Risk Aversion and Barriers to Firm Growth: Experimental Evidence from Small Retailers[*]

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Abstract

Firms in low and middle-income economies often grow slowly. This paper examines whether firm risk aversion prevents risk taking necessary to grow. While economists tend to model firms as risk neutral, I posit that this assumption is unlikely to hold for the modal developing country firm, which is owner-operated, so that uncertain investments may directly threaten owners' consumption. I develop a model that shows how risk aversion can reduce firms' willingness to experiment with new technologies, impeding investment and growth. I test the model's predictions within the context of retail firms' decision to adopt and sell a new consumer product using two field experiments with over 1,200 Kenyan firms. First, offering firms an insurance contract that creates a mean-preserving contraction of profits increases new product adoption by 50%. Effects are concentrated among firms whose owners exhibit higher levels of risk aversion. Second, temporarily inducing firms to try selling a new product with a supplier returns policy leads to a 70% increase in stock purchases after the intervention ends. Third, consistent with bandit models of learning, experimentally increasing the continuation value of learning increases adoption by 80%. These persistent effects arise through a reduction in the variance of beliefs, rather than through changes in mean beliefs. These results show that a feature inherent to developing country environments – small firms in the presence of missing financial markets – itself creates a barrier to firm innovation and growth.

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1 Introduction

Firms in low and middle-income (LMIC) economies often grow slowly and have low productivity (Bloom et al., 2010). Most of the existing literature on firms, even small and poor, assumes risk neutrality, meaning only expected returns should drive decisions. Yet most production in developing economies occurs among small enterprises where owners bear a significant share of profits and losses. In the absence of complete insurance markets, owners' consumption may be sensitive to business performance, causing their risk preferences to affect production decisions. This may deter enterprises from engaging in risk taking required to grow.

This paper investigates whether small firms are risk averse, causing them to forego profitable but risky investments. The returns to new business practices, such as employing a new technology or stocking a new product, are inherently uncertain. If firms are risk averse, they may fail to engage in experimentation to identify profitable opportunities, limiting innovation and growth.

I explore this question in the context of retail firms' decision to stock a new product. Offering new goods can increase retailers' profits. But shops are often uncertain about whether consumers will demand new products, creating a risk that inventory investments will lead to losses.

My empirical setting is the Kenyan market for new motorcycle helmets. In 2020, a corporation built a USD $3 million Kenyan factory and began producing one of the first effective helmets affordable to consumers, selling at a wholesale price of USD PPP 15 (high-quality imports cost $100 or more). But their retail diffusion was slow. Two years after their introduction, over 50% of shops near the factory believed that selling helmets would yield positive expected profits, yet only 6% stocked them. Although many firms held optimistic expectations, their beliefs were often diffuse, and retailers did not stock the good if they were unsure about its profitability.

I begin by developing a model that draws stark contrasts between the experimentation and learning behavior of risk neutral vs. risk averse firms when faced with an uncertain investment. In the model, an entrepreneur decides whether to stock a new good with unknown demand. This embeds a bandit learning problem into a standard model of small firm behavior, generating predictions identifying risk aversion and demand uncertainty. The core insight is that when firms are more uncertain about a product's profitability, risk neutral firms invest more because upside is high. This ensures that risk neutral retailers quickly discover profitable new products. In contrast, risk aversion creates disutility from uncertainty, which can lead firms to invest less in products the more uncertain they are about their profitability, undermining new product adoption.

Guided by the model, I design two field experiments to test if risk aversion prevents enterprises from efficiently adopting new products. The first experiment, the Insurance Experiment, constructs a precise test for whether risk aversion affects firms' stocking decisions. The second experiment, the Learning Experiment, isolates the importance of risky experimentation to learn in an uncertain environment. This experiment shows that temporarily lowering the risk of experimenting leads to permanent increases in helmet stocking. I leverage a treatment arm and model predictions to argue that persistence is driven by risk averse firms overcoming uncertainty and cannot be explained by confounding explanations such as incorrect beliefs or learning by doing.

The Insurance Experiment ($N = 350$ firms) tests if risk aversion affects firms' stocking decisions. I offered shops the opportunity to stock helmets with or without an insurance contract designed to be strictly dominated under risk neutrality but valuable under risk aversion. I test if enterprises are risk neutral by examining the effect of this contract on stocking.

Testing firms' risk preferences is complicated by possible capital constraints, since insurance contracts typically charge upfront premiums, affecting liquidity. To isolate risk aversion, my design offered control shops an unconditional future payment. Randomly selected *insurance* shops chose between this payment and a contract paying more if they stocked helmets and failed to sell out, but nothing otherwise. Offers were calibrated so that, given firms' beliefs, the contract's expected value fell below the fixed payment, creating a mean-preserving (technically decreasing) contraction of helmet profits. Therefore, *insurance* should increase stocking only if firms are risk averse.

The rate of *insurance* shops that immediately stocked helmets was 7 percentage points (157%, $p = .01$) higher. Firms were given up to two weeks to order to search for customers. The *insurance* effect was 10pp (50%, $p = .02$) after this period. This implies a rejection of risk neutrality. The results are not driven only by new or unproductive firms: effects remained large and significant among firms in business for at least 4 years, with employees, and with above median profits.

The design is robust to two additional confounds. First, surveyors followed a script to inform all shops about *insurance* and to explain that the shops receiving the offer would be selected at random. Then the shop's treatment assignment was revealed. This prevents *insurance* from sending a signal about demand. Second, the contract included conditions ensuring that stocking helmets and intentionally failing to sell out to obtain the *insurance* payout was never optimal.

The Learning Experiment ($N = 929$ firms) tests if risk aversion prevents firms from engaging in experimentation needed to discover profitable new products. The design builds on the model's prediction that firms face a risk-reward trade-off: by stocking a new product, they risk short-term losses but may gain long-term profits by restocking the product if they learn that it is profitable. The sample consists of firms that could have acquired helmets from the manufacturer for over two years but did not stock them. Two treatments test whether risk aversion prevented firms from learning about helmets' profitability in this setting where their background diffusion was slow.

The first *returns* treatment was designed to test if temporarily lowering the risk of selling helmets caused firms to learn about demand and permanently stock them. All firms received access to helmet stock at market prices. The intervention had two phases. In phase one, *returns* shops could return unsold stock for a refund whereas control shops could not. This mirrors common policies in high-income settings (Li and Kim, 2022). In phase two, all firms were given the option to buy new stock without a return option, so that offers were identical for *returns* and control shops. The key test is whether the one-time reduction in risk led to increases in phase two stock purchases.

Consistent with the Insurance Experiment, the temporary reduction in inventory risk led to a large increase in helmet uptake in phase one: 16.4% of *returns* shops stocked helmets compared to 6.8% of control firms (241%, $p < .01$). Reflecting firms' baseline expectations that the product would be profitable, few firms exercised the return option and 90% of shops directly reported that they were profitable, with magnitudes amounting to 10% of total firm profits.

These initial uptake differences translated into persistently higher rates of helmet stocking in phase two, when all firms faced the same stocking conditions. *Returns* shops were 7 percentage points (70%, $p = .03$) more likely to purchase helmet stock (from any supplier) and twice as likely to purchase inventory at least twice. This was driven by high restocking rates consistent with reported profitability: $2/3$ of phase one adopters restocked and 80% planned to stay in the market. The increase in phase two stocking is consistent with *returns* inducing risk averse firms to try selling helmets, who then resolved demand uncertainty by learning.

A confounding explanation is that shops held incorrect beliefs that helmets were unprofitable, and the intervention positively updated their expectations. The model predicts divergent patterns in firms' beliefs under these two mechanisms, which I test using novel firm survey data.

If returns mitigate risk aversion, (1) belief variance should negatively predict uptake among untreated firms (stocking is perceived as risky), (2) *returns* should induce stocking among firms uncertain about demand, and (3) beliefs should become more precise with experience. Under the alternative of changing mean beliefs, (1) uncertainty should positively predict uptake among untreated firms (information is more valuable), (2) the treatment should attract firms with pessimistic priors, and (3) beliefs should become more optimistic with experience. The empirical evidence supports the importance of risk aversion: all three predictions associated with this mechanism hold, versus none under the alternative of positively updating expectations.

These results highlight the importance of firm learning under uncertainty. When experimentation is risky but can generate long-run gains, a firm's willingness to experiment depends not only on risk but also on the perceived value of future opportunities. This implies an additional prediction of the model: firms should be more willing to undertake experimentation in the present when they anticipate larger payoff potential in the future, even if current-period payoffs are unchanged.

The Learning Experiment tests this hypothesis using a second *supplier commitment* treatment. Firms expressed concerns that the manufacturer may stop supplying small shops in the future, reducing future upside from discovering if helmets were profitable. I therefore offered, during phase one, to help *supplier commitment* shops restock directly from the supplier after the study. As in the Insurance Experiment, control shops were aware of this arm but informed that they would need to identify a supplier themselves. *Supplier commitment* firms were 6 percentage points (84%, $p = .03$) more likely to stock helmets during phase one, matching with the model's prediction.

This finding shows that firms are sophisticated and forward-looking. This suggests that low product experimentation absent *returns* does not simply reflect firms failing to internalize the learning value of stocking. Rather, the results are consistent with risk aversion creating high utility costs that deter experimentation, constraining firm innovation and productivity growth.

The model further predicts that *returns* and the *supplier commitment* should be substitutes, helping rule out mechanisms that could confound this interpretation. If *returns* effectively eliminate risk, then all firms that believe helmets may be profitable should stock. Therefore uptake should not be affected by also receiving the *supplier commitment*. A subset of firms received both offers to test this prediction. Results align with the model: the *supplier commitment* has no effect conditional on receiving *returns*. This helps to rule out confounding forces, such as learning-by-doing, that would typically cause the *supplier commitment* to affect uptake even with *returns*.

Another implication of demand uncertainty as the key source of risk constraining helmet adoption is that information acquired from other sources should also affect stocking. I document evidence of information spillovers across firms consistent with this view. These externalities also help to explain why competition does not drive risk averse firms out of business, suggesting that it may be possible for shops to free ride off the product experimentation of less risk averse competitors.

Three results highlight spillovers. First, *returns* had no effect when firms were located near an existing seller. In such cases, uptake was common in all arms, consistent with learning from incumbent sellers. Second, providing a random group of firms with peers' helmet sales data increased stocking by 2 percentage points ($p = .02$). Third, I induced helmet sellers to enter selected treated markets and later offered a new *spillover* sample helmet stock in treated and control markets. Firms in treated markets were nearly twice as likely to purchase helmet stock ($p < .01$).

Finally, the expansion of the helmet market produced by the Learning Experiment was economically important. *Returns* shops stocked twice the number of helmets during the study (3.4 vs 1.5, $p = .03$), and five times the volume in markets with no pre-existing seller (3.5 vs 0.7, $p < .01$). Moreover, *returns* enterprises remained 8 percentage points ($p = .04$) more likely to sell helmets or report a nearby vendor at endline, indicating persistent increases in market access. The magnitude of these effects two years after the helmet factory was built is reconciled with externalities by the fact that spillovers are highly localized: survey data shows shops rarely observed helmet stocking among firms more than 0.25 km from them (about 3 blocks), limiting information diffusion.

This study's principal contribution is to the literature studying barriers to firm growth in developing countries. Prior work has studied how inputs such as capital (de Mel et al., 2008), labor (de Mel et al., 2019), and management (Bloom et al., 2013) affect productivity. This paper provides the first direct evidence that risk aversion can prevent firms from engaging in entrepreneurial risk taking that is essential to growth, lowering uptake of a profitable product. I also introduce a new test of risk aversion that addresses key confounding factors and designed survey data to distinguish from competing models. This builds on prior studies examining correlations between risk preferences and firm outcomes (de Mel et al., 2008; Kremer et al., 2013; Meki, 2025), evidence that farmers are risk averse (Karlan et al., 2014), and observations that risk preferences may rationalize features of small enterprise behavior (Pelnik, 2024).

This finding implies that the common practice of modeling small firms as risk neutral may not be appropriate. Risk neutrality plays an important role in constructing empirical tests and interpreting findings in a vast range of topics within development economics, including capital constraints, collusion, technology adoption, and misallocation (Fafchamps et al., 2014; Bergquist and Dinerstein, 2020; Bassi et al., 2022; Buera et al., 2011). However, if risk aversion means that firm behavior cannot be interpreted solely through the lens of profit maximization, the interpretation of tests that are standard across these varied topics may change. For instance, the marginal product of capital may vary across firms due to heterogeneous risk preferences, not only borrowing access.

The results also show that risk aversion and demand uncertainty can slow the diffusion of new goods. This is important to economic development, as the slow diffusion of products and technologies is widely recognized as an important constraint to economic growth (Comin and Mestieri, 2018). Slow retail diffusion may also reduce manufacturers' incentives to introduce new products in developing markets. These findings build on research on barriers to technology diffusion and firm upgrading in developing economies (Atkin et al., 2017; Verhoogen, 2021), extending results to retail settings. More broadly, the finding that a temporary return intervention permanently boosts product adoption suggests that such policies could be a cost effective way to promote diffusion.

A further contribution of this study is to a literature on learning about demand. The model of small firm learning developed in this paper expands on a theoretical literature examining demand uncertainty (Rothschild, 1974; Bolton and Harris, 1999) and on empirical studies from other contexts (Foster and Rosenzweig, 1995; Doraszelski et al., 2018). The model highlights that risk neutral firms should hold accurate beliefs about demand in equilibrium because uncertainty incentivizes experimentation with goods. However, I show that risk aversion can prevent shops from experimenting, rationalizing evidence that firms in developing countries can hold inaccurate beliefs about the profitability of products (Bai et al., 2025). I also show evidence that firms internalize the learning value of investments, empirically validating a key prediction of bandit models of learning.

2 A model of small firm learning about demand

In this section, I model an entrepreneur faced with the decision of whether or not to stock a new product. The model embeds the problem of learning about demand for a new product into the optimization problem of a small firm owner. I begin by describing the problem faced by the agent, then derive equilibrium conditions. The section concludes by constructing tests of risk aversion and learning about demand that guide the design and interpretation of the experiments.

2.1 Model setup

I consider an infinitely repeated, single agent dynamic optimization problem in discrete time. The model is single agent because the hypotheses studied are not strategic. The problem is dynamic, as learning enables agents to refine their future decisions.

The entrepreneur's problem: The entrepreneur chooses how much of a safe product $j = s$ and a new product $j = n$ to stock in each period t. Inventory is sold in period $t+1$. The agent knows the residual demand curve of the safe good $p_s(q_{st}, \nu_{st})$, but realized demand is subject to iid stochastic fluctuations $\nu_{st} \sim \mathcal{N}(0, \Sigma_s)$.

Demand for the new product, $p_n(q_{nt}, \nu_{nt} + \theta)$, is indexed by a parameter $\theta \in \mathbb{R}^k$. The true value, θ_0, is unknown to the agent. $\nu_{nt} \sim \mathcal{N}(0, \Sigma_n)$ again captures iid fluctuations in demand. These influence the rate at which agents learn and allow for a distinction between two sources of uncertainty: diffuse prior beliefs, which can be resolved through learning, and stochastic demand fluctuations, which introduce variance in profits even when the demand curve is fully known.

The demand curves are continuously differentiable, downward sloping $\left(\frac{\partial p_j}{\partial q_{jt}} \leq 0 \; \forall q_{jt}, \nu_{jt}\right)$ and continuously differentiable and increasing in ν_{jt}. p_{jt} and q_{jt} are observed, but ν_{jt} is not observable.

Flow profits from the safe good, conditional on stocking q_{st}, are given by $\pi_{st} = q_{st} p_j(q_{st}, \nu_{st}) - \zeta_s(q_{st})$ where $\zeta_s(\cdot)$ is a known and differentiable function capturing non-stock costs of selling q_{st}, such as labor and capital used for marketing. Flow profits from the new product are determined by $\pi_{nt} = q_{nt} p_j(q_{nt}, \nu_{nt}; \theta) - \zeta_n(q_{nt})$ where $\zeta_n(\cdot)$ is also known and differentiable. Agents may invest in any non-negative stock of the safe product each period, $I_{st} \geq 0$ at wholesale cost w_s per unit. Wholesalers impose a minimum order size χ for the new product, so $I_{nt} \in \{0\} \cup [\chi, \infty)$.[1] The new product has a wholesale price of w_n. Since inventory is sold each period, $q_{jt} = I_{jt-1}$.

In addition to demand uncertainty, agents face supply chain uncertainty when stocking the new product. This captures concerns that unfamiliar manufacturers may fail or shift focus, cutting small firms off from future supply. To capture this, the model includes a fixed continuation cost Γ that

[1] This condition captures realistic features of the market and allows for an equilibrium in which the agent does not fully learn demand. Imposing fixed delivery costs without a minimum order size yields similar results.

the agent expects to incur in period $t_c > 1$ if they wish to keep stocking the new product. This provides a way to study how agents respond to changes in the continuation value of learning.

Learning about demand: A core feature of the model is that the agent learns about new product demand. They begin with a prior $\theta \sim \mathcal{N}(\mu_1, \Sigma_1)$ and Bayesian update when their information set, $\mathcal{I}_t(I_{n0}, \ldots, I_{nt-1})$, changes. If the agent stocks I_{nt}, then in period $t+1$ they receive a signal $x(I_{nt}) \sim \mathcal{N}\left(\theta_0, I_{nt}^{-1}\Sigma_x + \Sigma_n\right)$. The agent knows that the signal is centered around the truth and knows its precision, but does not know θ_0. The precision of information about demand the agent receives is increasing in their level of investment, reflecting the fact that a greater stock provides more opportunities for customer interactions, price experimentation, and data about sales. However, the learning becomes more difficult if demand fluctuates substantially from period to period.

The information set depends on time t because agents may also learn from external sources, such as neighboring retailers. Each period the retailer receives a signal $x_{ot} \sim \mathcal{N}(\theta_0, \Sigma_o)$ with probability φ where Σ_o (o for "other source") is known. Beliefs update according to Bayes' Rule.

$$\begin{aligned}
&\theta_t \sim \mathcal{N}(\mu_t, \Sigma_t) \\
&\mu_t = \Sigma_t \left(\Sigma_1^{-1}\mu_1 + \sum_{\tau=1}^{t-1} (I_{n\tau}^{-1}\Sigma_x + \Sigma_n)^{-1} x(I_{n\tau}) + \sum_{x_{ot} \in \mathcal{I}_t} \Sigma_o^{-1} x_{ot} \right) \\
&\Sigma_t = \left(\Sigma_1^{-1} + \sum_{\tau=1}^{t-1} (I_{n\tau}^{-1}\Sigma_x + \Sigma_n)^{-1} + |x_{ot} \in \mathcal{I}_t| \Sigma_o^{-1} \right)^{-1}
\end{aligned} \tag{1}$$

The agent's objective: The entrepreneur receives flow utility of consumption given by a continuously differentiable function $u(\cdot)$. They may save or borrow at interest rate r and discount the future at rate $\delta = \frac{1}{1+r}$ and are subject to borrowing limit $\underline{a} \leq 0$, capturing possible capital constraints. The agent begins with assets $a_0 > 0$ but no stock. Their objective is

$$\max_{\{c_t, a_t, I_{st}, I_{nt}\}} \mathbb{E}_0 \sum_{t=1}^{\infty} \delta^{t-1} u(c_t) \tag{2}$$

subject to a budget constraint $a_t + c_t + w_s I_{st} + w_n I_{nt} \leq (1+r)a_{t-1} + \pi_s(I_{st-1}, \nu_{st}) + \pi_n(I_{nt-1}, \nu_{nt} + \theta_0)$, minimum order sizes of the new product $I_{nt} = 0$ or $I_{nt} \geq \chi$, the borrowing limit $a_t \geq \underline{a}$, non-negative investment $I_{st} \geq 0$, and a transversality condition. Expectations are over θ, due to incomplete information, and ν_{jt}, due to stochastic variation in demand.

The value of learning affects an agent's new product stocking because investment facilitates learning, which allows better future optimization. This enters their objective as reductions in future "regret," lost utility due to incomplete knowledge of θ. Let $y_t = (1+r)a_{t-1} + \pi_s(I_{st-1}, \nu_{st}) + \pi_n(I_{nt-1}, \nu_{nt} + \theta_0)$ be the agent's cash on hand. Define the conditional value function

$$V^*(y_t, \Gamma, \theta) = \max_{\{c_\tau, a_\tau, I_{s\tau}, I_{n\tau}\}} \sum_{\tau=t+1}^{\infty} \delta^\tau \, \mathbb{E}[u(c_{t+\tau})|\theta] \tag{3}$$

subject to the some conditions as Equation 2, but treating θ as known. Let $\bar{c}_t$ denote consumption along the path that solves the original objective, maximizing over beliefs about θ instead of treating it as known. Define

$$V(y_t, \mathcal{I}_t, \Gamma, \theta) = \sum_{\tau=1}^{\infty} \delta^\tau \, \mathbb{E}[u(\bar{c}_{t+\tau})|\theta] \tag{4}$$

This is the expected utility the agent will receive from their planned actions if $\theta_0 = \theta$.

Regret is given by $R(y_t, \mathcal{I}_t, \Gamma, \theta) \equiv V^*(y_t, \theta, \Gamma) - V(y_t, \mathcal{I}_t, \theta, \Gamma) \geq 0$. This captures lost utility due to incomplete information about demand if $\theta_0 = \theta$. An agent's Bayesian regret is

$$\bar{R}(y_t, \mathcal{I}_t, \Gamma) \equiv \mathbb{E}_\theta \left[R(y_t, \mathcal{I}_t, \Gamma, \theta)|\mathcal{I}_t\right] \tag{5}$$

Which is the lifetime utility that the agent expects to lose because of uncertainty about θ.

2.2 Model solution

The solution to the model is derived in appendix A.1. I present and interpret the results for optimal investment in this section.

Optimal investment in the safe good: The utility maximizing level of investment in the safe good, I^*_{st}, satisfies

$$\begin{aligned} \delta \Bigg\{ & \mathbb{E}_t \left[\frac{u'(c_{t+1})}{u'(c_t)}\right] \mathbb{E}_t \left[\frac{\partial}{\partial I_{st}} \pi_s(I^*_{st}, \nu_{st+1})\right] \\ & + \frac{1}{u'(c_t)} Cov_t \left(u'(c_{t+1}), \frac{\partial \pi_s(I^*_{st}, \nu_{st+1})}{\partial I_{st}}\right)\Bigg\} = w_s - \frac{1}{u'(c_t)} \iota_{st} \end{aligned} \tag{6}$$

where $\mathbb{E}_t[\cdot] = \mathbb{E}[\cdot|\mathcal{I}_t]$ and $Cov_t(\cdot) = Cov(\cdot|\mathcal{I}_t)$. $\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right]$ will equal 1 when capital constraints do not bind and captures the fact that investment will fall when agents hit their borrowing limit, in which case $u'(c_t)$ will be greater than $\mathbb{E}_t[u'(c_{t+1})]$ so the shadow cost of investment increases. ι_{st} is a Lagrangian multiplier ensuring non-negative investment.

$\frac{1}{u'(c_t)} Cov_t \left(u'(c_{t+1}), \frac{\partial \pi_s(I^*_{st}, \nu_{st+1})}{\partial I_{st}} \right)$ captures possible risk aversion and will be zero if agents are risk neutral ($u'(\cdot)$ is then constant). Low profits reduce consumption (if insurance markets are incomplete). If $u(\cdot)$ is concave, this covariance will be negative and increasing in magnitude with the variance of profits and the agent's risk aversion, leading to inefficiently low stocking levels.

Optimal investment in the new good: I^*_{nt} is defined by the Euler equation

$$\delta \left\{ \underbrace{\mathbb{E}_t \left[\frac{u'(c_{t+1})}{u'(c_t)} \right]}_{\text{Capital constraints}} \underbrace{\mathbb{E}_t \left[\frac{\partial}{\partial I_{nt}} \pi_n(I^*_{nt}, \nu_{nt+1} + \theta) \right]}_{\text{Expected marginal profits}} + \underbrace{\frac{1}{u'(c_t)} Cov_t \left(u'(c_{t+1}), \frac{\partial \pi_n(I^*_{nt}, \nu_{nt+1} + \theta)}{\partial I_{nt}} \right)}_{\text{Risk aversion}} \right.$$
$$\left. - \underbrace{\frac{1}{u'(c_t)} \delta\, \mathbb{E}_t \left[\frac{\partial \bar{R}(y_{t+1}, \mathcal{I}_{t+1}, \Gamma)}{\partial I^*_{nt}} \right]}_{\text{Marginal learning value}} \right\} = \underbrace{w_n - \frac{1}{u'(c_t)} \kappa_{\chi t}(2I_{nt} - \chi)}_{\text{Marginal investment costs}} \tag{7}$$

given that the utility of I^*_{nt} satisfies the participation constraint that it exceeds that of not stocking n. Since a change in primitives which increases I^*_{nt} also relaxes the participation constraint, I focus on Equation 7 to study extensive and intensive margin decisions.

There are two important differences from equation 6. First, expected profits and the covariance between profits and marginal utility depend on θ, and beliefs about this parameter change with information. For instance, if the agent receives a signal centered on their prior, expected profits will be unchanged but a risk averse agent will perceive less stocking risk, reducing this term in magnitude. Thus learning can overcome demand uncertainty.

Second, learning has value because it reduces regret. Let $V_{nt} = I_{nt}^{-1} \Sigma_x + \Sigma_n$ be the variance of the signal the agent receives. Appendix A.2 shows

$$\mathbb{E}_t \left[\frac{\partial \bar{R}(y_{t+1}, \mathcal{I}_{t+1}, \Gamma)}{\partial I^*_{nt}} \right] = -\frac{1}{2} Cov_t \left(\underbrace{R(y_{t+1}, \mathcal{I}_{t+1}, \Gamma, \theta)}_{\text{Sensitivity of utility to } \theta}, \underbrace{(\theta - \mu_t)' I_{nt}^{-2} V_{nt}^{-1} \Sigma_x V_{nt}^{-1} (\theta - \mu_t)}_{\text{Reduction in uncertainty from investment}} \right) \leq 0 \tag{8}$$

In words, the increase in future utility one obtains from stocking more of the new product is a function of how strongly utility varies with θ and the sensitivity of learning to investment. Agents will stock past the point where the immediate payoff is 0 because investment allows them to better optimize in the future. This is the "risk-reward" trade-off typical of bandit models.

For instance, consider a risk neutral agent that expects new product profits to be barely negative, is uncertain about demand, and can immediately learn demand by stocking. Then $I^*_{nt} > 0$ because the firm can exit the market if the product is not profitable but restock if it is. The present value of possible restocking profits will exceed the one period expected losses, the cost of experimenting.

A core insight of this model is that the value of learning is increasing in uncertainty about demand, $|\Sigma_t|$, because high uncertainty implies a probability that the product is very profitable. As a result, a risk neutral firm's willingness to stock a new product is increasing in demand uncertainty, ensuring that profitable goods are discovered. However, for risk averse firms, the potential downside associated with uncertain payoffs creates disutility, constraining experimentation. If risk aversion is sufficiently high, this relationship may reverse: greater uncertainty can prevent firms from investing. Thus, deviations from risk neutrality can fundamentally impede product diffusion.

Two additional insights inform the predictions used to test the model. First, supply chain uncertainty, Γ, lowers the continuation value of learning without affecting short-run returns, so it may be used to test the "risk-reward" trade-off. Beginning in period t_c, the agent would only stock $I^*_{nt} > 0$ if the present value of expected lifetime utility gains from stocking it exceeded Γ. An increase in Γ thus reduces $\mathbb{E}_t\left[\frac{\partial \bar{R}(y_{t+1}, \mathcal{I}_{t+1}, \Gamma)}{\partial I^*_{nt}}\right]$ in magnitude. But profits are unaffected for $t < t_c$.

Second, indirect tests of risk aversion used in past work – such as examining heterogeneous returns to capital or portfolio choices – are sensitive to modeling assumptions. For instance, de Mel et al. (2008) model an enterprise stocking a single good with uncertain demand that cannot be overcome by learning. Given this structure, risk averse firms are further from the efficient stocking level and have higher returns to capital. However, this test is ambiguous when firms stock multiple products: more risk averse agents may invest funds in safer products with lower expected returns. This paper instead focuses on predictions relating to the response of new product stocking to changes in risk, which yields robust tests of risk neutrality.

2.3 Comparative static predictions

I next derive testable predictions of the model. I first derive three propositions that examine whether risk aversion and demand uncertainty consequentially affect new product adoption. A fourth proposition then considers information externalities.[2]

Definition 1 (Mean-preserving contraction) *A mean-preserving contraction of the profit function at the minimum order size, χ, is a one-period perturbation, $\pi_n^p(\cdot)$, such that*

1. $\mathbb{E}_t\left[\pi_n^p(\chi, \nu_{nt+1} + \theta)\right] = \mathbb{E}_t\left[\pi_n(\chi, \nu_{nt+1} + \theta)\right]$

2. $\int_{-\infty}^{x} F_p(\chi, y)dy \leq \int_{-\infty}^{x} F(\chi, y)dy \quad \forall x$*, and strictly for some* x. $F(I_{nt}, y)$ *denotes the probability that* $\pi_n(I_{nt}, \nu_{nt+1} + \theta) \leq y$*, and* F_p *is the same for the perturbed profit function.*

3. $\pi_n^p(I_{nt}, \nu_{nt+1} + \theta) = \pi_n(I_{nt}, \nu_{nt+1} + \theta) \quad \forall I_{nt} \neq \chi$

Expected profits are unaffected by a mean-preserving contraction, but the distribution is less spread out.[3] Since expected profits are unchanged, this will not affect a risk neutral firm's decision. However, a risk averse agent's expected utility of stocking will increase.

Proposition 1 (Risk aversion) *A mean-preserving contraction leads a firm to stock* $I_{nt}^* > 0$ *that otherwise would not if and only if they are risk averse.*

This provides an unambiguous test for firm risk aversion that is not affected by capital constraints. This result is sensitive to the fact that the contraction lasts for one period, which ensures that it does not reduce the continuation value of learning.

I next derive tests of learning about new product demand by analyzing the model's predictions about two policies: (i) a one-time return offer, and (ii) a reduction in supply chain uncertainty. A return policy guarantees that the firm receives at least the good's stocking cost ($p_{nt+1} \geq w_n$), reducing downside risk and increasing the expected marginal return from stocking under uncertainty. A reduction in supply chain uncertainty, Γ, increases the continuation value of learning. Therefore both policies should increase new product investment if demand uncertainty binds.

A sharper implication arises from the interaction between the return offer and the reduction in Γ, which helps distinguish targeted responses from alternative explanations such as learning-by-doing, fixed costs, or other omitted dynamics. If the return policy fully insures the firm against

[2]I focus on intuition for the propositions and their implications in the text. Proofs are presented in Appendix A.3.

[3]I focus on a mean-preserving contraction at the minimum order size to show that only modifying this portion of the profit function, as in the Insurance Experiment, is sufficient to identify risk aversion.

losses, then any agent who believes the product could be profitable will stock it. Under this condition, a reduction in Γ has no additional effect (at the extensive margin) if it operates through the targeted channel. In contrast, if supply chain frictions matter for reasons other than the continuation value of learning, then a change in Γ may still affect behavior when returns are guaranteed.

A final and more direct test of learning relates to the persistence of effects of a one-time return policy. If firms learn about demand, then receiving returns once should cause them to experiment and update beliefs, affecting their future stocking behavior. A risk neutral firm would stock less in subsequent periods unless expectations positively updated as the value of information is lower. By contrast, a sufficiently risk averse firm would increase stocking as learning reduces risk.

Proposition 2 (Learning about demand) *If firms face uncertain demand for a new product that they can overcome through learning*

a.) New product stocking increases when the firm is offered returns.

b.) Reducing future supply chain uncertainty, Γ*, increases stocking.*

c.) If $\delta \approx 1$*, non-stock costs of helmet sales are low, and capital constraints are not binding, then a reduction in supply chain uncertainty has no effect on Pr(*$I^*_{nt} > 0$*) if a firm has access to returns.*

d.) A one-time return offer will have persistent effects on new product stocking.

Suppose that a one-time returns policy has a persistent positive effect on stocking. This is consistent with risk aversion flipping the relationship between demand uncertainty and new product adoption, deterring firms without access to a form of insurance from stocking the new product when they are uncertain about demand. Under this explanation, returns should cause firms with uncertain priors to stock, who then overcome uncertainty about demand and stay in the market. An alternative explanation is that returns cause firms with incorrect and pessimistic expectations to try stocking helmets, who then correct their beliefs. The model predicts a different relationship between beliefs and product adoption under these mechanisms.

Proposition 3 (Learning mechanism) *Suppose that a one-time returns policy has a persistent positive effect on new product adoption.*

a.) If returns correct the pessimistic priors of risk neutral firms, each of the following should hold:

i.) Prior uncertainty about demand and uptake are positively correlated absent returns.

ii.) Returns cause firms with pessimistic priors to stock.

iii.) Experience selling the new product causes firms to positively update beliefs.

b.) If returns cause risk averse firms to stock and overcome demand uncertainty:

i.) Prior uncertainty about demand and uptake are negatively correlated absent returns.

ii.) Returns cause firms with uncertain priors to stock.

iii.) Experience reduces uncertainty about demand.

If learning is consequential to firms' investment decisions, then a natural question is whether they can learn from each other. Information externalities may contribute to risk averse firms remaining in the market if they can free-ride off of competitors to compensate for their own lack of risk taking. If information spills over, then receipt of a signal from neighbors should increase the propensity of a firm to stock if they are risk averse since uncertainty is reduced, and the effects of returns or a relaxation of supply chain frictions should be smaller. Formally,

Proposition 4 (Information externalities) *If firms can learn about demand from competitors*

a.) The effects of a returns offer or change in Γ are decreasing in φ.

b.) Exposure to a seller increases I^*_{nt} if agents are risk averse or have pessimistic priors.

c.) Receipt of a signal x_{ot} below an agent's expectation increases I^*_{nt} only if agents are risk averse.

3 Setting and design of the experiments

I test the model's predictions using two field experiments in Kenya. First, an Insurance Experiment induces a mean-preserving contraction to test if firms are risk averse. Second, a Learning Experiment traces out the longer-run trajectory of helmet adoption by temporarily alleviating risk using a realistic policy, then examining the persistence of effects.

Both experiments offer a motorcycle helmet introduced 2.5 years before the study began. Previously, helmets affordable to a typical Kenyan household offered minimal safety benefits. However, a motorcycle importer invested in a modern helmet factory in Kenya in 2020. Lower transportation and labor costs brought more effective helmets into the budget set of consumers.[4]

This market provides an ideal setting to study new product adoption because retail adoption of helmets was slow despite a large base of motorcycle users. A census showed that more than

[4]East Africa lacked testing labs at the time of the study, but the helmets used are considered high quality and were approved by the Kenyan Bureau of Standards. For details, see Strzyzynska, Weronika (2023). "Africa sees sharp rise in road traffic deaths as motorbike taxis boom." *The Guardian*.

two years after helmets were introduced, fewer than 3% of surveyed firms sold them. Yet motorcycle use is widespread: in 2022 an estimated 2.4 million motorcycles were operated each day, accounting for 22 million trips. Helmet use is limited, and the annual number of motorcycle deaths doubled between 2018 and 2021.[5]

3.1 The insurance experiment

Setting and timeline: The Insurance Experiment (November 2024 to April 2025) was conducted in 140 markets in West Kenya, spanning 9 counties. In each county, 15-16 large markets (identified by field officers) were selected. The experiment included a baseline survey followed by a follow-up 2 months later. A survey with a separate *spillover* sample was conducted after the follow-up.

The studied helmets are produced about 300 kilometers from this sample and were not widely available in West Kenya at the time of the experiment. Low-quality imports and alternative helmet types, such as construction helmets sometimes used by motorcycle riders, were more common. Approximately half of enterprises reported knowing of a nearby shop selling such products.

Recruitment and sample: The baseline survey included 70 randomly selected markets, stratified by county. The remaining 70 markets were pure controls visited only for the *spillover* survey.

In each baseline market, field officers identified 10 shops suitable for helmet sales, prioritizing motorcycle spare parts and repair shops. If fewer than 10 existed, other shops such as hardware stores were added. Eligibility required that shops did not currently sell helmets and had a fixed physical location. From the 10 eligible shops, 5 were randomly selected, yielding 350 firms.[6]

The focus on urban and semi-urban markets, plus the restriction that enterprises have a permanent location, resulted in a sample of relatively large and established firms. Median profits the month before the survey were about $365 (summary statistics are reported in Appendix Table A1).[7] The average shopkeeper was 34 years old with 13 years of education and about 30% were female.

More than half of the shops sold motorcycle spare parts. General shops and hardware stores were the next most common categories. The average business age exceeded 4 years, and $1/4$ of firms had an employee. Half of shop owners reported having no employees or other sources

[5] Fred Matiang'i, "The urgency of bodaboda reforms", *Nation.Africa*, 2022.

[6] One market was inadvertently visited in piloting and at baseline, resulting in some firms receiving a control and treatment offer. Analysis focuses on the 345 remaining firms, but results are similar including the full sample.

[7] Egger et al. (2022) report *annual* revenues of $240 among retail firms in rural Siaya County, Kenya.

of family income. These enterprises may be sensitive to profit risks since they are the residual claimant to all firm revenues, which fund all household consumption.

The 5 shops not surveyed at baseline were targeted for the *spillover* survey. The aim was to offer these shops plus identically sampled firms in the control markets a stock of helmets to test for information externalities (Proposition 4). However, only the name of shops was recorded at baseline to prevent priming, making it challenging to confidently re-identify shops.

As a result, field officers instead targeted the 5 best shops to sell helmets in pure control markets. In baseline markets, they recruited the 5 best shops not already surveyed (including skipped firms).[8] Typically, all motorcycle parts shops in each market were recruited, and assignment to the *insurance* or *spillover* sample was determined randomly in baseline markets. Consequently, the sample of motorcycle-related shops should be comparable across baseline and pure control markets within the *spillover* survey. Because fewer motorcycle shops were available to survey in baseline markets (since some had already been visited), firms in these markets are generally less suitable to sell helmets. Appendix Table A2 validates this, showing that *spillover* shops in baseline markets are less likely to sell motorcycle products, but shops are balanced across other dimensions.

Design and randomization: The Insurance Experiment was designed to test if risk aversion affects firms' stocking decisions (Proposition 1). The design randomly assigned firms to an *insurance* or control status, stratified by market. *Insurance* firms were offered helmet stock with a contract designed to induce a mean-preserving contraction of profits. Control firms were offered the same stock of helmets without this contract, allowing one to test if firms are risk averse by examining if *insurance* firms stock at a higher rate.

Both arms were offered a stock of 3 helmets at the end of the baseline survey and informed that they could purchase 3 or more helmets at a follow-up, unconditional of prior stocking. The order size (80% of median weekly profits) was set to the manufacturer's minimum. Shops paid full price but received free delivery. They were allowed two weeks to place an order, giving time to search for customers and raise capital.

Insurance shops were offered firm-specific contracts calibrated based on subjective beliefs about demand. To elicit beliefs, *insurance* and control firms were asked how many of 10 helmet-

[8] Some shops stocking helmets prior to the study, which were ineligible, reported adopting helmets after the study began to obtain subsidized stock. Back checks were conducted after the delivery of subsidized stock, so misreporting incentives were removed. Firms that disclosed eligibility violations in back checks are excluded from analysis.

stocking shops would sell out by follow-up, and whether their own shop would earn more or less revenue than the average firm. Respondents earned small rewards for accurate predictions. Field officers then reported the implied probability of selling out, and each shopkeeper stated their own belief about this probability, so_i.

Control firms were promised an unconditional payment (in Kenyan shillings) at follow-up of

$$P_i = \begin{cases} 1000 \cdot (1 - so_i) + 25, & so_i < 0.8 \\ 225, & so_i \geq 0.8 \end{cases}$$

Insurance firms could choose P_i or a contract paying 1,000 if they stocked helmets and failed to sell out by the follow-up, but 0 otherwise. This exceeds P_i when helmet demand is low, mitigating losses. But the expected payout is less than P_i. Appendix A.4 shows that setting $P_i' = 1000 \cdot (1 - so_i)$ would make insurance a mean-preserving contraction. Since $P_i > P_i'$, *insurance* is strictly dominated under risk neutrality. Thus, higher stocking rates under insurance imply risk aversion.

The design also addressed confounding explanations. First, *insurance* shops were given the choice between a future transfer or insurance, ruling out any treatment effect due to capital constraints (Casaburi and Willis, 2018). Second, both *insurance* and control firms were informed of the contract offer and its random assignment, so that receipt did not provide a signal about demand. Third, firms accepting the payout could not restock, eliminating incentives to intentionally forgo sales (and further lowering the expected insurance payout). Finally, a mystery shopper visited 1 in 5 firms, buying a helmet if available (or another item otherwise). Firms refusing to sell helmets but reporting unsold inventory were denied payouts and barred from restocking.

The primary outcome is an indicator equal to 1 if a firm ordered helmets within two weeks. A secondary outcome captures "immediate adoption" (orders within 24 hours). The first measures adoption overall; the second isolates behavior before firms could mitigate risk by finding customers. Surveys also collected firm demographics, demand beliefs, realized sales, and risk preferences via a lottery-choice game conducted at the follow-up (Strobl, 2022). This exercise was omitted from the baseline to ensure that payouts did not confound effects of *insurance*.

Markets had 2 or 3 *insurance* firms with equal probability. Within markets, simple random assignment allocated *insurance* to firms. Appendix Table A1 shows that balance across arms.

If no firm in a market ordered helmets, shops were unexpectedly approached again (in a random order) and offered a large discount, ensuring that each baseline market had a seller to test for information externalities using the *spillover* sample.

3.2 The learning experiment

Setting and timeline: The Learning Experiment (January-July 2024) was conducted in Nairobi, Kenya. A census identified eligible firms and collected data for randomization. Data collection included a baseline survey (February-March), a midline (May) and en endline (June-July). Chronologically, this exercise was conducted before the Insurance Experiment since the design relied on an environment where shops had prevailing access to stock.

A key feature of this setting is that the manufacturer is local, so helmets were available to firms for over two years. This allows one to test if varying exposure to risk changes stocking in a setting where product diffusion had been slow despite the producer's efforts to promote their adoption.

Recruitment and sample: The Learning Experiment included 929 retail firms. Shops with prior experience selling motorcycle helmets were excluded since the study focuses on new product adoption. Recruitment focused on areas where few existing helmet shops were present. Consequently, recruitment was low in the Central Business District, where firms are large. Shops without a permanent building were excluded. Finally, shops that were certain they would never stock helmets were omitted to approximate the population of firms a producer may target when seeking retailers.

Recruitment occurred via a census: surveyors visited shops, showed them a sample helmet, explained eligibility, and recorded information for randomization. Of 1,152 eligible firms listed, 950-1,000 were targeted. Shops were placed in either a primary or replacement pool. The primary pool deterministically included firms located far from existing sellers, with others randomly assigned. Surveyors first recruited from the primary pool, drawing replacements if firms declined. The final sample included 929 firms: 22 interested shops failed to complete the baseline, and five were dropped after being deemed ineligible. Treatments were introduced after baseline, ensuring participation was not influenced by assignment.

Appendix table A1 presents summary statistics. Firms in the Learning sample were larger than those in West Kenya, with average monthly profits over \$600, and a smaller share (37%) sold motorcycle parts. Respondent age, gender, and education levels were similar across samples, as were firms' likelihood of having an employee or adopting a new product in the prior year.

Returns treatment: The first arm was designed to test if temporarily reducing inventory risk led shops to permanently stock helmets (Proposition 2). This was carried out over two phases.

In phase one, all shops were offered a stock of helmets at prevailing market prices. *Returns* shops were given the option to return stock from their first order for a refund, whereas control shops could not. In phase two, both groups could restock without returns, making conditions identical. The key outcome is whether reduced risk in phase one raised stocking in phase two by resolving demand uncertainty. As in the Insurance Experiment, all firms were informed about the existence of treatments and their random assignment to avoid signaling.

Returns were used to reduce risk for two reasons. First, return policies are widespread in high-income settings (Padmanabhan and Png, 1995). This intervention therefore helps to address whether return markets are thin (less than 15% of *spillover* firms reported ever being offered returns).[9] Second, *returns* do not involve payouts that could affect phase two stocking.

During phase one, shops could order at least 5 helmets, half the manufacturer's minimum. This reduced liquidity needs and maintained goodwill among controls. Returns were permitted only once, at the midline, to target demand uncertainty rather than ongoing inventory risks. Phase two ran from midline to endline, with orders accepted anytime. Shortly after midline, the manufacturer unexpectedly cut its minimum order size to 3, and the study followed suit.

If a shop indicated intent to order, surveyors revealed an installment option: firms could pay for their first order in three weekly payments, regardless of treatment. This helped shops with limited liquidity while preserving time for learning about demand. To prevent contamination, surveyors disclosed this option only after a shop expressed intent to buy.[10] The default rate was under 3%.[11]

Supplier commitment treatment: The second treatment was designed to increase the continuation value of learning without affecting phase one helmet profitability or risks (Proposition 2).

I targeted uncertainty about future supply chain reliability to achieve this. Surveyors told *supplier commitment* shops that the study would help them restock directly with the manufacturer at

[9] Anecdotally, the helmet producer cited the fact that contracting costs, mistrust, and shipping make returns costly when selling to many small retailers as barriers to offering returns.

[10] In three cases, the option was revealed early; those firms were dropped.

[11] 91% of shops paid on time and 97% paid before the midline survey. Most shops that paid late received incorrect information about the repayment schedule and paid within 1-2 weeks of the intended time.

endline, ensuring continued access if helmets proved profitable. Controls had to establish this link themselves. This design reflected pilot evidence that most shops did not know how to reach the supplier and feared that small retailers would be de-prioritized if the supplier's focus shifted. The focus of this treatment is on phase one uptake.

A secondary information treatment: The endline survey included a randomized *information* treatment designed to test if firms learn from peers (Proposition 4). *Information* shops received data from 5 randomly selected phase one helmet adopters about how many helmets they sold, the prices that they charged, and whether they restocked. Using 5 shops generated randomly varying signals: average sales were 3.8 helmets with a standard deviation of 1.2. This relied on trust built over prior surveys, making it a stylized test of information value and not a market intervention.

Randomization: Stratified random assignment was used to allocate the *returns* and *supplier commitment* treatments. I followed a 2x2 randomization design so that 1/4 of shops received *both* treatments to test predictions about the interaction between the offers (Proposition 2). Randomization was stratified on neighborhood (for power), an indicator for whether the enterprise reported having any employees during the census (for power), and distance to the nearest existing helmet seller (for heterogeneity analysis). Appendix Table A1 verifies balance across arms.

The *information* treatment was delivered unexpectedly to half of shops that had not stocked helmets by the endline. Randomization was stratified on knowledge of a nearby helmet seller.

3.3 Primary variables collected

The baseline survey captured information about the demographics of the shopkeeper and the enterprise's costs, revenues, and profits. Field officers also elicited beliefs about helmet demand using a frequentist approach (Benjamin et al., 2017). Respondents first stated the price at which they would sell helmets, then were asked to imagine stocking 10 units. They received 20 beans and a sheet labeled 0–10, and were instructed to place beans across boxes to represent the probability of selling each quantity over a month.[12] From this distribution, both the expected value and the variance of beliefs can be calculated.

Reported expectations strongly aligned with a separate direct question about anticipated

[12] Field officers often entered totals that did not sum to 20 on the first day of data collection. Surveyors were retrained and the variable is set to missing for observations from day one.

monthly sales ($t > 18, R^2 = 0.49$). Most respondents also provided plausible full distributions. A minority placed all beans on their expectation (or nearly all), yielding degenerate distributions that likely reflect measurement error. Results are stronger when excluding these cases, but they are retained in main specifications to be conservative. Measures of variance also face a censoring issue for very high expectations since boxes do not exceed 10. I elicited responses up to twice the *learning experiment* order size of 5 so that observations where censoring binds are unlikely to be affected by the *returns* arm.

The midline and endline surveys recorded helmet stocking from both the study and other suppliers. For adopters, they captured helmet sales, costs, revenues, and profits, as well as stocking intentions. All firms provided updated information on overall revenues, costs, and profits, plus new data on product offerings. Beliefs about helmet demand were re-elicited at each round.

Analysis focuses on phase one and phase two stocking decisions and on beliefs about helmet profitability. Helmet profits are also examined, but given that only a minority of firms stocked, the experiment was not powered to detect profit effects. For this reason, repeat stocking serves as the primary revealed-preference measure of profitability (this focus was pre-registered).

4 Empirical specifications

I examine the effects of the Insurance Experiment by estimating the regression

$$Stocked_i = \alpha + \beta Insurance_i + X_i'\gamma + \mu_m + \epsilon_i \tag{9}$$

$Stocked_i$ indicates that shop i stocked helmets, $Insurance_i$ denotes receipt of the *insurance* offer, X_i denotes controls, and μ_m market fixed effects. Proposition 1 predicts $\beta > 0$, stocking increases in response to the expected profit and variance lowering contract, only if firms are risk averse.

Turning to the Learning Experiment, I test if agents face demand uncertainty by estimating

$$Stocked_i = \alpha + \beta_1 R_i + \beta_2 S_i + \beta_3 R_i \times S_i + X_i'\gamma + \mu_k + \epsilon_i \tag{10}$$

$Stocked_i$ denotes phase one stocking, R_i indicates receipt of *returns*, S_i captures receipt of the *supplier commitment*, and μ_k is strata fixed effects. The interaction captures the model's prediction that the offers are substitutes. Proposition 2 yields four predictions: (a) $\beta_1 > 0$, since returns reduce loss risk; (b) $\beta_2 > 0$, since supplier commitment increases the continuation value of learning; and

(c) $\beta_3 < 0$, since supplier commitment should have little additional effect when returns already eliminate risk. Part (d) predicts that *returns* will have persistent effects, reflecting direct evidence of learning. This is tested by replacing the outcome with phase two stocking and testing if $\beta_1 = 0$.

If returns have a positive effect on phase two stocking, Proposition 3 provides tests to differentiate between a case where *returns* overcome risk aversion versus incorrect beliefs. This is tested with three equations. First, I estimate

$$\begin{aligned} Stocked_i = \alpha + \beta_1 \mathbb{E}[Sales]_i + \beta_2 SD[Sales]_i + \beta_3 R_i \\ + \beta_4 R_i \times \mathbb{E}[Sales]_i + \beta_5 R_i \times SD[Sales]_i + X_i'\gamma + \epsilon_i \end{aligned} \tag{11}$$

$\mathbb{E}[Sales]_i$ is the agent's prior expectation about the number of helmet sales in the next month and $SD[Sales]_i$ is the standard deviation of their belief. Under the targeted mechanism (risk aversion), Proposition 3 predicts that $\beta_2 < 0$, meaning control firms with more uncertain beliefs are less likely to stock, versus $\beta_2 > 0$ if firms are risk neutral, reflecting the value of learning.[13]

Second, I examine the composition of adopters by estimating

$$Returns_i = \alpha + \beta_1 \mathbb{E}[Sales_i] + \beta_2 SD[Sales]_i + X_i'\gamma + \epsilon_i \tag{12}$$

among phase one adopters not receiving the *supplier commitment*. β_1 captures whether sales expectations differ among *returns* versus control adopters. Under risk neutrality, $\beta_1 < 0$, since returns should draw in firms with pessimistic priors. If firms are risk averse, β_1 need not be negative. Instead, $\beta_2 > 0$ would indicate that returns primarily induce firms with uncertain beliefs to adopt.

To examine how stocking affected beliefs, I estimate the two-stage least squares system

$$\Delta \log(1 + \mathbb{E}[Sales]_i) = \alpha_e + \beta_1 Stocked_i + \rho_e KS_i + X_i'\gamma_e + \mu_k + \epsilon_{ei} \tag{13a}$$

$$\Delta \log(1 + V[Sales]_i) = \alpha_s + \beta_2 Stocked_i + \rho_s KS_i + X_i'\gamma_s + \mu_k + \epsilon_{si} \tag{13b}$$

$$\begin{aligned} Stocked_i = \pi_0 + \pi_1 R_i + \pi_2 S_i + \pi_3 R_i \times S_i + \pi_4 KS_i + \pi_5 R_i \times KS_i \\ + \pi_6 S_i \times KS_i + \pi_7 R_i \times S_i \times KS_i + \nu_i \end{aligned} \tag{13c}$$

$Stocked_i$ denotes phase one stocking. I control for knowing a seller at baseline, KS_i, and include interactions between KS_i and treatment assignment as instruments to improve power, since treat-

[13] I estimate this model among the restricted sample of shops not receiving the supplier commitment since the focus is on untreated behavior and the effect of returns. Results are similar if the full sample is used.

ment effects should be smaller when firms can learn from peers. $\Delta \log(1 + \mathbb{E}[Sales]_i)$ is the log change in i's expected helmet sales from baseline to midline or endline. $\Delta \log(1+V[Sales]_i)$ is the analogous change in the variance of i's sales beliefs. If *returns* correct pessimistic priors, Proposition 3 predicts that $\beta_1 > 0$, meaning agents became more optimistic about demand. In contrast, it predicts that $\beta_2 < 0$ if *returns* mitigate risk aversion, meaning experience reduces uncertainty.

The final class of predictions address spillovers. I test (i) whether exposure to incumbent sellers substitutes for a firm's own experience, (ii) whether proximity to entrants raises adoption, and (iii) whether the information treatment corrects pessimistic priors or reduces uncertainty.

Let $TM_i = 1$(Market visited at *insurance* baseline). IT_i denotes receipt of the *information* treatment, HS_i equals one if the average sales presented in the *information* signal is greater than an agent's expectation, and μ are strata fixed effects. I test for information externalities by estimating

$$\begin{aligned} Stocked_i = \alpha_1 + \beta_1 R_i + \beta_2 S_i + \beta_3 R_i \times S_i + \beta_4 KS_i + \beta_5 KS_i \times R_i \\ + \beta_6 KS_i \times S_i + \beta_7 KS_i \times R_i \times S_i + X_i'\gamma_1 + \epsilon_{1i} \qquad \text{(14a)} \end{aligned}$$

$$Stocked_i = \alpha_2 + \beta_8 TM_i + \mu_c + \epsilon_{3i} \qquad \text{(14b)}$$

$$Stocked_i = \alpha_3 + \beta_9 IT_i + \beta_{10} HS_i + X_i'\gamma_2 + \mu_n + \epsilon_{2i} \qquad \text{(14c)}$$

Proposition 4 part (a) predicts that learning from incumbents lowers the value of returns and supplier commitment, implying $\beta_5 < 0$ and $\beta_6 < 0$. Part (b) states that externalities exist if exposure to a seller increases adoption, implying $\beta_8 > 0$. Finally, Equation 14c tests whether spillovers operate through expectations or uncertainty. If they correct pessimistic priors, a signal only raises stocking when it exceeds expectations ($\beta_9 < 0$, $\beta_{10} > 0$). If they mitigate uncertainty for risk averse firms, then even a slightly negative signal can increase stocking, implying $\beta_9 > 0$.

5 Results

5.1 Demand priors and baseline stocking

Before turning to experimental results, I present descriptive evidence from firm beliefs suggesting that risk aversion may constrain helmet stocking (Appendix Table A3). I focus on the Learning sample, where helmets were offered at market prices. When asked about a hypothetical purchase of 10 helmets, the median firm reported a 30% chance that the investment would reduce overall profits, compared to a 50% chance that it would increase them. Frequentist measurements similarly indicate that 50% of control enterprises expected to earn enough revenue to pay for the cost of stocking 5 helmets by the midline survey. Appendix Table A7, discussed later in this section, validates that beliefs predict realized outcomes, suggesting that this evidence of optimism among many firms is not likely a product of measurement error.

This indicates that about half of firms believed that helmets are profitable in expectation. However, beliefs are uncertain, and most firms reported a risk of helmets resulting in losses. Consistent with risk aversion, under 7% of control shops accepted stock when it was offered to them. I therefore turn to experimental variation to evaluate whether lowering risk affects stocking.

5.2 Firm risk aversion

The results of the Insurance Experiment (Equation 9) show that access to the insurance contract–which lowered expected profits while reducing stocking risk–substantially increased helmet adoption, implying a rejection of firm risk neutrality.

Insurance increased the rate of shops that acquired stock within 24 hours by 7 percentage points on a base of 5% (Table 1 Panel A, $p < .01$). Including shops that stocked later, the effect is about 10 percentage points (50%, $p = .02$). This shows that lowering profit variance (while reducing expected returns) substantially increases the propensity of firms to stock a new product, implying that risk aversion affects firms' investment decisions. Effects of *insurance* are more than twice as large among firms operated by more risk averse shopkeepers, consistent with a lack of separability between owners' consumption preferences and production decisions ($p = .009$, columns 5-6).

The rate and composition of *insurance* shops that opted into the contract suggest high comprehension of the offer. Half of *insurance* shops that immediately stocked helmets selected into the contract, while 40% of firms that ever stocked did (Table 1 Panel B). These rates align almost exactly with treatment effects on stocking. Shops with more uncertain beliefs were far more likely to choose the contract, consistent with risk aversion. A one helmet increase in a shop's standard deviation of sales was associated with a 24 percentage point increase in contract selection among immediate adopters (column 1), and a 15 percentage point increase among all adopters (bootstrapped $p = .049$, column 2).[14] The fact that shops with low uncertainty about demand typically declined the contract is consistent with shops understanding that expected payouts of the contract were lower than the guaranteed offer. Anecdotal reports also support this view: in nearly every survey, shops complained that *insurance* should be more generous, frequently objecting to the condition prohibiting restocking and collecting the payout.

A potential concern is that *insurance* could raise expected profits if beliefs were mismeasured. If this were driving the results, then contract acceptance would be more likely when the gap between the survey-estimated expected value of the *insurance* contract and guaranteed payment were smaller since less error would be required to flip the optimal choice. There is no such relationship in the data (Table 1, Panel B). Moreover, average ex-post payouts would have been higher among *insurance* adopters had they declined the contract. Thus, under both elicited beliefs and rational expectations, insurance is strictly dominated for risk neutral firms. Finally, beliefs about their probability of selling out strongly predict the outcome, with an elasticity of 0.2 among all baseline adopters ($p = .05$) and an elasticity of 0.4 among those that accepted the contract.

Treatment effects remain large and statistically significant among larger and older firms, suggesting that results are not driven by new or struggling enterprises. *Insurance* increased immediate helmet adoption by 20 percentage points ($p < .01$) among firms with employees and the rate of shops that ever stocked helmets by 25 percentage points ($p = .02$), more than double the control mean (Table 2). Point estimates are also large and significant among firms with higher profits and those that are older. These results suggests that risk aversion constrains entrepreneurship even when enterprises have liquidity available to make investments. From a policy perspective, this may also reduce the returns to capital in firms, since enterprises may direct additional resources towards low risk and low return investments absent insurance.

Appendix B estimates a simple structural model of the decision of firms exhibiting constant relative risk aversion to stock helmets, instrumenting for the expected utility of helmets using random assignment to *insurance*. The aim of this exercise is to examine how the level of risk aversion implied by insurance uptake compares to measures from individuals. The model suggests a mean CRRA coefficient of about 0.62, with the less risk averse sample measured via the lottery

[14] On average, the bad state insurance payout was about \$11 larger than the guaranteed payment among firms that accepted insurance, corresponding to about 25% of the price of the helmet stock.

choice game exhibiting a coefficient of 0.53 and the more risk averse firms an average of 2.21. These estimates align with the measures from the game and suggest that modest levels of distaste for risk can meaningfully distort enterprise decisions. However, these results are only suggestive as the parameters are imprecisely estimated and depend on strong assumptions.

5.3 Learning about demand

The effects of *returns* and the *supplier commitment* on stocking in phases one and two of the Learning Experiment (Equation 10) match the predictions of Proposition 3, consistent with demand uncertainty and learning consequentially affecting helmet uptake.

Returns increased the rate of firms that stock helmets during phase one, either from the study or any other supplier, by 9 percentage points relative to a 6.8% control rate ($p < .01$, Table 3). The effect remains stable, also 9 percentage points ($p = .02$), when the dependent variable is an indicator for ever stocking helmets by the endline survey. This suggests that *returns* increased firms' willingness to experiment with selling helmets, rather than simply accelerating adoption among firms that would have stocked regardless.

The *supplier commitment* increased phase one helmet stocking by about 6 percentage points ($p = .03$, Table 3), but only among firms without *returns*. Consistent with the model, the treatment had no effect among shops offered *returns*, in which case firms that perceive any chance of profitability should already adopt. This pattern supports the interpretation that firms internalize a "risk-reward" trade-off, increasing investment when the future value of learning is high, at the cost of short-run utility. The absence of effects from the *supplier commitment* conditional on *returns* suggests that alternative explanations – namely learning by doing or fixed costs – are unlikely to explain the results, since these forces would predict an effect regardless of *returns*.[15]

The null effect of the *supplier commitment* conditional on *returns* suggests that the firms moved by the treatments were unlikely facing binding capital constraints and that non-stock costs of selling helmets are low. Consistent with this, under 15% of shops in the Learning Experiment reported any fixed costs of selling helmets, accounting for under \$2.50 on average (Appendix Table A8). The Insurance Experiment asked directly if firms incurred any costs to sell helmets other than

[15]The binary outcome could generate this result due to ceiling effects, but Appendix Figure A1 shows that many shops interested in stocking did not make purchases.

buying stock. Under 3% of shops reported such costs, averaging \$4. Furthermore, shops did not typically report greater work hours or effort to sell helmets, consistent with slack capacity (Walker et al., 2024). The view that compliers did not face binding liquidity constraints when purchasing matches the result from the Insurance Experiment that effects were larger among bigger firms.

More strikingly, *returns* had large and significant persistent effects on stocking in phase two, when *returns* and control shops faced identical stocking conditions. *Returns* firms were 7 percentage points (70%, $p = .03$) more likely to stock in this phase (Table 3). In markets without a pre-existing helmet seller (Table A4), this rises to 8 percentage points (105%, $p = .02$). *Returns* also had large effects on market entry, defined by restocking and reporting intent to continue selling helmets (Table 3). *Returns* effects doubled the number of permanent market entrants based on this definition.[16] These results suggest that agents learned about demand since the groups differ only in that *returns* shops had more experience selling helmets in phase one, matching Proposition 3.

The persistent effect of *returns* on stocking provides revealed preference evidence that many shops found helmets profitable, which matches descriptive data. On average, phase one adopters reported profits of \$70 from helmet sales by endline, with fewer than 7% reporting losses. 65% restocked, and over 80% reported intent to stay in the market (Figure 1). Shops sold about 10 helmets on average by the end of the study at a typical price 1.5 times the wholesale cost. By endline, shops that stocked helmets reported that they accounted for about 10% of total profits, and they expected the share to rise to 20%. Consistent with these patterns, very few *returns* shops (7% of those that stocked) returned any helmets. These were concentrated among firms that reported losses: 80% of returning firms reported losses, versus 3% of shops not making returns.

5.4 Does risk aversion prevent experimentation with uncertain new products?

These results suggest that *returns* helped unlock a growth opportunity for many firms and expanded consumer access to helmets. The findings are consistent with risk aversion inhibiting experimentation with products whose demand is uncertain, but could also be driven by *returns* increasing expected profits, correcting pessimistic expectations.

The results of Equations 11-13 indicate that *returns* caused risk averse firms to try stocking helmets who overcame demand uncertainty through learning. Each of the predictions of Proposition

[16] Results are robust to other entry definitions, including stated intent to sell helmets or stocking three or more times.

3 is satisfied under this mechanism, while none of the predictions under the alternative of *returns* changing expectations hold. The results suggest that risk aversion can substantially inhibit new product adoption by deterring firms from experimenting with risky products, limiting firm growth.

First, the results of Equation 11 show that, absent *returns*, firms more uncertain about the profitability of helmets are significantly less likely to stock (Table 4). This is consistent with risk aversion undermining experimentation: firms uncertain about demand avoid stocking. Otherwise, one would expect the opposite, since the option value of learning rises with uncertainty. I estimate regressions of phase one stocking on $SD[Sales]_i$ plus this variable interacted with assignment to *returns*, controlling for expectations. As discussed in Section 3.3, $SD[Sales]_i$ is truncated when expected sales approach 10. Furthermore, if expectations are sufficiently high, $SD[Sales]_i$ is an imperfect proxy for uncertainty about the profitability of stocking 5 helmets.[17] I thus exclude cases where firms perceive no risk that 1 month helmet revenues will fail to exceed costs (25% of cases).

Estimates in column 1 of Table 4, which includes rich firm and respondent controls, indicate that when the standard deviation of beliefs about sales increased by one, firms' propensity to stock fell by 5pp (80%, $p = .03$). Appendix Table A5 shows that this relationship is similar using beliefs about revenue rather than sales, excluding covariates, and selecting controls using double-post LASSO. Estimates are also qualitatively similar, albeit underpowered, when observations prone to censoring are kept. Dropping cases where respondents report degenerate belief distributions strengthens effects. One may construct an additional robustness check by leveraging the fact that the outcome is firms' binary decision to stock 5 helmets. If agents' beliefs are sufficiently high that they perceive no risk of a loss from acquiring 5 helmets, theory no longer predicts a negative relationship between accepting stock and the standard deviation of beliefs. Consistent with this, the standard deviation of sales positively predicts stocking among firms perceiving no loss risk (Appendix Table A5, column 6), although the difference is not statistically significant ($p = .24$).

Second, the results of Equation 12 indicate that the composition of firms that stocked helmets with *returns* held more uncertain but similarly optimistic beliefs versus control firms at baseline (Appendix Table A6). This is consistent with treatment compliers being constrained from stocking by demand uncertainty and risk aversion, not incorrect beliefs. The standard deviation of *returns*

[17] Put differently, if a shop is uncertain about selling 5 helmets or 10, risk aversion may lead to under stocking, but should not prevent purchasing 5. So a censoring issue affects the binary outcome measure for high expectations.

adopters beliefs about the number of sales they will make in the subsequent month is 0.2 units higher versus control shops (24%, bootstrapped $p = .069$). There is no significant difference in expectations. This compositional effect is also evident in Table 4, which shows a large and significant interaction between treatment assignment and belief uncertainty: *returns* disproportionately increased adoption among firms with uncertain priors.

Third, and most directly, there is no evidence that the expectations of phase one adopters positively updated, but there is clear evidence of reduced demand uncertainty. Table 5 reports the results of Equation 13, which examines the effect of stocking in phase one (instrumented for using treatment assignment) on the change in an agent's belief about expected sales or the variance of sales from baseline to later surveys. There is no significant evidence expectations positively updated, and the point estimate is negative at endline. In contrast, estimates indicate that stocking caused the variance of compliers' beliefs about demand to fall by over 60%[18] ($p = .032$). The reduced-form also supports these conclusions: *returns* is associated with a small and insignificant decrease in expected sales but a 0.1 unit reduction in posterior variance ($p = 0.09$).

The view that learning reduced uncertainty without substantially changing expectations is robust to alternative variable definitions and specifications.[19] Running the same IV specifications pooling survey waves on expected revenue, obtained by combining frequentist beliefs about sales with prices, indicates a small and insignificant effect on expected profits but a \$26 decrease in the standard deviation of revenue ($p = .05$). And regressing the change in expected sales or the standard deviation of sales on stocking in phase one (with no instrument) suggests adopters expect to make $.03$ fewer sales but the standard deviation of their belief is smaller by 0.19 helmets ($p = .04$).

Reductions in belief variance among phase one adopters translate into substantially improved predictive accuracy over time. This validates the belief measures and indicates that declines in reported uncertainty are not likely driven by experimenter demand effects or overconfidence. Appendix Table A7 reports the elasticity of realized sales with respect to expectations at baseline and at midline, restricting the sample to shops that accepted stock in phase one. At baseline, firms hold diffuse priors, so attenuation bias implies an elasticity below one. Consistent with this, the estimate is 0.28 ($p = .024$, $R^2 = .045$). At midline, firms report substantially less uncertainty, which should

[18]Pooled point estimate: $\exp(.062 - 1.098) - 1 \approx -.64$. This is an approximation since 1 is added to logs.
[19]These results are only reported in the text for brevity given their similarity to Table 5.

yield stronger predictive power. The elasticity rises sharply to 0.69 ($p < .001$, $R^2 = .239$), and one can reject the null hypothesis of no change predictive accuracy with 95% confidence ($p = .047$).

Direct tests of learning present a consistent picture: for *returns* compliers, experience selling helmets left expectations about demand essentially unchanged but substantially reduced the variance of beliefs. This suggests that effects of *returns* on phase two stocking reflect risk aversion preventing firms uncertain about demand from stocking, causing a learning breakdown. Importantly, these results apply specifically to the subset of firms induced to try helmets by *returns*, who on average held more optimistic but uncertain beliefs. Some firms likely held inaccurate (pessimistic) beliefs, but the intervention was not strong enough to induce stocking among them.

5.5 Information externalities

The results of the Learning Experiment suggest that learning about demand is a consequential determinant of new product adoption. Because firms are risk averse, investing in new products involves high utility costs. Can firms learn from each other to avoid making risky investments themselves? The results of Equation 14 suggest that firms can substitute for learning from their own experiences by observing neighbors stock, validating Proposition 4.

Externalities matter for two reasons. First, they further support the argument that the persistence of *returns* reflects learning about demand since exposure to information from sources other than one's own experience generates similar effects. Second, information spillovers suggest a market imperfection that helps rationalize how risk averse firms survive competition. Absent externalities, one would expect more risk averse firms to have lower productivity and be driven out of business. But spillovers may allow risk averse firms to mimic neighbors product discoveries, rationalizing their survival (and potentially undercutting risk taking incentives of neighbors).

Treatment effect heterogeneity aligns with the model's predictions. Neither *returns* nor the *supplier commitment* affected stocking when there was a pre-existing helmet seller near the firm, and one can reject the equality of effects of *returns* with 95% confidence (Table A4). The adoption rate of untreated firms near a pre-existing seller is also approximately equal to that of *returns* firms in markets without incumbent motorcycle shops (Table A4).

Results of the *spillover* survey, presented in Table 7, experimentally validate the view that firms learn from each other. The coefficients on "BL market" reports the effect of being in a market where a shop was induced to stock helmets three months before the survey. Results are reported over motorcycle-related shops and all shops in the sample. As detailed in the design, the sample of motorcycle shops is likely balanced across markets, whereas across the full sample firms in baseline markets were ex-ante less likely to stock.[20]

Firms observed competitors adopt helmets, leading to an increase in their own propensity to stock. Across the sample of motorcycle shops, firms were about 13 percentage points more likely to report knowing a helmet seller in their market ($p = .03$) and to purchase stock ($p < .01$). Across all shops, the estimated effect on stocking falls to 6 percentage points but remains statistically significant ($p < .01$). Anecdotal reports from shops help shed light on how information spillovers occurred. Several firms reported that customers entered their shops with pictures of the study helmets and asked if they could provide them at a better price. Appendix Table A9 shows similar results using non-random variation in the Learning Experiment.

I further validate that information matters directly by examining the effects of the *information* treatment (Table 6). Receipt of the anonymous sales data increased the rate of shops that stocked helmets from 0.6% to 2.8% ($p = .02$). As with *returns*, the evidence suggests that information primarily affected behavior by reducing uncertainty. When a "high signal" indicator – equal to 1 if the data the shop received exceeded their expectation – is added (Equation 14c), the effect of receiving any data, whether above or below expectations, remains large and significant.[21]

While spillovers were strong, evidence suggests that they were localized, explaining slow helmet diffusion in Nairobi. In the *spillover* sample, only 42% of shops in baseline markets reported knowing of a helmet seller in their market. And in the Learning Experiment, shops within a quarter kilometer (about 3 blocks) of a study shop that adopted helmets were 14 percentage points more likely to report knowing a seller near them ($p < .01$), whereas the presence of a shop within 0.25-0.5 km had no effect (Appendix Table A9). This suggests minimal spillovers across markets, preventing social learning from ensuring rapid product diffusion despite firm risk aversion.

[20] Validating this argument, surveyed shops in baseline markets are 20 percentage points less likely to sell motorcycle parts, and motorcycle-related shops are more than four times as likely to stock helmets in pure control markets.

[21] This leverages the fact that signals were constructed from 5 random shops. The mean signal was 3.8 sales (SD 1.18). In columns 2 and 4, "high signal" is constructed by comparing the signal to the agent's elicited expectation about demand, controlling for expectations. In column 3 and 5, I examine whether the signal exceeds the median.

6 Discussion

The results indicate that inducing experimentation with helmets led to large and permanent increases in stocking. Could the persistent effect of *returns* on helmet stocking be driven by a mechanism other than learning resolving demand uncertainty?

One possibility is that *returns* increased phase one profits, relaxing capital constraints. But in practice, *returns* had no significant effect on overall firm profits because overall profit volatility dominated reported gains. Stocking effects also survived the fall in order size from 5 to 3 helmets, which lowered liquidity requirements for all firms. Direct evidence of learning and information spillovers further support the view that persistence reflects firms overcoming uncertainty. Lastly, flooding impacted about 1/5 of shops at the end of phase one, resulting in a 20% ($96) profit decline among exposed firms (Appendix Figure A2, $p = .024$). Despite this shock, phase two stocking among *returns* shops affected by flooding exceeded that of unaffected control firms ($p = .021$).[22]

A second potential mechanism is learning by doing. However, helmet profits between midline and endline were not higher than those between baseline and midline among adopters, or higher among shops that stocked helmets earlier (Appendix Table A8). The lack of effects of the *supplier commitment* given *returns* and null treatment effects in markets with a pre-existing seller also provide evidence against this explanation. And finally, the fact the information about demand from sources other than one's own experience (neighbors or the study) increases stocking suggests that learning about helmet profitability, not learning by doing, is the primary mechanism.

Third, behavioral biases such as loss aversion or cognitive uncertainty may make *insurance* or *returns* valuable for reasons distinct from neoclassical risk aversion. The central argument of this paper is that small firms' production decisions depend on owners' risk preferences. This still holds, and in fact may be more important, if behavioral biases affect consumption preferences. However, the evidence is more consistent with standard models of risk aversion. The response to the *supplier commitment* intervention rules out preferences like maxmin utility since investment expands when the future value of learning increases, with no change in risk. Moreover, appendix B suggests that modest levels of risk aversion can rationalize the observed response to *insurance*.

The large effect of *returns* on firm entry at endline suggests that the Learning Experiment

[22]Phase two stocking among returns firms was largely unaffected by flooding. Qualitatively, flooded shops were sometimes unable to restock immediately but purchased helmets once they recovered.

was effective at expanding consumers' access to motorcycle helmets, an important safety product. Smoothing risk aversion, such as by facilitating returns, could be an effective tool for practitioners that aim to increase firm profitability or expand product access. However, there are two concerns with the results that affect the policy interpretation. First, did the intervention expand helmet access or displace economic activity, crowding out firms that otherwise would have began stocking helmets? Second, would all of the *returns* firms that began selling helmets have adopted them anyways once they observed peers stock?

Information spillovers indicate that displacement is unlikely, suggesting that if anything the entry of *returns* shops made competitors more likely to enter the market. I also test for displacement in columns 1-2 of Appendix Table A10. The dependent variable in column 1 equals 1 if the surveyed shop was stocking helmets at endline or reported that a shop near them was.[23] *Returns* increased the likelihood that the shop or an enterprise near them sold helmets by about 8 percentage points (33%, $p = .04$). The effect is larger if I examine whether a seller was ever reported near the sampled shop: *returns* had about a 13 percentage point effect (42%, $p = .03$). These results are consistent with the finding that effects are concentrated in markets with no baseline seller, suggesting that the intervention crowded in firms in areas where there were no peers to learn from.

The question of whether *returns* entrants would have eventually stocked absent the treatment is more challenging to answer since the study ended after six months. However, helmets were available to shops for over two years before the study, and the manufacturer made a strong effort to market to shops. This suggests that, at least in the near term, shops were unlikely to enter absent the intervention. The effects on helmet sales during the study period are also economically important even if the study did not affect long-run helmet access. Appendix Table A10 suggests that *returns* induced shops to stock about 1.9 additional helmets on average during the study and sell about 1.4 more (144%, $p = .07$). In markets with no baseline helmet seller, these point estimates jump to 2.8 additional helmets stocked and 2.1 sold (528%, $p = .03$). These values correspond to *returns* inducing over 600 additional helmet sales in this time, accounting for around $15,000 in sales.

[23]This approach relies on shops to determine what firms they consider to be proximate. Field officers' assessment of the presence of shops and those of respondents were highly correlated at baseline.

7 Conclusion

This paper demonstrates that small retail firms in Kenya are risk averse, which inhibits them from adopting a profitable new product. I first show that offering firms an insurance contract that lowered profit risk from low demand, without increasing expected returns, led to a 50% increase in product adoption. This result is based on a new experimental test for risk aversion that could be adapted to study risk preferences in other settings. I then study the longer-run stocking decision of firms in an experiment designed to test a model of firm learning. Temporarily lowering inventory risk led to large increases in stocking that persisted after the policy ended. This indicates that risk aversion prevents firms from learning about demand, a view supported by a second experimental arm which shows that increasing the value of information expanded firms' propensity to adopt risk.

The large effects of smoothing risk on helmet adoption are consistent with risk aversion leading to consequential distortions. The setting of the Learning Experiment may be one where efficiency gains from *returns* are limited because helmet demand is easily observed by peers. Despite this, *returns* firms sold over twice the volume of motorcycle helmets over the course of the study. Speculatively, welfare gains from offering similar policies to increase access to products whose demand is more difficult to observe, such as female hygiene items (due to stigma) or digital commodities, may be larger. Theory also predicts that diffusion of expensive items, such as productive assets like machines, may be more impacted because the discrete costs of learning are high. Future research testing these predictions would be valuable as such goods are important to economic growth.

More broadly, the finding that small firms are risk averse calls into question many economic models of small firms and policies designed to promote their growth. These results have broad relevance as over half of workers in LMICs are self-employed, and a third of high-income country workers operate at companies with fewer than 10 employees (International Labour Organization, 2019). Risk aversion may help rationalize slow technology adoption (Cirera et al., 2022), inefficient location choices (Pelnik, 2024), and foregone investments with positive net returns (de Mel et al., 2008). It may also lower the effectiveness of capital transfers if risk averse firms direct capital towards low-risk, low-return investments. Investigating the role of risk aversion in these choices, and the effectiveness of insurance policies, could be valuable to better align academic understanding of LMIC firms with their behavior and to identify effective policies to promote growth.

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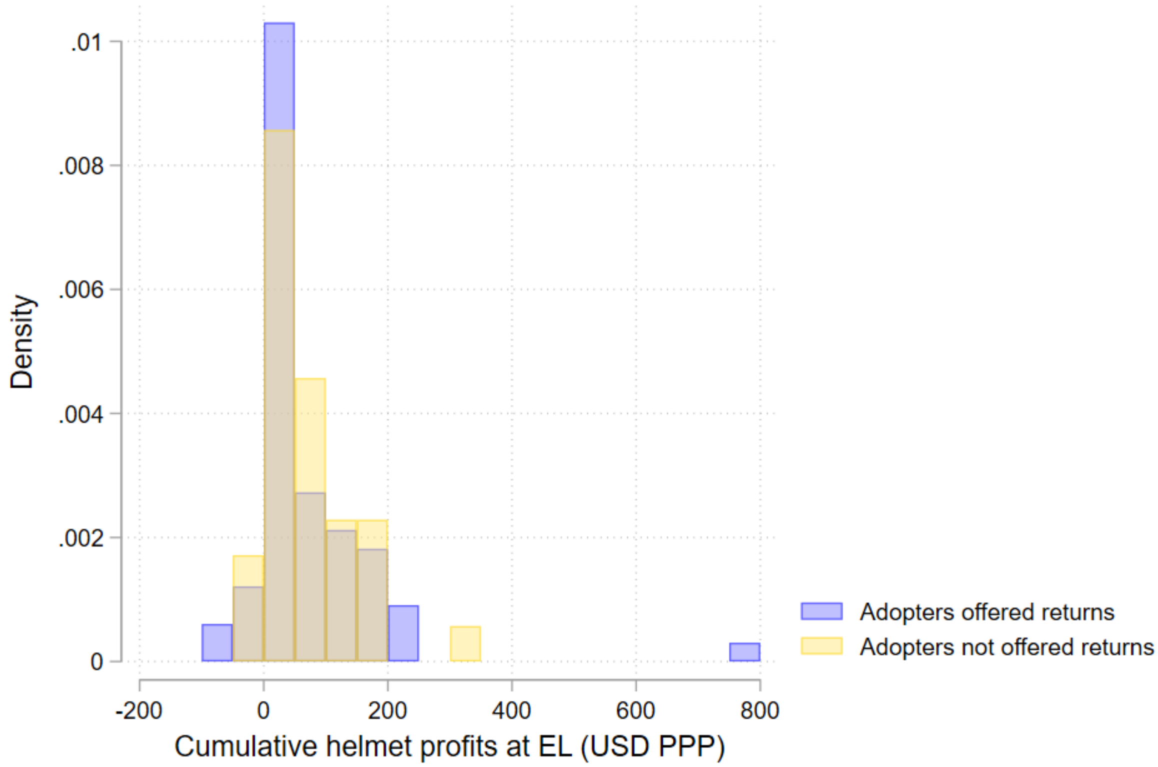
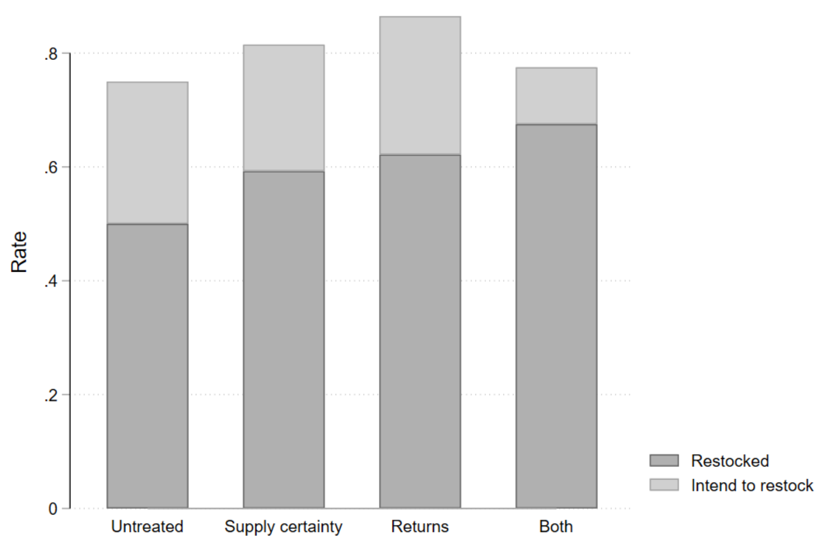
Figure 1: Learning experiment: Realized helmet profits and restocking rates

Panel A: Realized helmet profits



Panel B: Helmet restocking rates



Panel A reports realized helmet profits at endline among firms that adopted helmets within a month of the baseline survey, breaking down the sample by those that received access to returns versus not. Profit estimates are net of lost profits on items shops were unable to stock to afford helmets. Panel B reports rates of restocking among the same set of shops. The darker bar indicates that the shop purchased at least 1 additional stock of helmets by endline and reported an intent to stay in the market, and the lighter bar denotes shops that had yet to restock but reported planning to.

Table 1: Insurance experiment: Helmet stocking and contract uptake

Panel A: Effects of insurance offer on helmet stocking

	(1) Stocked ($\leq$ 24H)	(2) Stocked	(3) Stocked ($\leq$ 24H)	(4) Stocked	(5) Stocked ($\leq$ 24H)	(6) Stocked
Offered insurance	0.075***	0.092**	0.073***	0.099**	0.002	-0.028
	(0.029)	(0.044)	(0.028)	(0.043)	(0.046)	(0.066)
High risk aversion					-0.066	-0.171***
					(0.045)	(0.065)
Offered insurance $\times$ High RA					0.168**	0.267***
					(0.075)	(0.102)
Observations	345	345	345	345	327	327
Control mean	0.047	0.203	0.047	0.203	0.047	0.203
Controls	Market FE	Market FE	Yes	Yes	Market FE	Market FE

Panel B: Insurance offer take-up and expected foregone returns among treated adopters

	Insurance uptake		Insurance expected payout $-$ guaranteed		Insurance realized payout $-$ guaranteed	
	(1) Stocked ($\leq 24H$)	(2) Stocked	(3) Dollars	(4) Share	(5) Dollars	(6) Share
$\mathbb{E}$[Sales]	-0.017	-0.033				
	(0.113)	(0.036)				
SD[Sales]	0.239	0.153**				
	(0.221)	(0.078)				
Accepted insurance			-0.024	-0.003	0.932	-0.044
			(0.274)	(0.063)	(2.748)	(0.266)
Constant	0.224	0.290*	-0.822***	-0.117***	-6.046***	-0.472**
	(0.254)	(0.152)	(0.158)	(0.038)	(1.983)	(0.213)
Observations	22	51	51	51	47	47
Mean	0.500	0.392	-0.812	-0.113	-6.048	-0.493

$* \; p < 0.1$, $** \; p < .05$ $*** \; p < .01$. Robust standard errors in parenthesis in Panel A. Bootstrapped standard errors in parenthesis in Panel B (constructed with 500 draws).

This table reports results of the Insurance Experiment. In columns (1), (3) and (5) the dependent variable equals 1 if the shop stocked helmets within 24 hours of the survey while columns (2), (4) and (6) report effects on stocking after baseline, which could occur up to two weeks after the survey. Columns (3) and (4) include controls for industry, baseline revenue, days open per week, knowledge of a nearby helmet seller, and indicators for having space to store helmets, selling multiple products, and stocking a new product in the past year. High risk aversion indicates that the agent's coefficient of relative risk aversion, measured via a lottery choice game at the follow-up, exceeds the sample median. Panel B reports (endogenous) uptake of the insurance offer among treated enterprises that stocked helmets within 24 hours in columns (1) and after baseline in (2). Columns (3) - (6) examine *insurance* shops that ever stocked helmets. Column (3) reports the expected value of the insurance offer less the guaranteed payment offered to firms, and column (4) reports column (3) normalized by the size of the guaranteed payment. Columns (5) and (6) are similar but use realized payouts rather than expectations. One market that was visited in both piloting and at baseline due to a sampling error is excluded from Panel A.

Table 2: Insurance experiment: Heterogeneity by firm size and age

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Stocked within 24 hours			Dependent variable: Stocked		
	Employees	Firm profits	Firm age	Employees	Firm profits	Firm age
Treatment effect of insurance: smaller/younger firms						
Below median $\times$ insurance	0.025	0.020	0.036	0.046	0.039	0.043
	(0.032)	(0.045)	(0.034)	(0.047)	(0.060)	(0.060)
Treatment effect of insurance: larger/older firms						
Above median $\times$ insurance	0.197***	0.118**	0.120**	0.249**	0.173***	0.170**
	(0.071)	(0.047)	(0.054)	(0.100)	(0.066)	(0.066)
Pr(below = above)	0.039	0.160	0.219	0.070	0.130	0.169
Control mean ($\leq$ median)	0.044	0.072	0.033	0.193	0.193	0.228
Control mean ($>$ median)	0.054	0.027	0.062	0.243	0.216	0.175
Observations	344	316	345	344	316	345

$* \, p < 0.1, \;\; ** \; p < .05 \;\;\; *** \, p < .01$. Robust standard errors in parenthesis.

This table reports heterogeneous treatment effects of *insurance* with respect to firm size and age. Panel A presents treatment effects of receiving an insurance offer on helmet stocking. In columns (1) - (3) the dependent variable equals 1 if the shop stocked helmets within 24 hours of the survey, while in columns (4) - (6) the dependent variable captures stocking within the 2 week window provided after the baseline. The below median group of firms by employee size contains no paid employees, while the above median group includes all firms with at least 1. Median firm profits in the last month were PPP USD 325. The median enterprise age is 3 years. Pr(below=above) reports the p-value of the test that the treatment effects are equal. Control means capture the untreated average of the dependent variable within the respective groups. All estimates include market fixed effects and controls for industry, baseline revenue, days open per week, knowledge of a nearby helmet seller, and indicators for having space to store helmets, selling multiple products, and stocking a new product in the past year.

Table 3: Learning experiment: Treatment effects on stocking and entry

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Stocked in Phase 1		Ever Stocked		Stocked in Phase 2		Entrant	
Returns	0.096***	0.089***	0.085**	0.078**	0.070**	0.065**	0.069**	0.066**
	(0.029)	(0.028)	(0.034)	(0.032)	(0.031)	(0.030)	(0.028)	(0.027)
Supplier commitment	0.053*	0.060**	0.038	0.045	0.041	0.044	0.022	0.027
	(0.027)	(0.027)	(0.033)	(0.031)	(0.030)	(0.028)	(0.025)	(0.024)
Returns $\times$	-0.056	-0.042	-0.061	-0.043	-0.063	-0.047	-0.032	-0.021
Supplier commitment	(0.043)	(0.042)	(0.049)	(0.046)	(0.044)	(0.042)	(0.039)	(0.038)
Observations	929	929	929	929	929	929	929	929
Control mean	0.068	0.068	0.131	0.131	0.102	0.102	0.068	0.068
Controls	Strata FE	Yes	Strata FE	Yes	Strata FE	Yes	Strata FE	Yes

$* \, p < 0.1, \;\; ** \; p < .05, \;\; *** p < .01$

This table plots treatment effects on helmet stocking and helmet market entry from the Learning Experiment. Column (1)-(2) report effects on phase 1 stocking, supplied by the study or any other source. Columns (3)-(4) examine effects on stocking at any point prior to the endline survey. Columns (5)-(6) examine effects on phase 2 stocking. Column (7)-(8) examine effects on entry into the helmet market, defined by two or more stock purchases and reported intent to permanently stock helmets. The dependent variable in columns (5)-(8) is coded to 0 if a shop withdrew from the study because they did not wish to sell helmets or if the outcome was confirmed without the survey. All estimates include strata fixed effects. Columns where controls indicates “Yes” also include industry fixed effects, controls for distance to the manufacturer, log revenue, days open per week, and indicators for stocking a new product in the year before the baseline survey, selling multiple products at baseline, and having space to store helmets without stocking less of another item.

Table 4: Learning experiment: Relationship between beliefs and adoption

	SD Sales			log(1 + Var sales)		
	(1) LPM	(2) Logit	(3) LPM	(4) LPM	(5) Logit	(6) LPM
Returns	-0.032	0.290	0.232	-0.042	0.947	0.243
	(0.077)	(1.339)	(0.185)	(0.117)	(2.207)	(0.184)
E[sales]	0.021	0.479		0.069	1.906	
	(0.015)	(0.311)		(0.046)	(1.208)	
$\sigma(sales)$	-0.052**	-0.933**	-0.053**	-0.061**	-1.127**	-0.067**
	(0.023)	(0.406)	(0.024)	(0.027)	(0.506)	(0.030)
Returns $\times$ E[sales]	0.013	-0.145		0.038	-0.743	
	(0.034)	(0.394)		(0.110)	(1.556)	
Returns $\times$ $\sigma(sales)$	0.058	0.984*	0.096**	0.079	1.274*	0.121**
	(0.055)	(0.586)	(0.048)	(0.064)	(0.721)	(0.059)
Observations	304	271	304	304	271	304
Control mean	0.068	0.068	0.068	0.068	0.068	0.068
Controls	Full	Full	Full	Full	Full	Full
Expected Sale x Returns FEs	No	No	Yes	No	No	Yes

$* \, p < 0.1, \;\; ** \; p < .05, \;\; * * *p < .01$

Robust standard errors in parenthesis. The dependent variable in all regressions is an indicator equal to 1 if the firm stocked helmets in the month after the baseline survey. In columns (1) - (3), $\sigma(sales)$ is the standard deviation of the agent's beliefs about helmet sales, and in columns (4) - (6) this value denotes the log of 1 plus the variance of sales. The sample excludes firms offered *supplier commitment*. The sample further excludes shops with 0 loss probability from stocking 5 helmets to avoid top censoring. Estimates include controls for storage space, education, baseline profits, respondent characteristics, and firm characteristics.

Table 5: Learning experiment: Instrumental variable estimates of entry on posterior beliefs

	Instrumental Variables Estimates						Reduced form	
	(1) $\Delta\,\mathbb{E}[Sales]$	(2) $\Delta V[Sales]$	(3) $\Delta\,\mathbb{E}[Sales]$	(4) $\Delta V[Sales]$	(5) $\Delta\,\mathbb{E}[Sales]$	(6) $\Delta V[Sales]$	(7) $\Delta\,\mathbb{E}[Sales]$	(8) $\Delta V[Sales]$
Stocked (phase 1)	0.217	-1.173**	-0.026	-0.999*	0.095	-1.098**		
	(0.382)	(0.582)	(0.387)	(0.530)	(0.346)	(0.496)		
Returns							-0.001	-0.108*
							(0.047)	(0.063)
Supplier commitment							0.036	-0.040
							(0.043)	(0.058)
Returns $\times$ Supplier commitment							0.056	0.089
							(0.064)	(0.089)
Observations	820	820	832	832	1,652	1,652	1,652	1,652
Dep. var. mean	-0.111	0.059	-0.100	0.130	-0.069	0.062	-0.091	0.090
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV robust p-val	0.561	0.050	0.859	0.073	0.794	0.032		
Survey	ML	ML	EL	EL	Pooled	Pooled	Pooled	Pooled

$*\,p < 0.1,\ **\ p < .05,\ ***p < .01$

Robust standard errors in parenthesis in columns (1) - (4). Standard errors clustered by firm in columns (5) - (8). The dependent variables are the change in the log of 1 plus the moment of beliefs of firm i since baseline, for instance $\log(1 + \mathbb{E}$[Sales at midline]$) - \log(1 + \mathbb{E}$[Sales at baseline]$)$. Columns (1) - (6) instrument for stocking in the month following baseline using treatment assignment and treatment interacted with an indicator for a seller at baseline, controlling for the presence of a seller. The baseline seller interaction absorbs heterogeneity in the first stage, yielding better mean-squared error (Abadie et al., 2023). In columns (1) - (2), midline data is used, in columns (3) - (4) endline data is used, and in columns (5) - (8) both surveyed are considered. All estimates include controls for the saturation of baseline helmet sellers in the market, firms' reported willingness to take risks to experiment, the firm owner's performance on a digit span recall score, an indicator for whether the firm sold multiple products at baseline, and the firm's proximity to the helmet manufacturer. These controls were selected because they are likely to affect access to and retention of information affecting learning. Without covariates, the endline coefficient on expectations is $-.015(p = .969)$ and the coefficient on variance is $-.87(p = .096)$. Using CHS post-lasso-orthogonalized controls, selected from a list including those selected in the main specification as well as a wider list, yields a coefficient on endline expectations of $.004(p = .990)$ and one on endline variance of $-.86(p = .098)$.

Table 6: Information treatment: Effects on helmet uptake

		Helmet sales		Helmet revenue	
	(1)	(2)	(3)	(4)	(5)
1(Information treatment)	0.022**	0.030**	0.028**	0.029*	0.039**
	(0.009)	(0.015)	(0.014)	(0.017)	(0.017)
Signal > Expectation		-0.014		-0.011	
		(0.021)		(0.023)	
Signal > Median			-0.011		-0.030
			(0.017)		(0.018)
Expectation		-0.000		0.001	
		(0.000)		(0.003)	
Observations	727	722	727	722	727
Control mean	0.006	0.006	0.006	0.006	0.006

$* \, p < 0.1, \; ** \; p < .05, \; *** p < .01$

Robust standard errors in parenthesis. The dependent variable is an indicator for ordering helmets at the endline survey and the sample is restricted to shops that had not stocked helmets prior to that point. 1(Information treatment) indicates that a shop received helmet sale and price data from 5 randomly selected shops. Signal > expectation denotes that the signal average sales or revenue value exceeded the respondent's beliefs and is 0 otherwise or if the shop received no information, and Signal > median is one if the average sales or revenue value was greater than the median value across signals and 0 otherwise or if the shop received no signal. Columns (2) - (3) consider signals about helmet sales, and columns (4) - (5) examine signals about helmet revenue. All estimates include controls for knowing of a helmet seller at midline, log revenue in the month before the survey, and an indicator equal to 1 if the shop was affected by floods that occurred near midline.

Table 7: Spillover survey: Effect of helmet entrant on neighbor adoption

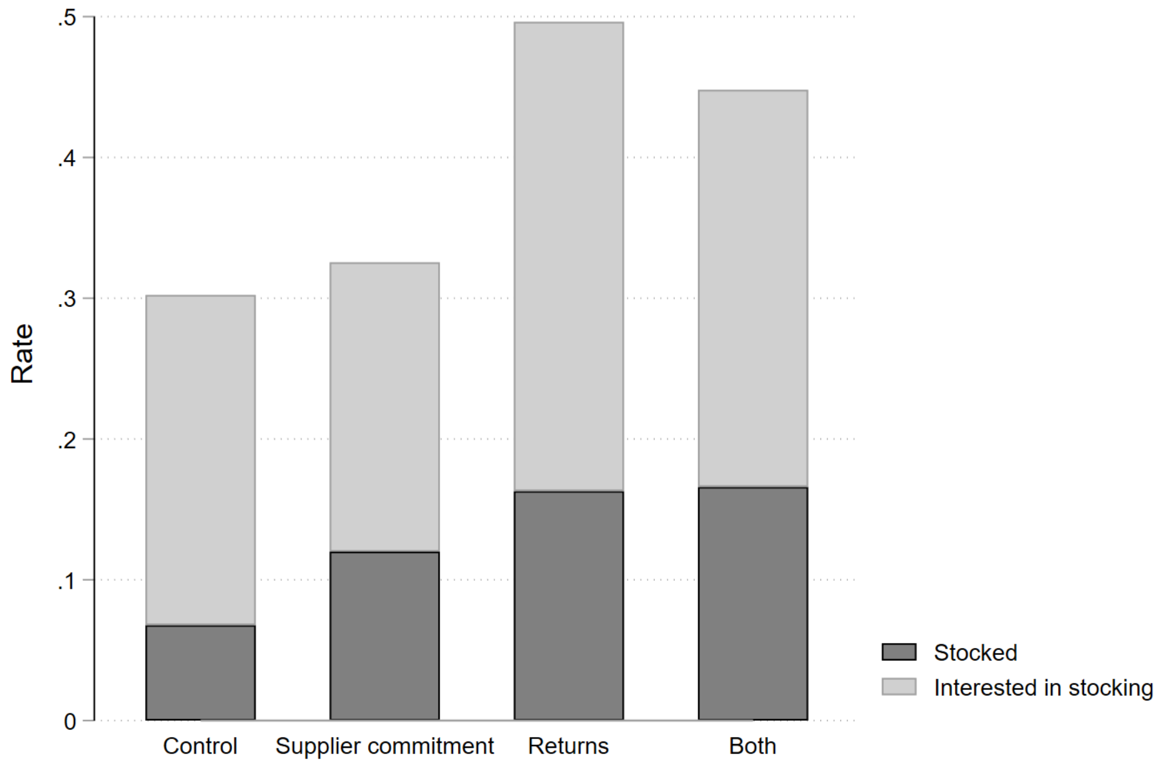
	(1)	(2)	(3)	(4)
	Know seller in market		Ordered helmets	
Panel A: Motorcycle shops				
Baseline (treated) market	0.115*	0.133**	0.098***	0.104***
	(0.069)	(0.061)	(0.035)	(0.036)
Observations	376	376	376	376
Control mean	0.361	0.361	0.091	0.091
Controls	No	Yes	No	Yes
Panel B: All shops				
Baseline (treated) market	0.122**	0.143***	0.048**	0.059***
	(0.052)	(0.049)	(0.022)	(0.022)
Observations	665	665	665	665
Control mean	0.300	0.300	0.070	0.070
Controls	No	Yes	No	Yes

$* p < 0.1, \;\; ** \; p < .05, \;\; ***p < .01$

Standard errors in parenthesis clustered by market. Panel a restricts the sample to shops selling motorcycle-related products and panel b includes all firms. The variable "Baseline (treated) market" equals 1 if the market was randomly selected for surveys at baseline, and is 0 if the market was a pure control and was skipped. All results are from the spillover survey with non-baseline shops, collected 3-4 months after baseline. The dependent variable in columns (1)-(2) equals 1 if the shop reported knowing a helmet seller in their market (selling the studies variety, or any other variety). The dependent variable in columns (3)-(4) equals 1 if the shop purchased helmet stock. All estimates include county fixed effects. Estimates with controls include additional covariates for revenue, business age, employees, and the number of products the firm sells. All estimates exclude 35 observations from shops that started selling helmets before the study began. These shops were ineligible put initially misreported their sales start date to obtain subsidized stock. Back checks were conducted after delivery (when no misreporting incentive is present) with all shops selling helmets at the time of the survey to confirm eligibility. Shops that revealed they were ineligible during these back checks are excluded.

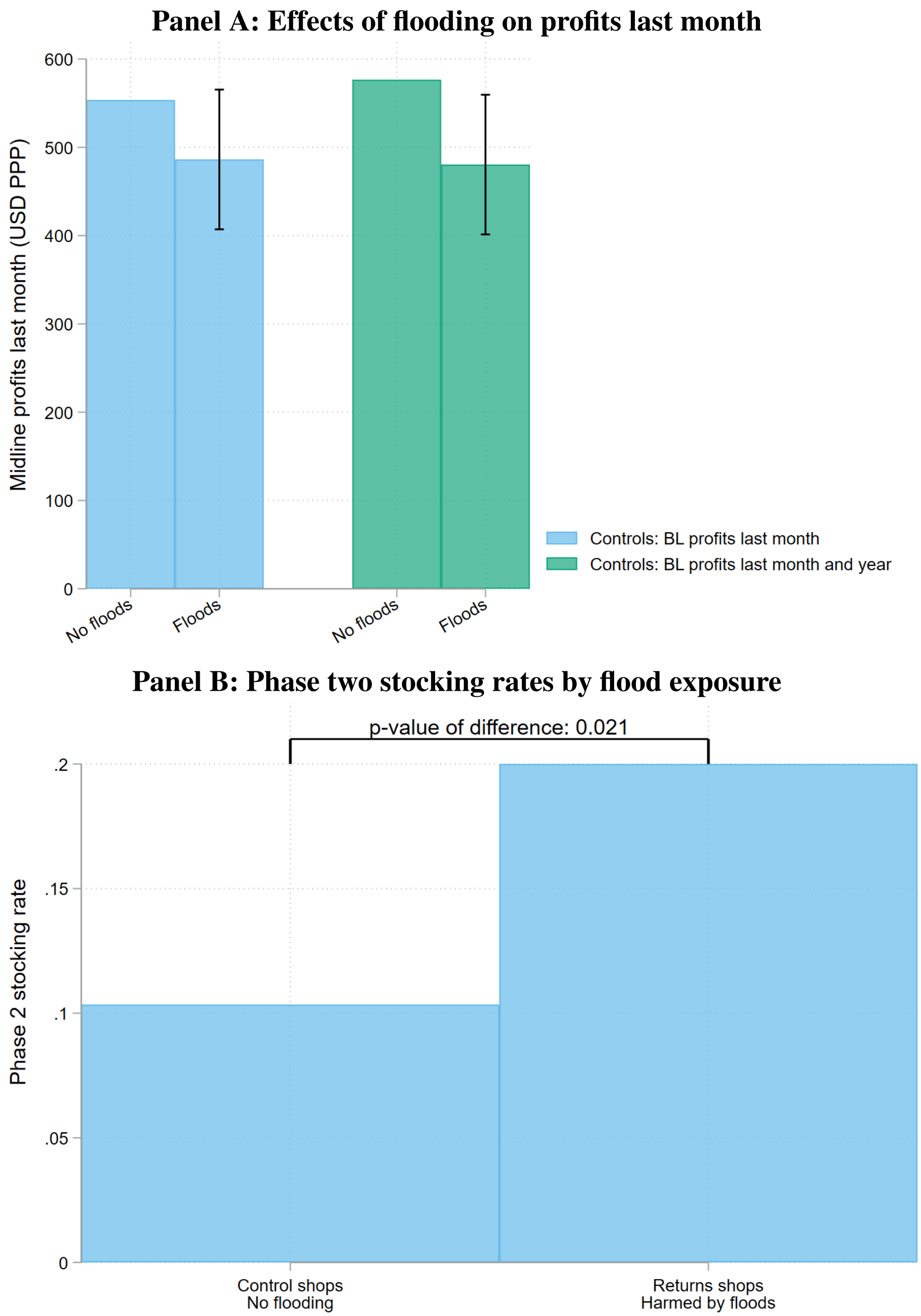
Appendix

Figure A1: Learning experiment: Treatment effects on phase one helmet adoption plus interest



This figure reports the rate of shops that stocked helmets in phase one, from the study or a different source, or that requested the field officer call them back in two days for a final purchase decision. The first bar presents the control stocking rate. The second reports the adoption rate among shops receiving the *supplier commitment*. The third bar is similar, but reports the effect of *returns*. The fourth bar reports the estimated effect of receiving both treatments.

Figure A2: Learning experiment: Phase two stocking effects by flood exposure



Panel A plots profits the month before the midline survey among shops that reported they were negatively impacted by severe flooding and those that were not. Both results control for strata and industry fixed effects, controls for distance to the manufacturer, log revenue, days open per week, and indicators for stocking a new product in the year before the baseline survey, selling multiple products at baseline, and having space to store helmets without stocking less of another item. The first estimate further controls for baseline profits last month, and the second controls for baseline monthly and annual profits. Panel B reports control, no flooding stocking and returns, flooding stocking rates. The p-value for equality is from a regression of stocking on returns among these firm types, including the controls used in profit estimates.

Table A1: Summary statistics and baseline balance

	Insurance experiment		Learning experiment Returns		Learning experiment Supplier commitment	
Variable	(1) Control mean [SD]	(2) T - C	(3) Control mean [SD]	(4) T - C	(5) Control mean [SD]	(6) T - C
Respondent age	34.18	0.77	28.92	-0.77	28.68	-1.32
	[10.86]	(1.12)	[6.75]	(1.85)	[6.97]	(2.08)
Female	0.30	0.03	0.28	-0.03	0.27	0.00
	[0.46]	(0.05)	[0.45]	(0.03)	[0.44]	(0.03)
Years of education	12.78	0.19	13.23	0.16	13.34	-0.02
	[2.79]	(0.27)	[2.48]	(0.16)	[2.54]	(0.17)
Business age	4.53	-0.22	5.29	0.18	5.68	-0.57
	[5.46]	(0.52)	[5.45]	(0.36)	[5.77]	(0.36)
Motorcycle spares shop	0.59	-0.01	0.37	-0.02	0.37	-0.00
	[0.49]	(0.05)	[0.48]	(0.03)	[0.48]	(0.03)
Revenue last month	1,235.46	-132.01	1,459.28	153.17	1,590.50	-125.91
	[1,482.82]	(152.00)	[2,107.67]	(159.08)	[2,458.95]	(168.67)
Costs last month	972.27	15.70	655.92	138.05*	747.21	-49.55
	[1,280.78]	(185.11)	[835.61]	(79.85)	[1,045.04]	(83.74)
Profits last month	508.77	-46.39	622.90	61.17	674.88	-32.39
	[593.92]	(57.29)	[977.04]	(66.44)	[937.10]	(66.60)
1(paid employees)	0.22	0.05	0.25	-0.01	0.25	-0.01
	[0.41]	(0.04)	[0.43]	(0.03)	[0.43]	(0.03)
Wage bill last week	58.26	10.85	34.69	-5.05	21.60	18.33**
	[348.89]	(29.99)	[164.33]	(10.47)	[47.16]	(9.01)
Hours open/week	77.41	0.24	85.35	-0.16	86.06	-1.74*
	[19.50]	(1.57)	[15.37]	(0.95)	[15.07]	(0.95)
KM to helmet seller	NA	NA	2.24	0.06	2.28	-0.06
	–	–	[3.17]	(0.15)	[3.27]	(0.15)
Know helmet seller	0.56	-0.10**	0.25	0.03	0.27	0.00
	[0.50]	(0.05)	[0.44]	(0.03)	[0.44]	(0.03)
Min. to motorcycle taxi stand	NA	NA	3.16	-0.07	3.00	0.24
	–	–	[6.01]	(0.36)	[5.77]	(0.36)
New product in last year	0.28	0.03	0.34	-0.01	0.36	-0.05
	[0.45]	(0.04)	[0.48]	(0.03)	[0.48]	(0.03)
E[sales]	3.43	-0.04	3.75	0.06	3.89	-0.21
	[1.73]	(0.13)	[2.01]	(0.13)	[2.06]	(0.14)
V(sales)	2.96	-0.13	1.26	0.08	1.33	-0.06
	[2.88]	(0.25)	[1.84]	(0.12)	[1.80]	(0.12)
Observations	172	173	461	468	463	466
Joint p-value		0.87	0.91		0.47	

$*\,p < 0.1,\ **\ p < .05\ \ ***\,p < .01$. Standard deviations in brackets. Standard errors in parenthesis.

Column (1) reports the mean and standard deviation of the indicated variable across control enterprises in the Insurance Experiment. Columns (3) and (5) report the same value across control surveys in the *returns* and *supplier commitment* arms of the Learning Experiment. Columns (2), (4) and (6) report the difference between the treatment and control group in the sample and arm as denoted in the table header, estimated via OLS including strata fixed effects. The last row reports the p-value associated with a test for joint orthogonality, constructed by estimating a seemingly unrelated regression model then estimating a Wald test to allow for missing variables.

Table A2: Summary statistics and balance
Spillover survey

	Motorcycle shops		All shops	
	(1)	(2)	(3)	(4)
Variable	Control mean [SD]	T - C	Control mean [SD]	T - C
Respondent age	35.04	0.90	35.20	-0.00
	[10.41]	(0.99)	[10.59]	(0.82)
Business age	4.58	-0.33	4.95	-0.45
	[4.11]	(0.48)	[5.04]	(0.39)
Revenue last month	990.57	14.04	982.30	-60.27
	[759.23]	(101.57)	[805.51]	(82.68)
Costs last month	729.83	-53.75	710.49	-24.51
	[825.97]	(91.45)	[812.68]	(81.38)
Profits last month	465.55	-8.88	458.62	-27.24
	[342.76]	(50.30)	[349.98]	(41.93)
1(Employees)	0.30	-0.04	0.29	-0.03
	[0.46]	(0.06)	[0.45]	(0.04)
Helmet storage capacity	22.63	-10.77*	21.46	-2.12
	[57.87]	(6.07)	[51.71]	(6.64)
Owner hours of work/week	76.55	-1.30	76.71	-1.64
	[15.39]	(1.98)	[15.68]	(1.83)
Motorcycle spares shop	NA	NA	0.67	-0.20***
	–	–	[0.47]	(0.05)
Observations	219	157	327	338
Joint p-value		0.51		0.01

$*\, p < 0.1,\ **\, p < .05\ \ ***p < .01$. Standard deviations in brackets. Standard errors in parenthesis clustered by market.

Column (1) reports the mean and standard deviation of the indicated variable across enterprises in pure control markets, restricting the sample to motorcycle spare part/repair shops. Column (3) is similar but with no sample restriction. Columns (2) and (4) report the difference between the treatment and control group in the sample and arm as denoted in the table header, estimated via OLS including county fixed effects. The last row reports the p-value associated with a test for joint orthogonality, constructed by estimating a seemingly unrelated regression model then estimating a Wald test to allow for missing variables. 35 shops that sold motorcycle helmets prior to the beginning of the study are excluded.

Table A3: Baseline beliefs about helmet profitability

	Mean	SD	25th percentile	50th percentile	75th percentile	Obs
Panel A: Insurance experiment						
Pr(Loss| Stock 10 helmets)	0.307	0.233	0.100	0.300	0.500	350
Pr(Helmets profits > current stock)	0.504	0.219	0.400	0.500	0.600	349
Pr(Sell out in 8 weeks|Stock)	0.442	0.256	0.200	0.450	0.600	350
$\mathbb{E}$[8 week revenue] − stock cost	0.200	16.189	-8.850	1.996	10.672	340
8 week expected sales	3.436	1.800	2.050	3.250	4.575	340
8 week SD sales	1.489	0.824	0.829	1.303	2.041	340
Panel B: Learning experiment						
Pr(Loss | Buy 10 helmets)	0.369	0.241	0.200	0.300	0.500	922
Pr(Helmets profits > current stock)	0.506	0.214	0.400	0.500	0.600	922
Pr(Representative firm restocked)	0.442	0.205	0.333	0.444	0.556	929
$\mathbb{E}$[1 month revenue] − stock cost	-5.078	29.082	-28.330	-3.384	16.139	873
1 month expected sales	3.765	2.052	2.300	3.500	5.000	873
1 month SD sales	0.928	0.658	0.500	0.829	1.221	873

This table reports baseline beliefs about helmet profitability. The first row in each panel is the firm's belief about the likelihood that they would lose money, inclusive of the opportunity cost of funds, if they stocked 10 helmets. The second row is the likelihood that stocking 10 helmets would raise the firm's profits, inclusive of any losses from stocking less of other items. The third row in Panel A denotes the firm's belief about their probability of selling 3 helmets in 8 weeks if they stocked them, and in Panel B the row denotes the enterprise's perceived likelihood that a representative shop would restock if given helmets to learn about the market. The fourth row captures expected revenue net of stocking cost for the study offers over an 8 week period in Panel A and a 1 month period in Panel B. The final two rows present beliefs about expected sales and the standard deviation of sales, measured with a frequentist mechanism, over an 8 week and 1 month period respectively.

Table A4: Learning experiment: Heterogeneous effects by presence of a pre-existing seller

	(1)	(2)	(3)	(4)
	Stocked in Phase 1		Stocked in Phase 2	
Returns	0.127***	0.126***	0.078**	0.077**
	(0.032)	(0.031)	(0.034)	(0.033)
Supplier commitment	0.085***	0.085***	0.069**	0.055*
	(0.029)	(0.028)	(0.032)	(0.030)
Returns × Supplier commitment	-0.099**	-0.079*	-0.080	-0.051
	(0.048)	(0.046)	(0.049)	(0.047)
BL seller	0.125**	0.092*	0.092*	0.029
	(0.050)	(0.047)	(0.053)	(0.048)
Returns × BL seller	-0.122	-0.143**	-0.038	-0.047
	(0.075)	(0.072)	(0.080)	(0.074)
Supplier commitment × BL seller	-0.128*	-0.099	-0.112	-0.043
	(0.073)	(0.071)	(0.077)	(0.071)
Returns × Commitment	0.166	0.142	0.074	0.019
× BL seller	(0.107)	(0.104)	(0.111)	(0.106)
Observations	929	929	929	929
Control mean	0.034	0.034	0.073	0.073
Controls	Strata FE	Yes	Strata	Yes

$* \, p < 0.1, \;\; ** \; p < .05, \;\; *** p < .01$

This table plots heterogeneous treatment effects on helmet stocking and helmet market entry from the Learning Experiment based on proximity to an existing helmet seller. "BL Seller" indicates that a shop reported that a pre-existing helmet seller operated near them at baseline. Columns (1)-(2) reports effects on phase 1 stocking, supplied by the study or any other source. Column (3)-(4) report phase 2 stocking effects. The dependent variable in columns (3)-(4) is coded to 0 if a shop withdrew from the study because they did not wish to sell helmets or if the outcome was confirmed without the survey. All estimates include strata fixed effects. Columns (2) and (4) include industry fixed effects, controls for distance to the manufacturer, log revenue, days open per week, and indicators for stocking a new product in the year before the baseline survey, selling multiple products at baseline, and having space to store helmets without stocking less of another item.

Table A5: Learning experiment: Belief-adoption relationship robustness to controls and sample

	Sample: Pr(Loss|Stock 5) > 0			All	Non-degenerate	All
	(1) Revenue	(2) Sales	(3) Sales	(4) Sales	(5) Sales	(6) Sales
Returns	-0.193	-0.006	0.024	0.015	0.022	-0.022
	(0.213)	(0.073)	(0.072)	(0.067)	(0.086)	(0.076)
$\mathbb{E}$[sales/revenue]	0.052*	0.021	0.021	0.008	0.023*	0.023
	(0.028)	(0.015)	(0.015)	(0.008)	(0.013)	(0.015)
σ(sales/revenue)	-0.041**	-0.042**	-0.031*	-0.022	-0.054**	-0.054**
	(0.020)	(0.020)	(0.018)	(0.017)	(0.024)	(0.022)
Returns × $\mathbb{E}$[sales/revenue]	-0.001	0.014	0.008	0.013	-0.018	0.013
	(0.065)	(0.033)	(0.033)	(0.015)	(0.021)	(0.033)
Returns × σ(sales/revenue)	0.045	0.039	0.032	0.023	0.092*	0.051
	(0.038)	(0.053)	(0.053)	(0.039)	(0.053)	(0.054)
Pr(Loss|Stock 5) = 0						-0.071
						(0.109)
Returns × Pr(Loss|Stock 5) = 0						0.239
						(0.220)
$\mathbb{E}$[sales/revenue] × Pr(Loss|Stock 5) = 0						-0.010
						(0.024)
σ(sales/revenue) × Pr(Loss|Stock 5) = 0						0.101
						(0.086)
Returns × $\mathbb{E}$[sales/revenue] × Pr(Loss|Stock 5) = 0						-0.022
						(0.047)
Returns × σ(sales/revenue) × Pr(Loss|Stock 5) = 0						-0.136
						(0.146)
Observations	283	304	304	427	286	427
Control mean	0.068	0.068	0.068	0.068	0.068	0.068
Controls	Full	DPLASSO	None	Full	Full	Full

$* \, p < 0.1, \;\; ** \; p < .05 \;\; *** \, p < .01$

Robust standard errors in parenthesis. This table reports the relationship between helmet stocking and belief uncertainty under different measures and controls. The dependent variable equals 1 if the firm stocked helmets during phase 1. The sample excludes *supplier commitment* firms. In column (1), the independent variables are log(1 + expected helmet revenue) and the same transform of variance. Columns (2)-(6) examine expected sales and SD sales. Columns (1)-(3) exclude shops with 0 loss probability from stocking 5 helmets. Columns (4)-(6) include all observations, but 5 screens for comprehension of belief elicitation by dropping respondents reporting degenerate belief distributions (Var(sales) $\leq$ 0.25). Covariates in column (2) were selected with double post LASSO. Columns (1) and (4)-(6) include controls for storage space, education, baseline profits, respondent characteristics, and firm characteristics.

Table A6: Learning experiment: Beliefs of adopters with versus without returns

	(1) Belief levels	(2) Belief logs
$\mathbb{E}$[sales/revenue]	-0.001	
	(0.038)	
σ(sales/revenue)	0.202*	
	(0.111)	
log(1 + E[sales])		-0.012
		(0.175)
log(1 + V(sales))		0.246**
		(0.113)
Know helmet seller	-0.309**	-0.313**
	(0.148)	(0.140)
Observations	46	46

$* \, p < 0.1, \;\; ** \; p < .05 \;\; *** \, p < .01$

Bootstrapped standard errors in parenthesis (500 draws). This table reports beliefs about demand for helmets of adopters with and without *returns*. The dependent variable in each column is an indicator equal to 1 if the agent received *returns*, and the sample is restricted to those that stocked helmets in phase one and did not receive the supplier commitment offer. Robust standard errors in parenthesis.

Table A7: Learning experiment: Elasticity of outcomes w.r.t. expectation (phase 1 adopters)

	log(Sales)		log(Revenue)	
	(1)	(2)	(3)	(4)
	BL $\rightarrow$ ML	ML $\rightarrow$ EL	BL $\rightarrow$ ML	ML $\rightarrow$ EL
log(Expectation at BL)	0.279**		0.239*	
	(0.122)		(0.131)	
log(Expectation at ML)		0.690***		0.317**
		(0.139)		(0.126)
Constant	0.921***	0.425***	3.298***	2.717***
	(0.184)	(0.139)	(0.587)	(0.513)
Observations	98	91	97	93
Adjusted R^2	0.045	0.239	0.028	0.038
Pr(η BL= η ML)		0.047		0.666

$* \, p < 0.1, \;\; ** \; p < .05 \;\; *** \, p < .01$

Robust standard errors in parenthesis. This table reports the elasticity of realized helmet sales or revenue with respect to firms' expectation among phase 1 adopters. Columns (1)-(2) report beliefs about sales while columns (3)-(4) consider beliefs about revenue, constructed from expected sales and expected prices. Beliefs were elicited using a frequentist method. Columns (1) and (3) report regressions of the log of sales or revenue from baseline to midline on log baseline expectations of this value. Columns (2) and (4) report regressions of the log of sales or revenue from midline to endline on log midline expectations of this value. The p-values in columns (2) and (4) report results of a test that the baseline and midline elasticities are equal, estimated using seemingly unrelated regressions.

Table A8: Learning experiment: Helmet profit dynamics and selling costs

	(1) Helmet profits per month EL	(2) Total costs selling helmets	(3) Helmet costs net est. stock	(4) Any fixed costs	(5) Fixed costs	(6) Δ capacity utilization
Stocked in phase 1	-9.419					0.046*
	(5.956)					(0.024)
Estimated cost of helmet stock (PPP)		0.451***				
		(0.167)				
Returns			-8.747	0.050	-0.632	
			(8.575)	(0.060)	(2.009)	
Observations	127	132	132	128	128	712
Dep. var. mean	21.552	73.735	-10.925	0.133	2.381	0.064

$* \, p < 0.1, \;\; ** \; p < .05 \;\; *** \, p < .01$

Robust standard errors in parenthesis. The sample consists of shops that reported selling helmets, supplied by the study or a different source. Column (1) regresses helmet profits accumulated between midline and endline on an indicator equal to 1 if the shop stocked helmets in the month after baseline to test for learning by doing. Column (2) regresses total reported costs of stocking helmets on the cost of the helmet stock, measured via administrative data or estimated. The dependent variable in column (3) equals reported helmet costs net of stock price. Column (4) examines whether shops reported any fixed costs of helmet sales, and column (5) reports total fixed costs. Column (6) reports the change in capacity utilization of the firm from baseline to endline, measured as worst week over best week profits in the last month.

Table A9: Learning experiment: Helmet adoption rates and proximity of other sellers

	(1) Ordered Baseline	(2) Ordered Midline	(3) Ordered Endline	(4) Ordered	(5) Observed Seller
Know helmet seller	0.129**				0.325***
	(0.055)				(0.034)
Know of large seller, BL	-0.198***				
	(0.068)				
Noticed seller by ML		0.179**			
		(0.085)			
Noticed helmet seller by EL			0.201***		
			(0.063)		
Study adopter within 0.25km				0.031**	0.138***
				(0.014)	(0.031)
Sample shops in quarter km				-0.001***	
				(0.000)	
Study adopter 0.25-0.5km					-0.003
					(0.030)
Observations	231	165	165	771	929
Control mean	0.034	0.028	0.029	0.010	0.237
Controls	DPLASSO	DPLASSO	DPLASSO	DPLASSO	None
Sample restriction	Control	No BL order control	No BL order control	No BL order	

$* p < 0.1, \;** \; p < .05, \; ***p < .01$

Robust standard errors in parenthesis. This table reports correlations between proximity to a shop selling helmets and the firm's own stocking decisions. In column (1), the dependent variable equals 1 if the shop stocked in phase one, in column (2) the dependent variable captures stocking between a month after baseline and a month after midline, column (3) looks at stocking more than a month after midline, and column (4) captures ever stocking. Column (5) indicates that the shop reported knowing a helmet seller in their location at midline. Noticed seller indicates that the shop did not know of a shop selling helmets before the indicated survey, then observed one. Columns (1) - (3) include only untreated shops, with (2) and (3) further excluding those that stocked in the 4 weeks after baseline. Column (4) includes all firms that did not stock helmets within a month of baseline. Column (1)-(4) includes enterprise and shopkeeper demographic controls selected using double-post selection LASSO (Belloni et al., 2014).

Table A10: Learning experiment: Effect of returns on helmet access

	(1)	(2)	(3)	(4)	(5)	(6)
	Helmet shop in market at endline	Ever helmet shop in market	Cumulative helmets stocked at endline		Cumulative helmets sold at endline	
Returns	0.077**	0.135***	1.870**	2.828***	1.374*	2.111**
	(0.038)	(0.040)	(0.835)	(1.014)	(0.747)	(0.951)
Supplier commitment	0.044	0.083**	0.663	1.281***	0.322	0.763**
	(0.038)	(0.042)	(0.480)	(0.460)	(0.348)	(0.343)
Returns × Commitment	-0.035	-0.096	-1.455	-2.745**	-1.238	-2.105**
	(0.054)	(0.058)	(0.978)	(1.159)	(0.829)	(1.043)
BL Seller				2.013		1.492
				(1.322)		(0.967)
Returns × BL Seller				-3.651**		-2.828**
				(1.750)		(1.357)
Commitment × BL Seller				-2.508		-1.817
				(1.579)		(1.141)
Returns × Commitment × BL seller				4.876**		3.327**
				(2.359)		(1.690)
Observations	929	929	929	929	926	926
Control mean	0.242	0.314	1.525	0.689	0.966	0.407

$* p < 0.1, \; ** \; p < .05, \; ***p < .01$

Robust standard errors in parenthesis. This table reports evidence of the effect of the intervention on helmet access. Column (1) equals 1 if the respondent shop entered the helmet market by endline or reported that a shop near them sold helmets. Column (2) is similar but equals 1 if the respondent ever stocked helmets or ever reported that a shop near them stocked helmets. Columns (3) - (4) examine the total number of helmets stocked by the shop by endline, and columns (5) - (6) examine total helmet sales by endline. In 3 cases, shops stocked helmets and did not complete the endline survey because their enterprises closed, so endline sales are imputed using midline values. Estimates include strata fixed effects and controls for industry, proximity to the manufacturer, days open per week at baseline, log baseline revenue, indicators for adopting a new product in the year before the survey, selling multiple products at baseline and having space to store helmets. I also control for exposure to floods at midline.

A Model Details

A.1 Lagrangian and solution to optimal investment

The Lagrangian of the entrepreneur's optimization problem may be written as

$$\begin{aligned}\mathcal{L} = u(c_1) &+ \lambda_1\left[(1+r)a_0 - c_1 - a_1 - w_s I_{s1} - w_n I_{n1}\right]\\ &+ \kappa_{a1}\left[a_1 - \bar{a}\right] + \kappa_{\chi 1}\left[I_{n1}\cdot(I_{n1}-\chi)\right] + \iota_{s1} I_{s1}\\ &+ \delta\,\mathbb{E}_{\theta,\nu_{s2},\nu_{n2}}\left\{u(c_2) + \lambda_2\left[\pi_s(I_{s1},\nu_{s2}) + \pi_n(I_{n1},\nu_{n2}+\theta) + (1+r)a_1 - c_2 - a_2 - w_s I_{s2} - w_n I_{n2}\right]\right.\\ &\left.+\ \kappa_{a2}\left[a_2 - \bar{a}\right] + \kappa_{\chi 2}\left[I_{n2}\cdot(I_{n2}-\chi)\right] + \iota_{s2} I_{s2} \middle| \mathcal{I}_1\right\}\\ &+ \delta\,\mathbb{E}_{\nu_{s2},\nu_{n2},\theta}\left[V^*(y_t,\theta) - \bar{R}(y_2,\mathcal{I}_2(I_{n1})|\mathcal{I}_1\right]\end{aligned} \tag{15}$$

Differentiating, we get the set of first order conditions

$$\begin{aligned}&\mathcal{L}_c : u'(c_t) = \lambda_t\\ &\mathcal{L}_a : \lambda_t = \mathbb{E}_t\,\lambda_{t+1} + \kappa_{at}\\ &\mathcal{L}_{I_s} : \lambda_t w_s + \iota_{st} = \delta\,\mathbb{E}_t\left[\lambda_{t+1}\frac{\partial}{\partial I_{st}}\pi_s(I_{st},\nu_{st+1})\right]\\ &\mathcal{L}_{I_n} : \lambda_t w_n + \kappa_{\chi t}(2I_{nt}-\chi) = \delta\,\mathbb{E}_t\left[\lambda_{t+1}\frac{\partial}{\partial I_{nt}}\pi_s(I_{nt},\nu_{nt+1}+\theta)\right] - \delta^2\,\mathbb{E}_t\left[\frac{\partial}{\partial I_{nt}}\bar{R}(y_{t+1},\mathcal{I}_{t+1}(I_{nt}))\right]\end{aligned} \tag{16}$$

where $\mathbb{E}_t[\cdot] = \mathbb{E}[\cdot|\mathcal{I}_t]$.

From the FOCs for consumption and assets, we get the consumption Euler equation $u'(c_t) = \mathbb{E}_t\,u'(c_{t+1}) + \kappa_{at}$. By Karush–Kuhn–Tucker conditions, whenever capital constraints are not binding, $u'(c_t) = \mathbb{E}_t\,u'(c_{t+1})$, and whenever they bind $\kappa_{at} > 0 \Rightarrow u'(c_t) > \mathbb{E}_t\,u'(c_{t+1})$, so $u'(c_t) \geq \mathbb{E}_t\,u'(c_{t+1})$.

Next solving for optimal investment in the safe good,

$$\delta\,\mathbb{E}_t\left[\lambda_{t+1}\frac{\partial}{\partial I_{st}}\pi_s(I_{st},\nu_{st+1})\right]=\lambda_t w_s+\iota_{st}$$
$$\delta\,\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\frac{\partial}{\partial I_{st}}\pi_s(I_{st},\nu_{st+1})\right]=w_s+\frac{1}{u'(c_t)}\iota_{st}$$
$$\delta\left\{\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right]\mathbb{E}_t\left[\frac{\partial}{\partial I_{st}}\pi_s(I_{st},\nu_{st+1})\right]+\right.$$
$$\left.\frac{1}{u'(c_t)}Cov_t\left(u'(c_{t+1}),\frac{\partial}{\partial I_{st}}\pi_s(I_{st},\nu_{st+1})\right)\right\}=w_s+\frac{1}{u'(c_t)}\iota_{st}$$

where ι_{st} is a Lagrangian multiplier ensuring non-negative investment, which will not bind and be zero whenever the safe product is stocked. Optimal investment in the new good is

$$\delta\left\{\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right]\mathbb{E}_t\left[\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt},\nu_{nt+1}+\theta)\right]+\frac{1}{u'(c_t)}Cov_t\left(u'(c_{t+1}),\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt},\nu_{nt+1})\right)-\right.$$
$$\left.\frac{1}{u'(c_t)}\delta\,\mathbb{E}_t\left[\frac{\partial}{\partial I_{nt}}\bar{R}(y_{t+1},\mathcal{I}_{t+1}(I_{nt}))\right]\right\}=w_n+\frac{1}{u'(c_t)}\iota_{\chi t}(2I_{nt}-\chi)$$

given that this value satisfies the participation constraint that the utility exceeds that from investing 0, where this verification is needed since minimum order sizes create a non-convex choice set. This Euler equation characterizes optimal investment given adoption. Any change in primitives that raises the optimal inventory level given entry, I^*_{nt}, strictly raises the value of entry, and thus weakly increases the probability of stocking. Therefore comparative statics on this Euler equation are sufficient to study entry probability.

A.2 Derivation of the derivative of Bayesian regret w.r.t investment

This section shows that

$$\frac{\partial}{\partial I_{nt}}\mathbb{E}\left[\bar{R}(y_{t+1},\mathcal{I}_{t+1}(I_{nt}))|\mathcal{I}_t\right]=-\frac{1}{2}Cov\left(R_{t+1}(\theta),(\theta-\mu_t)'I_{nt}^{-2}V_{nt}^{-1}\Sigma_x V_{nt}^{-1}(\theta-\mu_t)\middle|\mathcal{I}_t\right)\leq 0$$

where $V_{nt}=I_{nt}^{-1}\Sigma_x+\Sigma_n$. The inequality is typically strict whenever expected regret is positive.

Consider an agent in time t making investment I_{nt}. In period $t+1$, they will receive the signal from this investment, which will affect their Bayesian regret associated with decisions beginning in period $t+2$, $\bar{R}(y_{t+1},\mathcal{I}_{t+1}(I_{nt}))$. The aim is to find

$$\frac{\partial}{\partial I_{nt}}\mathbb{E}\left[\bar{R}(y_{t+1},\mathcal{I}_{t+1}(I_{nt}))|\mathcal{I}_t\right]$$

Let $c^*_\tau(x_t, \theta)$ denote the expected utility maximizing consumption path given θ and $\bar{c}_\tau(x_t, \mathcal{I}_{nt+1}, \theta)$ be consumption along the agent's planned expected utility maximizing path if θ is the true parameter. Applying the definition of Bayesian regret and the Law of Iterated Expectations

$$\bar{R}(y_{t+1}, \mathcal{I}_{t+1}(I_{nt})) = \sum_{\tau=t+2}^{\infty} \delta^{\tau-t-1} \mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_s} \left[\int_{\mathbb{R}^k} (u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))) f(\theta | \mathcal{I}_\tau(x(I_{nt}))) d\theta \Big| \mathcal{I}_{t+1}, x(I_{nt}) \right]$$

The expectation over $\mathcal{I}_\tau$ captures expected future learning about demand, due to planned investment along the consumption path or learning from neighbors. $x(I_{nt})$ gives the realized draw of x given I_{nt}. Plugging this in

$$\begin{aligned} &\mathbb{E}\left[\bar{R}(y_{t+1}, \mathcal{I}_{t+1}(I_{nt})) | \mathcal{I}_t\right] = \\ &\quad \mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_s} \left[\sum_{\tau=t+2}^{\infty} \delta^{\tau-t-1} \int_{\mathbb{R}^k} \int_{\mathbb{R}^k} (u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))) f(\theta | \mathcal{I}_\tau(x(I_{nt}))) f(x(I_{nt}) | \mathcal{I}_t) \, d\theta \, dx(I_{nt}) \Big| \mathcal{I}_t \right] \end{aligned}$$

The integral over $x(I_{nt})$ captures the fact that at time t, the agent does not know what draw $x(I_{nt})$ they will receive, so they must take an expectation over the possible signals. Focusing on some arbitrary τ,

$$\frac{\partial}{\partial I_{nt}} \bar{R}_\tau \equiv \frac{\partial}{\partial I_{nt}} \mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_s} \left[\int_{\mathbb{R}^k} \int_{\mathbb{R}^k} (u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))) f(\theta | \mathcal{I}_\tau(x(I_{nt}))) f(x(I_{nt}) | \mathcal{I}_t) \, d\theta \, dx(I_{nt}) \Big| \mathcal{I}_t \right]$$

where $f(\theta | \mathcal{I}_\tau(x(I_{nt})))$ is conditioning on the draw of $x(I_{nt})$. I apply the Law of Iterated Expectations and integrate over the distribution of expected draws $f(x(I_{nt}) | \mathcal{I}_t)$. The notation $\mathbb{E}_{\mathcal{I}_\tau}$ refers to expected draws to the information set, capturing expected evolution aside from the signal $x(I_{nt})$, including information from peers and other planned investments in the new product. By the Envelope Theorem,

$$\begin{aligned} \frac{\partial}{\partial I_{nt}} \bar{R}_\tau = \mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_s} \Bigg[&\int_{\mathbb{R}^k} \int_{\mathbb{R}^k} (u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))) \frac{\partial f(\theta | \mathcal{I}_\tau(x(I_{nt})))}{\partial I_{nt}} f(x(I_{nt}) | \mathcal{I}_t) \, d\theta \, dx(I_{nt}) \\ &+ \int_{\mathbb{R}^k} \int_{\mathbb{R}^k} (u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))) f(\theta | \mathcal{I}_\tau(x(I_{nt}))) \frac{\partial f(x(I_{nt}) | \mathcal{I}_t)}{\partial I_{nt}} \, d\theta \, dx(I_{nt}) \Big| \mathcal{I}_t \Bigg] \end{aligned}$$

The agent knows $x(I_{nt}) \sim \mathcal{N}(\theta_0, V_{nt})$ where $V_{nt} = I_{nt}^{-1}\Sigma_x + \Sigma_n$, but does not know θ_0. Expected signal draws are thus distributed $\mathcal{N}(\mu_t, \Sigma_t + V_{nt})$ where μ_t and Σ_t are posteriors given $\mathcal{I}_t$. I first show that the second term is 0, which is intuitively the case since a change in investment level should not change the location of the signal. Formally,

$$
\begin{aligned}
\int_{\mathbb{R}^k} \frac{\partial f_\tau(x(I_{nt}))}{\partial I_{nt}} dx(I_{nt}) &= \int_{\mathbb{R}^k} \frac{1}{2} I_{nt}^{-2} f(x(I_{nt})|\mathcal{I}_t) \left\{ Tr\left([V_{nt} + \Sigma_t]^{-1} \Sigma_n \right) \right. \\
&\quad \left. - \left[(x-\mu_t)' [V_{nt} + \Sigma_t]^{-1} \Sigma_n [V_{nt} + \Sigma_t]^{-1} (x - \mu_t) \right] \right\} dx(I_{nt}) \\
&= \frac{1}{2} I_{nt}^{-2} \left\{ Tr\left([V_{nt} + \Sigma_t]^{-1} \Sigma_n \right) - Tr\left([V_{nt} + \Sigma_t]^{-1} \Sigma_n \right) \right\} = 0
\end{aligned}
$$

where the first line applies Jacobi's formula for the derivative of a trace and the second line leverages results about the expectation of a quadratic form.

Now I turn to the first term in the derivative. This will be non-zero since the agent expects investment to lower posterior variance, which affects $u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))$. For a fixed draw $x(I_{nt})$,

$$
\begin{aligned}
\frac{\partial f(\theta|\mathcal{I}_\tau(x(I_{nt})))}{\partial I_{nt}} &= -\frac{1}{2} Tr\left(\Sigma_\tau^{-1} \frac{\partial \Sigma_\tau}{\partial I_{nt}} \right) f(\theta|\mathcal{I}_\tau) + \frac{1}{2} \left[(\theta - \mu_t)' \Sigma_t^{-1} \frac{\partial \Sigma_t}{\partial I_{nt}} \Sigma_t^{-1} (\theta - \mu_t) \right] f(\theta|\mathcal{I}_\tau) \\
&\quad + (\theta - \mu_\tau)' \Sigma_\tau^{-1} \frac{\partial \mu_\tau}{\partial I_{nt}} f(\theta|\mathcal{I}_\tau)
\end{aligned}
$$

From the known form of the posterior mean and variance, it follows that

$$
\begin{aligned}
\frac{\partial \mu_\tau}{\partial I_{nt}} &= I_{nt}^{-2} \Sigma_\tau V_{nt}^{-1} \Sigma_x V_{nt}^{-1} (x(I_{nt}) - \mu_\tau) \\
\frac{\partial \Sigma_t}{\partial I_{nt}} &= -I_{nt}^{-2} \Sigma_\tau V_{nt}^{-1} \Sigma_x V_{nt}^{-1} \Sigma_\tau
\end{aligned}
$$

Terms with $\frac{\partial \mu_\tau}{\partial I_{nt}}$ will be zero, reflecting the fact that the agent doesn't expect a marginal increase in investment to change the location parameter of beliefs. In particular,

$$
\begin{aligned}
&\mathbb{E}_\theta \left[(u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta)))(\theta - \mu_\tau)' \Sigma_\tau^{-1} \frac{\partial \mu_\tau}{\partial I_{nt}} \big| x(I_{nt}) \right] \\
&= \mathbb{E}_\theta \left[(u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))) \big| x(I_{nt}) \right] \underbrace{\mathbb{E}_\theta \left[(\theta - \mu_\tau)' \big| x(I_{nt}) \right]}_{=0} \Sigma_\tau^{-1} \frac{\partial \mu_\tau}{\partial I_{nt}} \\
&\quad + Cov \left[(u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))), (\theta - \mu_\tau)' \big| x(I_{nt}) \right] \Sigma_\tau^{-1} \frac{\partial \mu_\tau}{\partial I_{nt}} \\
&= Cov \left[(u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))), (\theta - \mu_\tau)' \big| x(I_{nt}) \right] I_{nt}^{-2} V_{nt}^{-1} \Sigma_x V_{nt}^{-1} (x(I_{nt}) - \mu_\tau)
\end{aligned}
$$

Since $\mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_t}[x(I_{nt}) - \mu_\tau] = 0$, it follows that

$$
\mathbb{E}_{\mathcal{I}_\tau, \nu_n, \nu_t} \left[\mathbb{E}_\theta \left[(u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta)))(\theta - \mu_\tau)' \Sigma_\tau^{-1} \frac{\partial \mu_\tau}{\partial I_{nt}} \big| x(I_{nt}) \right] \right] = 0
$$

Now denote $P_{nt} = V_{nt}^{-1}$ so that $\frac{\partial P_{nt}}{\partial I_{nt}} = I_{nt}^{-2} P_{nt} \Sigma_x P_{nt}$. Substituting for $\frac{\partial \Sigma_\tau}{\partial I_{nt}}$, collecting the terms containing this expression, and evaluating

$$\mathbb{E}_\theta\left[(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)))\cdot\left(\frac{1}{2}Tr\left(\frac{\partial P_{nt}}{\partial I_{nt}}\Sigma_\tau\right)-\frac{1}{2}(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)\right)\big|x(I_{nt})\right]$$

$$=\mathbb{E}_\theta\left[(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)))\big|x(I_{nt})\right]\cdot\mathbb{E}_\theta\left[\frac{1}{2}Tr\left(\frac{\partial P_{nt}}{\partial I_{nt}}\Sigma_\tau\right)-\frac{1}{2}\underbrace{(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)}_{=Tr\left(\frac{\partial P_{nt}}{\partial I_{nt}}\Sigma_\tau\right)}\big|x(I_{nt})\right]$$

$$-\frac{1}{2}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)\big|x(I_{nt})\right)$$

$$=-\frac{1}{2}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)\big|x(I_{nt})\right)$$

Substituting,

$$\frac{\partial}{\partial I_{nt}}\bar{R}_\tau=-\frac{1}{2}\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t}\left[\int_{\mathbb{R}^k}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)\big|x(I_{nt})\right)dx(I_{nt})\big|\mathcal{I}_t\right]$$

$$=-\frac{1}{2}\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t}\Bigg[\int_{\mathbb{R}^k}Cov\Big(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),$$
$$(\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t)-2\theta'\frac{\partial P_{nt}}{\partial I_{nt}}(\mu_\tau-\mu_t)+(\mu_\tau-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\mu_\tau-\mu_t)\big|x(I_{nt})\Big)dx(I_{nt})\big|\mathcal{I}_t\Bigg]$$

$$=-\frac{1}{2}\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t}\left[\int_{\mathbb{R}^k}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t)\big|x(I_{nt})\right)dx(I_{nt})\big|\mathcal{I}_t\right]$$
$$+\underbrace{\mathbb{E}\left[(\mu_\tau-\mu_t)\big|\mathcal{I}_t\right]}_{=0}\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t}\left[\int_{\mathbb{R}^k}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),\theta\right)dx(I_{nt})\big|\mathcal{I}_t\right]$$

Applying the Law of Total Covariance,

$$\frac{\partial}{\partial I_{nt}}\bar{R}_\tau=-\frac{1}{2}\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t,x}\left[-\frac{1}{2}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t\big|x(I_{nt})\right)\big|\mathcal{I}_t\right]$$

$$=-\frac{1}{2}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t)\big|\mathcal{I}_t\right)$$

$$-\frac{1}{2}Cov\left(\mathbb{E}_\theta\left[u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta))|x(I_{nt})\right],\underbrace{\mathbb{E}_\theta\left[(\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t)|x(I_{nt})\right]}_{=Tr(\frac{\partial P_{nt}}{\partial I_{nt}}\Sigma_t)}\big|\mathcal{I}_t\right)$$

$$=-\frac{1}{2}Cov\left(u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta)),(\theta-\mu_\tau)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_\tau)\big|\mathcal{I}_t\right)$$

Therefore

$$\frac{\partial}{\partial I_{nt}}\mathbb{E}[\bar{R}_{t+1}|\mathcal{I}_t] = -\frac{1}{2}Cov\left(\sum_{\tau=t+2}^{\infty}\delta^{\tau-2}\left[u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))\right], (\theta-\mu_t)'\frac{\partial P_{nt}}{\partial I_{nt}}(\theta-\mu_t)\big|\mathcal{I}_t\right)$$
$$= -\frac{1}{2}Cov\left(R(y_{t+1}, \mathcal{I}_{t+1}, \Gamma, \theta), (\theta-\mu_t)' I_{nt}^{-2} V_{nt}^{-1}\Sigma_x V_{nt}^{-1}(\theta-\mu_t)\big|\mathcal{I}_t\right)$$

So the expected marginal reduction in regret associated with increasing I_{nt} is a function of the expected marginal reduction in the variance of posteriors times the covariance of regret and deviations of θ from its expectation. This expression is typically negative since regret is minimized when $\theta = \mu_t$, in other words beliefs are correct.

This derivation also reveals several other intuitive features. $\frac{\partial}{\partial|\Sigma_x|}\frac{\partial \bar{R}}{\partial I_{nt}} > 0$, meaning that the marginal reduction in regret falls in magnitude if signals are less precise. Conversely, the value of information increases when learning from θ from sources other than I_{nt} become less precise. Since one must take an expectation over changes to the information set, this means that if φ increases so the agent expects to receive more information from neighbors, $\frac{\partial \bar{R}}{\partial I_{nt}}$ will fall in magnitude, reflecting the fact that the information is expected to be less useful. Similarly, if the agent has more precise priors, then information will have less value. Finally, if $u(c_\tau^*(\theta)) - u(\bar{c}_\tau(\theta))$ falls, then $\frac{\partial \bar{R}}{\partial I_{nt}}$ will fall in magnitude because the utility cost of not knowing θ is lower, so information holds less value.

A.3 Proof of propositions

Proof of Proposition 1

I focus on the discrete choice of whether to stock at least χ units or not. This generates tractable predictions without requiring conditions on the smoothness of the hedging value of the mean-preserving contraction to ensure that it is differentiable with respect to I_{nt}. It also matches the mean-preserving contraction implemented in the experiment.

First note that the value of learning, $\mathbb{E}_t\left[\bar{R}(y_{t+1}, \mathcal{I}_{t+1})|I_{nt} = \chi\right]$, is unaffected by the mean-preserving contraction since it affects profit realizations only in period $t+1$, and regret is a function of periods beginning with $t+2$. The costs of stocking I_{nt} are also unaffected, and so the problem reduces to examining how the mean-preserving contraction affects the present value of expected utility associated with the contracted profits.

Applying Rothschild and Stiglitz (1970), there exists some random variable ϵ such that

$\pi_n(\chi, \nu_{nt} + \theta) = \pi_n^p(\chi, \nu_{nt} + \theta) + \epsilon$ and satisfying $\mathbb{E}[\epsilon | \pi_n^p(\chi, \nu_{nt} + \theta)] = 0$.

Case 1: If the agent is risk neutral, then $u'(c_t) = \bar{u}$ is constant. The firm therefore is indifferent between redistributing profits across periods versus not, and so the present value of expected utility from investing $I_{nt} = \chi$ is proportional to $\delta\, \mathbb{E}_t[\pi_n^p(\chi, \nu_{nt} + \theta)]$ under the contraction and $\delta\, \mathbb{E}_t[\pi_n(\chi, \nu_{nt} + \theta)]$. By definition of the mean-preserving contraction, these values are the same and so the firm's utility from stocking $I_{nt} > 0$ is unaffected.[24]

Case 2: If the agent is risk averse, then $u''(c_t) < 0$. Consumption smoothing will lead the agent to borrow against future periods if profit realizations are low and save if they are high, but the present value of utility gains will remain a concave function of realized profits, which we may denote by $\varphi(\pi_n(\chi, \nu_{nt} + \theta))$ or $\varphi(\pi_n^p(\chi, \nu_{nt} + \theta))$.[25] By Jensen's Inequality,

$$\mathbb{E}_t[\varphi(\pi_n(\chi, \nu_{nt}+\theta))] = \mathbb{E}_t[\varphi(\pi_n^p(\chi, \nu_{nt}+\theta)+\epsilon)] < \varphi(\mathbb{E}_t[\pi_n^p(\chi, \nu_{nt}+\theta)+\epsilon]) = \varphi(\mathbb{E}_t[\pi_n^p(\chi, \nu_{nt}+\theta)])$$

Meaning that a risk averse agent gets strictly higher expected utility under the mean-preserving contraction, reflecting the fact that payouts are moved in towards the mean.

Case 3: If the agent is risk loving, the firm gets strictly lower expected utility under the mean-preserving contraction by a reverse argument to case 2.

Therefore the mean-preserving contraction increases the likelihood that $I_{nt}^* > 0$, in the sense that any agent that stocks under π_n will stock under π_n^p and there exists some agents (those with a particular distribution of ν_{nt}, θ) such that agents that do not stock under π_n stock under π_n^p, if and only if the agent is risk averse.

It immediately follows that any variation $\pi_n^{p'}$ that is first order stochastic dominated by π_n^p will increase the likelihood that $I_{nt}^* > 0$ since all agents strictly prefer π_n^p.

Proof of Proposition 2

Part a: The result follows immediately for any increasing utility function since the realization of

[24] This argument relies on the fact that risk neutral firms can always reduce consumption to reach the optimal point of investment. Absent this, a concave production function would yield a buffer stock savings problem that would induce risk averse behavior even with a constant marginal utility of consumption. This decision is based on the fact that firms empirically have relatively stationary monthly costs even if they have a negative shock, implying that they are able to reduce consumption to cover firm investments. Results would be similar if households had an exogenous flow of external income that they could invest in the business and profit functions were restricted such that losses do not exceed monthly consumption.

[25] If capital constraints are unbinding, then a Permanent Income Hypothesis result would imply that the agent's change in consumption is a fixed proportion of their change in profits. Capital constraints will lead to larger consumption reductions for low realizations.

profits under returns first order stochastic dominates the profit function without returns.

Part b: An agent in period t_c will stock n if and only if the present value of expected utility gains from stocking it versus not exceed the utility loss of paying Γ. And for values where it is stocked, the present value of utility gains from stocking helmets is reducing in Γ since the agent must pay the restocking expense. Therefore regret is lower for any θ after this period since the possibly utility loss from not stocking n is lower, while regret is unaffected before that point. Thus, $\frac{\partial R_{t+1}(\theta)}{\partial \Gamma} < 0$. Therefore Equation 7 and 8 show that $\frac{\partial I^*_{nt}}{\partial \Gamma} < 0$ if and only if $|\Sigma_t| > 0$.

Part c: Capital constraints are not binding means that $\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right] = 1$ and $\zeta_n = 0 \Rightarrow \pi_n(\chi, \nu_{nt} + \theta) = \chi \cdot p_j(\chi, \nu_{nt+1}; \theta) \geq \chi \cdot w_n$.

Agents will therefore stock helmets if $Pr(\pi_n(\chi, \nu_{nt} + \theta) > w_n) > 0$. This is immediate if agents are risk neutral. If they are risk averse, then if profit realizations of the safe good are high, the firm can save the consumption for the following period, ensuring that the utility of the consumption gains exceeds the foregone utility from stocking.

If $Pr(\pi_n(\chi, \nu_{nt} + \theta) > w_n) = 0$, then regret is 0 and there is no learning value, so changes in Γ do not affect decisions and the agent never stocks. Therefore, if offered returns that eliminate loss risk, $\frac{\partial}{\partial \Gamma} I^*_{nt} = 0$.

Part d: If agents are risk neutral and have correctly centered beliefs, then expected profits from stocking χ are unaffected but learning value is lower so investment falls. If expected profits positively update or agents are sufficiently risk averse, then the expected utility of stocking increases so there will be persistent positive effects on stocking.

Proof of Proposition 3

Part i: Appendix A.2 shows that $\frac{\partial}{\partial |\Sigma_t|} \frac{\partial}{\partial I_{nt}} \mathbb{E}_t\left[\bar{R}_{t+1} | \mathcal{I}_t\right] < 0$, meaning that information has more value when priors are diffuse. Therefore investment is higher under more diffuse θ if the agent is risk neutral by Equation 7. The magnitude of $\frac{\partial}{\partial I_{nt}} \mathbb{E}_t\left[\bar{R}_{t+1} | \mathcal{I}_t\right]$ is falling as agents become more risk averse and the disutility of stocking increases. Therefore for a sufficiently risk averse agent, I^*_{nt} falls as uncertainty in θ increases.

Part ii: Returns do not affect the value of learning since regret is a function of payoffs beginning in period $t+2$. From Equation 7, it therefore follows that a risk neutral agent that stocks only with returns must have a lower $\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)} \pi_n(\chi, \nu_{t+1} + \theta)\right]$ versus an agent that stocks without returns.

If the agent is risk averse, then returns also increases $Cov_t\left(u'(c_{t+1}), \frac{\partial}{\partial I_{nt}} \pi_{nt}(\chi, \nu_{nt+1} + \theta)\right)$, and so a firm that stocks only with returns may not have lower $\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\pi_n(\chi,\nu_{t+1}+\theta)\right]$ than one that stocks without them if their priors are more diffuse, so that the increase in $Cov_t\left(u'(c_{t+1}),\frac{\partial}{\partial I_{nt}}\pi_n(\chi,\nu_{nt+1}+\theta)\right)$ is larger.

Part iii: Restocking shrinks $|\Sigma_t|$ which lowers in magnitude $\frac{\partial}{\partial I_{nt}}\mathbb{E}_t\left[\bar{R}_{t+1}|\mathcal{I}_t\right]$, meaning that learning has less value. By Equation 7, it follows that a risk neutral agent will obtain lower expected utility from stocking $I_{nt+1}=\chi$ unless $\mathbb{E}_{t+1}\left[\frac{u'(c_{t+1})}{u'(c_t)}\pi_n(\chi,\nu_{nt+2}+\theta)\right]$ increases since $u'(c)$ is constant so $Cov_t\left(u'(c_{t+2}),\frac{\partial}{\partial I_{nt+1}}\pi_n(I_{nt+1},\nu_{nt+2}+\theta)\right)=0$.

If agents are risk averse, then the contraction of beliefs about the profitability of helmets raises expected utility as proved earlier, so an agent may obtain higher expected utility from stocking $I_{nt+1}=\chi$ even if expected profits are unchanged or fall.

Proof of Proposition 4

Part a: Beginning with a change in Γ, a higher φ lowers $\bar{R}(\theta)$ since the agent expects to learn demand without entering themselves, lowering regret whenever helmets can be stocked. Therefore a change in Γ has a smaller impact since expected regret in period t_c is small under a high φ regardless of whether the agent can continue stocking n or not. Returns will also have lower effects since an agent with high φ has more precise beliefs about demand.

Parts b and c: The argument is similar to the prior proposition.

A.4 Proof that the insurance contract induces a mean-preserving contraction

First suppose that the premium payments to enterprises are

$$P_i = 1000\cdot(1-p_i)$$

and let the insurance contract be as given, paying out $1,000$ if sales are less than 3 and 0 if the shop sells 3 helmets. The payout P_i is strictly lower for all p_i compared to that used in the study, meaning the study version first order stochastic dominates it. Therefore proving that the version of the offer here is a mean-preserving contraction is sufficient to conclude that the insurance offer increases investment only if firms are risk averse.[26]

The restriction that shops cannot restock if they accept the insurance payout and the audits

[26] The structure on the profit function differs from that imposed in the model, which requires strict monotonicity and smoothness conditions for tractability. Those conditions are imposed for model tractability (particularly with respect to learning), and neither is required in the proof of Proposition 1, so results are not sensitive to these differences.

ensure that it is not profit increasing to intentionally sell fewer helmets or inflate prices after accepting the insurance contract, so I will assume that the agents' expected distribution of helmet sales is unaffected by opting into insurance. In particular, average profits conditional on selling 3 helmets exceed 800, so a firm capable of selling out always has higher expected profits by doing so than intentionally not and so has no incentive to follow a different sales strategy with insurance. Consistent with this assumption, prices were no higher on average among those that opted into insurance, those with insurance sold out at a higher rate than they anticipated, and all enterprises passed audits.

Let $\pi_n(3, \nu_{nt+1} + \theta)$ be profits, inclusive of P_i, under the control offer and $\pi_n^p(3, \nu_{nt+1} + \theta)$ under the treatment offer. By construction, and recalling that $p_i = Pr(\text{sales}= 3)$,

$$\begin{aligned}
\mathbb{E}_t[\pi_n^p(3, \nu_{nt+1} + \theta)] &= Pr(\text{sales} < 3) \cdot (1000 - P_i + \mathbb{E}_t[\pi_n(3, \nu_{nt+1} + \theta)|\text{sales} < 3]) \\
&\quad + Pr(\text{sales} = 3) \cdot (-P_i + \mathbb{E}_t[\pi_n(3, \nu_{nt+1} + \theta)|\text{sales} = 3]) \\
&= \underbrace{1000 \cdot (1 - p_i)}_{=P_i} - P_i + Pr(\text{sales} < 3) \cdot \mathbb{E}_t[\pi_n(3, \nu_{nt+1} + \theta)|\text{sales} < 3] \\
&\quad + Pr(\text{sales} = 3) \cdot \mathbb{E}_t[\pi_n(3, \nu_{nt+1} + \theta)|\text{sales} = 3] \\
&= \mathbb{E}_t[\pi_n(3, \nu_{nt+1} + \theta)]
\end{aligned}$$

where the last line leverages the Law of Total Probability. Therefore the expected profits are the same under the two offers. Assume that $p_i < 1$ since trivially the offers are the same if the agent perceives no risk of failing to sell out.

Let h_i be the price charged of a helmet. The proof requires that agents sell helmets for at least $1000 - P_i$, which is always true empirically. Let F denote the CDF of profits under π_n and F_p under π_n^p. Observe that F will make discrete jumps at P_i, $h_i + P_i$, $2h_i + P_i$ and $3h_i + P_i$ and F_p at 1000, $h_i + 1000$, $2h_i + 1000$ and $3h_i + 1000$ since payouts only change when demand crosses these thresholds.

For $x < 3 \cdot h_i$, $F_p(x) \leq F(x)$ and so we immediately have that $\int_{-\infty}^{x} F_p(y)dy \leq \int_{-\infty}^{x} F(y)dy$ and the inequality is strict at $x = 2 \cdot h_i + P_i$ since $p_i < 1$. For $y \geq 3h_i + P_i$, $F_p(y) = F(y) = 1$ and so if $\int_{-\infty}^{x} F_p(y)dy \leq \int_{-\infty}^{x} F(y)dy$ holds for $x \in [3h_i, 3h_i + P_i]$ we may conclude that π_p is a mean-preserving contraction. Observe that $F_p(y) = 1$ for all $y \in [3h_i, 3h_i + P_i]$ whereas $F(y) = 1 - p < 1$. Therefore it suffices to verify that $\int_{-\infty}^{3h_i+P_i} F_p(y)dy \leq \int_{-\infty}^{3h_i+P_i} F(y)dy$.

Let $\mathbb{P}_0 = Pr(\text{sales} = 0)$, $\mathbb{P}_1 = Pr(\text{sales} = 1)$ and $\mathbb{P}_2 = Pr(\text{sales} = 2)$.

$$\begin{aligned}\int_{-\infty}^{3h_i+P_i} F(y)dy &= \int_{-\infty}^{P_i} F(y)dy + \int_{P_i}^{P_i+h_i} F(y)dy + \int_{P_i+h_i}^{P_i+2h_i} F(y)dy + \int_{P_i+2h_i}^{P_i+3h_i} F(y)dy \\ &= h_i \cdot \mathbb{P}_0 + h_i \cdot (\mathbb{P}_0 + \mathbb{P}_1) + h_i \cdot (\mathbb{P}_0 + \mathbb{P}_1 + \mathbb{P}_2) \\ &= h_i \cdot (3\mathbb{P}_0 + 2\mathbb{P}_1 + \mathbb{P}_2)\end{aligned}$$

Case 1: Suppose that $h_i \geq 1,000$.

$$\begin{aligned}\int_{-\infty}^{3h_i+P_i} F_p(y)dy &= \int_{-\infty}^{1000} F_p(y)dy + \int_{1000}^{1000+h_i} F_p(y)dy + \int_{1000+h_i}^{1000+2h_i} F_p(y)dy \\ &\quad + \int_{1000+2h_i}^{3h_i} F(y)dy + \int_{3h_i}^{3h_i+P_i} F(y)dy \\ &= h_i \cdot \mathbb{P}_0 + h_i \cdot (\mathbb{P}_0 + \mathbb{P}_1) + (h_i - 1000) \cdot (\mathbb{P}_0 + \mathbb{P}_1 + \mathbb{P}_2) \\ &\quad + \underbrace{P_i}_{=1000\cdot(\mathbb{P}_0+\mathbb{P}_1+\mathbb{P}_2)} \cdot (\mathbb{P}_0 + \mathbb{P}_1 + \mathbb{P}_2 + \mathbb{P}_3) \\ &= h_i \cdot (3\mathbb{P}_0 + 2\mathbb{P}_1 + \mathbb{P}_2)\end{aligned}$$

Case 2: Suppose that $1000 - P_i \leq h_i < 1000$.

$$\begin{aligned}\int_{-\infty}^{3h_i+P_i} F_p(y)dy &= \int_{-\infty}^{1000} F_p(y)dy + \int_{1000}^{1000+h_i} F_p(y)dy + \int_{1000+h_i}^{3h_i} F_p(y)dy + \int_{3h_i}^{2h_i+1000} F(y)dy \\ &\quad + \int_{2h_i+1000}^{3h_i+P_i} F(y)dy \\ &= \int_{1000+h_i}^{3h_i} F_p(y)dy + \int_{3h_i}^{2h_i+1000} F(y)dy + \int_{3h_i}^{3h_i+P_i} dy - \int_{3h_i}^{2h_i+1000} dy \\ &= h_i \cdot \mathbb{P}_0 + (2h_i - 10000) \cdot (\mathbb{P}_0 + \mathbb{P}_1) + (1000 - h_i) \cdot (\mathbb{P}_0 + \mathbb{P}_1 + \mathbb{P}_3) \\ &\quad + P_i - (1000 - h_i) \\ &= h_i \cdot (3\mathbb{P}_0 + 2\mathbb{P}_1 + \mathbb{P}_2)\end{aligned}$$

Therefore the insurance contract is a mean-preserving contraction.

B Coefficient of relative risk aversion estimation

This section estimates the coefficient of relative risk aversion of firms' implied by the effects of the contract offered in the Insurance Experiment. Agents indexed by i face a discrete choice to stock

3 helmets, $s_i = 1$, or not $s_i = 0$. Utility from profits is given by the utility function

$$u(\pi_i) = \begin{cases} \frac{\pi_i^{1-\theta}}{1-\theta}, & \theta \neq 1 \\ \log(\pi_i), & \theta = 1 \end{cases}$$

where θ is the coefficient of relative risk aversion. Absent insurance, agents' expected utility from stocking is given by

$$\begin{aligned}\mathbb{E}[u(\pi_i^{noins})] &= \gamma + u(P_i) \cdot Pr(Sales_i = 0) + u(price_i + P_i) \cdot Pr(Sales_i = 1) \\ &+ u(2 \cdot price_i + P_i) \cdot Pr(Sales_i = 2) + u(3 \cdot price_i + P_i) \cdot Pr(Sales_i = 3)\end{aligned}$$

where γ captures utility or disuility from stocking helmets not related to their expected profits, in particular capital constraints or a hassle cost of learning to stock. P_i denotes the insurance premium offered to the firm and $price_i$ denotes the price firms charge per helmet.

Treated firms have access to insurance. Stocking with insurance yields expected utility of profits

$$\begin{aligned}\mathbb{E}[u(\pi_i^{\text{ins}})] &= \gamma + u(1000) \cdot Pr(Sales_i = 0) + u(price_i + 1000) \cdot Pr(Sales_i = 1) \\ &+ u(2 \cdot P_i + 1000) \cdot Pr(Sales_i = 2) + u(3 \cdot P_i) \cdot Pr(Sales_i = 2)\end{aligned}$$

Let z_i denote treatment assignment. Assuming agents' optimally opt into insurance when offered,

$$\mathbb{E}[u(\pi_i^{stock}|z_i)] = \begin{cases} \max\{\mathbb{E}[u(\pi_i^{\text{noins}})], E[u(\pi_i^{\text{ins}})]\} + \epsilon_{is}, & z_i = 1 \\ \mathbb{E}[u(\pi_i^{\text{noins}})] + \epsilon_{is}, & z_i = 0 \end{cases}$$

where $\epsilon_{ih} \sim EV1$ denotes firm-specific determinants of utility from stocking unobserved to the econometrician. Absent stocking, agents retain the investment cost and receive the insurance premium. Therefore

$$\mathbb{E}[u(\pi_i^{nostock})] = u(2210 + P_i) + \epsilon_{in}$$

where $\epsilon_{in} \sim EV1$. A firm's probability of stocking is therefore given by

$$Pr(s_i = 1|z_i) = \frac{\exp(u(\pi_i^{stock}|z_i))}{\exp(u(\pi_i^{stock}|z_i)) + \exp(u(\pi_i^{nostock}))}$$

Probabilities of selling each number of helmets and prices are observed. The aim is to identify the coefficient of relative risk aversion, θ, and γ, which absorbs features such as liquidity constraints. I leverage the moment conditions $\mathbb{E}[s_i - Pr(s_i = 1|z_i)] = 0$ and $\mathbb{E}[z_i(s_i - Pr(s_i = 1|z_i))] = 0$ where the second moment condition holds by random assignment of insurance, assuming that insurance only affects decisions via expected payouts.

The model is estimated in Python using differential evolution, searching over $\theta \in [0, 8]$ and $\gamma \in [-25, 25]$. The estimated value of θ is 0.62, although bootstrapped confidence intervals are uninformaive since the model is non-linear and estimated with only 350 observations. I also estimate the model for firms exhibiting below or above-median risk aversion measured via lottery choice, and find that the less informative group has an estimated $\theta = 0.53$ and the more risk averse $\theta = 2.21$. Although suggestive, these estimates align well with the heterogeneity from the lottery choice measures of risk aversion. This suggests that even modest levels of risk aversion can substantially affect firms' new product stocking.