Risk Aversion and Demand Uncertainty Among Small Firms

Evidence From New Product Adoption*

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Abstract

Economists typically view firms as risk neutral. Yet many enterprises, especially in low and middle-income economies, are small and owner-operated, making household consumption sensitive to business risk. As a result, owners' risk preferences may influence firm decisions. This paper demonstrates that small retailers in Kenya are risk averse, leading them to under-adopt a new product when they face uncertain demand. I model risk averse firms who learn about demand through stocking decisions, then test the model's predictions using two field experiments. The first establishes that risk aversion affects the stocking decisions of enterprises. I test for risk aversion by offering treated firms an insurance contract that lowers expected profits from a new product while reducing the risk of losses. This leads to a 50% increase in adoption, rejecting risk neutrality. The second experiment shows that *temporarily* reducing inventory risk leads firms to *permanently* stock a profitable new product because they overcome demand uncertainty through learning. These results show that risk aversion in firms can impede product diffusion, potentially limiting growth.

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1 Introduction

Firms in low and middle-income (LMIC) economies are often small and have low productivity (Bloom et al., 2010). Understanding why such firms fail to grow remains a core topic of development economics. A substantial body of work has studied the role of features such as capital market imperfections and management practices in constraining firm performance (e.g. de Mel et al., 2008; Bloom et al., 2013). But the reasons why firms are slow to innovate and adopt new products are not well understood.

This paper investigates whether small firms are risk averse, causing them to forego profitable but risky investments. The returns to entrepreneurial activities, such as employing a new technology or stocking a novel product, are fundamentally uncertain. 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 business decisions. This may deter enterprises from engaging in risk taking that is necessary to grow.

I explore this question in the context of retail firms' decision to adopt a new product. Stocking 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, an East African motorcycle importer built a factory to produce effective helmets in Kenya. This brought safe helmets within reach of typical consumers.[1] 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 enterprises generally held optimistic expectations, their beliefs were often diffuse, and retailers did not stock the good if they were unsure about its profitability. Motivated by these patterns, this paper demonstrates that risk aversion constrains new product adoption when firms are uncertain about demand.

I begin by developing a model in which entrepreneurs, who may be risk averse, decide 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 uncer-

[1] The wholesale price of the studied helmets was about PPP USD 15. High-quality imports often exceeded $100.

tainty. The core insight is that risk neutral firms should experiment more with new products the less certain they are about their profitability because upside is high. This ensures that risk neutral retailers quickly discover profitable new products. But risk aversion deters firms from stocking goods whose profitability is uncertain, 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 *insurance* experiment shows that risk aversion affects firms' stocking decisions. The second *learning* experiment traces out the longer-run trajectory of helmet adoption, demonstrating that risk aversion prevents new product diffusion when retailers are uncertain about demand. The *learning* experiment shows that temporarily lowering risk leads to persistent increases in helmet stocking. I then leverage a second treatment 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 offer shops helmet stock 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.

A challenge with testing firms' risk preferences is that they may face capital constraints. Insurance products typically charge upfront premiums, affecting liquidity. My design overcomes this by offering control shops an unconditional future payment. I allowed randomly selected *insurance* shops to choose between this fixed payment and an insurance contract that paid out more if the firm stocked helmets and failed to sell out, but nothing otherwise. Offers were calibrated so that the expected value of the contract, based on firms' beliefs, was always lower than the fixed payment. Put differently, the contract induces 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 two weeks to order helmets to allow shops to raise capital or search for customers. The effect of *insurance* was 10 percentage points (50%, $p = .02$) after this period. These results imply a rejection of risk neutrality. The results are not driven by new or unproductive enterprises: effects remain statistically significant and are often larger among firms in business for at least 4 years, with employees, and with above median baseline 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) traces out the longer-run effects of risk aversion on helmet stocking and tests if it prevents firms from efficiently learning about new product demand. The experiment is based on the theoretical prediction that firms engage in a "risk-reward" trade-off in which they sacrifice short-run utility to learn. The sample consists of firms that had access to the helmets for over two years but did not stock them. The experiment offered two randomly assigned treatments to test whether risk aversion and demand uncertainty were binding constraints to helmet adoption in this context where background diffusion was slow.

The first *returns* treatment was designed to temporarily lower the risk of stocking helmets. I test if this led to persistent increases in helmet stocking. All firms received access to helmet stock at prevailing market prices. The intervention had two phases. In phase one, *returns* shops were permitted to 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. I examine stocking rates in phase two to test if the one-time reduction in risk led to lasting increases in helmet stocking, as one would expect if learning resolves demand uncertainty.

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 ($p < .01$). 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 stock helmets (from any supplier) and twice as likely to have purchased 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 distinct 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 uncertainty 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 expectations, (1) belief 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. I find evidence consistent with the risk aversion channel: all three predictions associated with this mechanism hold, versus none under the alternative of positively updating expectations.

The second *supplier commitment* treatment in the *learning* experiment is designed to increase the value of information without changing the risk of stocking. This tests the other side of "risk-reward" trade-off. Bandit models predict that firms should be more willing to incur short-run risk when the returns to learning are higher. I test this by informing *supplier commitment* shops at the beginning of the study that the team could help them transition to stocking from the manufacturer at the end of the study (6 months later). As in the *insurance* experiment, control shops were aware of this treatment but informed that they would need to form a supplier relationship themselves. This intervention was motivated by qualitative reports that small shops feared suppliers would fail to restock them if their priorities changed. The *supplier commitment* therefore increases the time horizon over which firms can act on what they learn, without affecting phase one profits.

The model predicts that phase one helmet adoption should be higher among *supplier commitment* shops, reflecting the greater value of information. Consistent with this, *supplier commitment* firms were 6 percentage points (84%, $p = .03$) more likely to stock helmets during this phase. This suggests that firms make sophisticated forward-looking decisions and value learning about demand, with a greater propensity to take risks when future upside is greater.

A third group of shops received *both* the returns and supplier commitment offers. This helps to rule out confounding explanations. If *returns* effectively eliminate risk, then all firms that believe helmets might be profitable should stock. Therefore uptake should not be affected by also receiving the *supplier commitment*. The model shows that phase one helmet adoption should be higher among *returns* versus *supplier commitment* shops, and similar among enterprises in the *returns* and *both* arms. Results align exactly with these predictions, the *supplier commitment* has no effect conditional on receiving *returns*. This helps to rule out many alternative mechanisms that could make the *supplier commitment* valuable. For instance, under learning by doing shops would expect profits to grow over time and thus value the *supplier commitment* even with *returns*.

The findings indicate that firms learn about demand through their own experience. If learning is the principle mechanism, then information from other sources may also matter. I document evidence of information spillovers consistent with this view. The existence of externalities also helps to explain why competition does not force out risk averse firms and restore allocative efficiency, suggesting that it may be possible for shops to free-ride off of risk neutral competitors.

Three results highlight the existence of spillovers. First, *returns* had no effect when firms were located near an existing seller. In these cases, uptake was common in all arms, consistent with learning from incumbent sellers. Second, providing a random subset of firms with peers' helmet sales data increased stocking by 2 percentage points ($p = .02$). Third, the study randomly selected markets where helmet sellers were induced to enter, then surveyed new *spillover* shops located nearby in both these and control markets 3 months later. Firms in treated markets were nearly twice as likely to stock ($p < 0.01$).

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 about four 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 helmets' introduction 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 some of the first 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 in economics of modeling small firms as risk neutral may not be appropriate. Prominent and well-designed recent studies of collusion (Bergquist and Dinerstein, 2020), capital constraints (Fafchamps et al., 2014), technology adoption (Bassi et al., 2022), and misallocation (Buera et al., 2011) rely on this assumption. It is unclear to what extent the core finding of such research may be sensitive to this assumption. For instance, depressed stocking due to risk aversion could mimic risk neutral firms colluding and heterogeneous risk preferences may prevent marginal products from equalizing.

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, revealing the truth. However, I show that risk aversion can prevent shops from engaging in such experimentation, rationalizing evidence that firms in developing countries can hold inaccurate beliefs about the profitability of products (Bai et al., 2025).

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 $I_{nt} \in \{0\} \cup [\chi, \infty)$.[2] The

[2]This condition captures realistic features of the market and allows for an equilibrium in which the agent does not

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 common anecdotal fears that unknown manufacturers will fail or change focus, cutting small firms off from stock. To capture this, the model includes a fixed future utility cost Γ that the agent expects to incur in period $t_c > 1$ if they wish to keep stocking the new product. In addition to capturing salient features of the market, this parameter provides a way to examine how entrepreneurs respond to anticipated future costs that do not directly affect short-run profits.

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

fully learn demand. Imposing fixed delivery costs without a minimum order size yields similar results.

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

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

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.

The term $\frac{1}{u'(c_t)}Cov_t\left(u'(c_{t+1}),\frac{\partial \pi_s(I^*_{st},\nu_{st+1})}{\partial I_{st}}\right)$ captures possible risk aversion and will be zero if agents are risk neutral (since $u'(\cdot)$ is then constant). Low profits reduce consumption, so 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 given by

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

The condition $\frac{1}{u'(c_t)}\kappa_{\chi t}(2I_{nt}-\chi)$ requires investment be 0 or exceed the minimum order size. This leads to a lower probability of stocking the new product at the extensive margin.

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 changes 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 risk from stocking it, 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

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

In words, the increase in future utility an entrepreneur 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 optimally stock the new product past the point where the immediate payoff is 0 because the investment allows them to better optimize in the future. This is the "exploration versus exploitation" 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.

The 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 consumers efficiently gain access to new goods. However, for risk averse firms, the potential downside associated with uncertain payoffs creates disutility, lowering incentives to experiment. When risk aversion is sufficiently high, this relationship may reverse: greater uncertainty can lead firms to reduce investment in new products. 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 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 in magnitude $\mathbb{E}_t\left[\frac{\partial \bar{R}(y_{t+1}, \mathcal{I}_{t+1}, \Gamma)}{\partial I^*_{nt}}\right]$. However, for periods $t < t_c$, the immediate utility from selling the product is unchanged, so supply chain uncertainty depresses the future value of information without distorting short-run incentives.

Second, common approaches to test for risk aversion – such as examining heterogeneous returns to capital or portfolio choices – can be 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. Given the sensitivity of such tests, 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.[3]

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.[4]

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

1. $\mathbb{E}_t\left[\pi^p_n(\chi, \nu_{nt+1} + \theta)\right] = \mathbb{E}_t\left[\pi_n(\chi, \nu_{nt+1} + \theta)\right]$
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. $\pi^p_n(I_{nt}, \nu_{nt+1} + \theta) = \pi_n(I_{nt}, \nu_{nt+1} + \theta) \quad \forall I_{nt} \neq \chi$

Expected profits are unaffected by a mean-preserving contraction, but the distribution is less spread out.[5] 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.

[3]This model could also be used to study cross-product spillovers among risk averse firms. But empirically, heterogeneity in the products shops stock makes it infeasible to test such predictions in this setting.

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

[5]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.

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 learning value, confounding the test. This highlights the value of experimentally testing risk preferences, since real-world shocks are likely to last for more than one period.[6]

I next derive empirical tests for 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, captured by a decrease in the parameter Γ. A return policy guarantees that the firm receives at least the good's purchase cost ($p_{nt+1} \geq w_n$), reducing downside risk and increasing the expected marginal return from stocking under uncertainty. A reduction in Γ enhances the value of experimentation, provided that firms face unresolved demand uncertainty (i.e., $|\Sigma_t| > 0$). Both interventions raise the expected utility of stocking the product and should therefore increase adoption if firms learn about demand.

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 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 if it operates solely through the targeted channel. In contrast, if supply chain frictions matter for reasons other than learning about demand, then a change in supply chain uncertainty 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 stocking behavior after the return policy is no longer available. 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.

[6]Empirically, it can be desirable to instead offer a perturbation $\pi_n^{p'}$ of π_n^p that is first-order stochastic dominated to produce robustness against measurement error in expected profits. This provides a stronger test of risk aversion.

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 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 consistent with the model 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 randomized controlled trials 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 tests for firm learning by examining the persistence of effects.

Both experiments offer a motorcycle helmet introduced 2.5 years before the study began. Historically, only low quality helmets with minimal safety benefits were affordable to most Kenyans. However, a motorcycle importer invested in a large helmet factory in Kenya in 2020. Lower transportation and labor costs brought higher quality helmets into the budget set of typical consumers.[7]

I study this market because the retail diffusion of the helmets was slow yet the market of motorcycle users is large, suggesting a possible inefficiency. A census conducted before the *learning* experiment found that, despite the fact that shops had access to helmet stock for over 2 years, under 3% of surveyed firms sold helmets, and over 75% reported that they were unaware of any local retailer offering helmets. Yet motorcycle use is ubiquitous. There were 2.4 million motorcycle operated per day in 2022, accounting for 22 million trips. Motorcycle deaths are rapidly rising, with the recorded number doubling between 2018 and 2021.[8]

3.1 The insurance experiment

Setting and timeline: The *insurance* experiment was conducted between November 2024 and April 2025 across 140 markets in western Kenya, spanning nine counties. In each county, the 15

[7] 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*.

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

to 16 largest markets – identified by local field officers – were selected. The markets are typically urban (e.g. Kisumu, Kenya's third largest city) or semi-urban (e.g. Siaya Town, with a population near 30,000). The experiment included a baseline and a follow-up survey, conducted two months apart. A survey with a separate *spillover* sample was also completed shortly after the follow-up.

The studied helmets are produced about 300 kilometers from the 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 was conducted in 70 markets, randomly selected with stratification by county. The remaining 70 markets served as pure controls and were visited only during the *spillover* survey.

In each of the 70 baseline markets, field officers identified 10 eligible shops that were effective fits for motorcycle helmets. Priority was given to motorcycle spare parts and repair shops. If fewer than 10 such establishments were available, other shop types (e.g. hardware stores) were included to complete the list. Eligibility criteria required that shops did not currently sell any type of motorcycle helmet and operated from a fixed physical location. From each set of 10 eligible shops, 5 were randomly selected for inclusion in the baseline sample, yielding a total of 350 firms.[9]

The focus on urban and semi-urban markets combined with the restriction that enterprises have a permanent location resulted in a sample of firms that is larger than average. Median profits the month before the survey were about $365 (summary statistics and balance are reported in Appendix Table A1), compared to annualized revenues of $240 reported among retail firms in Egger et al. (2022) in rural Siaya County, Kenya. The average shopkeeper was 34 years old, about 30% were female, and respondents had about 13 years of education.

More than half of the shops sold motorcycle spare parts, mainly replacement tires and motor oil. General shops and hardware stores were the next most common categories. The average business had been open for more than 4 years, and under 30% reported trying to sell a new product in the past year. About a quarter of firms had an employee, and half of shop owners reported having no employees or other sources of family income. These enterprises may be sensitive to profit risks

[9]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.

since they are the residual claimant to all firm revenues, which fund household consumption.

The 5 skipped shops were targeted for recruitment during 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. However, only the name of skipped shops was recorded at baseline to limit interactions with respondents that may prime them to subsequently stock helmets. Many shops lacked clear names, making it challenging to confirm if targeted shops were visited.

As a result, field officers were instead instructed to target the 5 best suited shops to sell helmets in pure control markets. In baseline markets, they recruited the 5 best shops not already surveyed (including the shops skipped at baseline).[10] In general, all motorcycle parts shops in each market were recruited, and assignment to the *insurance* or *spillover* sample was determined randomly within 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 goal of the *insurance* experiment was to empirically test Proposition 1 to determine if risk aversion constrains small firms' stocking decisions. The design randomly assigned firms in the baseline sample to a *insurance* or control status, stratified by market. *Insurance* firms were offered a contract designed to induce a mean-preserving contraction of helmet 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.

Insurance and control firms were offered a stock of 3 helmets at the end of the baseline survey and informed that they could purchase helmets in order sizes of 3 or more at a follow-up, unconditional of prior stocking. This was based on the manufacturer's minimum quantity so that the investment size (80% of median weekly profits) was realistic. Shops were charged the full price of the helmets, but delivery was fully subsidized. Respondents were allowed two weeks after the

[10] Some retailers already stocking helmets prior to the study, which were ineligible, reported stocking helmets only after the study began to qualify for subsidized stock. To mitigate misreporting, back checks were conducted with all firms selling helmets at the time of enrollment, following the delivery of subsidized stock, at which point the incentive to misreport had been removed. Firms that reported stocking prior to the study are excluded from analysis.

survey to place an order to allow time to search for customers to mitigate risk and to raise capital.

Insurance shops were offered firm-specific contracts that were calibrated based on subjective beliefs about demand. To elicit beliefs, field officers asked both *insurance* and control firms to estimate how many out of every 10 shops that chose to stock helmets would sell out by the follow-up survey. Firms were then asked whether they expected to earn more or less revenue than other shops if they chose to stock helmets. Respondents received small cash rewards at follow-up for each accurate prediction. Based on these responses, field officers communicated the implied probability, conditional on stocking, that their shop would sell all three helmets by the follow-up. The survey then elicited each shopkeeper's belief about this probability, denoted so_i.

Control enterprises were informed that they would receive a fixed payment of

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

Kenyan shillings at the follow-up, regardless of whether they purchased helmet stock.

Insurance firms were given the option to choose between P_i and a contract paying 1,000 shillings if they purchased 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 of this contract is less than P_i. Appendix A.4 shows that offering fixed payments of $P_i' = 1000 \cdot (1 - so_i)$ would make insurance a mean-preserving contraction. Since $P_i > P_i'$, *insurance* is strictly dominated under risk neutrality. Thus, an increase in helmet stocking under this offer implies risk aversion.

The design includes several features to help rule out confounding mechanisms. First, treated shops were given the choice between a future transfer or insurance so that capital constraints do not affect the test (Casaburi and Willis, 2018). Second, both *insurance* and control firms were informed of the insurance offer and its random assignment, so that receipt of the contract did not provide a signal about demand. Third, shops were informed that accepting the Ksh 1,000 *insurance* payout would preclude them from restocking, removing incentives to intentionally forgo sales. Finally, all firms were informed that 1 of 5 shops would be randomly visited by a mystery shopper. The shopper would attempt to purchase a helmet if the firm stocked them and an item of similar value otherwise. Firms unwilling to sell a helmet to this auditor that later reported unsold inventory would be denied the Ksh 1,000 *insurance* payout and barred from restocking.

The primary outcomes is an indicator equal to 1 if the enterprise ordered helmets in the two weeks after the baseline survey. A secondary outcome captures if the firm ordered within 24 hours. The first measure better captures helmet adoption, while the second "immediate adoption" variable is useful for studying the behavior of firms before they can mitigate risk by searching for customers. The baseline and follow-up surveys also captured detailed demographic and business characteristics, including questions related to beliefs about helmet demand and realized sales. The follow-up also measured each respondent's risk aversion over personal financial decisions using a lottery choice game from Strobl (2022).

Markets were randomized to have 2 or 3 *insurance* firms with equal probability. Firms were randomized to *insurance* or control status within markets using simple random assignment. Appendix Table A1 shows that observable variables are balanced across arms.

If no firm in a market ordered helmets, shops were unexpectedly approached a second time (in a random order) and offered stock with a large discount until one accepted. This ensured that each baseline market had a seller. This allows for a test of information externalities by examining if helmet uptake in the *spillover* sample is higher in baseline versus control markets.

3.2 The learning experiment

Setting and timeline: The *learning* experiment was conducted between January and July 2024 in the Nairobi, Kenya metropolitan area. A census in January identified eligible firms and collected basic information used for stratified randomization. The baseline survey was administered in February-March, followed by a midline in May and an endline in June-July.

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

The study coincided with two significant adverse shocks. The midline overlapped with flooding that displaced over 40,000 people near Nairobi. During the endline, protesters stormed the Kenyan parliament, triggering a wave of riots that led many businesses to temporarily shut down. Survey data show that average firm profits declined by 20% relative to baseline during each follow-up.

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 of Nairobi and its suburbs where few or no existing helmet shops were present to limit selection and information spillovers from existing sellers. Consequently, recruitment was low in the Central Business District, where firms are large. Shops without a permanent building were also excluded since they lacked a location to store helmets. Finally, shops that were certain they would never stock helmets were omitted to improve statistical power and to better approximate the population of firms a producer may target when seeking retailers.

Study enterprises were recruited through a census. Field officers visited firms, showed them a sample helmet, and explained the inclusion criteria for the study. If the shop was eligible, the surveyor collected the information needed for randomization and informed the shop that they would be notified if they were invited to take part in the study, at which point details would be provided and the firm could decide whether or not to participate. A total of 1,152 eligible firms were listed.

The targeted baseline sample size was 950-1,000 firms. Censused firms were assigned to either a primary or replacement sample pool. The primary pool deterministically included enterprises located far from existing sellers. Remaining firms were randomly assigned to the primary or replacement status. Field officers first attempted to recruit shops from the primary pool, substituting from the replacement pool if a firm declined to participate. The final sample consists of 929 firms. This is slightly below the target because 22 firms were interested but failed to complete the baseline survey, and five shops were discovered to be ineligible during the survey and dropped. Treatments were not presented to shops before baseline, so participation cannot be driven 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 treatment was designed to test if temporarily reducing inventory risk led shops to permanently stock helmets. This intervention 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 unsold stock from their first order for a full refund, whereas control shops could not. In phase two, all shops were given the option to restock helmets without access to returns, so that stocking conditions were identical for *returns* and control firms. The focus of this treatment is whether the temporary reduction in stocking risk in phase one led firms to stock at a higher rate in phase two, which one expect if firms overcame demand uncertainty by learning. As in the *insurance* experiment, respondents were informed about the existence of all experimental arms and the randomization process to prevent signaling.

Stock returns are used to reduce risk in phase one, rather than an insurance contract, for two reasons. First, contracts such as the one used in the *insurance* experiment are not realistic, whereas return policies are widespread in high-income settings (Padmanabhan and Png, 1995). This intervention therefore helps to address whether thin return markets (less than 15% of *spillover* firms reported ever being offered returns) contributes to firm risk aversion.[11] Second, *returns* do not involve payouts that may affect phase two stocking.

Shops were offered helmet stock with a minimum order size of 5 during phase one. This was half the manufacturer's minimum at the time. The lower order size reduced liquidity needs and helped foster goodwill with control enterprises. *Returns* shops were permitted to return stock only from their first order during the midline survey. Returns were permitted only at this time to ensure that *returns* targeted demand uncertainty and not risks inherent to holding stock. Phase two lasted from the midline to endline, although shops were permitted to place orders at any time. Shortly after the midline survey, the manufacturer unexpectedly lowered its minimum order size to 3, leading the study to implement the same change.

If a shop stated that they planned to place an order at the end of the baseline survey, surveyors unexpectedly revealed that the shop could pay for their first order over 3 weeks, regardless of treatment assignment. This allowed enterprises to begin gaining experience selling helmets while they raised funds. Piloting showed that many did not have enough cash on hand to purchase on the spot without prior warning, and the design required that shops had time to learn about demand before they were given the opportunity to make returns. Surveyors followed a strict protocol ensuring that the existence of this offer was not made prior to shops stating that they intended to make a purchase to avoid contaminating the treatment. [12] The default rate was under 3%.[13] Installments

[11] 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.

[12] In 3 cases, this was revealed to firms before they expressed interest in helmets, so the observations were dropped.

[13] 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.

were not permitted on subsequent orders.

Supplier commitment treatment: The second treatment was designed to increase the value of learning about demand without affecting phase one helmet profitability or stocking risks. I targeted uncertainty about future supply chain reliability to achieve this.

Surveyors informed *supplier commitment* shops that the study could help them restock directly with the manufacturer at the end of the study. This ensured they could continue selling helmets if they found them profitable. The control group was responsible for identifying a supplier on their own. The focus of this treatment is on phase one uptake since the goal is to establish if firms are forward looking and internalize future changes in the value of information.

The *supplier commitment* was based on pilot reports that even if shops were aware of the existence of helmets, most did not know how to contact the supplier. In addition, shops expressed concerns that suppliers may fail to restock small shops if their priorities shifted.

A secondary information treatment: The endline survey included a randomized *information* treatment designed to test if firms learn from peers. *Information* shops received data from 5 randomly selected phase one helmet adopters about how many helmets they sold, the prices that they charged, and about whether they restocked. The data was provided from only 5 shops to generate randomly varying signals. The standard deviation in average helmet sales is 1.2 (compared to a mean of 3.8) across the information presented to firms.[14]

This treatment relied on trust established over the prior surveys. It is unlikely that entities such as the helmet manufacturer could offer a similar product due to concerns about conflict of interest and mistrust. The *information* treatment is therefore narrowly designed to validate the role of learning, not to test a realistic product.

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.[15] 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

[14] I also piloted an arm with a small subset of firms that informed shops no one near them was sampled so would not receive helmets. Shops did not value this information because they were typically isolated and inferred this fact.

[15] In strata where the sample was not mod 4, shops were assigned to both or neither treatment with higher probability.

heterogeneity analysis). Appendix Table A1 demonstrates that the experimental arms are well balanced across shopkeeper and firm characteristics and beliefs about helmet demand.

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, including household finances and a proxy for risk preferences, plus data about the enterprise's costs, revenues, profits, and main products using questions from Egger et al. (2022). Field officers also elicited beliefs about helmet demand using a frequentist approach (Benjamin et al., 2017). [16]

The midline and endline surveys recorded helmet stocking from the study and alternative suppliers. Detailed measures of helmet sales, costs, revenues, and profits were captured if the firm stocked them. Shops were also asked if they planned to remain in the helmet market. The survey then collected updated enterprise revenue, cost, profit, and product data from all shops, and concluded by eliciting beliefs about helmet demand.

Analysis focuses on phase one and phase two helmet stocking, and beliefs about helmet profitability. Helmet profits are also analyzed, but the experiment was not powered to detect treatment effects on firm profits since a minority of firms stocked helmets. I therefore focus on repeat stocking as a revealed preference indicator of helmet profitability.

4 Empirical tests and results

This section empirically tests the predictions presented in section 2.3. I first present the empirical models used, then present the results of the field experiments. The section concludes by considering the policy and welfare implications of the findings.

[16] The survey asked respondents to consider a scenario where they stocked 10 helmets, then presented them with 20 beans and a piece of paper with boxes for 0 to 10 helmets. Respondents were instructed to place beans in the boxes proportionally to the likelihood of selling each of the number of helmets over the following month. Field officers often entered totals that did not sum to 20 on the first day of data collection, so the variable was set to missing on this day.

4.1 Empirical specifications

I examine the effects of the *insurance* experiment by estimating the regression

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

where $Stocked_i$ indicates that shop i stocked helmets, $Insurance_i$ denotes receipt of an insurance offer, X_i is a vector of controls, and μ_m captures market fixed effects. Proposition 1 predicts that $\beta > 0$ only if firms are risk averse.

Turning to the *learning* experiment, I test if agents face demand uncertainty by estimating

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

where $Stocked_i$ 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 between the treatments is included since the model predicts that the offers are substitutes. Parts a-c of Proposition 2 predicts that *returns* and the *supplier commitment* should increase helmet adoption if agents are uncertain about demand, corresponding to $\beta_1 > 0$ and $\beta_2 > 0$, and that the *supplier commitment* should have little effect conditional on receipt of returns, meaning $\beta_3 < 0$. Part d of Proposition 2 predicts that returns should have a persistent effect on stocking if firms learn about demand, which is tested by changing the outcome variable to stocking during phase two and testing the null hypothesis $\beta_1 = 0$.

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 pessimistic priors. This is tested with three equations. First, I estimate

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

$\mathbb{E}[Sales]_i$ is the agent's prior expectation about the number of helmet sales in the next month, obtained through the frequentist elicitation, 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 untreated firms with more uncertain beliefs are less likely to stock, whereas the model predicts

$\beta_2 > 0$ if firms are risk neutral, reflecting the value of learning.[17]

Next, I estimate

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

This is estimated among phase one adopters not receiving the *supplier commitment*. It captures the composition of firms that stock with and without *returns*, allowing one to make an inference about what types of firms the treatment caused to stock. β_1 captures whether sales expectations differ among *returns* versus control adopters. Under risk neutrality, the model predicts $\beta_1 < 0$, meaning beliefs were more pessimistic on average among those that stock helmets with *returns* versus not, but if agents are risk averse β_1 may not be negative. In this case $\beta_2 > 0$, meaning the *returns* crowded in shops with uncertain priors about demand.

Finally, 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 the interventions are likely to have smaller impacts if firms can already learn demand from peers. $\Delta \log(1 + \mathbb{E}[Sales]_i)$ is the log change in shopkeeper i's expected helmet sales from baseline to midline or endline, and $\Delta \log(1 + V[Sales]_i)$ is the same but using the log change in the variance of i's beliefs about sales. If *returns* correct pessimistic priors, Proposition 3 predicts that $\beta_4 > 0$, meaning agents became more optimistic about the helmet market. In contrast, it predicts that $\beta_2 < 0$ if *returns* mitigate risk aversion, meaning experience reduces demand uncertainty.

The final class of model predictions relates to the effects of information from other shops on

[17] 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.

helmet stocking. I first examine heterogeneous effects of *returns* and the *supplier commitment* by exposure to an existing seller, testing if knowledge spillovers substitute for learning from a firm's own experience. I then test if shops near an entrant are more likely to stock helmets, leveraging the *spillover* survey. I finally turn to the *information* treatment, which can be used to verify that information is valuable and test if spillovers matter because they positively update priors or because they mitigate risk aversion.

Let TM_i equal 1 if a firm falls in a market visited at baseline in the *spillover* sample. IT_i denotes receipt of the *information* treatment, HS_i equals one if the average sales presented in the *information* signal is greater than the agent's expectation, and μ represents strata fixed effects. I test for information externalities by estimating

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

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

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

Proposition 4 part a predicts that the ability to learn from an incumbent seller should lower the value of the *returns* and the *supplier commitment*, corresponding to $\beta_3 < 0$ and $\beta_4 < 0$, since stocking is no longer necessary to learn. Proposition 4 part b states that externalities exist if exposure to a seller changes a firm's propensity to stock, which is tested by examining if $\beta_1 > 0$. I examine if spillovers positively update expectations or mitigate uncertainty using Equation 14c. If spillovers matter because they correct pessimistic priors, then a signal should only increase stocking if it exceeds expectations so $\beta_3 > 0$. But if externalities mitigate uncertainty faced by risk averse agents, we may have $\beta_2 > 0$ and $\beta_3 = 0$, meaning receipt of a signal that marginally lowers expected profits (and lowers posterior variance) increases a firm's propensity to stock.

4.2 Results

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 to shops 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.

Figure 1 validates these frequentist belief measures by comparing them to realized outcomes for shops that subsequently stocked helmets. The distribution of reported beliefs closely mirrors the distribution of realized sales, and the elasticity of actual sales with respect to expectations is positive and statistically significant ($p = .02$). These results suggest that the data capture a meaningful signal about firms' beliefs.

These measures indicate that about half of firms believed that helmets are profitable in expectation. However, beliefs are not certain, and most firms reported a risk of helmets resulting in a loss. Consistent with risk aversion, under 7% of untreated shops stocked them. Motivated by these patterns, I turn to experimental variation to evaluate whether lowering risk affects stocking.

Firm risk aversion: The results of the *insurance* experiment (Equation 9) show that receiving access to *insurance* substantially increases helmet adoption, rejecting firm risk neutrality.

Insurance increased the rate of shops that acquired stocked within 24 hours by 7 percentage points on a base of 5% (Table 1 Panel A, $p < .01$). Including shops that stocked later, the treatment 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. Therefore risk aversion affects firms' investment decisions. The effects of *insurance* are larger among firms operated by more risk averse shopkeepers, consistent with a lack of separability between owners' consumption risk preferences and production decisions (columns 5-6).

About 40% of treated firms that stocked helmets accepted the insurance contract (50% of shops that immediately stocked did so, Table 1, Panel B). Shopkeepers that reported they were only willing to take personal financial risks when they know that returns are high – a proxy for higher risk aversion – were twice as likely to opt into the contract ($p = .07$). And agents with more diffuse beliefs about demand, controlling for expectations, accepted insurance at a higher rate ($p = .03$).[18]

One concern with these results is that *insurance* could be mean-increasing if beliefs were mis-

[18]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.

measured. 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 option. 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. Put differently, *insurance* is strictly dominated under risk neutrality if one calculates expectations using firms' subjective beliefs or assuming rational expectations. Finally, firms beliefs about their probability of selling out also predict the true 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). In contrast, the offer had no significant effect among firms without employees. Point estimates also suggest larger effects among firms with higher profits and those that are older. This heterogeneity likely reflects the presence of additional barriers (e.g. liquidity constraints) faced by smaller and younger enterprises, not greater risk tolerance. But the large and statistically significant effects of *insurance* among larger and longer-tenured firms suggests that risk aversion constrains entrepreneurship even when enterprises have liquidity available to make investments.

Appendix B estimates a simple 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 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 parameter is imprecisely estimated and depends on strong assumptions.

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 remarkably 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*.[19]

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 view, under 15% of shops in the *learning* experiment reported any fixed costs of selling helmets, accounting for under \$2.50 on average (Appendix Table A6). The *insurance* experiment asked directly if firms incurred any costs to sell helmets other than 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 adopters do not face binding liquidity constraints at the time of the investment matches the result from the *insurance* experiment that effects were larger among bigger firms. This suggests that among firms capable of affording stock, investment may be deterred by exposure to demand risk in the absence of an insurance mechanism.

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 percent-

[19]The fact that the outcome is binary could generate this result in certain cases, but Appendix Figure A1 shows that results are similar if the outcome captures stocking or interest in stocking, measured by the respondent asking for 2 days to make a final stocking decision.

age points (70%, $p = .03$) more likely to stock during this phase (Table 4). In markets without a pre-existing helmet seller, the treatment effect 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. *Returns* effects under this definition imply more than a doubling of the number of active helmet sellers.[20] These results suggest that agents learned about demand since the groups differ only in that *returns* shops had more experience selling helmets in the first phase of the experiment, matching Proposition 3.

The persistent effect of *returns* on stocking provides revealed preference evidence that many shops found helmets profitable, matching descriptive evidence. On average, phase one adopters reported profits of $70 from helmet sales by endline, with fewer than 7% reporting any losses. 65% restocked, and over 80% reported intent to stay in the market (Figure 3). 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 negative profits, versus 3% of shops not making returns.

Risk aversion and demand uncertainty: 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 3 is satisfied under this mechanism, while none of the predictions under the alternative where *returns* correcting pessimistic priors hold. The results suggest that risk aversion can substantially inhibit new product adoption by deterring firms from experimenting with risky products, limiting small firm growth and consumers' access to goods.

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 5). Estimates indicate that

[20] Results are robust to alternative definitions of entry, including stated intent to permanently sell helmets or stocking helmets three or more times.

when the standard deviation of beliefs about sales increases by one helmet, firms' propensity to stock helmets reduces by 5 percentage points (80%, $p = .03$).[21] This pattern is consistent with risk aversion: firms uncertain about demand avoid stocking due to downside risk. Under risk neutrality, one would expect the opposite, since the option value of learning rises with uncertainty. Appendix Table A4 shows that, although correlational and not causal, this relationship is similar using beliefs about revenue rather than sales, excluding covariates, selecting controls using double-post LASSO, and dropping observations where baseline belief distributions are near-degenerate, suggesting a misunderstanding of the mechanism.

Second, the results of Equation 12 indicate that the composition of firms that stocked helmets with *returns* held more uncertain but not more pessimistic beliefs at baseline (Appendix Table A5). The standard deviation of *returns* adopters beliefs about the number of sales they will make in the subsequent month is about 0.2 units higher versus untreated shops (24%, $p = .03$). In contrast, there is no significant difference in expectations. This compositional effect is also evident in Table 5, 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 a substantial reduction in the uncertainty of their beliefs. Table 6 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 midline or baseline to endline. Estimates pooling the surveys show a small positive and statistically significant effect of market experience on expected sales, and the point estimate is negative examining only endline data. In contrast, the pooled point estimate on the variance of beliefs about demand suggests that experience in the market reduces uncertainty about demand by over 60%[22] ($p = .032$). The reduced-form also supports these conclusions: *returns* is associated with a small and insignificant decrease in expectations but a 0.1 unit reduction in posterior variance ($p = 0.09$).

The view that learning reduced agents' belief uncertainty without moving levels is robust to

[21] The sample of firms considered in these estimates consists of those whose baseline beliefs indicate that they are neither certain that helmets are profitable or are not profitable to avoid censuring issues with the binary outcome. Results are similar, although power is somewhat lower, if all firms are included.

[22] $\exp(.062 - 1.098) - 1 \approx -.64$

alternative definitions of the variables and specifications.[23] 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 $26 decrease in the standard deviation ($p = .05$). And regressing the change in expected sales or the standard deviation of sales on stocking in phase one (without instrumenting for adoption) suggests adopters expect to make $.03$ fewer sales but the standard deviation of their belief is smaller by 0.19 helmets ($p = .04$).

Direct measures of firm learning therefore present a consistent story that experience selling helmets left expectations about demand essentially unchanged, but substantially lowered the variance of agents' beliefs. This suggests that effects of *returns* on phase two stocking are likely attributable to the intervention helping risk averse agents overcome uncertainty about demand.

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.

First, 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 3). The adoption rate of untreated firms near a pre-existing seller is also approximately equal to that of *returns* firms in markets far from incumbent motorcycle shops (Figure 2).

Second, I turn to experimental evidence of externalities from the *spillover* survey to examine whether firms learn from each other in a real-world setting in Table 8. 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.[24]

Results of the *spillover* survey show that firms observe changes in the products that their competitors sell and that exposure to a helmet seller increases a firm's propensity to stock helmets

[23] These results are only reported in the text for brevity given their similarity to Table 6.

[24] 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.

themselves. Across the sample of motorcycle shops, firms are about 13 percentage points more likely to report knowing a helmet seller in their market ($p = .03$) and to stock helmets ($p < .01$). Across all shops, the estimated effect on stocking reduces to 6 percentage points but remains statistically significant ($p < .01$). Anecdotal reports from shops help shed light on how information spillovers occur. Several shops 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 A7 shows similar results using non-random variation in the *learning* experiment.

Third, I verify that information mattered directly by examining the effects of the *information* treatment. Receipt of the anonymous sales data (Equation 14b) increased the rate of shops that stock helmets from 0.6% to 2.8% ($p = .02$, Table 7). As with the *returns* intervention, the data support the view that information mattered because it mitigated uncertainty and not because it positively updated expectations. When a "high signal" indicator equal to 1 if the signal the shop received exceeded their expectation (Equation 14c), the estimated effect of receiving any information (below or above expectations) remains large and significant, and the coefficient on the "high signal" indicator is essentially zero (Table 7, columns 2 and 4).[25] In other words, a firm that received *information* slightly below or above priors was more likely to stock, consistent with externalities mattering primarily because they lowered uncertainty.

While spillovers were strong, evidence suggests that they were localized, explaining why firms in Nairobi held uncertain beliefs more than two years after helmets' introduction. 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 such a shop within a quarter and half a kilometer had no effect.

These results are valuable 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 compared to the treatment. Second, information externalities suggest a market imperfection that helps rationalize why risk averse firms

[25] This leverages the fact that signals were constructed from 5 shops selected at random, resulting in a mean signal of 3.8 sales and a standard deviation of 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, and in column (3) and (5) I examine whether the signal is more optimistic than the median value presented.

can survive competition. Put differently, the effects of *returns* and *insurance* suggest that insurance markets are imperfect. The presence of information externalities indicate a second market failure that can sustain a violation of allocative efficiency.

Confounding explanations: Additional tests help to rule out several potential confounding explanations for the results.

First, the persistence of *returns* could alleviate capital constraints, generating stocking persistence. But in practice, the profits of *returns* firms are not significantly higher, and average firm profits were down at midline and endline due to flooding and political instability. Effects also survive the reduction in order size from 5 to 3 helmets, which lowered liquidity requirements for all firms. The direct evidence of learning and effects of the *information* treatment also validate the interpretation that persistence is driven by learning.

Second, the data are inconsistent with persistence being driven by learning by doing. 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 A6). 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.

Helmet market expansion and policy implications: The large effect of *returns* on firm entry at endline suggests that the *learning* experiment 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?

The positive 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) and (2) of Appendix Table A8. The dependent variable in column (1) equals 1 if the respondent shop was stocking helmets at endline or reported that a shop near them stocked helmets.[26] *Returns* increased the likelihood that the shop or an enterprise near them

[26] This approach relies on shops to determine what firms they consider to be proximate. Field officers' assessment

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 A8 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.

5 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 using return policy similar to those frequently employed in high-income settings led to large increases in stocking that persisted after the policy ended. This suggests 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 deviations from risk neutrality leading to consequential distortions. The setting of the *learning* experiment is likely

of the presence of shops and those of respondents were highly correlated at baseline.

one where efficiency gains from *returns* are limited because helmet demand is easily observed by peers and markets in Nairobi are integrated, giving firms alternative means to learn. Despite this, *returns* firms sold over twice the volume of motorcycle helmets, an important safety product, over the course of the study. Speculatively, the 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 (since competitors cannot see sales), may be larger, and using similar strategies to boost adoption in poorly integrated markets outside of urban centers could help consumers gain access to products that would otherwise not be stocked in their markets. Future research testing these predictions would be valuable.

More broadly, the finding that small firms are risk averse calls into question many economic models of such enterprises and policies designed to expand growth. These results have broad relevance as over half of workers in LMICs are self-employed, and about a third of high-income country workers operate at companies with fewer than 10 employees (International Labour Organization, 2019). This fact could help rationalize the slow adoption of new technologies (Cirera et al., 2022), firm location choices that appear to violate profit maximization (Pelnik, 2024), and foregone investments with positive net returns (de Mel et al., 2008). Risk aversion may also explain why the returns to capital among firms using technologies inside the frontier are not higher in some cases, as risk averse agents may direct capital towards low-risk, low-return opportunities rather than more uncertain investments with higher average yields. Investigating the role of risk aversion in these choices, and the effectiveness at policies that insure agents against risk, could be valuable to better align academic understanding of LMIC firms with their real world behavior and to identify effective policies to increase growth. Entrepreneurship is fundamentally risky, and therefore insuring enterprises against risk has the potential to increase their propensity to engage in activities that drive economic growth and development.

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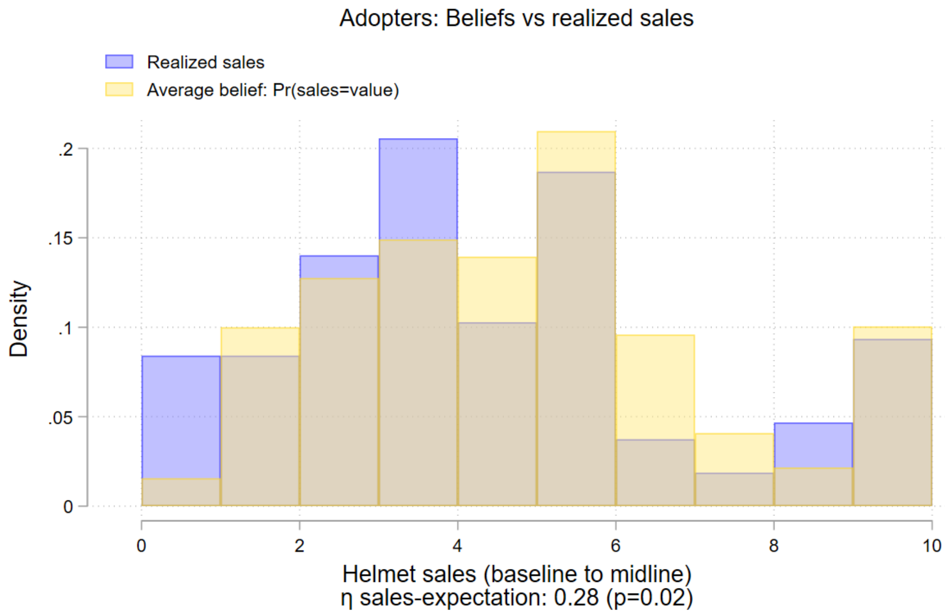
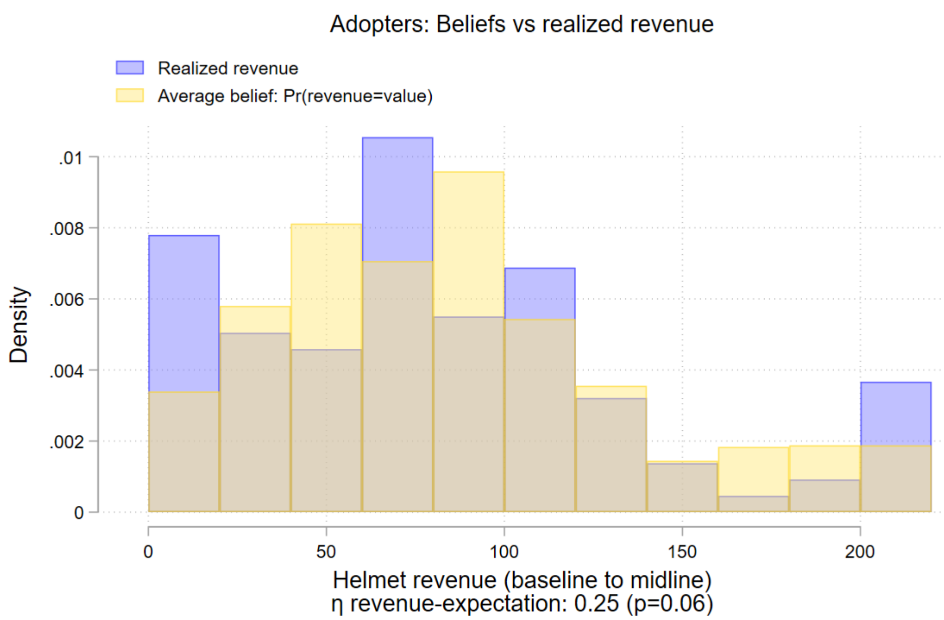
Figure 1: Learning experiment: Beliefs versus realized outcomes among adopters

Panel A: Helmet sales

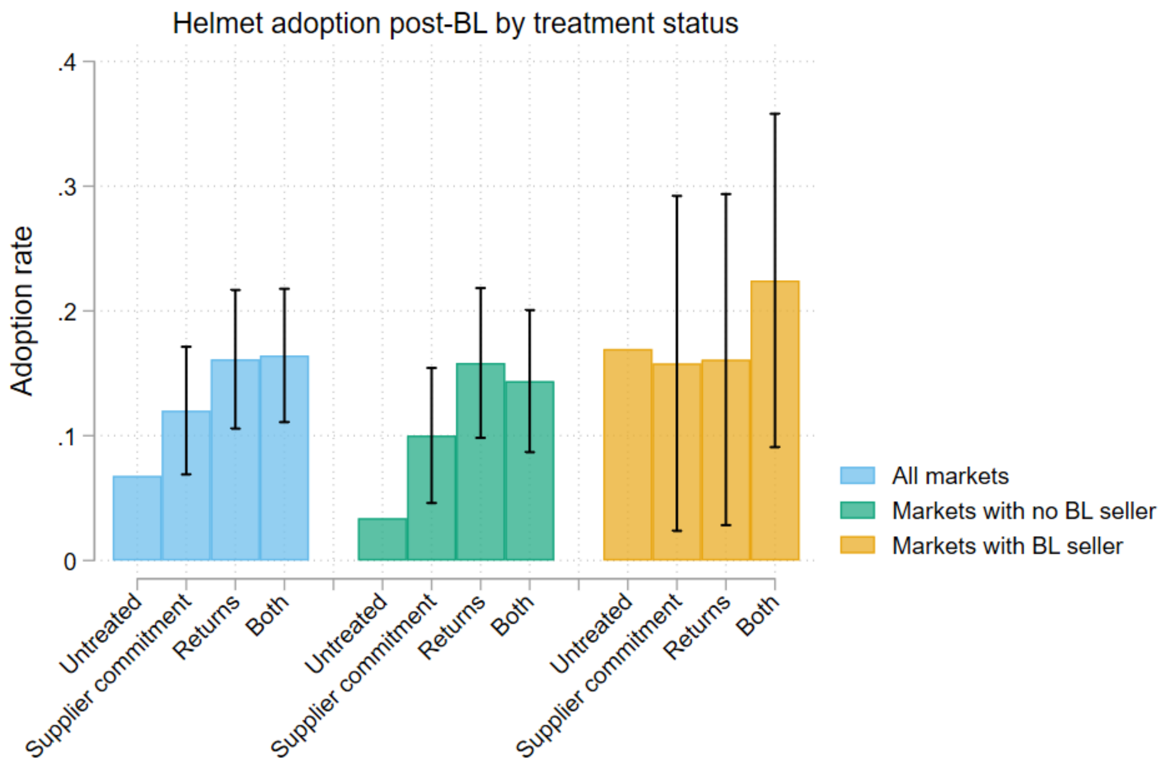


Panel B: Helmet revenue



This figure the average perceived probability of selling a given number of helmets (or earning a given revenue) from frequentist belief measures versus the realized outcome measured at the midline survey among 108 phase one helmet adopters. The belief measure estimates an individual's full distribution of beliefs. These measures are then averaged across the population. Elasticities of outcomes with respect to beliefs are obtained by estimating a regression of expected sales (revenue) on realized sales (revenue) with robust standard errors.

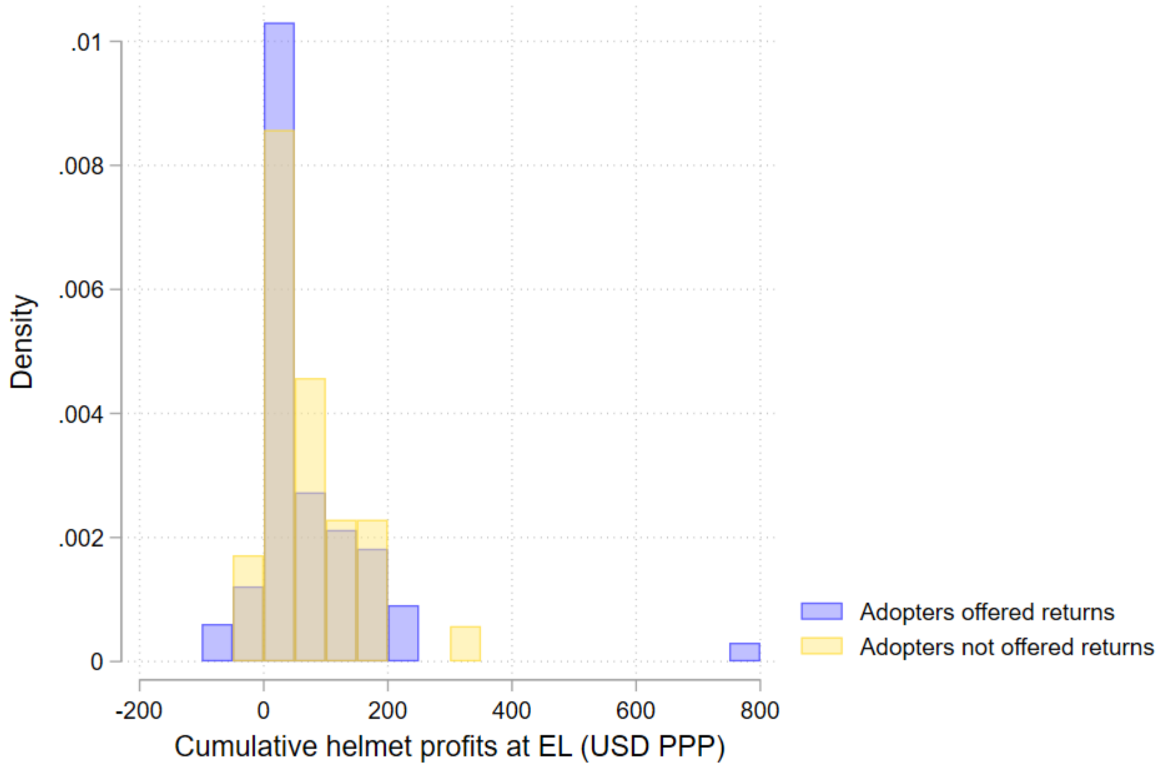
Figure 2: Learning experiment: Treatment effects on phase one helmet adoption



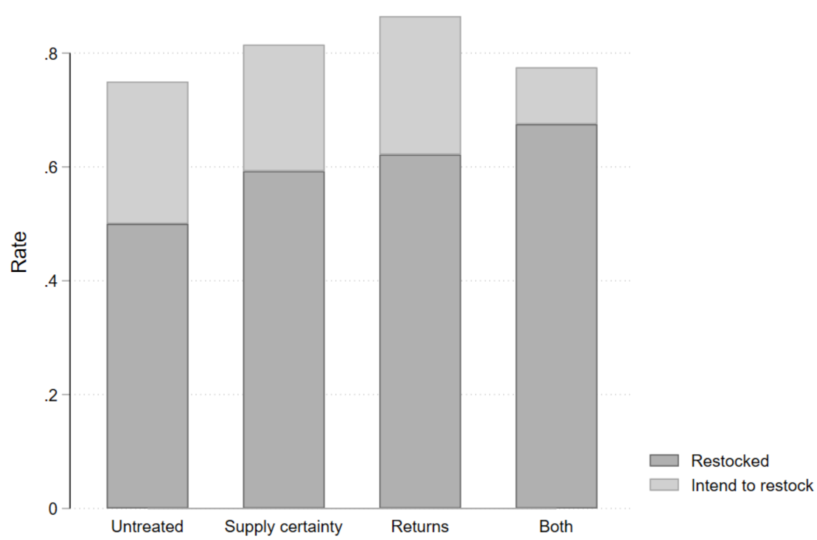
This figure reports the rate of shops that stocked helmets within a month of the baseline survey in the learning experiment, from the study or a different source. The first four bars include all markets ($n = 929$), the next four restrict the sample to markets with no baseline helmet seller present as reported by the respondent firm (about 75% of cases), and the final four bars correspond to markets with a pre-existing seller. The first bar in each block presents the control stocking rate. The second reports the control rate plus the estimated treatment effect of the *supplier commitment*, with the black line denoting a 95% confidence interval. The third bar is similar, but reports the effect of *returns*. The fourth bar reports the estimated effect of receiving *both* treatments.

Figure 3: 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 × 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

	Insurance uptake		Insurance expected payout − guaranteed		Insurance realized payout − guaranteed	
	(1) Accepted insurance	(2) Accepted insurance	(3) Dollars	(4) Share	Dollars	Share
Risk averse	0.274*					
	(0.144)					
$\mathbb{E}$[Sales]		-0.036				
		(0.032)				
SD[Sales]		0.136**				
		(0.061)				
Accepted insurance			-0.040	-0.007	0.663	-0.071
			(0.292)	(0.065)	(2.824)	(0.274)
Constant	0.281***	0.316**	-0.822***	-0.117***	-6.046***	-0.472**
	(0.081)	(0.151)	(0.167)	(0.037)	(2.008)	(0.214)
Observations	50	50	50	50	46	46
Mean	0.380	0.380	-0.818	-0.114	-6.141	-0.493

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

This table reports results of the *insurance* experiment. Panel A presents treatment effects of receiving an insurance offer on helmet stocking. 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 in columns (1) and (2). 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.

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 × 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 × 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 (≤ 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 helmet stocking

	Strata fixed effects				Full controls			
	Phase 1	Phase 1	By EL	By EL	Phase 1	Phase 1	By EL	By EL
Returns	0.096***	0.127***	0.085**	0.114***	0.089***	0.126***	0.078**	0.114***
	(0.029)	(0.032)	(0.034)	(0.037)	(0.028)	(0.031)	(0.032)	(0.035)
Supplier commitment	0.053*	0.085***	0.038	0.065*	0.060**	0.085***	0.045	0.056*
	(0.027)	(0.029)	(0.033)	(0.035)	(0.027)	(0.028)	(0.031)	(0.033)
Returns × Supplier commitment	-0.056	-0.099**	-0.061	-0.091*	-0.042	-0.079*	-0.043	-0.060
	(0.043)	(0.048)	(0.049)	(0.054)	(0.042)	(0.046)	(0.046)	(0.051)
BL seller		0.125**		0.146**		0.092*		0.077
		(0.050)		(0.060)		(0.047)		(0.054)
Returns × BL seller		-0.122		-0.117		-0.143**		-0.136*
		(0.075)		(0.087)		(0.072)		(0.080)
Supplier commitment × BL seller		-0.128*		-0.112		-0.099		-0.045
		(0.073)		(0.086)		(0.071)		(0.081)
Returns × Supplier commitment × BL seller		0.166		0.119		0.142		0.063
		(0.107)		(0.121)		(0.104)		(0.115)
Observations	929	929	929	929	929	929	929	929
Control mean	0.068	0.034	0.131	0.090	0.068	0.034	0.131	0.090

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

This table plots treatment effects from the *learning* experiment. Dependent variable in odd columns: shop stocked helmets during phase one (within 1 month of the baseline). Dependent variable in even columns: shop stocked helmets by the endline survey. Robust standard errors in parenthesis. Estimates include strata fixed effects. Firms that did not complete the endline survey had the outcome coded to 0 if they withdrew from the study because they did not wish to sell helmets or if the outcome was confirmed without completing a full survey. Columns (5) - (8) 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: Persistent effects on stocking

	Stocked in phase 2				Entrant (restocked and intent)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Returns	0.070**	0.078**	0.065**	0.077**	0.069**	0.076***	0.066**	0.077***
	(0.031)	(0.034)	(0.030)	(0.033)	(0.028)	(0.029)	(0.027)	(0.029)
Supplier commitment	0.041	0.069**	0.044	0.055*	0.022	0.054**	0.027	0.046*
	(0.030)	(0.032)	(0.028)	(0.030)	(0.025)	(0.027)	(0.024)	(0.026)
Returns × Commitment	-0.063	-0.080	-0.047	-0.051	-0.032	-0.051	-0.021	-0.030
	(0.044)	(0.049)	(0.042)	(0.047)	(0.039)	(0.044)	(0.038)	(0.042)
BL Seller		0.092*		0.029		0.091*		0.040
		(0.053)		(0.048)		(0.049)		(0.045)
Returns × BL Seller		-0.038		-0.047		-0.036		-0.041
		(0.080)		(0.074)		(0.073)		(0.069)
Commitment × BL Seller		-0.112		-0.043		-0.126*		-0.074
		(0.077)		(0.071)		(0.066)		(0.063)
Returns × Commitment		0.074		0.019		0.082		0.037
× BL seller		(0.111)		(0.106)		(0.099)		(0.095)
Observations	929	929	929	929	929	929	929	929
Control mean	0.102	0.073	0.102	0.073	0.068	0.040	0.068	0.040
Controls	Strata	Strata	Yes	Yes	Strata	Strata	Yes	Yes

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

Robust standard errors in parenthesis. Dependent variable in columns (1) - (4): shop purchased helmet stock in phase 2. Dependent variable in columns (5) - (8): shop purchased helmets two or more times during the study (restocked) and reported intent to permanently keep selling them. All estimates include strata fixed effects. Firms that did not complete the endline survey had the outcome coded to 0 if they withdrew from the study because they did not wish to sell helmets or if the outcome was confirmed without the survey. Estimates where controls are included ("Yes") 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 5: 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	-0.026	0.318	0.325*	-0.030	0.991	0.342*
	(0.078)	(1.341)	(0.181)	(0.125)	(2.219)	(0.179)
E[sales]	0.022	0.477		0.081*	1.902	
	(0.015)	(0.311)		(0.048)	(1.207)	
$\sigma(sales)$	-0.051**	-0.934**	-0.052**	-0.061**	-1.128**	-0.067**
	(0.023)	(0.405)	(0.024)	(0.028)	(0.505)	(0.030)
Returns × E[sales]	0.011	-0.150		0.027	-0.772	
	(0.034)	(0.394)		(0.115)	(1.564)	
Returns × $\sigma(sales)$	0.057	0.979*	0.097**	0.081	1.274*	0.122**
	(0.055)	(0.585)	(0.048)	(0.065)	(0.720)	(0.059)
Observations	302	270	302	302	270	302
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 expected sales or 0 loss probability from stocking 5 helmets since there is mechanically no variation in the distribution of utility from helmets in these cases. Estimates include controls for storage space, education, baseline profits, respondent characteristics, and firm characteristics.

Table 6: 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 × 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 $\log(1 + \mathbb{E}[\text{Sales at midline}]) - \log(1 + \mathbb{E}[\text{Sales at baseline}])$. 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. 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 shop and respondent characteristics.

Table 7: 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 8: Spillover survey: Effect of helmet entrant on neighbor adoption

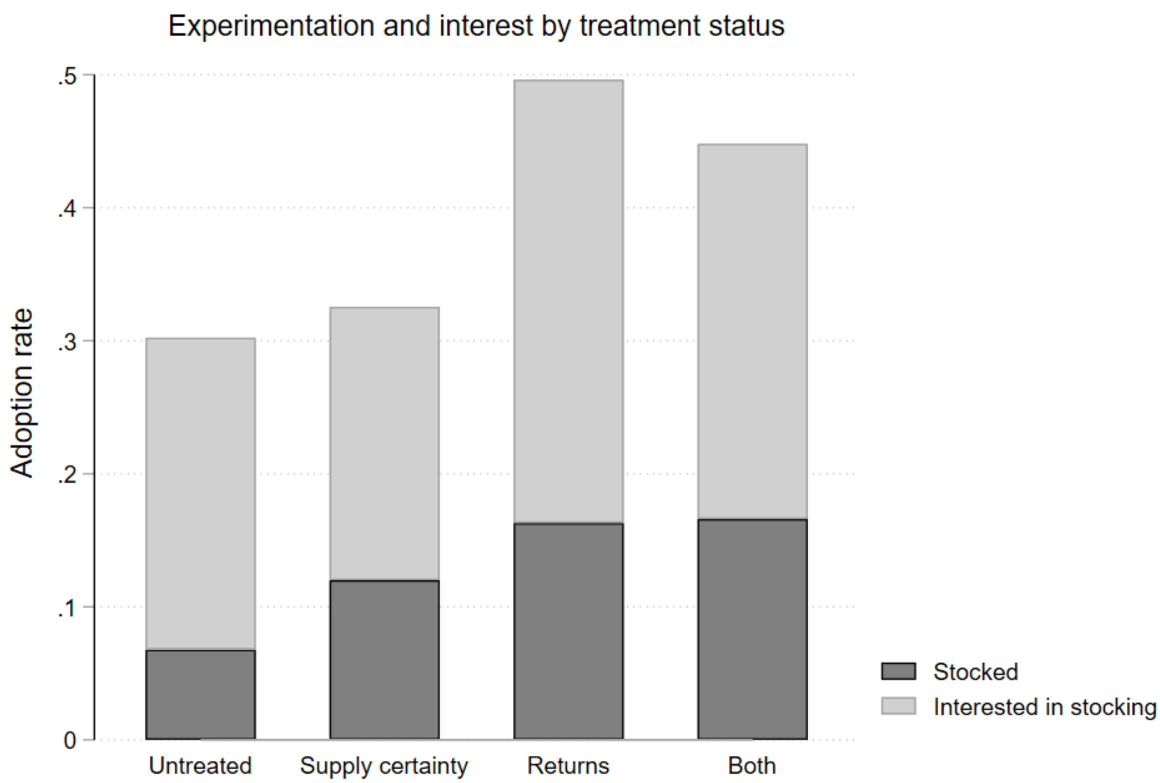
	Motorcycle shops				All shops			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Know seller in market		Ordered helmets		Know seller in market		Ordered helmets	
BL market	0.115*	0.133**	0.098***	0.104***	0.122**	0.143***	0.048**	0.059***
	(0.069)	(0.061)	(0.035)	(0.036)	(0.052)	(0.049)	(0.022)	(0.022)
Observations	376	376	376	376	665	665	665	665
Control mean	0.361	0.361	0.091	0.091	0.300	0.300	0.070	0.070
Controls	No	Yes	No	Yes	No	Yes	No	Yes

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

Standard errors in parenthesis clustered by market. Columns (1)-(4) restrict the sample to shops selling motorcycle-related products and columns (5)-(8) include all firms. The variable "BL 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) and (5)-(6) equals 1 if the shop reported knowing a seller in their market. The dependent variable in the remaining columns equals 1 if the shop purchased helmets. 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, which were ineligible but still surveyed.

Appendix

Figure A1: Learning experiment: Treatment effects on short-run 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.

Table A1: Summary statistics and baseline balance

	Insurance experiment		Learning experiment Returns		Learning experiment Supplier commitment	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	Control mean [SD]	T - C	Control mean [SD]	T - C	Control mean [SD]	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: Belief-adoption relationship under alternative controls

	Sample: interior beliefs			Sample: non-degenerate beliefs		
	(1) Revenue	(2) Sales	(3) Sales	(4) Sales	(5) Sales	(6) Sales
Returns	-0.193	-0.002	0.025	0.022	0.058	0.068
	(0.213)	(0.076)	(0.076)	(0.086)	(0.085)	(0.084)
$\mathbb{E}$[sales/revenue]	0.052*	0.021	0.021	0.023*	0.022*	0.023*
	(0.028)	(0.015)	(0.015)	(0.013)	(0.013)	(0.013)
σ(sales/revenue)	-0.041**	-0.042**	-0.031*	-0.054**	-0.037*	-0.042*
	(0.020)	(0.020)	(0.018)	(0.024)	(0.022)	(0.021)
Returns $\times$ $\mathbb{E}$[sales/revenue]	-0.001	0.013	0.008	-0.018	-0.018	-0.020
	(0.065)	(0.033)	(0.033)	(0.021)	(0.020)	(0.020)
Returns $\times$ σ(sales/revenue)	0.045	0.039	0.032	0.092*	0.069	0.068
	(0.038)	(0.053)	(0.053)	(0.053)	(0.051)	(0.051)
Observations	283	302	302	286	286	286
Control mean	0.068	0.068	0.068	0.068	0.068	0.068
Controls	Full	DPLASSO	None	Full	DPLASSO	None

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

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 expected sales or 0 loss probability from stocking 5 helmets. Columns 4-6 screen for comprehension of belief elicitation by dropping respondents reporting degenerate belief distributions (Var(sales) ≤ 0.25). Covariates in columns 2 and 5 selected with double post LASSO. Columns 1 and 3 include controls for storage space, education, baseline profits, respondent characteristics, and firm characteristics.

Table A5: Learning experiment: Beliefs of adopters with versus without returns

	(1) Belief levels	(2) Belief logs
E[sales]	-0.001	
	(0.034)	
$\sigma(sales)$	0.202**	
	(0.089)	
log(1 + E[sales])		-0.012
		(0.178)
log(1 + V(sales))		0.246**
		(0.103)
Know helmet seller	-0.309**	-0.313**
	(0.143)	(0.142)
Observations	46	46

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

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 A6: 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 A7: Learning experiment: Helmet adoption rates and proximity of other sellers

	(1) Ordered Baseline	(2) Ordered Midline	(3) Ordered Endline	(4) Ordered
Know helmet seller	0.129**			
	(0.055)			
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**
				(0.014)
Sample shops in quarter km				-0.001***
				(0.000)
Observations	231	165	165	771
Control mean	0.034	0.028	0.029	0.010
Controls	DPLASSO	DPLASSO	DPLASSO	DPLASSO
Sample restriction	Untreated	No BL order untreated	No BL order untreated	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. 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. Each column includes enterprise and shopkeeper demographic controls selected using double-post selection LASSO (Belloni et al., 2014).

Table A8: Learning experiment: Effect of returns on helmet access

	(1) Helmet shop in market at endline	(2) Ever helmet shop in market	(3) Cumulative helmets stocked at endline	(4)	(5) Cumulative helmets sold at endline	(6)
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

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

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

$$\delta\left\{\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right]\mathbb{E}_t\left[\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt},\nu_{nt+1}+\theta)\right]+\frac{1}{u'(c_t)}Cov_t\left(u'(c_{t+1}),\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt},\nu_{nt+1})\right)-\right.$$

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

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

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

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

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

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,

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

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 $x(I_{nt})$,

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

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

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

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,

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

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,

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

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

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

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

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

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.[27]

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 $\varphi(\pi_n^p(\chi, \nu_{nt} + \theta))$.[28] 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. The other direction need not hold.

Proof of Proposition 2

[27]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.

[28]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.

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

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

Part c: 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 beginning 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, so returns integrate out fewer losses.

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.[29]

The restriction that shops cannot restock if they accept the insurance payout and the audits 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 $\int_{-\infty}^{x} F_p(y)dy \leq \int_{-\infty}^{x} F(y)dy$

[29] 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.

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.