Risk Aversion and Demand Uncertainty Among Small Firms Evidence From New Product Adoption*

Grady Killeen

University of California, Berkeley

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Abstract

I report the results of two randomized controlled trials in Kenya demonstrating that retail firms under-adopt new products because they are risk averse and face uncertain demand. The experiments study the market for motorcycle helmets, which many shops believe are profitable, yet few adopt. One experiment ($N = 350$) offers shops a contract that induces a mean-preserving contraction of profits. This increases stocking from 20% to 30%, indicating that risk aversion constrains adoption. The second experiment ($N = 929$) allows treated firms to return unsold stock, a common high-income policy. Shops offered returns are more than twice as likely (10 p.p.) to try selling helmets, yet only 5 shops make returns. Effects are persistent. Treated firms are 7 p.p. (70%) more likely to stock after the returns window ends and most stay in the market. Belief data indicates that returns crowd in shops with diffuse priors about demand whose uncertainty is resolved by experience, consistent with learning by risk averse agents. A second experimental arm supports this interpretation by reducing future supply chain uncertainty, making learning more valuable. This doubles adoption if shops cannot return stock but has no effect if they can. This matches theoretical predictions since returns eliminate losses, inducing any firm uncertain about the profitability of helmets to stock. These results suggest that products may diffuse inefficiently in low and middle-income economies because small retailers pay a high utility cost to experiment with new goods. The findings also call into question the common practice of modeling small enterprises as risk neutral.

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1 Introduction

Firms in low and middle-income (LMIC) economies are often small, homogeneous, and have low productivity (Lewis, 1954; Bloom et al., 2010). Yet economists have documented that small enterprises sometimes forego investment opportunities that have positive expected returns. For instance, average returns to capital have been shown to exceed market interest rates (de Mel et al., 2008), firms forego large discounts for stocking in bulk (Kremer et al., 2013), and enterprises fail to move when doing so raises average profits (Pelnik, 2024). Why do firms fail to take advantage of opportunities to grow?

This paper demonstrates that retail firms in Kenya are risk averse, causing them to under-adopt a new product because of uncertainty about demand. I study the market for motorcycle helmets that were introduced over two years before the study began but saw limited retail diffusion. I sample small firms not selling motorcycle helmets and find that many enterprises report positive expected profits, yet most control firms (93%) choose not to adopt them.[1] While enterprises' expectations are optimistic, their beliefs about helmet demand are uncertain. In most cases, shops perceive a risk that helmet demand will be low and stocking helmets will result in losses even when expected returns are positive. Motivated by these patterns, I investigate whether risk aversion, combined with uncertain demand, constrains new product adoption.

An established result in development economics is that the consumption and production decisions of farming households are not always separable (LaFave et al., 2020), and there is evidence that farmers are risk averse (Karlan et al., 2014). But non-agricultural firms are commonly modeled as profit maximizing. For instance, 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) assume risk neutrality. This assumption can influence conclusions: risk aversion may depress stocking in a way that mimics collusive behavior, or prevent the marginal product of capital from equalizing across firms. The primary argument for assuming risk neutrality is that competition should eliminate firms with non-standard objective functions (Kremer et al., 2019). However, empirical tests of this prediction are scarce. As Kremer et al. (2019) note, "we have a limited understanding of what the actual objectives of firm-owners in developing countries are and

[1]Ohnsorge and Yu (2022) estimate that microenterprises represent over 70% of employment in LMICs.

the extent to which the preferences and skills of household members affect firm decisions."

Learning about demand for new products provides a natural setting in which to test if firms are risk neutral since uncertainty about profits is the defining feature of decisions. This topic is also of independent interest, as little research has examined whether demand uncertainty plays a significant role in firm behavior or whether firms approach learning in a manner consistent with theoretical predictions. Moreover, few studies have assessed whether retailers in LMICs adopt new products efficiently.

I present the results of two field experiments showing that retail firms in Kenya are risk averse, leading many enterprises not to try selling motorcycle helmets to avoid the risk that they are unprofitable. Lowering loss risk once induces many firms to permanently enter the market because they learn about demand so no longer view stocking as risky, suggesting that demand uncertainty interacting with risk aversion can be one of the principle barriers to new product diffusion. These results are based on newly developed tools that can be applied to study risk aversion and demand uncertainty in other settings.

I begin by developing a model in which entrepreneurs, who may be risk averse, choose stock levels of an existing product and a new good with unknown demand. This framework embeds a bandit learning problem into a standard model of firm behavior in low and middle-income countries, generating predictions identifying risk aversion and demand uncertainty. I test these predictions across two samples of permanent enterprises using the field experiments.

The first "mechanism" experiment ($N = 350$) generates a mean-preserving contraction of helmet profits to test if firms are risk averse. A randomly selected treatment group was offered an insurance contract that reduces profit variance without increasing expected returns. All firms were offered a guaranteed cash payment at a follow-up period, regardless of whether they stocked helmets. Treated shop could forego this payment in exchange for an insurance contract that provided a larger payout if they stocked helmets and failed to sell out but paid nothing otherwise, smoothing profit risk. The payouts were calibrated so that the expected value of the insurance contract was always lower than the guaranteed sum. Therefore a risk-neutral firm would never opt in or change its stocking decision.[2] However, offering insurance increased stocking from 20% to 30% (SE 4.4%),

[2]The insurance offer is technically a first order stochastic dominated modification to a mean-preserving contraction. This makes the test strictly stronger and does not change the intuition.

demonstrating that risk aversion constrains helmet adoption. 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, demonstrating that these results are not driven by new or small firms.

The mechanism experiment offers a choice over future payments to ensure that capital constraints do not confound the test of risk aversion (Casaburi and Willis, 2018). Field officers also explained the insurance contract to treated and control firms and revealed that insurance would be offered at random before a firm's treatment assignment was revealed, ruling out signaling effects. This is one of the first case where economists have translated the idea of a mean-preserving contraction, the gold standard for identifying risk aversion, into an empirical test. The method can be adapted to other settings where risk is salient by eliciting beliefs and then offering a similar insurance contract.

The second "impact" experiment ($N = 929$) studies how real world tools affect risk averse firms' decision to adopt new products. The intervention also tests whether uncertainty about helmet demand arises because helmets are new to firms – meaning learning can mitigate risk – or because demand is stochastic. This complements the results of the mechanism experiment which does not address what real-world market for risk sharing is missing and lacks clear tests of learning about demand since the premium and insurance payouts affect restocking decisions.

The first of two experimental arms examines a policy that is often offered by manufacturers to retailers in high-income markets: returns of unsold stock (Padmanabhan and Png, 1995; Li and Kim, 2022). All firms could access helmet stock at prevailing market prices. Randomly selected treatment shops were offered an initial stock with the option to return unsold units at a midline survey, while control firms were offered stock without returns.[3] Access to returns increased helmet adoption (from the study or a different source) after the baseline survey from 6.8% to 16.4% (SE 2.9%). Only 5 shops (7% of those eligible) returned helmets, and 90% of adopters reported profits.

The key feature of the experimental design is that after the midline survey, firms retained the ability to purchase helmet stock, but returns were no longer available in either treatment arm. This reveals if firms learned about demand and found helmets profitable by examining the persistence of effects. Consistent with learning, firms initially offered returns were 7 percentage points more likely to stock helmets during this phase (control mean 10 p.p., SE 3.1%) and twice as likely to

[3] Firms were informed of both offers and their random assignment to rule out signaling.

have stocked at least twice. Revealed preference evidence suggests that helmets were profitable for most shops: $2/3$ of baseline adopters restocked and 80% reported that they intended to permanently remain in the market.[4]

Do returns correct pessimistic priors held by risk neutral firms, or do they mitigate risk aversion? Firms' beliefs about demand suggest that the primary function of returns in this setting is to help risk averse firms overcome uncertainty through learning. In the control group, helmet adoption is decreasing in the variance of priors about demand. Returns crowd firms with uncertain, but not pessimistic, beliefs into the market. Instrumenting for helmet-selling experience with treatment assignment shows that experience reduces posterior uncertainty but does not change expected profits. This pattern of results matches model predictions of risk averse firms that are deterred from stocking helmets without returns because of diffuse beliefs at baseline, whose uncertainty is resolved by learning. In contrast, risk neutral firms with diffuse priors would be expected to stock at a higher rate because the value of learning is higher, and a reduction in posterior variance would make them less likely to restock.

I further test for learning about demand through a second, cross-randomized, "supplier commitment" arm of the experiment. Treated firms were informed at baseline that the study team could help them restock directly from the manufacturer when the study ended. Those in the control group were informed that they would need to find a supplier themselves.[5]

The model predicts that the supplier commitment should increase the rate of shops that stock helmets after baseline when returns are not offered because those that discover helmets are profitable can continue stocking them, a form of the "exploration versus exploitation" trade-off in bandit models. However, if returns are offered, the model predicts no effect, since eliminating loss risk should induce any firm with even moderate profit expectations to stock. Consistent with this prediction, the supplier commitment doubles adoption when returns are unavailable (TE 5.3 p.p., SE 2.7), but has no effect among firms receiving the returns treatment. This result helps rule out confounding explanations for treatment effect persistence, such as learning by doing, which would predict increased stocking even when returns are available.

Do firms learn about demand from their neighbors? In the cross-section, I find that untreated

[4]The minimum order cost is about 80% of median weekly profits. On average, firms report helmet profits of $70 at endline, just over 10% of median baseline monthly firm profits.

[5]As in the returns arm, all shops were informed about both arms and the randomization process to prevent signaling.

enterprises near a pre-existing helmet seller stock at approximately the same rate (17%) as those offered returns in markets with no existing seller (16%) and that the treatments have no effect in these markets. Experimentally, I randomize whether shops that had never tried stocking helmets before the endline survey receive access to anonymous sales data from other shops or not. Those that receive information are about 4.5 times as likely to subsequently stock helmets (TE 2.2 p.p., SE 0.9). I further test if information externalities are consequential in the real world by randomizing which markets the mechanism experiment induces an entrant in, then examining the propensity of other firms to stock helmets in these markets and randomly determined control markets with no entrant. Firms in markets with an entrant are about twice as likely to stock helmets ($p < .01$), consistent with consequential information externalities.

The findings indicate that risk aversion can be a primary barrier to new product diffusion. Firms offered returns stocked 1.9 additional helmets on average (SE 0.8, control mean 1.5) and more than 2.8 additional helmets in markets with no pre-existing helmet seller (SE 1.0, control mean 0.7). Furthermore, treated shops are about 8 percentage points (SE 3.8) more likely to sell helmets or report a seller near them at endline, suggesting that the intervention led to persistent increases in helmet access. These results are particularly striking because the manufacturer aggressively marketed helmets to small retailers in Nairobi near the study's midline and reduced their minimum order size, relaxing other constraints to adoption. Shops that stocked helmets report about a 10% business expansion on average, consistent with revealed preference evidence from restocking choices.[6]

The principal contribution of this study is to demonstrate that risk aversion can constrain firm entrepreneurship in LMICs. While previous research has examined correlations between firm owners' risk preferences and enterprise outcomes, few studies have directly tested whether firms behave as risk neutral agents. For instance, de Mel et al. (2008) find no heterogeneity in returns to capital based on microenterprise owners' risk tolerance, while Kremer et al. (2013) show that loss averse entrepreneurs are more likely to make inefficient stocking decisions. Meki (2025) similarly shows that loss-averse individuals prefer equity contracts which lower risk and may stimulate investment. Studies such as Pelnik (2024) have also noted that risk aversion could rationalize apparent departures from profit maximization. This study advances the literature by exogenously varying in-

[6]The experiment is underpowered to detect effects on profits since the first stage is only about 10 percentage points.

vestment risk for small Kenyan firms, providing direct evidence that risk aversion shapes business decisions. In doing so, I introduce a new method for generating a mean-preserving contraction that can be used to study risk preferences in other settings. Furthermore, I show that alleviating risk aversion substantially increase new product diffusion in this context, demonstrating that departures from profit maximization can have first-order effects on firm decisions.

This study also establishes that uncertainty about demand for new products can be an important barrier to their adoption in LMICs. This builds on work studying demand uncertainty theoretically and in high-income settings (Rothschild, 1974; Bolton and Harris, 1999; Doraszelski et al., 2018) and extends results showing that learning is important to LMIC households and farmers (Dupas, 2014; Foster and Rosenzweig, 1995). The experimental results show that demand uncertainty is a consequential feature of new product adoption in LMICs and that its interaction with risk aversion can lead to inefficient product diffusion. This extends prior work on technology diffusion to downstream firms in developing countries (Cirera et al., 2022; Atkin et al., 2017; Verhoogen, 2021). I also prevent evidence that firms learn in a manner consistent with theoretical predictions, supporting the validity of models commonly used to study firm learning. In addition, I develop a model of microenterprise learning about demand that can be used to study these topics in LMICs.

A third, more practical, contribution of this study is to a literature evaluating the efficacy of policy interventions in LMICs. Many policies designed to improve firm productivity focus on alleviating capital constraints or improving business practices (e.g. de Mel et al., 2008; Bloom et al., 2010). I present evidence that return policies may be an effective way to help small firms grow and increase consumers' ability to access products. The results suggest that such policies can be cost competitive since they only incur costs when firms experience bad state realizations, highlighting the importance of risk aversion in designing polices to help firms grow.

2 A model of firm experimentation with separation failures

In this section, I model the decision of an expected utility maximizing entrepreneur about whether to experiment with selling a new product. This model embeds the problem of learning about demand for a new product considered by Rothschild (1974) into the joint firm and consumption optimization problem of a household, similar to de Mel et al. (2008). I begin by describing the learning problem faced by the agent, then derive equilibrium conditions. The section concludes by constructing comparative static tests of risk aversion and optimal learning that guide the design of the experiment and analysis. The goals of these tests are to establish if learning about demand is an important barrier to new product adoption, examine whether firms trade off short-run utility and learning in a manner consistent with theory, and test if firm choices are distorted by risk aversion.

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 non-competitive.[7] 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 then 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 stochastic fluctuations drawn iid from a multivariate normal distribution with known variance $\nu_{st} \sim \mathcal{N}(0, \Sigma_s)$.

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. ν_{nt} are again iid stochastic fluctuations in demand, $\nu_{jt} \sim \mathcal{N}(0, \Sigma_n)$. Allowing for stochastic fluctuations in demand influences the rate at which agents learn and enables 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.

Both demand functions are continuously differentiable and downward sloping in prices $\left(\frac{\partial p_j}{\partial q_{jt}} \leq 0 \; \forall q_{jt}, \nu_{jt}\right)$ and continuously differentiable and increasing in ν_{jt}. [8] The agent observes p_{jt} and q_{jt} each period, but ν_{jt} is not observable.

Flow profits from the safe good, conditional on having 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 display. Flow profits from the new product

[7]The question of whether competition affects learning is important, but the topic falls outside the scope of this paper and including competitive interactions significantly reduces the tractability of the model and would not typically change the predictions studied.

[8]These assumptions are sufficient for tractability, but they allow flexibly for most commonly considered demand functions including linear, quadratic, monomial, semi-log and log-log with additive or multiplicative normal or log-normal fluctuations in demand.

are similarly 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 $I_{nt} \in \{0\} \cup [\chi, \infty)$.[9] The new product is purchased at a wholesale price of w_n. Since all 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 associated with the risk that new manufacturers may exit the market or update policies, cutting the entrepreneur off from stock entirely or imposing a hassle cost to secure a new supplier. 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. This feature is modeled because supply chain instability is a salient characteristic of the studied market. Moreover, incorporating future supply chain frictions provides a way to examine how entrepreneurs respond to anticipated future costs that do not directly affect short-run profits.

Learning about demand: The agent has a prior $\theta \sim \mathcal{N}(\mu_1, \Sigma_1)$ about the unknown parameter of demand for the new product. The agent's information set $\mathcal{I}_t(I_{n0}, \ldots, I_{nt-1})$ is a function of their previous stocking decisions and time. If the agent stocks I_{nt} helmets, then in period $t + 1$ they receive a signal $x(I_{nt}) \sim \mathcal{N}\left(\theta_0, I_{nt}^{-1}\Sigma_n\right)$. 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.[10]

The information set depends on t because agents may also learn from external sources, such as neighboring retailers. Specifically, 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.

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

[10] Appendix C.1 describes how characteristics of this setting combined with the distribution of ν_{nt} microfound this signal distribution.

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

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

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

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 $I_{nt} \cdot (I_{nt} - \chi) \geq 0$, the borrowing limit $a_t \geq \underline{a}$, non-negative investment $I_{st} \geq 0$, and a transversality condition $\lim_{t\to\infty} \left(\prod_{s=1}^{t}(1+r)\right)^{-1} a_t = 0$. Since the agent does not know θ_0 or observe realizations of ν_{nt}, expectations are over θ and ν_{st}, ν_{jt}.

A central feature of this model is that investment in the new product facilitates learning, allowing the entrepreneur to better optimize in future periods. The value of learning will be captured in reductions in regret, which I next define.

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 in period t. 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, which maximizes expected utility over the belief distribution of θ instead of treating it as fixed. Define

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

which is the expected utility that the agent gets from their planned actions if $\theta_0 = \theta$ (meaning that θ governs π_n).

The agent's regret is defined by $R(y_t, \mathcal{I}_t, \Gamma, \theta) \equiv V^*(y_t, \theta, \Gamma) - V(y_t, \mathcal{I}_t, \theta, \Gamma) \geq 0$ which is always weakly positive since the agent will optimize less effectively if θ is unknown. Intuitively, an agent may posit some value of θ, then calculate how much utility they lose implementing their planned strategy if θ is true. The agent's Bayesian regret is

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

In words, the Bayesian regret captures the present value of lifetime utility that the agent expects to lose from period $t+1$ onward because of incomplete information about θ.

2.2 Model solution

The solution to the model is derived in appendix C.2. 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} must satisfy

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

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

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.[11] This shows that risk aversion can lead to inefficient stocking levels if demand for products is stochastic (Kremer et al., 2013). However, this study focuses on identifying risk aversion through uncertainty due to incomplete information, which differs since agents can overcome the lack of knowledge through learning.

Optimal investment in the new good: Prior to t_c, the period when a restocking cost is realized, I^*_{nt} is given by

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

The condition $\frac{1}{u'(c_t)} \kappa_{\chi t}(2I_{nt} - \chi)$ requires investment be 0 or exceed the minimum order size. This can lead agents to invest more or less at the intensive margin, but it leads to a lower probability of stocking the new product.

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

[11] Capital constraints will also increase this covariance penalty due to a buffer stock savings problem in which poor profit realizations will cause capital constraints to bind the following period, preventing consumption smoothing.

Second, learning has value because it lowers future optimization error. Appendix C.3 shows

$$\mathbb{E}_t\left[\frac{\partial R(y_{t+1},\mathcal{I}_{t+1},\Gamma)}{\partial I^*_{nt}}\right] = -\frac{1}{2}Cov_t\left(R(y_{t+1},\mathcal{I}_{t+1},\Gamma,\theta),(\theta-\mu_t)'\Sigma_n^{-1}(\theta-\mu_t)\right)\leq 0 \qquad (8)$$

Agents should therefore invest in the new product even if the expected change in utility next period is small and negative because the investment allows them to learn and better optimize in the future. This is the "exploration versus exploitation" trade-off typical of bandit models.

Modeling investment in multiple goods also provides insights about how risk aversion affects returns to capital. de Mel et al. (2008) model a single good and conclude that more risk averse firms should have higher returns to capital. This model would often predict the opposite, since risk averse agents may invest more capital into the safe good to avoid risk at the expense of higher expected returns.

Beginning in period t_c, the agent would only stock positive I^*_{nt} if the present value of expected lifetime utility gains from stocking it exceeded Γ. An increase in Γ thus reduces in magnitude $\mathbb{E}_t\left[\frac{\partial \bar{R}(y_{t+1},\mathcal{I}_{t+1},\Gamma)}{\partial I^*_{nt}}\right]$ since the potential long-run profit gains from stocking n fall. But for $t<t_c-1$, the expected utility from selling I_{nt} next period is unaffected, so a rise in supply chain uncertainty only affects the future value of information.

2.3 Comparative static predictions

I next turn to deriving testable model predictions. I first show that a mean-preserving contraction increases firm investment if and only if enterprises are risk averse. This mirrors typical results in expected utility theory and aligns with the notion that a risk averse enterprise is one which dislikes the addition of noise to their profits. The limitation of this test is that it does not generate separate predictions if agents can learn about demand versus not. In addition, it does not allow one to determine if returns, a common real-world policy, primarily smooth risk aversion or raise expected profits. I therefore derive further predictions that may be used to understand the economic function of return policies and test if learning about demand for new products is important.[12]

Definition 1 (Mean-preserving contraction of the profit function)

[12] I focus on intuition for the propositions and their implications in the main body of the text. Formal proofs are presented in Appendix C.4.

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

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

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

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

Intuitively, expected profits are unaffected by a mean-preserving contraction, but the distribution of possible realizations is less spread out.[13] Since expected profits are unchanged, this will not affect a risk neutral firm's decision. However, a risk averse agent's expected utility of stocking will increase.

Proposition 1 (Investment response to a mean-preserving contraction)
A mean-preserving contraction increases the likelihood that $I_{nt}^* > 0$ *if and only if a firm is risk averse.*

To be precise, an increase in the likelihood of stocking $I_{nt}^* > 0$ means that all agents that would stock the new product absent the contraction will do so, and for certain priors agents that otherwise would not stock do. The following corollary follows immediately.

Corollary 1 (Dominated modification of a mean-preserving contraction)
If π_n^p *first order stochastic dominates* $\pi_n^{p'}$*, then a perturbation of the mean-preserving contraction that pays* $\pi_n^{p'}$ *in place of* π_n^p *increases* I_{nt}^* *only if a firm is risk averse.*

Responses to a return policy or changes in supply chain uncertainty: A policy allowing unsold stock to be returned implies $p_{nt+1} \geq w_n$. This effectively ensures that the firm will not experience a loss, raising expected marginal profits when demand is uncertain. The increase in expected utility will be particularly large if agents are risk averse since the worst outcomes are eliminated. Firms will increase investment if $|\Sigma_n| > 0$, meaning demand realizations are stochastic, or if $|\Sigma_t| > 0$, meaning that agents face uncertainty about the demand curve that can be overcome by learning.

[13] 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 mechanism experiment, is sufficient to identify risk aversion.

Changes in future supply chain frictions help to identify whether agents perceive the ability to learn, i.e. $|\Sigma_n| > 0$. A reduction in future supply chain frictions has no effect on short-run profits, but it increases the value of learning if n is profitable since the agent can more easily restock in the future. Therefore investment will increase only if an agent anticipates learning about demand.

This prediction may differ if the model included learning by doing or fixed costs, which would make reductions in Γ valuable due to profit growth over time. However, since returns almost eliminate risk, any shop that believes the new product could be profitable will adopt with the offer. A change in Γ should therefore have no effect if supply chain frictions only matter due to learning. In contrast, one would still expect a change in Γ to affect investment if profits grow over time. Formally,

Proposition 2 (Short-run responses to returns and supply chain frictions) *a.) An entrepreneur faces demand uncertainty for the new product if I^*_{nt} increases when offered returns in period $t+1$.*

b.) If $|\Sigma_t| > 0$, then $\frac{\partial}{\partial \Gamma} I^*_{nt} < 0$.

c.) If $\delta \approx 1$, non-stock costs of helmet sales are low so that $\pi_n(I_{nt}, \theta + \nu_{nt}) \approx p_n(I_{nt}, \theta + \nu_{nt}) \cdot I_{nt}$, and capital constraints are not binding, then $\frac{\partial}{\partial \Gamma} I^*_{nt} = 0$ if a firm has access to returns.

Returns could increase helmet stocking because they crowd firms with pessimistic priors into the market or by alleviating distortions from risk aversion. Understanding which mechanism makes returns valuable is informative to determine if thin return markets cause risk aversion to affect firms stocking decisions.

One may test if effects of a returns offer are consistent with the behavior of risk neutral or risk averse firms by considering agents' priors about demand. Under risk neutrality, investment increases in the variance of beliefs since the value of learning is larger. In contrast, sufficiently risk averse agents will stock less when their priors are diffuse since distaste for risk dominates.

Similarly, a returns offer could only crowd risk neutral firms into the market via increasing expected profits. But returns could also bring risk averse firms with diffuse priors into the market. A scenario where firms that stock only with returns have similar expected profits but more uncertain beliefs versus those that always stock suggests that returns predominately mitigate risk aversion.

Proposition 3 (Stocking effects of returns under risk neutrality and risk aversion)

a.) Suppose that $\theta_1 \sim \mathcal{N}(\mu_1, \Sigma_{t1})$ *and* $\theta_2 \sim \mathcal{N}(\mu_2, \Sigma_{t2})$ *such that* $E_t\left[\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt}, \nu_{nt+1} + \theta_1)\right] = E_t\left[\frac{\partial}{\partial I_{nt}}\pi_n(I_{nt}, \nu_{nt+1} + \theta_2)\right]$ *for all* I_{nt} *and* $|\Sigma_{t2}| > |\Sigma_{t1}|$*. If a firm is risk neutral,* I^*_{nt} *is higher under* θ_2*.* I^*_{nt} *is higher given* θ_1 *only if the agent is risk averse.*

b.) Under risk neutrality, an entrepreneur that stocks $I^*_{nt} > 0$ *only with returns must have lower* $\mathbb{E}_t\left[\frac{u'(c_{t+1})}{u'(c_t)}\right]\mathbb{E}_t\left[\pi_n(\chi, \nu_{nt+1} + \theta)\right]$ *than an agent that stocks without returns. This need not hold under risk aversion if the first entrepreneur has more diffuse priors.*

The restocking behavior of firms absent a return offer facilitates direct tests of learning. Furthermore, the persistence of effects of returns can identify if risk aversion constrains the diffusion of profitable new products.

If firms learn about demand, then experience in the market lowers uncertainty. This reduces the learning value of new product investment, which lowers stocking if firms are risk neutral (unless expected profits positively update). But if firms are risk averse, a reduction in uncertainty raises the expected utility of investment so temporarily inducing firms to stock helmets may result in permanent entry even if expected profits do not increase.

Proposition 4 (Restocking effects of returns)

Suppose entrepreneurs are offered returns in period t*.*

a.) If agents are risk neutral, I^*_{nt+1} *will fall unless* $\mathbb{E}_{t+1}\left[\frac{u'(c_{t+2})}{u'(c_{t+1})}\right]\mathbb{E}_t\left[\pi_n(\chi, \nu_{nt+2} + \theta)\right]$ *increases.*

b.) If agents are sufficiently risk averse, I^*_{nt+1} *will increase if the posterior variance of beliefs falls.*

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 does spillover, 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 5 (Information spillovers)

a.) Receipt of a signal x_{ot} *increases* I^*_{nt} *only if agents are risk averse or* $\mathbb{E}_t[\pi_n(\chi, \nu_{nt+1} + \theta)]$ *increases.*

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

3 Setting and design of the experiments

I test the predictions of the model using two randomized controlled trials in Kenya. The first "mechanism" experiment induces a mean-preserving contraction to test if firms are risk averse. The second "impact" experiment examines whether firms learn about demand for new products and assesses the economic consequences of reducing risk aversion through a realistic return policy.

The new product studied in both experiments is a motorcycle helmet introduced in 2021, about 2.5 years before the study began. Motorcycle helmets are not new in Kenya, but historically those affordable to a typical consumer were low quality. The producer of the studied product locally manufacturers helmets that are differentiated on quality yet low cost. Prices are competitive due to low local production costs and the absence of import duties.[14]

I study this market because the retail diffusion of the helmets was slow. Over 75% of firms interviewed in the impact experiment reported that they were unaware of a local retailer offering helmets at the start of the study. This suggests that, at least in the short-run, adoption would have likely remained low absent the experiments. Low diffusion exists despite a high volume of motorcycle users in Kenya. 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.[15]

3.1 The mechanism experiment

Setting and timeline: The mechanism experiment was implemented from November 2024 - April 2025 across 140 markets in West Kenya. The sample includes markets across nine counties. I include the 15-16 largest markets in each county, determined by a field officer from each location. The markets are typically urban (e.g. Kisumu, Kenya's third largest city) or semi-urban (e.g. Siaya Town, which has a population near 30,000). The experiment focuses on a baseline and single follow-up survey, conducted about two months apart. A survey with a separate sample of "spillover" firms was completed shortly after the follow-up survey.

[14] 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*.

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

The helmet manufacturer that produces the studied product is based in Nairobi, Kenya's capital city, which is about 300 kilometers East of the sample. As a result, the brand of helmets were not widely available in West Kenya at the time of the mechanism experiment. Low-quality imports and other varieties of helmets, such as welding or construction products sometimes used by motorcycle riders, were more prevalent: about half of enterprises reported knowing of a nearby shop selling helmets at baseline.

Recruitment and sample: I selected 70 markets at random, stratified by county, for inclusion in the baseline survey. The remaining 70 markets were kept as pure controls and not visited until after the follow-up survey.

In the 70 baseline markets, field officers identified 10 eligible shops that were effective fits for motorcycle helmets and 5 were randomly selected for the baseline sample, totaling 350 firms.[16] Field officers were instructed to first identify motorcycle spare part or repair shops, then recruit general shops, hardware stores, and clothing vendors if there were an insufficient number of retailers focused on motorcycles. Shops were eligible if they did not already sell any variety of motorcycle helmet and possessed a permanent physical location.

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 store categories. The average business had been open for more than 4 years, 25% had opened in the past year, and under 30% reported trying to sell a new product in the past year. About a quarter of firms have an employee, and half of shop owners report having no employees or other sources of family income. These enterprises may be sensitive to profit risks since they are the residual claimant to all firm revenues which form all household consumption.

[16]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.

The 5 skipped shops were targeted for recruitment during the follow-up period only as part of the spillover survey. The aim was to offer these 5 shops plus 5 identically sampled shops in each of the 70 pure control markets a stock of helmets to test for information externalities. However, field officers recorded only the name of skipped shops at baseline since collecting more information would require introducing the study, which may affect their propensity to purchase helmets at the follow-up. Since many shops lack clear names and we lacked the contact information to schedule an interview, we were only able to confirm that a targeted shop was visited around $1/3$ of the time.

We therefore instructed field officers to target the 5 most likely shops to acquire helmets in pure control markets and replace targeted shops that could not be identified with the shop types most likely to adopt helmets that had not yet been surveyed in markets visited at baseline.[17] In general, all motorcycle spare parts shops in each market were recruited, and assignment to the baseline 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. However, because fewer motorcycle shops were available to survey in baseline markets (since half had already been surveyed), shops in these markets are generally less suitable to sell helmets. Appendix Table A2 is consistent with this view, showing that shops in treated markets are less likely to sell motorcycle products, but shops are balanced across other dimensions, and overall if the sample is restricted to motorcycle-related shops.

Design and randomization: The goal of the mechanism experiment is to test Proposition 1. The design tests this proposition by randomly assigning firms in the baseline sample to a treatment or control status. Treated firms were offered a stylized insurance contract designed to induce a mean-preserving contraction of the distribution of helmet profits. Control firms were offered the same stock of helmets without insurance, allowing one to test if firms are risk averse by examining if treated firms stock at a higher rate.

Each firm was 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 whether they stocked previously. This order size was based on the manufacturer's minimum quantity so

[17] Shops stocking helmets prior to the start of the study in pure control markets often reported that they recently began stocking to ensure that they were eligible to obtain discounted stock. We therefore completed back checks with all firms already selling helmets after they received the helmet delivery so there was no longer an incentive to misreport and exclude these shops from analysis.

that the investment size (80% of median weekly profits) was realistic. Shops were charged the full price of the helmets, but no fee for delivery.[18] Respondents could purchase helmets beginning immediately after the survey, and they were allowed up to three weeks to place an order to allow firms without cash on hand to raise funds.

The insurance contract was designed based on firms' responses to several baseline questions. Field officers first asked respondents how many out of 10 shops that chose to stock helmets they expected to sell the full stock by the follow-up survey. They then asked if the shop expected to earn more or less revenue compared to other shops if they stocked helmets. Respondents received cash gifts at the follow-up for each correct prediction.[19] Next, shopkeepers were informed of the implied probability – based on their responses – that their shop would sell out. The survey then elicited the respondent's own belief, so_i, about the probability that they would sell out if they acquired stock.

Control enterprises were informed that they would receive a payment of

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

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

Treated firms were given the option to choose between the control offer or an insurance contract that paid 1,000 shillings if they purchased helmet stock but failed to sell all of the helmets by the follow-up and 0 otherwise. Intuitively, this moves payouts from states where helmet profits are high to one where they are low, making the distribution of possible outcomes less spread out. Appendix C.5 shows that offering guaranteed payments of $P'_i = 1000 \cdot (1 - so_i)$ would make the insurance contract a mean-preserving contraction. Since P_i is strictly higher, a risk neutral firm would always prefer the guaranteed payment, generating robustness against measurement error.

To prevent offering insurance from providing a signal to treated firms, the survey revealed the existence of the insurance offer to treatment and control firms and informed shops that access to insurance would be determined by random assignment. Offers were made in terms of future payouts to avoid confounding time-based liquidity effects with state-dependent risk mitigation

[18] This likely equates to about a 20-50% subsidy.

[19] The cash gifts were much smaller than the revenue from a helmet sale, so that this does not distort sales incentives.

(Casaburi and Willis, 2018).

One concern is that treated enterprises might accept insurance then strategically not sell a helmet to guarantee an insurance payout, increasing the expected value of the contract. Enterprises were therefore informed that they could not restock and receive an insurance payout. This further reduces the expected value of insurance and ensures manipulation is not incentive compatible. Moreover, all enterprises were informed that at least 1 of 5 shops, selected at random, would be visited by a mystery shopper who would attempt to buy a helmet if the firm stocked them and acquire an item of similar value if the firm did not stock them or none were available. If the shop was not willing to sell a helmet to the mystery shopper but then reported failing to sell out, they would receive no payout and be unable to restock.

The primary outcomes is an indicator equal to 1 if the enterprise ordered helmets 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 without binding capital constraints. 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 the survey choice game from Strobl (2022). This game was omitted from the baseline survey to ensure payouts did not affect helmet adoption.

Markets were randomized to have either 2 or 3 treated firms with equal probability. Firms were randomized to treatment or control status within markets using simple random assignment.[20] Appendix Table A1 shows that observable variables are balanced across arms.

If no firm in a market ordered helmets after 2 weeks, field officers unexpectedly approached the shops a second time (in a random order) and offered them stock with a large discount until one accepted.[21] This ensured that the 70 baseline markets each 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 pure control markets.

[20]There are 172 control and 173 treated firms after excluding a market erroneously visited in piloting and at baseline.

[21]In several cases, all shops declined the discounted offer. In these instances a shop was given helmets on credit.

3.2 The impact experiment

Setting and timeline: The impact experiment was implemented from January - July 2024 in the Nairobi, Kenya metropolitan area. A listing exercise recruited the sample of firms and collected basic information used for randomization in January 2024, then a baseline survey was completed in February-March, a midline was completed in May, and the endline was implemented from June-July. The midline survey overlapped with historic flooding that displaced over 40,000 people around Nairobi. Shortly after the endline began, protesters stormed the Kenyan parliament and a series of violent protests forced many businesses to temporarily close. Both events were negative shock to firms, and the data show that average firm profits were down at least 20% from baseline during each of the follow up surveys.

The helmet manufacturer offered local deliveries at no cost within Nairobi, so in principle each of the firms in the impact sample could have acquired stock for 2 years before the study. The manufacturer imposed a minimum order size of 10 helmets at the time of the baseline, resulting in a minimum investment cost of about $150 (1.5 weeks profits for the median firm in the sample). The company unexpectedly reduced their minimum order size to 3 midway through the experiment.[22]

Recruitment and sample: The sample for the impact experiment included 929 retail firms. As with the mechanism experiment, firms with prior experience selling motorcycle helmets were excluded. 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, an area where firms are typically large. Firms without a fixed permanent building were also excluded since they lacked a location to store motorcycle helmets safely, and those that were certain that they did not want to stock helmets were omitted from the sample to improve statistical power.

Study enterprises were recruited through a listing exercise. Field officers visited firms, showed them a sample helmet, and explained the inclusion criteria for the study. If the shop was eligible and indicated that they would ever consider stocking helmets, the surveyor collected the information needed for randomization and informed the shop that they would be notified if they were selected to take part in the study. A total of 1,152 eligible firms were listed. This recruitment strategy

[22] The manufacturer did not disclose the reason for the change, but we first documented it shortly after the company observed a high volume of orders associated with the study.

was not designed to reach a representative sample: shops are more permanent than average, and sampled firms are likely more interested in stocking motorcycle helmets than a randomly drawn retailer. But the composition of firms is likely similar to that which a helmet manufacturer aimed at marketing its products would reach.

The targeted baseline sample size was approximately 950 firms. Listed enterprises located far from existing helmet sellers were deterministically included in the sample, while some firms in highly saturated or unsafe neighborhoods were excluded. The remaining firms were randomized into either the primary sample or a replacement pool. If a targeted firm declined to participate in the baseline survey, surveyors surveyed a replacement.[23]

Appendix table A1 presents summary statistics from the impact and mechanism samples. Firms in the impact 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 of two cross-randomized primary treatment arms was a one-time return offer. The entire sample was granted the ability to purchase unsubsidized helmets from the study team at any point from the end of the baseline survey through the endline. The study imposed a minimum order size of 5 at baseline through midline, reduced from the manufacturer's requirement of 10 to lower liquidity requirements and generate goodwill with firms. The minimum order size was lowered to 3 immediately after the midline survey to reflect the change in the manufacturer's policy.

Treated shops were offered a stock of helmets after the baseline survey with the option to return unsold units during the midline survey for a full refund. Similar policies are common in high-income settings and their potential to mitigate risk aversion has been documented (Padmanabhan and Png, 1995). However, these contracts are rare in LMICs. Less than 15% of firms in the spillover sample reported ever being offered returns. The focus of this study is on testing if there is a missing market for returns, rather than showing why such arrangements are rare. But anecdotally, the helmet manufacturer cited the fact that contracting costs, mistrust, and shipping costs make

[23] The final sample consists of 929 firms, slightly short of the targeted size because more firms than anticipated could not be reached. Attrition occurred before field officers or shops knew treatment assignment and is balanced.

returns cost prohibitive when selling to many small retailers. Control shops were offered helmets without returns.

Returns were permitted during the midline survey only to ensure that the treatment targeted price uncertainty and not risks inherent to holding stock, such as the threat of liquidity needs emerging. And firms were informed that returns could not be made from any subsequent orders. The impact experiment used the same approach as the mechanism experiment to prevent signaling: informing all shops of the existence of each experimental arm and that treatment assignment was randomized so that each firm received the same signal. Treatment assignment was not revealed to field officers until after the offer had been explained to ensure that information was delivered identically across arms.

If a shop stated that they were planning to place an order, the field officer unexpectedly revealed that the shop could pay for their first order over 3 weeks, regardless of treatment status. 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 experimental 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 so that it did not contaminate treatment effects. [24] Appendix table A3 shows that the default rate was under 3%.[25]

Supplier commitment treatment: The second treatment arm was designed to reduce supply chain uncertainty after the study. I committed to help treated shops restock directly with the manufacturer at the end of the study so they could continue selling helmets (purchased at identical unit costs) if they found them profitable. The control group was informed that they would be responsible for identifying a supplier on their own if they wished to restock after the study ended. Both arms were again revealed to respondents to prevent signaling.

This treatment was based on pilot evidence which found that even if shops were aware of the existence of motorcycle helmets, most did not know how to go about locating the supplier. After

[24] In 3 cases, this was inadvertently revealed to firms before they expressed interest in helmets, so the observations are dropped. In 2 of them, the shops defaulted on the installments and stole helmets.

[25] 91% of shops completing payments on time and 97% completing payments 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. Only 2 firms never paid, and 2 had balances remaining at the time of the midline due to emergencies.

the midline survey, sample shops in many areas of the study reported that a vehicle operated by the manufacturer had begun regularly visiting their areas, likely reducing these concerns.

A secondary information treatment: The endline survey included a randomized information treatment designed to test if firms learn from peers and if product adoption responds to learning. Treated shops received information from 5 randomly selected baseline helmet adopters about how many helmets they sold between the baseline and midline surveys, the prices that they charged, and information about whether they restocked. The information about the shops was anonymized to protect privacy. The data was provided from only 5 shops to generate random variation in signal content. The standard deviation in average helmet sales is 1.2 (compared to a mean of 3.8) and the standard deviation of average adopter revenue is about $16 (compared to a mean of $63) across the signals presented to firms.[26]

Randomization: Stratified random assignment was used to allocate assignment of the return and supplier commitment treatments.[27] Randomization was stratified on neighborhood, an indicator for whether the enterprise reported having any employees during the listing exercise, and distance to the nearest existing helmet seller.[28] Neighborhood is likely to affect demand, so this dimension of stratification was included primarily for power. Employees were used as a proxy for firm size, which is a topic of interest for heterogeneity analysis and expected to improve power. Stratification based on distance to an existing seller was done for heterogeneity analysis to study information externalities. Appendix Table A1 demonstrates that the experimental arms are well balanced across shopkeeper and firm characteristics and priors about helmet demand.

The information treatment was delivered unexpectedly to half of shops that had never stocked helmets at the time of the endline survey, regardless of their original treatment status. Randomization was stratified on an indicator equal to 1 if the enterprise reported knowing of a nearby helmet seller by the midline survey, which may lower the value of the presented information.

[26] I 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.

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

[28] Stratification uses the calculated distance between the enterprise and the nearest known seller. Analysis focuses on whether the enterprise reports nearby sellers, which is more accurate but was unavailable for randomization.

3.3 Primary variables collected

The baseline survey captured information about the demographics of the shop owner, including their household finances and a proxy for risk preferences, plus detailed data about the enterprise's costs, revenues, profits, and primary products using questions adapted from Egger et al. (2022). The field officer then elicited the respondent's beliefs about helmet demand. The survey asked respondents questions such as how many helmets the shop owner expected to sell in the following month and the probability they would suffer a loss if they stocked 10 helmets and elicited beliefs about the first and second moment of demand using a frequentist approach (Benjamin et al., 2017). The mechanism 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.[29]

The midline survey asked if shops had stocked helmets since the prior visit and recorded the supplier if they were obtained from a source other than the study. Detailed questions were asked about sales, costs, revenues, and profits from helmets if shops sold them, including opportunity costs and reduced profits from other products if applicable. Shops were also asked if they planned to remain in the helmet market permanently, were not yet sure, or if they planned to exit the market. The survey then collected updated overall enterprise revenue, cost, profit, and product data from all shops, and concluded by collected posterior beliefs using the same questioned included at baseline. The endline survey was similar to the midline survey.

Analysis focuses on initial helmet uptake, subsequent helmet purchases, and beliefs about helmet profitability. Helmet profits are also analyzed, but the experiment was not powered to detect treatment effects on overall firm profits since I only expected a minority of firms to adopt helmets. I therefore focus on repeat purchases as a revealed preference indicator of helmet profitability and present suggestive evidence about the magnitude of profit gains and losses.

[29] Field officers often entered totals that did not sum to 20 on the first day of data collection. They were retrained and the variable was dropped from surveys completed on the first day.

4 Empirical tests and results

This section tests the predictions presented in section 2.3 using variation from the experiment. I first present the empirical models used, then show evidence that firms are risk averse from the mechanism experiment. I then turn to the impact experiment and provide evidence that uncertainty about demand and learning are important determinants of helmet adoption, then I present the results of tests showing that the effects of the returns intervention are driven by risk aversion and not increases in expected helmet profits. The section concludes by considering the policy and welfare implications of the findings.

4.1 Empirical specifications

I begin by examining the effects of the mean-preserving contraction induced by the mechanism experiment by estimating the regression

$$E_i = \alpha + \beta T_i + X_i'\gamma + \mu_m + \epsilon_i \tag{9}$$

where E_i (for experimented) indicates that shop i stocked helmets after the baseline offer was made, T_i denotes receipt of an insurance offer, X_i is a vector of controls, and μ_m captures market fixed effects. Since the insurance contract is a first order stochastic dominated perturbation of a mean-preserving contraction, Corollary 1 shows that $\beta > 0$ only if firms are risk averse.

Turning to the impact experiment, one may test if agents face demand and supply chain uncertainty by estimating a similar model

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

where R_i indicates receipt of the returns treatment, S_i captures assignment to the supply chain treatment, and μ_k captures strata fixed effects. The interaction between the treatments is included since the model predicts that the treatments are substitutes, not linearly additive. Proposition 2 predicts that the return and supplier commitment offers should increase helmet adoption if agents have diffuse priors about demand, corresponding to $\beta_1 > 0$ and $\beta_2 > 0$, and that the supplier commitment arm should have little effect conditional on receipt of returns, meaning $\beta_3 < 0$.

Proposition 3 places conditions on agents' beliefs needed to rationalize short-run treatment effects under risk neutrality versus risk aversion. Part a is examined by estimating versions of the regression

$$E_i = \alpha + \beta_1 f(\mathrm{Var}[Sales]_i) + \beta_2 R_i + \beta_3 R_i \times f(\mathrm{Var}[Sales]_i) + g(\mathbb{E}[Sales]_i, R_i) + X_i'\gamma + \epsilon_i \quad (11)$$

$\mathrm{Var}[Sales]_i$ is the individual's variance of beliefs about helmet sales in the next month, obtained through the frequentist elicitation, and $f(\cdot)$ is the standard deviation of sales)or $log(1 + Var[Sales]_i)$. $g(\mathbb{E}[Sales]_i, R_i)$ controls for expected sales. I consider linear controls for expectations and parsimonious fixed-effects that bucket expected sales into half-helmet increments to account for any non-linear effects, for instance if shops stock when expectations cross a threshold. Proposition 3 predicts that risk neutral agents should be more likely to stock when beliefs are diffuse, conditional on expectations, which implies $\beta_1 > 0$. In contrast, one would only expect $\beta_1 < 0$ under risk aversion. If $\beta_3 > 0$, then returns increase the willingness of agents to adopt risk.[30]

Part b of Proposition 3 is tested by estimating

$$R_i = \alpha + \beta_1 \, \mathbb{E}[Sales_i] + \beta_2 SD[Sales]_i + X_i'\gamma + \epsilon_i \quad (12)$$

This is estimated among baseline adopters not receiving the supplier commitment arm. β_1 captures whether sales expectations differ among adopters offered returns compared to the untreated. Under risk neutrality, the model predicts $\beta_1 < 0$, meaning beliefs are more pessimistic on average among those that stock helmets with a return offer versus not, but if agents are risk averse β_1 may not be negative. In this case $\beta_2 > 0$, meaning the returns offer is crowding in shops with diffuse priors about demand.

I next turn to examining restocking effects after the returns treatment expires. Letting RS_i indicate that a shop stocked helmets after the returned period, Proposition 4 is tested by fitting the following models. First, to test if receipt of a one-time returns offer increases subsequent helmet

[30] 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.

stocking, I estimate

$$RS_i = \alpha + \beta_1 R_i + \beta_2 S_i + \beta_3 R_i \times S_i + X_i'\gamma + \mu_k + \epsilon_i \tag{13}$$

To examine how experience in the market affects beliefs, I estimate the two-stage least squares system

$$\Delta\,\mathbb{E}[Sales]_i = \alpha_e + \beta_4 Stocked_i + \rho_e KS_i + X_i'\gamma_e + \mu_k + \epsilon_{ei} \tag{14a}$$

$$\Delta SD[Sales]_i = \alpha_s + \beta_5 Stocked_i + \rho_s KS_i + X_i'\gamma_s + \mu_k + \epsilon_{si} \tag{14b}$$

$$\begin{aligned} Stocked_i = {} & \pi_0 + \pi_1 R_i + \pi_2 S_i + \pi_3 R_i \times S_i + \pi_4 KS_i + \pi_5 R_i \times KS_i \\ & + \pi_6 S_i \times KS_i + \pi_7 R_i \times S_i \times KS_i + \nu_i \end{aligned} \tag{14c}$$

$Stocked_i$ indicates that the enterprise stocked helmets after the baseline survey. The models control for knowing a seller at baseline, KS_i, and include interactions between this variable and treatment assignment as instruments to improve power since shops are likely to have market knowledge obtained via observing peers if they are near a shop already offering helmets. $\Delta\,\mathbb{E}[Sales]_i$ is the change in individual i's expected helmet sales from baseline to midline or endline, and $\Delta SD[Sales]_i$ is the same but using the change in the standard deviation of i's beliefs about sales, measured via the frequentist mechanism.[31]

An effect of the one-time returns treatment on restocking, $\beta_1 \neq 0$, indicates that agents learned about demand. If agents are risk neutral, the model predicts that $\beta_1 < 0$ because learning reduces subsequent exploratory value from stocking helmets, or $\beta_1 > 0$ and $\beta_4 > 0$, meaning agents became more optimistic about the helmet market. $\beta_1 > 0$ may be sustained with any sign on β_4 if agents are risk averse and their uncertainty about helmet demand drops, $\beta_5 < 0$.

The final class of model predictions relates to the effects of information from other shops on helmet stocking. I first test if shops near an entrant are more likely to stock helmets, leveraging the spillover survey. I then turn to the information treatment included in the impact experiment, which can be used to verify directly that information is valuable and test if spillovers matter because they positively update priors or because they mitigate risk aversion. I finally turn to examining hetero-

[31] I use differences in beliefs from baseline to help remove idiosyncratic variation in the way that individuals complete the game.

geneous effects of the main impact experiment treatments by exposure to an existing seller, testing if knowledge spillovers substitute for learning from a firm's own experience. Let IT_i denote receipt of the information treatment, HS_i equal one if the average sales presented in the information signal is greater than or equal to the agent's expected sales, μ be strata fixed effects, and TM_i denote a market visited at baseline in the spillover sample. I test proposition 5 by estimating

$$E_i = \alpha_1 + \beta_1 TM_i + \mu_c + \epsilon_{1i} \tag{15a}$$

$$E_i = \alpha_2 + \beta_2 IT_i + \beta_3 HS_i + X_i'\gamma_1 + \mu_n + \epsilon_{2i} \tag{15b}$$

$$\begin{aligned} E_i = \alpha_3 &+ \beta_4 R_i + \beta_5 S_i + \beta_6 R_i \times S_i + \beta_7 KS_i + \beta_8 KS_i \times R_i \\ &+ \beta_9 KS_i \times S_i + \beta_{10} KS_i \times R_i \times S_i + X_i'\gamma_2 + \epsilon_{3i} \end{aligned} \tag{15c}$$

Proposition 5 part a predicts that receipt of a signal will only increase stocking for a risk neutral agent if it raises their expectations. I test if firms learn from each other in real world settings and if learning increases stocking by examining if $\beta_1 > 0$. I then examine if information spillovers positively update expectations or mitigate uncertainty using Equation 15b. 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 information 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.

Proposition 5 part b predicts $\beta_8 < 0$ and $\beta_9 < 0$, regardless of risk preferences. That is, if learning from a competitor substitutes for learning from experience, greater exposure to information from competitors should mitigate effects of the primary treatments of the impact experiment.

4.2 Results

Demand priors and baseline stocking: Before turning to the results of the tests, I present descriptive evidence from firm beliefs suggesting that risk aversion may constraint their decisions to adopt helmets. Appendix Table A4 suggests that firms in both the mechanism and impact experiment have optimistic priors about expected helmet demand. The median firm in both settings reported a 30% risk of a loss in firm profits if they purchased a stock of 10 helmets, larger than the minimum order size in either experiment but equal to the manufacturer's at the beginning of the impact experiment. The question asked respondents to factor foregone revenue from other items that they would need to stock less of to afford the helmets into their answer to account for capital constraints. The median firm estimated a 50% likelihood that stocking 10 helmets would increase the profits of their enterprise. Frequentist belief measurements indicate about 55% of firms in the mechanism experiment expected to earn more revenue from selling helmets within 8 weeks than the cost of stock, and 45% of enterprises in the impact experiment expected to earn enough revenue to pay for the cost of stocking 5 helmets within a month. Over 50% of shop keepers expected to sell more than 3 helmets within 8 weeks in the mechanism experiment and 1 month in the impact experiment.

These measures suggest that a majority of firms perceive positive expected helmet profits. However, beliefs are not certain, and most firms report some risk of helmets resulting in a loss. Furthermore, only 20% of untreated enterprises in the mechanism sample and under 7% in the impact sample stock helmets. This suggests that a large share of firms believe helmets have positive average returns and choose not to stock them, suggesting they may be maximizing a concave utility function and not profits. However, these results are purely motivational, so I turn to experimental variation to evaluate whether lowering demand risk increases investment.

Effect of insurance on helmet adoption: The results of the mechanism experiment show that receiving access to insurance substantially increases helmet adoption, rejecting firm risk neutrality. Table 1 reports the results of Equation 9. Panel A shows that receiving the offer increases the rate of shops that acquire stock within 24 hours ("Stocked $\leq$ 24H") by over 7 percentage points on a base of under 5%, more than a 100% increase ($p < .01$). Including shops that stock after 24 hours ("Stocked"), the treatment effect is about 10 percentage points, relative to a control adoption rate of 20% ($p < .05$). These results show that lowering profit variance – without raising expected returns – substantially increases the propensity of firms to stock a new product, indicating that deviations from risk neutrality have a consequential effect on helmet adoption. Columns (5) and (6) show that treatment effects are concentrated among firms in which the firm owner has above median risk aversion, measured through the lottery choice game at the follow-up. This suggests that firm risk aversion may reflect a lack of separability by firm owners' consumption preferences and business decisions. Appendix Figure A1 shows that effects are similar excluding firms that recently opened and remain statistically significant among firms operating for at least 4 years prior to the study. This finding is inconsistent with the argument that firms with non-standard objective function should be weeded out of the market by competition. Insurance also has a statistically significant 33 percentage point effect on helmet adoption among firms with employees (on a base of 14%)[32], and a statistically significant 18 percentage point effect among firms with above median baseline profits.

Panel B examines adoption of the insurance offer among treated firms that opted to stock helmets. About 40% of firms accepted the insurance contract (50% of shops that stocked within 24 hours selected insurance). Consistent with the interpretation that insurance increases the expected utility from stocking helmets among risk averse firms, 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 insurance contract ($p = .07$). And agents with more diffuse beliefs about demand, controlling for expectations, accepted insurance at a higher rate ($p < .05$).[33]

One concern with these results is that the insurance offer could be mean-increasing if beliefs were elicited incorrectly. If this were driving the results, then insurance 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 measurement error would be required to flip the optimal option for a risk neutral firm. Columns (3) and (4) demonstrate that there is no such relationship in dollars or as a share of the guaranteed payout. Moreover, the expected value of the insurance contract is lower by about 11% on average, so beliefs would need to be substantially misreported for this concern to bind. Columns (5) and (6) also show that realized payouts were not higher among firms that opted into insurance. In fact, insurance payouts were almost 50% lower on average than the guaranteed payouts the enterprises would have received if they had declined the contract. This reflects the fact that shops that selected into insurance sold out at 140% of the expected rate and several declined insurance payouts in favor of restocking.

The results of the mechanism experiment indicate that firm risk aversion limits new product adoption. But similar insurance contracts between producers and retailers do not, to my knowledge, exist in the real world. The delivery of payments to firms at the follow-up survey also

[32]The p-value on the heterogeneity of treatment effects is 0.07.

[33]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.

confounds tests of learning since both firms' information sets and cash on hand were affected by the experiment. I therefore turn to the mechanism experiment to examine if the absence of contracts permitting product returns helps explain the effect of risk aversion on helmet stocking in this setting and to test if learning about demand is consequential.

Effects of returns and supplier commitment on helmet adoption: Both the returns and supplier commitment interventions substantially increased helmet adoption, consistent with uncertainty about demand posing a barrier to retail diffusion. Table 2 and Figure 1 report the results of Equation 10. The returns arm increased the propensity of firms to stock helmets within a month of the baseline survey ("Post BL"), either from the study or any other source, by 9 percentage points relative to a 6.8% stocking rate in the control group ($p < .01$). The effect remains remarkably stable—also 9 percentage points ($p < .05$)—when the dependent variable is an indicator for ever stocking helmets by the endline survey ("By EL"). This suggests that returns increased firms' willingness to experiment with selling helmets, rather than simply accelerating adoption among firms that would have stocked regardless. Appendix Table A5 shows that effects are larger among shopkeepers that report high risk aversion, using the same proxy as the mechanism experiment, and in multi-product shops and those that sell motorcycle spare parts.

The supplier commitment intervention increased helmet stocking by approximately 5 percentage points ($p < .05$) in the month following the baseline, but only among firms without access to returns. Consistent with Proposition 2, the supplier commitment had no detectable effect among firms offered returns – a setting in which firms that perceive any chance of profitability should already adopt. This pattern supports the interpretation that firms internalize an "exploration versus exploitation" trade-off, increasing investment when the future value of learning is high, even at the cost of lower short-run utility. As formalized in the model, the absence of effects from the supplier commitment conditional on returns suggests that alternative explanations – such as learning by doing or fixed costs – are unlikely to explain the results, since these forces would predict an effect regardless of returns availability.[34]

Do returns crowd pessimistic or risk averse firms into the market? Results of Equations 11 and 12 suggest that the primary function of returns in this setting is to mitigate risk aversion. Table 3

[34]The fact that the outcome is binary could generate this result in certain cases, but Appendix Figure A2 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, suggesting this is unlikely.

shows that, in the absence of returns, firms with more diffuse priors about helmet sales are significantly less likely to stock helmets ($\beta_1 < 0$). This pattern is consistent with risk aversion: firms uncertain about demand avoid stocking due to downside risk. In contrast, under risk neutrality, one would expect the opposite pattern, since the option value of learning rises with uncertainty. Returns reverse this relationship. Firms offered returns are significantly more likely to stock when their priors are diffuse ($\beta_3 > 0$), implying that returns crowd risk-averse but uncertain firms into the market. [35]

Overall, there is no evidence that returns primarily crowd in firms with more pessimistic beliefs about helmet profitability. Instead, as shown in Appendix Table A6 (which reports the results of Equation 12), returns expand adoption primarily by attracting firms with diffuse but not systematically lower expectations. This finding reinforces the interpretation that the primary role of returns is to mitigate the effects of risk aversion, rather than correcting pessimistic priors.

Helmet sales, restocking, and market entry: The results presented so far suggest that risk aversion deters firms from experimenting with stocking helmets. However, lower rates of helmet adoption would be of limited economic significance if most firms ultimately found helmets unprofitable and exited the market after trying them. In practice, however, I show that the vast majority of adopters reported helmet sales to be profitable and most voluntarily restocked absent any further treatment benefits. These patterns, combined with revealed preference evidence from restocking decisions, suggest that selling helmets is expected utility maximizing for many firms. Consequently, the returns intervention generates large and persistent increases in stocking and entry even after returns are no longer available. These findings indicate that risk aversion and demand uncertainty are first-order barriers to market expansion and suggest that returns policies may address a missing market for risk-sharing in LMIC retail environments.

I first present descriptive evidence on helmet profitability and restocking decisions in Figure 2. On average, adopters reported profits of \$70 from helmet sales by endline, with fewer than 7% reporting any losses.[36] Appendix Table A7 suggests that the high rate of shops reporting profits is partially driven by small non-stock costs of helmets, consistent with the finding that the supplier

[35]These results hold using both linear and non-linear controls for expected sales, and are robust to alternative measures of belief variance, including the standard deviation or the log of the variance of beliefs about demand. Results are also similar when using beliefs about revenue rather than sales, although these data are noisier.

[36]These estimates are net of the respondent's estimate of any foregone profits from items that shop could not stock to afford the helmet stock.

commitment intervention has little value conditional on receiving returns. Reported costs, inclusive of foregone profits from other items the firm could not stock, are essentially equal to the price of the helmet stock that firms' purchased.[37] The mechanism experiment follow-up survey also asked directly if firms incurred any costs to sell helmets other than buying stock. Under 3% of shops reported any other costs, averaging $4 in such cases.

The lack of reported non-stock costs of selling helmets could be rationalized by under-utilized capacity (Egger et al., 2019; Walker et al., 2024). Providing suggestive evidence of this interpretation, less than 10% of firms in the mechanism experiment sample reported working extra hours to sell helmets, with a majority reporting that they were able to leverage excess labor capacity to sell them during downtime when no customers were present. Moreover, the ratio of worst to best week profits in the last month, a proxy for capacity utilization, increased from baseline to endline in the impact experiment among shops that adopted helmets compared to those that did not.[38] Shops sold about 10 helmets on average by the end of the study at a typical price of about 1.5 times the wholesale cost.

A concern with reported helmet profitability is that the measures may fail to capture unobserved costs like effort, meaning that stocking helmets may not be utility maximizing. I therefore turn to the restocking decisions of firms in Panel B of Figure 2, which demonstrates by revealed preference that shops found helmets profitable. Consistent with helmet sales generating positive expected utility, 65% of firms that purchased helmets within a month of the baseline restocked by the end of the study, and over 80% reported plans to continue selling helmets permanently. Notably, restocking rates were similar across treatment arms, suggesting that untreated adopters did not selectively enter based on superior private information about demand.

By endline, shops that stocked helmets after baseline reported that helmet sales accounted for about 10% of total profits, and they expected that share to rise to 20% within three months, indicating plans for expansion. Consistent with these patterns, very few firms made use of the returns policy at midline. Among those eligible to return helmets, fewer than 7% did so, and the

[37] The data also suggest fixed costs and learning by doing are not salient features of the market. Under 15% of shops reported any fixed costs of selling helmets, accounting for under $2.50 on average, the endline helmet profits of firms that entered the market earlier are not higher, and monthly helmet profits at endline are typically not higher than monthly profits at midline.

[38] Over 90% of shops also reported space to store at least 5 helmets without stocking less of another item at baseline, and almost 70% reported space for 10 or more helmets. This further suggests that capacity may not be fully utilized at baseline.

average number of helmets returned was just two. Returns were concentrated among firms that reported losses: 80% of returning firms reported negative profits, while only 3% of firms that did not return helmets reported losses.[39]

Turning to experimental variation and the tests outlined in Equation 13, Table 4 shows that the returns intervention had large and statistically significant persistent effects on helmet stocking after the return window ended. Firms that received a return offer in the first phase of the experiment were 7 percentage points more likely to stock helmets after the return window, a 70% increase compared to the control rate. In markets without a pre-existing helmet seller, the treatment effect rises to 8 percentage points, more than doubling the control stocking rate.[40] The returns intervention also had large effects on market entry, defined as placing at least two helmet orders and reporting an intention to continue selling helmets. Treatment effects under this definition imply more than a doubling of the number of active helmet sellers.[41] These results indicate that agents learned about demand since the groups differ only in that treated shops were more likely to have experience selling helmets in the first phase of the experiment.[42]

The large and persistent effects of the one-time returns offer on helmet stocking suggest a market inefficiency. As detailed in Proposition 4, this result could be rationalized by risk neutral firms with pessimistic priors correcting their expectations or by risk averse agents whose uncertainty about demand was mitigated by learning. To distinguish between these mechanisms, Table 5 presents estimates from Equation 14, which examines how market experience (instrumented using treatment assignment) affected firms' posterior beliefs about helmet demand.

Under risk neutrality, firms should update beliefs primarily through increases in expected sales as they become more optimistic ($\beta_4 > 0$). In contrast, under risk aversion, a reduction in the variance of beliefs alone ($\beta_5 < 0$) can rationalize increased adoption, even if expectations about average sales remain unchanged. The results are consistent with the latter mechanism: I find no

[39]The remaining return was from a firm whose shop was destroyed by flooding.

[40]There is also some evidence that the supplier commitment arm had persistent effects on stocking in markets with no pre-existing seller, but the focus of that intervention is on short-run effects.

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

[42]If profits from helmet sales were sufficiently large, it is possible that the treatment could also help shops overcome capital constraints and confound this interpretation. But this is unlikely given the size of reported profits, and firm profits were down by over 20% at midline and endline due to flooding and riots. The reduction of order sizes to 3 also lowered the capital needed to acquire helmets for all firms at endline. And qualitatively, firms overwhelmingly reported that the treatment was valuable because it showed them they could sell helmets profitably.

evidence that market experience raised firms' expected helmet sales, but I document a statistically significant reduction in the variance of posterior beliefs. This pattern supports the interpretation that the returns intervention operated primarily by allowing risk-averse firms to learn about demand, rather than correcting overly pessimistic priors.[43]

Further supporting the interpretation that firms learned about demand – and validating the credibility of the belief data – Appendix Table A8 shows that firms that adopted helmets shortly after baseline were increasingly accurate in predicting their own future sales. In particular, firms' ability to forecast their endline sales improved substantially between baseline and midline, after they gained experience selling helmets.

Learning from peers: The results of the impact 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 15 suggest that firms can overcome demand uncertainty by observing their neighbors, validating Proposition 5.

I begin by examining the results of the spillover survey in Table 6. The coefficients on "BL market" reports the effect of being in a market where the mechanism experiment induced a neighboring shop to enter the helmet market about three months before the survey. This produces exogenous variation in exposure to a seller since the markets entered at baseline were selected at random. 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 are ex-ante less likely to stock helmets.[44]

The results 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 themselves. Across the sample of motorcycle shops, firms are about 10 percentage points more likely to report knowing a helmet seller in their market ($p < .05$) and to stock helmets ($p < .01$). Across all shops, the estimated effect on stocking reduces to 5 percentage points but remains statistically significant, consistent with the lower bound interpretation. Anecdotal reports from shops help shed light on

[43]Results are similar using levels of expectations and the standard deviation of sales, or using beliefs about revenue instead of helmet sales.

[44]Validating this argument empirically, 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.

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 A9 shows similar results using non-random variation in the impact experiment.

As with returns, information externalities could positively update shops expectations about helmet demand or help risk averse shops overcome uncertainty. The information treatment leverages a drop of anonymous sales data to shops that had not stocked helmets by the endline of the impact experiment to verify that information matters and help disentangle these mechanisms. As shown in Table 7, the results suggest that information from competitors helps to mitigate risk aversion. Receipt of the information treatment (Equation 15a) increases the rate of shops that stock helmets from 0.6% to 2.8% ($p < .05$). Columns (2) - (5) implement Equation 15b, adding an indicator equal to 1 if the signal that treated enterprises received was optimistic.[45] Across specifications, the coefficient on the "high signal" indicator is approximately zero and the indicator for receiving any signal remains positive and significant. In other words, a firm that receives a signal slightly below or above priors is more likely to enter the market, consistent with information externalities mattering primarily because they lower uncertainty.

Figure 1 and Table 2 show that treatment effect heterogeneity also align with Proposition 5. Neither intervention has an effect when there is a pre-existing helmet seller in the same market as a study firm, and one can reject the equality of effects of the return arm across markets with and without a seller with 95% confidence. The adoption rate of untreated firms in markets with a pre-existing seller is also approximately equal to the adoption rate of firms with access to returns in markets with no pre-existing seller.

Helmet market expansion and policy implications: The large effect of returns on firm entry at endline suggests that the impact experiment was effective at expanding consumers' access to motorcycle helmets, an important public health 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,

[45]This leverages the fact that signals were constructed by selecting 5 shops at random to induce small sample variation in the content of signals, resulting in a mean signal of 3.8 sales and a standard deviation of 1.18. In columns (2) and (4) the indicator for a "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.

crowding our firms that otherwise would have began stocking helmets? Second, would all of the treated firms that began selling helmets have adopted them anyways once they observed peers selling them?

The positive information spillovers reported in the prior section indicate that displacement is unlikely, suggesting that if anything the entry of treated shops made competitors more likely to enter the market. I also test for displacement in columns (1) and (2) of Appendix Table A10. 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.[46] I estimate that the treatment increased the likelihood of a shop selling helmets in the area around the respondent by about 8 percentage points, a 33% increase over the untreated rate. The effect is larger if I examine whether a seller was ever reported near the sampled shop: returns had about a 14 percentage point effect on a base of just over 30%. These results are consistent with the finding that effects are concentrated in markets with no seller at baseline reported in the prior section, suggesting that the intervention crowded in risk averse firms in areas where there were otherwise no sellers to learn from.

The question of whether entrants offered returns would have ultimately entered the market absent the treatment is more challenging to answer since the study was not able to continue observing the long-run choices of shops. However, helmets had been available in the market for over 2 years when the study started, and the manufacturer made a strong effort to market to shops in the majority of the areas where the study took place beginning around the midline survey. This suggests that, at least in the near term, shops were unlikely to enter absent the intervention. Qualitatively supporting this view, around 40% of helmet adopters reported that they definitely would not have tried selling helmets absent a return offer and 80% of these firms cited the fact that returns allowed them to learn that they could sell helmets profitably as the reason the treatment was pivotal. Furthermore, the effects on helmet sales even during the study period are consequential even if the study did just move up entry dates. Appendix Table A10 suggests that returns induced shops to stock about 1.9 additional helmets on average during the study and sell about 1.4 more (both more than double the control mean values). In markets with no baseline helmet seller, these point

[46] Markets in Nairobi are difficult to define, so this approach relies on shops to determine what shops they consider to be proximate. Field officers' assessment of the presence of shops and those of respondents were highly correlated at baseline, suggesting that this measure is informative and that shops are aware of the products competitors sell. Shops nearby a study adopter were also more likely to report noticing an entrant near them ($p < .01$).

estimates jump to about 2.8 additional helmets stocked and 2.1 sold. These values correspond to the returns arm inducing over 600 additional helmet sales during the 5 month study period, a stock value of about $10,000 which accounted for around $15,000 in sales. These helmet sales are also likely to equate to large safety benefits given the low baseline utilization rate of helmets in Kenya.

Since the cost of returns came out to under $200, even these medium-run effects suggest that the intervention had a high benefit-cost ratio, a product of the fact that smoothing risk aversion only requires payout when state realizations are low, rather than subsidies on all units sold. These estimates are also likely lower bounds on the true market expansion since they fail to account for differences in future sales or positive information spillovers that caused control firms to enter the market. Thin markets for stock returns therefore seem to substantially constrain the expansion of markets for new products, suggesting that the returns to governments or NGOs from supporting similar programs may be large.

5 Conclusion

This paper demonstrates that retail firms in LMICs face uncertainty about demand for new products, producing a barrier to their adoption because they are risk averse. I first show using a stylized insurance contract that offering firms stock of a new product with a mechanism to lower the variance of profits, without increasing returns, lead 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 examine how a commonly used tool in high-income settings, product returns, affects firms' stocking decisions and examine whether demand uncertainty can be overcome through learning. Returns almost triple the rate of shops that experiment with helmet sales and double the rate of entry into the market after the intervention ends, suggesting large efficiency losses from risk aversion but suggesting that learning can resolve uncertainty and yield sustained effects. These results suggest that stock return markets may be thin in low and middle-income countries. Consistent with firms making sophisticated and forward looking investment decisions that internalize the value of learning about demand, an intervention that resolves future supply chain uncertainty also yields increases in short-run new product investment.

The large effects of smoothing risk on product adoption in two different settings are consistent with deviations from risk neutrality leading to consequential distortions. The setting of the impact experiment is likely 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 about demand. Despite this, treated firms sold over twice the volume of motorcycle helmets, an important public 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 them leaving shelves), 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 to construct policies to counter risk aversion.

I also find evidence of positive information externalities, both from an experimentally generated signal and observationally based on the response of firms to a neighbor adopting helmets. This study was not designed to investigate the competitive implications of these results, but theorists have long recognized that such spillovers may produce free-riding problems that lead to too little experimentation with new products (Bolton and Harris, 1999). Research examining these implications could be useful for understanding how competition affects entrepreneurship in LMICs and to inform competition policy.

Finally, this paper demonstrates that treating firms as risk neutral entities in LMICs may be a poor approximation of the on the ground reality. 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). Investigating the role of risk aversion in these choices, and the effectiveness at policies that insure agents against risk, could therefore be valuable to better align academic understanding of LMIC firms with their real world behavior and to identify cost effective policies to increase growth. For instance, policies designed to seed information about new goods similar to those used in agriculture (e.g. Beaman et al., 2021) may also be effective at promoting retail diffusion of new products, and tying microcredit loan payments to firm returns could motivate firms to pursue riskier growth opportunities (Meki, 2025).

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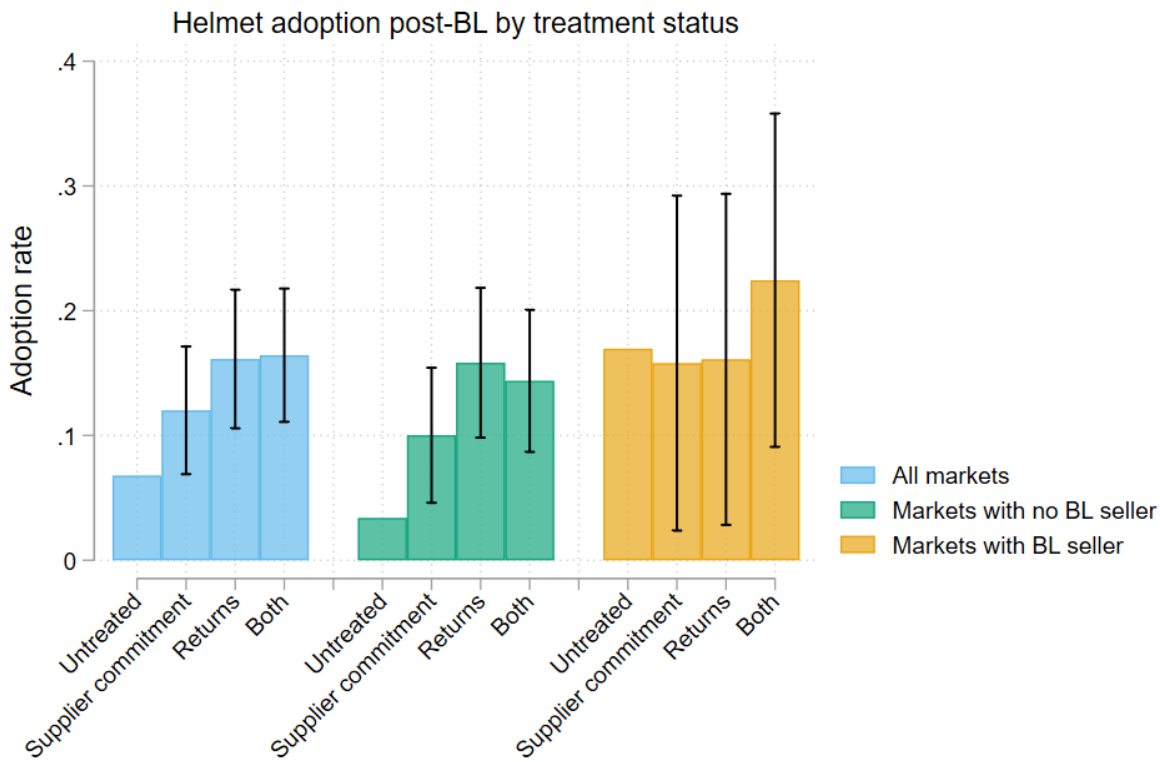
6 Figures

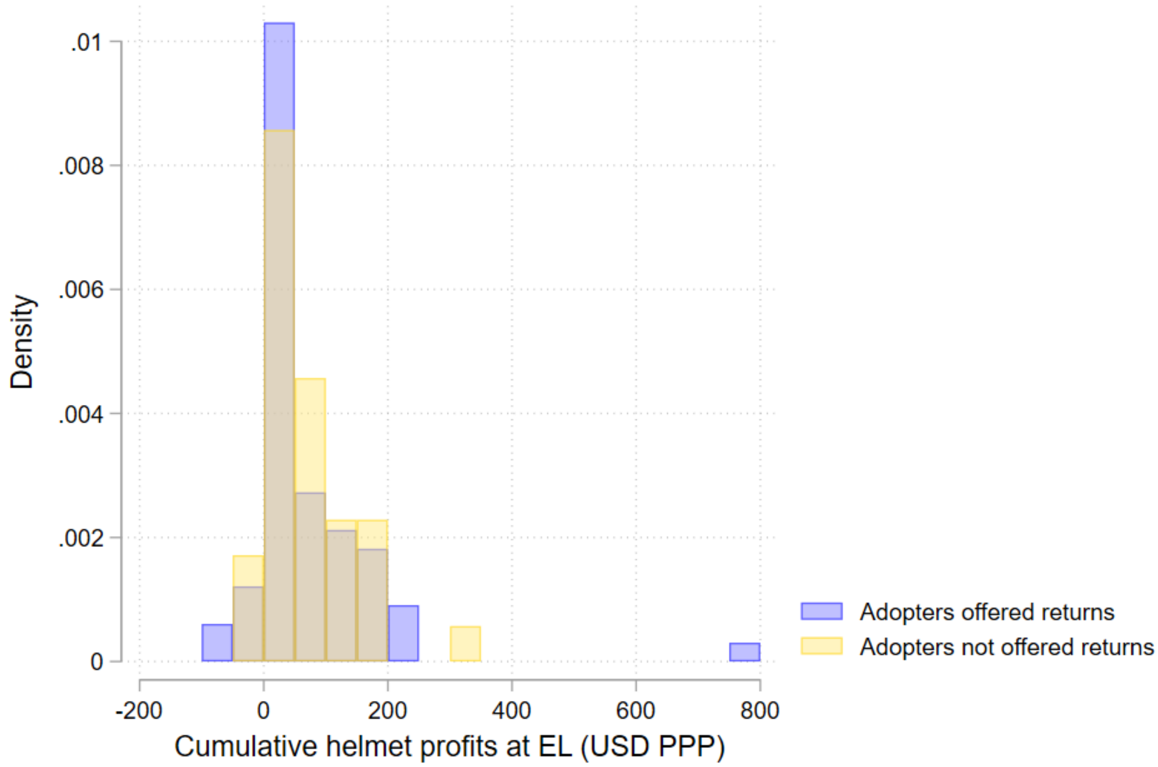
Figure 1: Impact experiment: Treatment effects on short-run helmet adoption



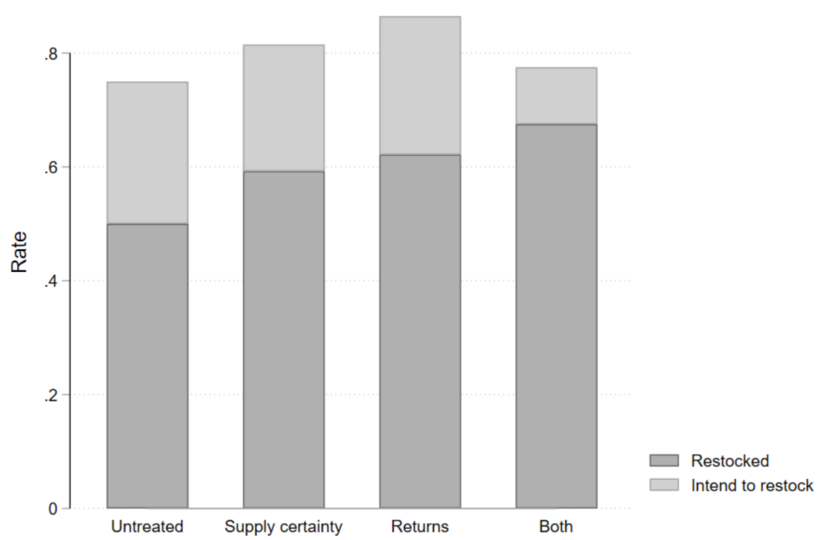
This figure reports the rate of shops that stocked helmets within a month of the baseline survey in the impact experiment, from the study or a different source. The first four bars include all markets ($n = 929$), the next four restrict the sample to markets with no baseline helmet seller present as 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 intervention, with the black line denoting a 95% confidence interval. The third bar is similar, but reports the effect of the returns arm. The fourth bar reports the estimated effect of receiving both treatments.

Figure 2: Impact 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.

7 Tables

Table 1: Mechanism experiment: Insurance offer effects and uptake

Panel A: Effects of insurance offer on helmet stocking

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

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

	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 mechanism experiment. Panel A presents treatment effects of receiving an insurance offer on helmet stocking. Columns (1), (3) and (5) report effects on stocking after baseline, which could occur up to two weeks after the survey, while in columns (2), (4) and (6) the dependent variable equals 1 if the shop stocked helmets within 24 hours of 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: Impact experiment: Treatment effects on helmet stocking

	Strata fixed effects				Full controls			
	Post BL	Post BL	By EL	By EL	Post BL	Post BL	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 impact experiment. Dependent variable in odd columns: shop stocked helmets after the baseline survey. Dependent variable in even columns: shop stocked helmets by the endline survey Robust standard errors in parenthesis. 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 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 3: Impact 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 $\times$ E[sales]	0.011	-0.150		0.027	-0.772	
	(0.034)	(0.394)		(0.115)	(1.564)	
Returns $\times$ $\sigma(sales)$	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 the supplier commitment arm. 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. All estimates include controls for storage space, education, baseline profits, respondent characteristics, and firm characteristics.

Table 4: Impact experiment: Effects of returns offer on post-intervention stocking

	Stocked post return offer				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 $\times$ 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 $\times$ BL Seller		-0.038		-0.047		-0.036		-0.041
		(0.080)		(0.074)		(0.073)		(0.069)
Commitment $\times$ BL Seller		-0.112		-0.043		-0.126*		-0.074
		(0.077)		(0.071)		(0.066)		(0.063)
Returns $\times$ Commitment		0.074		0.019		0.082		0.037
$\times$ 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 after the intervention ends. 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: Impact 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 post-BL	0.217	-1.173**	-0.026	-0.999*	0.095	-1.098**		
	(0.382)	(0.582)	(0.387)	(0.530)	(0.346)	(0.496)		
Returns							-0.001	-0.108*
							(0.047)	(0.063)
Supplier commitment							0.036	-0.040
							(0.043)	(0.058)
Returns $\times$ Supplier commitment							0.056	0.089
							(0.064)	(0.089)
Observations	820	820	832	832	1,652	1,652	1,652	1,652
Dep. var. mean	-0.111	0.059	-0.100	0.130	-0.069	0.062	-0.091	0.090
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weak IV robust p-val	0.561	0.050	0.859	0.073	0.794	0.032		
Survey	ML	ML	EL	EL	Pooled	Pooled	Pooled	Pooled

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

Robust standard errors in parenthesis in columns (1) - (4). Standard errors clustered by firm in columns (5) - (8). The dependent variables are the change in the log of 1 plus the moment of beliefs of firm i since baseline, for instance $\log(1 + \mathbb{E}[\text{Sales at midline}]) - \log(1 + \mathbb{E}[\text{Sales at baseline}])$. 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 6: 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.

Table 7: Effects of information 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

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

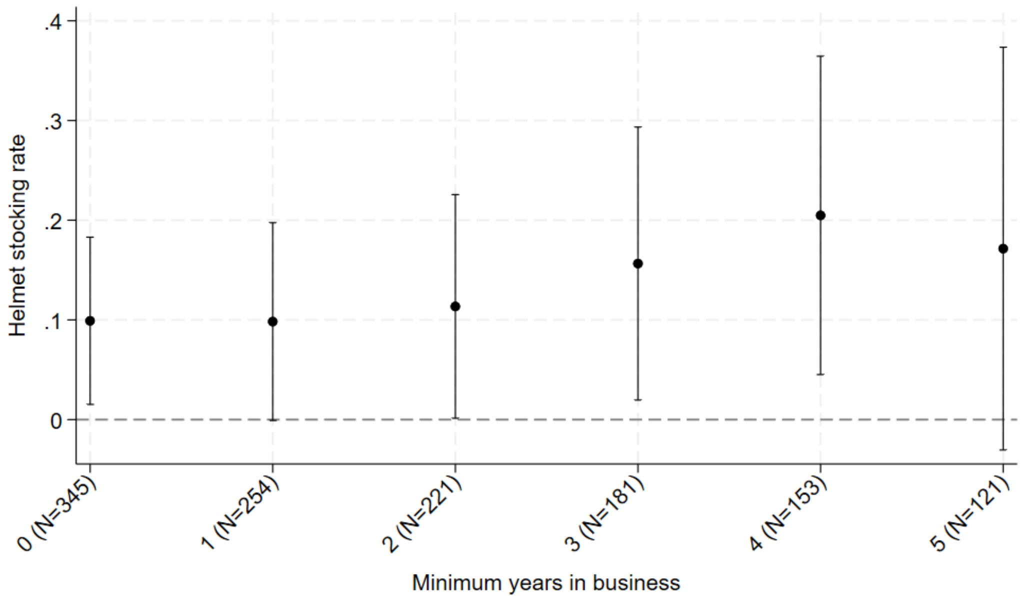
Robust standard errors in parenthesis. The dependent variable is an indicator for ordering helmets at the endline survey and the sample is restricted to shops that had not stocked helmets prior to that point. 1(Information treatment) indicates that a shop received helmet sale and price data from 5 randomly 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.

Appendix

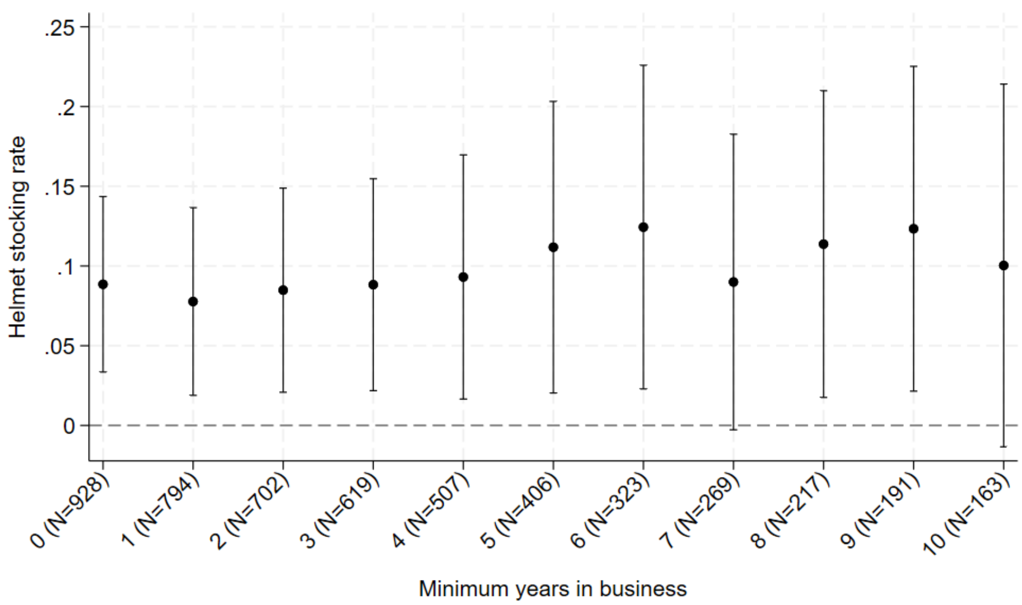
A Appendix Figures

A1: Treatment effects of insurance and returns by enterprise age

Panel A: Treatment effects of insurance by enterprise age

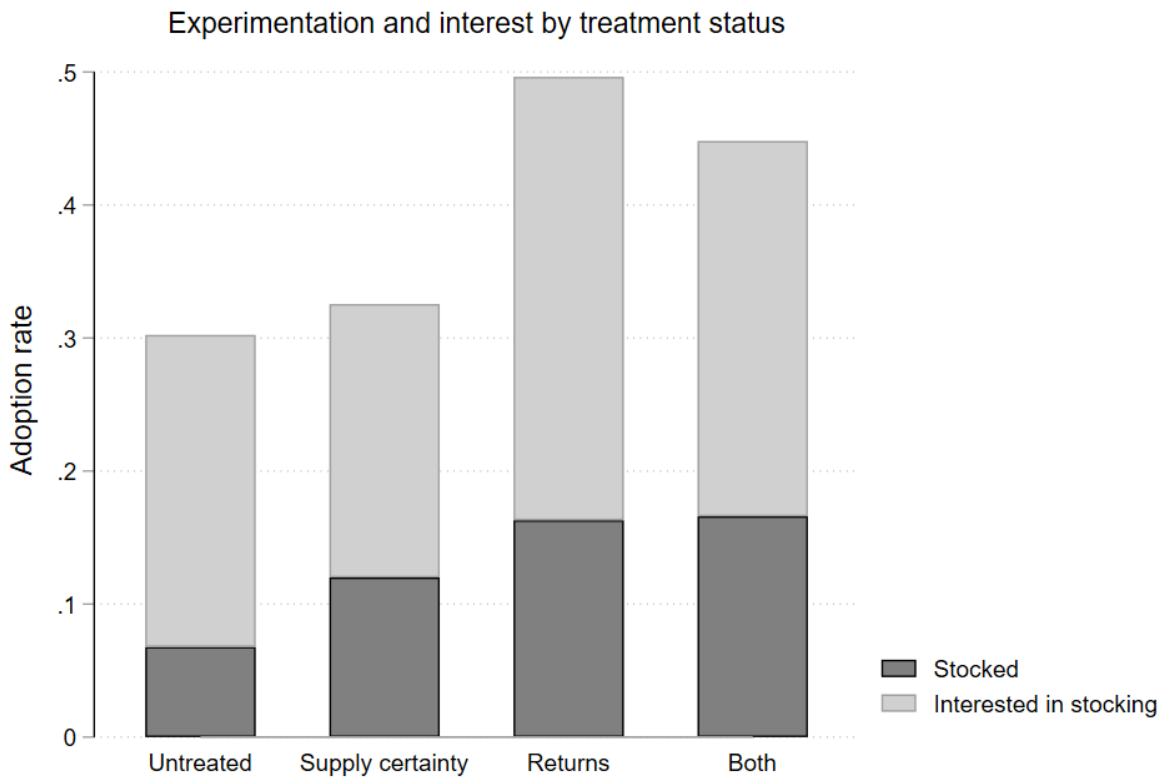


Panel B: Treatment effects of returns by enterprise age



Panel A plots estimates and 95% confidence intervals of the effects of insurance by enterprise age. Each regression controls for market fixed effects, excludes shops unsure of their enterprise age at baseline, and includes 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. Panel B plots estimates and 95% confidence intervals of the effects of returns by enterprise age, including strata fixed effects and industry fixed effects, controls for distance to the manufacturer, log revenue, days open per week, and indicators for stocking a new product in the year before the baseline survey, selling multiple products at baseline, and having space to store helmets without stocking less of another item.

A2: Impact experiment: Treatment effects on short-run helmet adoption plus interest



This figure reports the rate of shops that stocked helmets within a month of the baseline survey in the impact experiment, 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 treatment only. The third bar is similar, but reports the effect of the returns arm. The fourth bar reports the estimated effect of receiving both treatments.

B Appendix Tables

A1: Summary statistics and baseline balance

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

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.

A3: Installment take-up and repayment

	Mean	SD	25th percentile	50th percentile	75th percentile	Obs
Accepted installments	0.65	0.48	0.00	1.00	1.00	107
Balance owed	30.79	25.37	0.02	40.56	57.38	110
Initial payment/minimum	5.03	2.33	4.16	5.16	5.24	109
Balance cleared on time	0.91	0.29	1.00	1.00	1.00	110
Balance cleared pre-ML	0.97	0.16	1.00	1.00	1.00	110
Defaulted	0.02	0.13	0.00	0.00	0.00	110

This table reports take-up and repayment of installment offers among all shops that ordered helmets at baseline. “Balance owed” refers to the upfront balance that the shop faced after deducting any payments they chose to make upon helmet receipt. “Initial payment/minimum” equals the total upfront payment the shop made normalized by the minimum allowed amount. “Balance cleared on time” indicates the shop cleared their installment balance on the intended schedule, and “Balance cleaned pre-ML” equals 1 if all installments were paid before the midline survey. “Defaulted” equals 1 if the shop never finished paying for helmets.

A4: Baseline beliefs about helmet profitability

	Mean	SD	25th percentile	50th percentile	75th percentile	Obs
Panel A: Mechanism experiment						
Pr(Loss$\mid$ 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$\mid$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: Impact experiment						
Pr(Loss $\mid$ 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.

A5: Impact experiment: Heterogeneous treatment effects on helmet stocking

	(1) Risk averse	(2) Multi-product firm	(3) Female	(4) log(Revenue)	(5) 1(Employees)	(6) Motorcycle part shop
Returns	0.028	-0.027	0.095***	0.185	0.093***	0.040
	(0.050)	(0.048)	(0.034)	(0.274)	(0.035)	(0.025)
Supplier commitment	0.007	-0.032	0.064**	0.259	0.045	0.036
	(0.050)	(0.045)	(0.033)	(0.263)	(0.031)	(0.022)
Returns × Supplier commitment	0.066	0.102	-0.072	-0.583	-0.045	0.019
	(0.071)	(0.068)	(0.050)	(0.374)	(0.050)	(0.039)
Variable	-0.103***	-0.028	-0.008	0.067**	-0.046	0.099**
	(0.038)	(0.044)	(0.041)	(0.030)	(0.036)	(0.043)
Returns × Variable	0.105*	0.148**	-0.015	-0.013	-0.016	0.140**
	(0.063)	(0.060)	(0.067)	(0.041)	(0.063)	(0.070)
Supplier commitment × Variable	0.093	0.121**	-0.003	-0.030	0.069	0.060
	(0.059)	(0.056)	(0.060)	(0.039)	(0.065)	(0.064)
Returns × Supplier commitment × Variable	-0.217**	-0.202**	0.062	0.077	-0.035	-0.190*
	(0.089)	(0.086)	(0.098)	(0.056)	(0.097)	(0.100)
Observations	929	929	928	809	928	929
Control mean	0.068	0.068	0.068	0.068	0.068	0.068
Controls						

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

This table reports heterogeneous treatment effects with respect to stocking within a month of the baseline survey in the impact experiment. Estimates include controls for helmet storage space, log revenue, and an indicator for whether the enterprise sold multiple products at baseline. Robust standard errors in parenthesis. All estimates include strata fixed effects.

A6: Impact 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 the returns intervention. The dependent variable in each column is an indicator equal to 1 if the agent received access to returns, and the sample is restricted to those that stocked helmets within a month of the baseline survey and did not receive the supplier commitment offer. Robust standard errors in parenthesis.

A7: Impact 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
BL adopter	-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.

A8: Impact experiment: Correlation between expected and realized sales, midline and endline

	(1) Helmets sold Midline	(2) Helmets sold Endline	(3) Helmet revenue Midline	(4) Helmet revenue Endline
Baseline expectation	0.169		0.056	
	(0.123)		(0.203)	
Midline expectation		0.544***		2.138**
		(0.119)		(0.966)
Observations	107	111	105	101
Average deviation from expectation	-0.48	-0.60	-4.23	34.78
Controls	None	None	None	None

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

Robust standard errors in parenthesis. This table reports regressions of realized helmet sales or revenue on beliefs. In columns (1) and (3), the dependent variable is measured at midline and beliefs were elicited at baseline. In columns (2) and (4) beliefs were elicited at midline and the outcome was measured at endline. The sample consists of shops that accepted helmets after the baseline survey.

A9: Impact 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 within a month of the baseline survey, 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).

A10: Impact experiment: Effect of returns on helmet access

	(1) Helmet seller in market at endline	(2) Ever helmet seller 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. All 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.

C Model Details

C.1 Microfoundations for the Mutlivariate Signal

This section describes microfoundations that lead to signals about demand being distributed $x \sim \mathcal{N}\left(\theta_0, \frac{1}{I_{nt}}\Sigma_n\right)$.

Suppose that every time the retailer sells a helmet, they learn the individual's full realized demand curve. Then by the monotonicity of p_n with respect to its second argument it follows that they observe a realization of $\theta_0 + \nu_n$. Since $\nu_n \sim \mathcal{N}(0, \Sigma_n)$, it follows that $(\theta_0 + \nu_n) \sim \mathcal{N}(\theta_0, \Sigma_n)$. So if ν_n is iid across agents that the retailer sells to, the average signal across customers $\frac{1}{I_{nt}} \sum_{\tau=1}^{I_{nt}} \theta_0 + \nu_{n\tau} \equiv x \sim \mathcal{N}\left(\theta_0, \frac{1}{I_{nt}}\Sigma_n\right)$.

The assumption that the retailer gains a signal of an agent's demand curve from selling a single item would be implausible in settings where firms set prices and customers are price takers. However, in this setting shops reported negotiating extensively over prices for each sale from their first stock, often taking up to a month to complete the transaction after a potential customer first expressed interest in the product. It is therefore reasonable to assume that each transaction yields a signal about the demand curve, rather than just a single data point.

This argument has a known inconsistency with the model. If ν_n were iid across individuals, then variance in demand each period would drop in quantity sold. The correct approach to address this would be for there to be an iid idiosyncratic component to demand fluctuations and a market level shock that varies with time. However, this correction will yield the same comparative static predictions, so for simplicity I introduce only a single variance term to ease exposition. Adding the idiosyncratic component to demand fluctuations would imply that there's always learning value to additional stock, but that investment across multiple periods is also needed to smooth out time fluctuations. In practice, I find that agents are confident they wish to enter after 1 time period, so the time fluctuations seem small and the simplification that I make approximates the data reasonably well.

C.2 Lagrangian and solution to optimal investment

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

$$
\begin{aligned}
\mathcal{L} &= u(c_1) + \lambda_1 \left[(1+r)a_0 - c_1 - a_1 - w_s I_{s1} - w_n I_{n1}\right] \\
&\quad + \kappa_{a1}\left[a_1 - \bar{a}\right] + \kappa_{\chi 1}\left[I_{n1} \cdot (I_{n1} - \chi)\right] + \iota_{s1} I_{s1} \\
&\quad + \delta\, \mathbb{E}_{\theta,\nu_{s2},\nu_{n2}} \left\{u(c_2) + \lambda_2 \left[\pi_s(I_{s1},\nu_{s2}) + \pi_n(I_{n1},\nu_{n2}+\theta) + (1+r)a_1 - c_2 - a_2 - w_s I_{s2} - w_n I_{n2}\right]\right. \\
&\quad \left. + \kappa_{a2}\left[a_2 - \bar{a}\right] + \kappa_{\chi 2}\left[I_{n2} \cdot (I_{n2} - \chi)\right] + \iota_{s2} I_{s2} | \mathcal{I}_1\right\} \\
&\quad + \delta\, \mathbb{E}_{\nu_{s2},\nu_{n2},\theta}\left[V^*(y_t,\theta) - \bar{R}(y_2, \mathcal{I}_2(I_{n1}) | \mathcal{I}_1\right]
\end{aligned} \tag{16}
$$

Differentiating, we get the set of first order conditions

$$
\begin{aligned}
\mathcal{L}_c &: u'(c_t) = \lambda_t \\
\mathcal{L}_a &: \lambda_t = \mathbb{E}_t\, \lambda_{t+1} + \kappa_{at} \\
\mathcal{L}_{I_s} &: \lambda_t w_s + \iota_{st} = \delta\, \mathbb{E}_t \left[\lambda_{t+1} \frac{\partial}{\partial I_{st}} \pi_s(I_{st}, \nu_{st+1})\right] \\
\mathcal{L}_{I_n} &: \lambda_t w_n + \kappa_{\chi t}(2I_{nt} - \chi) = \delta\, \mathbb{E}_t \left[\lambda_{t+1} \frac{\partial}{\partial I_{nt}} \pi_s(I_{nt}, \nu_{nt+1} + \theta)\right] - \delta^2\, \mathbb{E}_t \left[\frac{\partial}{\partial I_{nt}} \bar{R}(y_{t+1}, \mathcal{I}_{t+1}(I_{nt}))\right]
\end{aligned} \tag{17}
$$

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,

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

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

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

C.3 Derivation of the derivative of Bayesian regret w.r.t investment

This section shows that

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

where the inequality is typically strict whenever expected regret is positive.

Consider an agent in time t making investment I_{nt}. Then in time period $t+1$, they will receive the signal from this investment, which will affect their Bayesian regret associated with decisions beginning in period $t+2$, $\bar{R}(y_{t+1}, \mathcal{I}_{t+1}(I_{nt})$. Our 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. We may leverage the definition of Bayesian regret and the Law of Iterated Expectations to obtain

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

where 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}. Thus

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

where 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 $x(I_{nt})$, then 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 separate 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 $x(I_{nt}) \sim \mathcal{N}\left(\theta_0, I_{nt}^{-1}\Sigma_n\right)$, but does not know θ_0. The distribution of expected signal draws is thus distributed $\mathcal{N}\left(\mu_t, \Sigma_t + I_{nt}^{-1}\Sigma_n\right)$ where μ_t and Σ_t are the posterior beliefs given $\mathcal{I}_t$. I first show that the second term is 0, which is intuitively the case since a small change in investment level should not change the value of the signal that the agent receives. Formally,

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

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

Now I turn to the first term in the derivative, which will not be equal to zero since the agent expects investment to lower posterior variance, which in term is correlated with $u(c^*_\tau(\theta))-u(\bar{c}_\tau(\theta))$. For a fixed draw $x(I_{nt})$,

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

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

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

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 mean beliefs. In particular,

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

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

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

Now we may substitute for $\frac{\partial \Sigma_\tau}{\partial I_{nt}}$, collect the two terms containing this expression, and evaluating one has

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

Substituting, we therefore have that

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

Applying the Law of Total Covariance,

$$
\begin{aligned}
\frac{\partial}{\partial I_{nt}}\bar{R}_\tau &= -\frac{1}{2}\,\mathbb{E}_{\mathcal{I}_\tau,\nu_n,\nu_t,x}\left[-\frac{1}{2}Cov\left(u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta)), (\theta-\mu_t)'\Sigma_n^{-1}(\theta-\mu_t\big|x(I_{nt})\right)\big|\mathcal{I}_t\right] \\
&= -\frac{1}{2}Cov\left(u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta)), (\theta-\mu_t)'\Sigma_n^{-1}(\theta-\mu_t)\big|\mathcal{I}_t\right) \\
&\quad - \frac{1}{2}Cov\left(\mathbb{E}_\theta\left[u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta))|x(I_{nt})\right], \underbrace{\mathbb{E}_\theta\left[(\theta-\mu_t)'\Sigma_n^{-1}(\theta-\mu_t)|x(I_{nt})\right]}_{=Tr(\Sigma_n^{-1}\Sigma_t)}\big|\mathcal{I}_t\right) \\
&= -\frac{1}{2}Cov\left(u(c^*_\tau(\theta)) - u(\bar{c}_\tau(\theta)), (\theta-\mu_\tau)'\Sigma_n^{-1}(\theta-\mu_\tau)\big|\mathcal{I}_t\right)
\end{aligned}
$$

Therefore

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

So in short the expected marginal reduction in regret associated with increasing I_{nt} is a function of the expected marginal reduction in the variance of posteriors Σ_n^{-1} 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. $\frac{\partial}{\partial|\Sigma_n|}\frac{\partial \bar{R}}{\partial I_{nt}} > 0$, meaning that the marginal reduction in regret falls in magnitude if signals are less precise. Conversely, $\frac{\partial}{\partial\sigma_\tau}\frac{\partial|\Sigma_t|}{\partial I_{nt}} < 0$ since the covariance term will grow if information about 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.

C.4 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 $\pi_n(\chi, \nu_{nt} + \theta) = \pi^p_n(\chi, \nu_{nt} + \theta) + \epsilon$ and satisfying $\mathbb{E}[\epsilon|\pi^p_n(\chi, \nu_{nt} + \theta)] = 0$.

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 $\delta\,\mathbb{E}_t[\pi^p_n(\chi, \nu_{nt} + \theta)]$ 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.[47]

Case 2: If the agent is risk averse, then $u''(c_t) < 0$. Consumption smoothing will lead the agent to

[47]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.

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 $\varphi(\pi_n^p(\chi, \nu_{nt} + \theta))$.[48] 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

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

[48]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.

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, $\frac{\partial}{\partial \Gamma} I^*_{nt} = 0$.

Proof of Proposition 3

Part a: Appendix C.3 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 θ_2 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, $Cov_t \left(u'(c_{t+1}), \frac{\partial}{\partial I_{nt}} \pi_n(I_{nt}, \nu_{nt+1} + \theta_2) \right) < Cov_t \left(u'(c_{t+1}), \frac{\partial}{\partial I_{nt}} \pi_n(I_{nt}, \nu_{nt+1} + \theta_2) \right)$ and the gap will grow as the agent becomes more risk averse. Therefore for a sufficiently risk averse agent, I^*_{nt} is higher under θ_1 than θ_2.

Part b: The price floor does 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 the price floor 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 the price floor.

If the agent is risk averse, then the price floor 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 the price floor 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 the price floor 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.

Proof of Proposition 4

Part a: 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 $Cov_t \left(u'(c_{t+2}), \frac{\partial}{\partial I_{nt+1}} \pi_n(I_{nt+1}, \nu_{nt+2} + \theta) \right) = 0$.

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

Proof of Proposition 4

Part a: The argument is identical to part a of the prior proposition.

Part b: 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.

C.5 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.[49]

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 $\pi_n^p(3, \nu_{nt+1} + \theta)$

[49] 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.

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 $x < 3 \cdot h_i$, $F_p(x) \le F(x)$ and so we immediately have that $\int_{-\infty}^{x} F_p(y)dy \le \int_{-\infty}^{x} F(y)dy$ and the inequality is strict at $x = 2 \cdot h_i + P_i$ since $p_i < 1$. For $y \ge 3h_i + P_i$, $F_p(y) = F(y) = 1$ and so if $\int_{-\infty}^{x} F_p(y)dy \le \int_{-\infty}^{x} F(y)dy$ 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 $\int_{-\infty}^{3h_i+P_i} F_p(y)dy \le \int_{-\infty}^{3h_i+P_i} F(y)dy$.

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 $h_i \ge 1,000$.

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

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.