Competition and Entry in Agricultural Markets: Experimental Evidence from Kenya

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Abstract

African agricultural markets are characterized by low farmer revenues and high consumer food prices. Many have worried that this wedge is partially driven by imperfect competition among intermediaries. This paper provides experimental evidence from Kenya on intermediary market structure. Randomized cost shocks and demand subsidies are used to identify a structural model of market competition. Estimates reveal that traders act consistently with joint profit maximization and earn median markups of 39%. Exogenously-induced firm entry has negligible effects on prices, and low take-up of subsidized entry offers implies large fixed costs. We estimate that traders capture 82% of total surplus.

JEL Classifications: D22, D43, F12, L13, L81, O13, Q13

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1 Introduction

The 1980s and 1990s saw a wave of liberalization sweep across African agricultural markets as part of broad structural adjustment plans. Inherent in the promise of these reforms was the presumption that a competitive private sector would emerge to take advantage of newly created arbitrage opportunities, with agricultural traders efficiently moving crops from surplus to deficit regions, and from harvest to lean seasons. However, recent empirical estimates suggest that agricultural markets remain poorly integrated, with prices varying widely across regions and seasons (Moser et al., 2009; Burke et al., 2019). High transaction costs contribute to this limited market integration. Transport costs in Africa are the highest in the world (Teravaninthorn and Raballand, 2009); also prevalent are harder-to-measure costs associated with search (Aker, 2010), contractual risk (Startz, 2017), and price uncertainty (Dillon and Dambro, 2016).

However, much less is known about the form of competition among intermediary traders in agricultural markets in developing countries. Whether, and how much, traders are exerting market power matters for policymaking: if intermediaries are operating in a competitive environment in which price gaps are purely due to high transactions costs, then policies that reduce these transaction costs – road improvements, preferential terms for business expansion loans, and trade intelligence systems for broadcasting prices to traders, for example – would yield savings that traders will pass on to farmers (in the form of higher prices) and consumers (in the form of lower prices). On the other hand, if traders are exercising a high degree of market power, gains from policies that reduce traders' operating costs may be captured mostly by intermediaries. To meaningfully improve farmer and consumer welfare in this environment, policies may need to explicitly target enhanced competition among intermediaries.

In this paper, we present some of the first experimental evidence on the market structure in which African agricultural traders operate. To this end, we implement three randomized control trials that deliver new empirical evidence on the extent of cost pass-through, the shape of demand, and the effects of entry on market prices. We interpret these estimates in the context of a structural model and estimate the model of imperfect competition that best fits the data and its welfare implications.

In the first experiment, we exogenously reduce traders' marginal costs by offering to all traders in a market a substantial, month-long subsidy per kg sold. We then observe how much of this reduction in costs is passed through to the price offered to consumers. We find that traders pass through only 22% of this reduction in costs to customers.

Nonetheless, the pass-through rate is insufficient to characterize imperfect competition as the curvature of demand could produce lower pass-through rates, holding behavior of intermediaries constant. For example, the observed rate of pass-through could be consistent with a Cournot competitive market structure with highly concave demand or with a perfectly collusive market structure with moderately concave demand. In order to distinguish between the roles played by intermediary conduct and consumer demand curvature, we run a second experiment to estimate the curvature of demand. In this experiment, we offer consumers random reductions in price spanning a range of counterfactual pass-through rates and measure the resulting quantities purchased. This experimental variation in prices allows us to identify consumer demand without having to rely on strong identification assumptions. We specify and estimate a highly flexible demand function.

To determine agricultural intermediaries' form of competition, we start with a simple model of demand and supply that nests Cournot competition and joint profit maximization. This model transparently maps the experimentally estimated pass-through rate and demand curvature into inference about the form of competition. We find that traders are not competing – the estimated parameter governing how traders value other traders' profits is statistically indistinguishable from that representing a perfectly collusive model in which traders form agreements (perhaps tacitly) about prices and act as a single profit-maximizing monopolist in the market. We can rule out Cournot competition with 95% confidence.

We then relax key assumptions in the simple model and estimate a more general model that allows for within-market trader heterogeneity and non-constant marginal costs, as well as an extensive margin to demand. We rely on the same experimental variation to identify the more general model, and introduce additional instruments describing how a trader's choices in one market depend on experimental variation in the other markets in which he sells. These multi-market traders, and their exposure to experimental variation across all of their markets, allow us to identify a non-constant marginal cost function. Our estimates from the more general model again imply conduct consistent with joint profit maximization; as with the simple model, we can reject Cournot competition. Traders capture high markups, with the median trader earning a 39% markup. We also offer non-nested tests of joint profit maximization and Cournot competition that again point toward joint profit maximization as describing traders' conduct.

Our third experiment generates exogenous entry by offering traders incentives to enter randomly selected markets for the first time. These results serve two purposes. First, we test whether policies that encourage market entry can decrease market power and promote competition. Second, traders' willingness to accept the entry subsidies – the size of which is randomized – reveals how they trade off fixed costs versus variable profits, which enables us to estimate total trader profits and conduct a complete welfare analysis. We find that the entry subsidies induce an additional 0.6 traders per market-day on average, a 16% increase over the mean market size (and 21% over the median). These additional traders have only small impacts on price. We find that competition in markets in which entrants have no prior connections increases to a level that is statistically indistinguishable from Cournot competition, but that markets in which entrants have prior connections continue to be collusive. Entrants with no prior connections, however, are less willing to take up the entry offer, indicating that the most likely compliers from a policy aimed at increasing entry may not be effective in increasing competition.

We use this experimental variation to identify an entry model in which potential entrants decide to enter a market when their variable profits upon entry – which depend on marginal costs and the impact of entry on market competition – exceed their fixed costs, net of the experimental entry subsidy. Our estimates indicate that potential entrants have high fixed costs that are positively correlated with marginal costs. Extending the estimates to incumbent traders, we find that the median trader's fixed costs constitute 71% of variable profits; the median (mean) trader keeps 12% (25%) of revenues as profit, but the largest traders earn the highest markups such that in the aggregate traders capture 82% of total surplus, while consumers capture just 18%.

We use our estimated demand, quantity-setting, and entry models to solve for counterfactual equilibria were traders to engage in Cournot competition. We estimate that switching from joint profit maximization to Cournot competition would have large effects on surplus division, as consumer surplus would triple and deadweight loss would fall by a third.

This paper is one of the first to experimentally test the competition model of rural agricultural markets directly. Previous attempts to measure competition or market efficiency have mainly relied on observational methods. Observational studies have typically found high rates of pass-through across major markets (Rashid and Minot, 2010), though these high transmission rates may not extend beyond major urban markets (Moser et al., 2009). Moreover, interpretation of this observational evidence is confounded by common shocks such as shared harvest times and reverse flows across seasons. One exception to this primarily observational literature is a recent paper by Casaburi and Reed (2016), which studies the effect of an experimental subsidy per unit purchased by cocoa traders in Sierra Leone. They find small pass-through in terms of price, but larger pass-through in credit, suggesting the importance of interlinked relationships in their context (a feature not relevant in the Kenyan maize markets we study, in which over 95% of transactions are conducted in cash).[1]

Another set of papers attempts to directly measure traders' profits in order to draw inference about the size of rents and model of competition. These have generally found that average trader profits are moderately large, though subject to significant variability, leaving a question mark on whether these returns represent rents or risk premia (Dillon and Dambro, 2016). Moreover, these direct measures are subject to potentially severe measurement error in the face of difficult-to-quantify search, own labor, and fixed costs, as well as a general lack of record keeping (Fafchamps et al., 2005).[2] The sensitivity of directly asking about profits in an environment in which traders are often labeled as exploitative presents a further challenge.

Finally, a set of papers has applied experimental methods to the somewhat related question of the impact of offering price information to farmers on their ability to extract better prices from traders. While most studies find null results (Fafchamps and Minten, 2012; Mitra et al., 2015),[3] it is unclear if this suggests traders are already offering competitive prices given their costs or whether farmers are simply unable to use this information to improve their bargaining position. There is therefore a paucity of causal evidence on how traders compete (Dillon and Dambro, 2016) despite a growing interest in the role these intermediaries play in determining the allocation of gains from trade (Antras and Costinot, 2011; Bardhan et al., 2013).[4]

More broadly, this paper follows a long literature on testing among different models of competition. We follow Nevo (2001) and Miller and Weinberg (2017), among many others, in testing among common models of competition nested in a single framework. We also implement non-nested tests of joint profit maximization and Cournot competition in the spirit of Berry and Haile (2014) and similar to Backus et al. (2019a), which avoids some

[1]Because their subsidy is offered only to a subset of traders in the market, Casaburi and Reed (2016) must ultimately rely on observational estimates of pass-through to measure the form of competition, as their experimental estimates appear to be affected by within-market spillovers. Further, in the absence of evidence on the shape of farmer supply, they are forced to make strong linearity assumptions. Because the curvature of the market facing traders (farmer supply in their case, consumer demand in ours) is crucial to interpreting the pass-through rate, we experimentally estimate this curvature.

[2]Only 58% of traders in our sample keep any written records and, among this group, most records are fairly rudimentary.

[3]The exception is Hildebrandt et al. (2015), which finds that farmers who receive price information earn 5% higher prices for their yams, but this effect disappears by the second year of the study.

[4]In a quasi-experimental variant of this literature, Casaburi et al. (2013) find that expansion of the road network in Sierra Leone led to price decreases that can be best explained under a search cost framework, and which are inconsistent with either Bertrand competition or Cournot oligopsony.

of the issues with the nested approach. To identify the form of competition, we rely on an experimental cost shock. This idea of identifying and quantifying deviations from perfect competition using pass-through rates dates back to Sumner (1981), who used cost variation from cigarette excise taxes. Sullivan (1985) and Ashenfelter and Sullivan (1987) also used excise tax pass-through while the trade literature has leveraged exchange rate pass-through (Feenstra, 1989; Knetter, 1989; Aw, 1993; Goldberg, 1995; Goldberg and Knetter, 1999).[5] Bulow and Pfleiderer (1983) clarified the role of demand functional form in inferring competition from pass-through, and more recent contributions to the literature on pass-through include Weyl and Fabinger (2013) and Atkin and Donaldson (2015). Unlike many of these papers, we use experimental variation to identify the model components, adding to the recent literature using experimentally estimated parameters to understand market structure in developing countries (Keniston, 2011).

This paper proceeds as follows: Section 2 describes maize markets in Kenya. Section 3 introduces a general and simplified theoretical supply and demand model. These motivate the experimental design, which we describe in greater detail in Section 4. Section 5 presents results on pass-through and demand, and then uses these estimates to identify the form of competition within the simple model. Section 6 relaxes some of the simple model's assumptions and presents results from the more general model. Section 7 describes results of the entry experiment and estimation of an entry model that we use to quantify welfare in Section 8. Section 9 concludes.

2 Maize Markets in Kenya

Staple commodities represent a major expenditure for consumers across Africa. In Kenya, maize – the country's primary staple commodity – is responsible for over a third of average gross caloric intake. The median household spends 9% of its annual expenditure on maize (and the poorest decile spends 14%). On the production side, about half of all Kenyan households grows maize (Argent and Begazo, 2015). The functionality of these staple commodity markets is therefore of significant importance for household welfare.

Figure A.1 displays the maize output market chain in western Kenya. Regional traders, the subjects of this study, are responsible for large-scale aggregation, storage, and transportation. They report purchasing 50% of their maize from small-medium farmers (selling < 5 tons), 16% from large farmers (selling ≥ 5 tons), and 33% from other traders. They buy primarily from counties throughout western Kenya and neighboring regions in eastern

[5] Relatedly, Panzar and Rosse (1987) developed predictions based on pass-through of cost shocks into revenues.

Uganda (the latter is more common during the lean season, when local supply is scarce in Kenya).[6] Traders tend to own a warehouse in a market center and either rent or own a truck which they use to purchase maize, bring it back to their warehouse for sorting, drying, and re-packaging, and then carry onward to their destination of sale. In our sample, 64% of sales take place in open-air markets in rural communities. 16% is sold to millers, who grind maize into flour for sale to stores that serve urban consumers. Another 16% is sold to other traders, who sell in other areas of Kenya or eastern Uganda. A small portion of sales – about 2% – is sold to restaurants, schools, and other institutions. Finally, 2% is sold to the Kenyan National Cereals and Produce Board, the former state maize marketing board that still has limited involvement in the market by purchasing, storing, and selling small reserves of maize with a goal of stabilizing prices.

2.1 Entry into Regional Trade

As part of a broad plan of structural adjustment in the 1980s and 1990s, Kenya pulled state-controlled marketing boards out of staple grain markets, lifted trade restrictions on export crops, and allowed prices to be determined by market forces, rather than by state mandate. Today, few legal barriers exist to entering the maize trade.[7] However, engaging in large-scale, regional wholesale trade still requires significant working capital in order to pay for inventory, storage facilities,[8] and transport vehicles.[9] Further, traders must develop extensive networks of contacts in order to glean information on prices and product availability, as this information is disseminated one-on-one through personal networks of fellow traders rather than through any centralized or open information clearinghouse. It is common for traders to enter the business with the support of siblings, spouses, or even former employers who already have experience in the business. Therefore, while entry is close to free legally, those who wish to enter regional trade still face significant barriers.

Table A.1 presents trader demographic details. The average trader has completed some

[6]Due to differences in crop calendars, farmers in eastern Uganda harvest maize several months earlier than those in western Kenya

[7]The few permits that are required are either easy to obtain or are unenforced. The primary license required is the Annual County Business License, which costs about USD $100/year and is issued by county officials. Traders report this license is easy to get and most have this license (though most also report that this license is not well-enforced). Other licenses are very poorly enforced, if at all, including a public health license and a transport permit. There are more serious inspections and permits required for cross-border trade. Finally, there is a small USD $2 "cess" tax charged to traders in the market each day.

[8]Though long-term storage is uncommon among traders, short run facilities are necessary for cleaning, drying, and sorting.

[9]For example, rental of a truck per day costs $250 (about 18% of annual GDP per capita), while purchasing a truck costs $30,000 (over 21x annual GDP per capita).

secondary school and is able to answer half of the Ravens matrices (Group B) questions. Only 58% keep written records, which typically include only sale prices and quantities; rarely are cost or accounting data recorded. However, 62% do report reviewing their financial strength monthly. Most traders operate one-man businesses, with only 37% having any employees.

2.2 Open Air Markets

This study takes place in the open air markets in which traders sell the majority of their produce. These markets typically occur on a set day each week. The traders present are a mix of those who have their warehouse in that particular market and those who arrive with a truck and sell out of its back for the day. Traders with trucks typically park next to each other in a particular area that they use each week, and warehouses are typically in a row or clustered. Importantly, trader prices, while not posted in any public way, are presumably common knowledge given the close physical proximity of traders. Figure A.2 presents the histogram of the number of traders per market, which varies from 1-10 with a median of 3. Traders commonly work in the same set of markets each week, with 96% of traders reporting working in that market most weeks and only 1% saying that this was their first time in the market (see Table A.1). 77% have worked previously with *all* other traders in the market that day. As a result, 68% say they know the other traders in the market that day "very well," 26% "somewhat well," and only 6% "not very well." When asked directly, only 38% of traders report "discussing a good price" with other traders and only 30% report engaging in an explicit price agreement with other traders; the vast majority claim they are rigorously competing on price. However, 72% of traders work in a market in which at least one trader has reported the existence of a price agreement that day. Most (83%) traders visit only one of our 60 sample markets during the 12-week experimental period, though multi-market traders visit an average of 2.7 markets.

Customers in these markets are comprised of two-thirds individual households and one-third rural retailers. The median consumer buys maize only from her local market, though a few retailers purchase from a larger number. We therefore model consumers as considering one local market.[10] The median customer buys maize for consumption every week; storage is rare (see Appendix C.1). The product itself is fairly homogenous (see Appendix C.2).

[10]Consistent with this, we find that the nearest market is on average 8km away (7km for the median market). On the same day, the nearest active market is 22km away (17km for the median market). Data on consumer behavior is drawn from a phone survey with 165 consumers randomly selected from the demand experiment sample. This survey was conducted in July and August 2016 immediately following data collection for the main experiment. In Appendix H we find some evidence of substitution from markets outside of our sample into markets in our sample and discuss how this might affect our results.

3 Theoretical Framework

The experimental design employed in this study is tightly tied to theory. In this section, we introduce a model of supply and demand for a homogeneous product, starting with a general model with limited assumptions. We then offer a simplified version of this model which demonstrates that, with the addition of a few key assumptions, a parameter nesting models of competition is a function of a small number of sufficient statistics. These sufficient statistics form the basis for the experimental design, with each of the three experiments implemented here designed to identify a specific parameter. Experiment 1 identifies pass-through, while Experiment 2 identifies the curvature of demand. We use these two estimates to infer the model of competition that best describes how traders operate. In Section 6, we relax these simplifying assumptions and return to estimating the more general model – still relying on the experimental variation for identification. We then return to the third experiment, where the number of traders in the market is experimentally manipulated, and estimate the effect on how traders compete. Figure 2 provides an overview of how the experiments map to theory.

3.1 Model Set-Up

We begin with a model of demand and supply of a homogeneous good.[11] Household i, in week w and its local market m, demands $q_{imw}(P_{mw}) \geq 0$ kilograms of maize at market price P_{mw}. Given the set $\mathcal{I}_{mw}$ households in local market m in week w, market demand is $Q_{mw}(P_{mw}) = \sum_{i \in \mathcal{I}_{mw}} q_{imw}(P_{mw})$ with corresponding inverse market demand $P_{mw}(Q_{mw})$. Traders choose quantities for each market they visit each week to maximize weekly profits.[12] We write trader j's maximization problem as:[13]

[11] Whether the product is differentiated matters for how we infer the form of competition from equilibrium prices and quantities, as the same equilibrium responses to shocks could be consistent with one model of competition under homogeneous products and a different model of competition under differentiated products. However, the weight of evidence suggests maize sold in these markets is fairly homogenous. There is little variation in quality, price, or other dimensions of the sale (e.g., credit is rarely used). See Appendix C.2 for further detail. Price discrimination also appears quite limited, with an intra-cluster correlation of 0.9 between the prices that a trader offers his various customers throughout the day (see Appendix C.3). This is likely because negotiations between traders and consumers are conducted in public, thereby limiting traders' ability to engage in dramatic price discrimination. We therefore assume a single market price.

[12] Specifying a weekly cost function allows for the possibility that the subset of traders active in multiple in-sample markets in the same week may have cost interdependencies across markets.

[13] The model employed here is static. While maize is in theory storable, empirically, consumer stockpiling is quite limited (see Appendix C.1). Related work in the region suggests that credit constraints limit households' ability to arbitrage these price fluctuations (Burke et al., 2019).

$$\max_{\{q_{jmw}\}_{m\in\mathcal{M}_{jw}}} \pi_{jmw} = \sum_{m\in\mathcal{M}_{jw}} P_{mw}(q_{jmw})q_{jmw} - C_{jw}(q_{j1w},...,q_{jMw}) \tag{1}$$
$$+\,\omega\left[\sum_{m\in\mathcal{M}_{jw}}\sum_{k\neq j} P_{mw}(q_{jmw})q_{kmw} - \sum_{k\neq j} C_{kw}(q_{j1w},...,q_{jMw})\right]$$

where $\mathcal{M}_{jw}$ is the set of markets j visits in week w and C_{jw} is j's total costs in week w across all markets. The second set of terms in brackets, multiplied by ω, captures other traders' profits in the same markets. $\omega \in [0,1]$ therefore serves as a profit weight summarizing how a trader internalizes the profits of other traders in his market, with $\omega = 0$ reducing to Cournot and $\omega = 1$ reflecting joint profit maximization.[14] Taking the derivative of Equation 1 with respect to the trader's quantity q_{jmw} in market m in week w yields the trader's first order condition:

$$P_{mw} = \frac{\partial C_{jw}}{\partial q_{jmw}} - \frac{\partial P_{mw}}{\partial q_{jmw}}\left(q_{jmw} + \omega\sum_{k\neq j} q_{kmw}\right) \tag{2}$$

where $\omega = 0$ and $\omega = 1$ reduce this to the familiar Cournot and monopoly first order conditions, respectively.

We will estimate a parametric form of this general model in Section 6. But to highlight how our experiments can directly identify the model of competition, we first offer a complementary simplified version of the model that reduces inference about the model of competition to a few statistics. This relies on two simplifying assumptions. First, consistent with Fafchamps et al. (2005), we assume marginal costs ($c_{jmw} = \frac{\partial C_{jw}}{\partial q_{jmw}}$) are constant with respect to quantities. This appears to be a good approximation of the empirical setting,

[14]Under this formulation, ω can be interpreted structurally – traders directly value the profits of other traders – or as a parameter summarizing conduct. For the structural interpretation, the literature has focused on cases where there is a common claimant on agents' profits (e.g., vertically integrated units in Crawford et al. (2018) or firms with common investors in Backus et al. (2019b)). In our setting, there are several features that could motivate a structural interpretation of the profit weight. Specifically, the structural profit weight encompasses relevant cases including traders from the same extended family, altruism toward fellow traders, and social insurance. If we proceed without a structural interpretation, the profit weight has no direct interpretation outside of $\omega = 0$ and $\omega = 1$. Fan and Sullivan (2018) derive a profit weight model consistent with a set of supergames, though they point out an additional term capturing rivals' deviation profits is necessary. We therefore primarily employ ω as a convenient formulation for nesting the two well-defined model of competition for which we will test empirically in our experiment. We also provide a direct non-nested test of Cournot and joint profit maximization. Finally, while versions of the reduced form approach can bias estimates towards too much competitiveness if the traders are colluding at a price below the monopoly price (Corts, 1999), our empirical estimates in Section 6 indicate joint profit maximization.

in which the major variable costs are constant with respect to quantity (see Appendix D). In the general model, we will relax this assumption (though estimates do suggest costs are fairly flat). Second, we assume symmetry across traders in a market-week, specifically with respect to initial marginal cost ($c_{jmw} = c_{mw}$). Again, we will relax this assumption in the general model.

Given these assumptions, our first order condition reduces to:

$$P_{mw} = c_{mw} - (1 + \omega(N_{mw} - 1)) \frac{\partial P_{mw}}{\partial Q_{mw}} \frac{Q_{mw}}{N_{mw}} \tag{3}$$

where N_{mw} is the number of traders in the market m and week w. ω continues to nest Cournot competition and full collusion.[15] We see that equilibrium markups depend on the shape of demand and two features of market structure and trader behavior: the number of traders N_{mw} and whether these traders interact according to Cournot competition ($\omega = 0$) or joint profit maximization ($\omega = 1$).

3.2 Pass-Through and Demand Curvature

In the first part of the paper, we use two experiments – one identifying pass-through and one identifying consumer demand – to estimate ω in the observed market equilibria.[16]

Our first experiment estimates pass-through by experimentally reducing traders' marginal costs. To link this to the model, we take the derivative of Equation 3 with respect to c_{mw}, which yields:

$$\rho_{mw} \equiv \frac{\partial P_{mw}}{\partial c_{mw}} = \left\{ 1 + \frac{1 + E_{mw}}{N_{mw}} (1 + \omega(N_{mw} - 1)) \right\}^{-1} \tag{4}$$

where $E_{mw} \equiv \left\{ \frac{Q_{mw}}{\frac{\partial P_{mw}}{\partial Q_{mw}}} \right\} \left\{ \frac{\partial \frac{\partial P_{mw}}{\partial Q_{mw}}}{\partial Q_{mw}} \right\}$ is the elasticity of the slope of inverse demand. Under the

[15] This equation also nests the solution to the Bertrand price-setting model, with $\omega = -\frac{1}{N_{mw}-1}$. We will not focus on this case, but point out that because Bertrand competition implies a negative ω, rejecting Cournot with a positive point estimate also implies rejecting Bertrand.

[16] We will use experimental variation across markets to estimate ω. This relies on the additional assumption that all sample markets operate under the same form of competition. In addition to being necessary for statistical power, this assumption also reflects the case in which multi-market contact leads traders to make potentially coordinated decisions at a level above the market. We are also assuming that the treatment itself – which will be a cost shock – does not change ω. The shock is unlikely to introduce any unfamiliar traders to the market. We investigate this further in Appendix I, where we find a very small, marginally significant effect that is likely to bias us toward Cournot competition. We relax this assumption when studying entry and allow ω to vary with whether traders have connections to each other.

specific models of competition tested here, this equation reduces to:

$$\rho_{mw} = \begin{cases} \left\{1 + \frac{1+E_{mw}}{N_{mw}}\right\}^{-1} & \text{when Cournot competitive} \\ \left\{2 + E_{mw}\right\}^{-1} & \text{when collusive.} \end{cases} \tag{5}$$

Therefore, the level of pass-through ρ_{mw} depends on the competitive structure of markets (ω=0 or $\omega = 1$) and the curvature of demand E_{mw} (as well as on the number of traders N_{mw} in the Cournot model).[17]

Accordingly, our first experiment identifies pass-through and our second experiment identifies demand curvature. We then insert these experimentally estimated parameters into Equation 4 from the sufficient statistics model and back out the profit weight ω in these markets. In Section 6, we will estimate ω in the more general model that relaxes the assumptions of constant marginal cost and symmetric traders. Finally, we provide a direct non-nested test of Cournot and joint profit maximization that does not depend on specifying a continuous parameter that nests Cournot competition and joint profit maximization.

3.3 The Effect of Entry on Competition

A commonly prescribed policy intervention to increase market competition is to encourage entry by new market participants. In the second part of this paper, we evaluate the impact of such policies and estimate whether the model of competition responds to changes in the number or identity of market participants. We then use these entry decisions to estimate a structural model of entry into a market. These estimates let us quantify the distribution of trader fixed costs and allow us to conduct counterfactual exercises where trader participation responds endogenously to the form of competition.

4 Experimental Design

4.1 Sample Selection and Experimental Schedule

The sample of markets in this study is drawn from six counties in Western Kenya. A listing exercise was conducted with the Director of Trade in each county to get a comprehensive list of all markets in the county. Markets without maize traders and urban markets in town centers were then excluded. See Appendix E for additional details on the sample selection procedure.

[17]Under Bertrand competition, $\rho_{mw} = 1$

The two market-level experiments (cost shock and entry) were each run for four weeks in a row. This time spans about a quarter of the full selling season in the region (March to July). This duration of treatment was selected to represent a relatively long-run cost or entry shock. It was also selected to match the frequency at which prices regularly vary to minimize concerns about sticky prices (see Figure 1, which displays the relative size of the subsidy compared to weekly fluctuations in market prices). Because piloting revealed that market and week fixed effects were important (cutting standard errors almost in half), the experiment was designed to provide each market each treatment status (cost shock treatment, entry treatment, and control) in a random order to allow for the inclusion of these fixed effects. Figure E.1 shows the six possible orders.[18] Each four-week block was broken by a one-week break during which the demand experiment was run in a subset of markets.

4.2 Experiment 1: Trader Cost Shock Experiment

In treatment market-days for the trader cost shock experiment, all traders in the market were offered a subsidy per kg sold. Enumerators arrived at the market at 7:30am (prior to the market start) and immediately made the offer to every trader present. Any traders who arrived later were also presented with the offer immediately upon arrival. Enumerators stayed in the market until 5pm (after the market conclusion). Maize sold during the enumerators' presence in the market was eligible for the subsidy.[19] When introducing the subsidy, enumerators first asked the trader to describe some of the major costs that he faced in his business (traders in control market days were also asked these questions, to avoid confounding treatment with any priming effects). The subsidy was then framed as a reduction of these costs. At no point were traders told that the purpose of the subsidy was to see how much would be passed on to the prices they set for customers; rather they were told the research was interested generally in how "reductions in cost affect your business."

In the first week, traders were informed that the offer would be available for four weeks. An identical script was read in each subsequent week to remind returning traders of the

[18] This randomization was first blocked by the day of the week of the market (done primarily for logistical ease as the trader cost-shock and entry treatment required additional management time to facilitate payments, and equal distribution of treatment across days of the week ensured an even flow of management duties) and then stratified by the number of traders typically in the market, as identified in the market census. See Appendix E for further details on this census.

[19] Only maize sold in cash was eligible for the subsidy due to concerns about the ability of enumerators to verify the authenticity of credit sales. However, over 95% of sales are conducted in cash, so this restriction was often irrelevant. The subsidy was capped at the first 75 90kg bags sold to limit budget exposure, but this cap was binding for only 1.5% of traders.

availability of the subsidy and to make the offer to any new traders who were absent in the previous week. All traders in the market therefore received an identical reduction in their marginal costs.

The 60 markets in the sample were divided into two groups: 45 markets received a "low" subsidy level of 200Ksh/90kg bag when they were in the cost shock treatment and 15 markets received a "high" subsidy level of 400Ksh/90kg bag (sales of partial bags were eligible at the same prorated amount). Note that "low" and "high" are merely relative titles: both represent large and meaningful changes to traders' costs. The "low" subsidy rate represents 7.5% of the average price, while the "high" subsidy represents 15% of the average price. Payments were made via mobile money twice a day. See Appendix J.1 for additional details about this experiment.

Enumerators monitored the sales of each trader throughout the day, recording the price and other details of each transaction as will be described below in the data section. The data collection process was identical in treatment and control markets.

4.3 Experiment 2: Demand Experiment

In the demand experiment, customers were first allowed to approach traders and negotiate a price and quantity in a natural way before being approached by an enumerator to invite them to the demand experiment.[20] If the customer consented, a random discount amount was drawn (using a randomization feature within SurveyCTO) and the customer was told that the price he had previously received from the trader would be reduced by that amount. The customer was then invited to select a new quantity he would like to purchase in light of this new price. The price discount was given to the customer in the form of a mobile money or a cash transfer, and the customer paid the trader the originally negotiated price.

Traders' consent was acquired at the beginning of each day. The trader was therefore aware that his customers would (potentially) receive price reductions. While this may have changed the baseline price charged by the trader (e.g., the trader may have raised his overall price to collect some of the anticipated discount), the trader did not know at the time of price negation with any one consumer the amount of the discount that would be offered

[20]The sample is therefore drawn from consumers who were already planning on purchasing maize that day. This was done out of necessity, in order to identify a pool of "customers" in which to randomize the discount amount. However, it does mean that the sample does not include customers who may have been induced on the extensive margin into market participation at these lower, discounted prices. The assumption therefore in the sufficient statistics model is that these customers would have exhibited the same curvature of demand as the customers observed in the sample. For the general empirical model, we directly model the extensive margin and identify substitution on the extensive margin using results from the trader cost shock experiment.

nor was the trader permitted to adjust the price following the announcement of the realized discount amount. Therefore, any variation in price driven by the discount is random.

Discounts were given per kg purchased (so as to lower the price/kg). Ten levels of discounts were offered, calibrated to span the range of price reductions one would have observed if 0-100% of the cost-reduction subsidy had been passed-through in the cost shock experiment. Per 90kg bag, they were: {0, 25, 50, 100, 150, 200, 250, 300, 350, 400} Ksh. See Appendix J.2 for additional details about this experiment.

4.4 Experiment 3: Entry Experiment

In the entry experiment, traders who had never before worked in the treated market were offered subsidies to enter and attempt to sell there. Three traders were given the offer for each market. This was designed (1) to increase the probability that at least one trader took up the offer and (2) to measure traders' willingness to enter, as the amount of each offer was randomized. Offers were given for four weeks in a row to generate somewhat long-run entry.

The pool of traders eligible to receive the entry offers was drawn from the sample of traders interviewed in pilot work (traders from markets in the same region in Kenya) and the universe of all traders found during the market census activity. Small traders who did not own or regularly rent trucks were then excluded from the pool as pilot work showed that these traders categorically did not take up the offer. A phone survey was conducted with the remaining 187 traders to determine markets in which they had ever worked. For each of the 60 sample markets, we then identified the set of eligible traders who (1) had never before worked in that market and (2) did not work in other study markets that occur on the same day of the week in order to avoid inducing exit in our sample. The median market had 37 eligible traders, the minimum had 28, and the maximum had 56. From each of these sets, we then randomly selected the three traders who would receive the entry offers.

Once the set of offers was established, each of the three selected traders for each market was randomized into a "low" offer of 5,000Ksh ($49 USD), a "medium" offer of 10,000Ksh ($99 USD), and a "high" offer of 15,000Ksh ($148 USD). The trader was eligible to receive this amount each time he visited the entry market on any of four offer days.[21] Payout was contingent on a few factors, of which traders were made aware during the offer call. They were that the trader must: (1) come to the specified market on the specified date; (2) arrive with a truck and at least 15 bags; (3) stay for at least one hour and show intention to attempt

[21] Traders were encouraged to attend all four days to receive four payouts of the above amounts. Offers for each day were independent because making payouts contingent on perfect attendance could have potentially discouraged overall take-up.

sales. Payment was made via mobile money immediately after these conditions were met.

Traders were informed of the offer via phone call one week prior to the first market-day for which they were eligible. During this call, a short survey was conducted to gather additional information about the potential entrant, including whether he had contacts in the market, his expected profits for the day should he take up and not take up the offer respectively, and his ethnicity. Following each offer week, a short follow-up phone survey was conducted, in which information was collected about the trader's activities on the day of the offer regardless of whether or not he accepted the offer. See Appendix J.3 for additional details about this experiment.

4.5 Data

Data was collected in an identical way in all markets and in all periods (cost shock treatment, entry treatment, and control). Depending on the activity level of each market, enumerators were assigned to monitor 1–4 traders.[22] Those surveys captured transaction-level price, quantity, payment method (cash or credit), and customer type (individual household consumer or retailer), all observed in real-time by the enumerator. Data on the value of any non-traditional reductions in price were also collected; these included: flat reductions in the total cost of the purchase (rather than in the per-unit price); "top-ups," quantities of maize added to the total purchase "for free"; and "after-bag services," such as free sacks, transport, or other services given to customers bundled with their transactions. The value of these additional services is incorporated into the price per kg.[23] Maize quality data was also collected for each trader (see Appendix C.2 for greater detail). In addition, traders were asked about their experience with other traders in the market that day: how often they had worked with others before, how well they knew others, whether they had "discussed a good price" at which to sell, and whether they had "agreed on a price" at which to sell.[24] Finally, the first time a trader was met in the sample, additional information was captured on the

[22]Busier markets with more quickly moving sales were allocated additional enumerators to ensure that all transactions could be recorded with accuracy.

[23]These non-traditional reductions in price were uncommon, but they do add 1–2 percentage points to our estimate of pass-through, so there is some indication that traders can use these less-traditional methods of price reductions to pass-through some of the cost reduction. It is possible that this is a more discreet method of deviating from price agreements maintained with fellow traders. It may also help traders to make price changes more continuous (as prices otherwise typically change in increments of 50 Ksh/bag or 5 Ksh/goro, a 2.2kg tin).

[24]Due to their sensitivity, these questions were asked at mid-day, after the enumerator had established good rapport with the respondent. For any traders who left the market before that time, enumerators attempted to ask these questions before the trader left, but these efforts occasionally failed due to short notice. As a result, there is higher attrition among this section of the survey.

trader's fixed characteristics, including ethnicity, location of home market, highest level of education achieved, and a battery of business management and record keeping questions drawn from McKenzie and Woodruff (2015). A Raven's test was also administered.

5 Estimating the Form of Competition in Simple Empirical Model

In this section we estimate the elements, or sufficient statistics, that enter Equation 4 in the simplified model. We start by estimating pass-through and then turn to the curvature of demand. We end by backing out the implied model of competition.

5.1 Pass-Through

To measure pass-through, we estimate:

$$P_{jmw} = \beta CC_{mw} + \boldsymbol{\gamma_w} + \boldsymbol{\zeta_m} + \epsilon_{jmw} \tag{6}$$

where P_{jmw} is trader j's quantity-weighted average price in market m and week w,[25] CC_{mw} is the level of cost change per kg offered in market m on week w (i.e., CC is the *negative* value of the marginal cost subsidy in cost shock treatment markets and zero elsewhere, such that CC_{mw}={0 Ksh, -200 Ksh, -400 Ksh }), $\boldsymbol{\gamma_w}$ and $\boldsymbol{\zeta_m}$ are week and market fixed effects, respectively, included to improve precision. The sample includes traders in market-days in which the market was in either the cost shock treatment or control period – market days assigned to the entry treatment are omitted. Under this specification, the coefficient of interest is β, which yields the pass-through rate, or $\frac{\partial P}{\partial c}$. Throughout we cluster standard errors by market-block, the unit of randomization, and weight the regressions by the inverse number of traders in the market to give equal weight to each market.

To measure heterogeneity in the pass-through rate by the level of the cost change, we estimate

$$P_{jmw} = \beta_1 CC_{mw} * Low_{mw} + \beta_2 CC_{mw} * High_{mw} + \boldsymbol{\gamma_w} + \boldsymbol{\zeta_m} + \epsilon_{jmw} \tag{7}$$

in which Low_{mw} ($High_{mw}$) is a dummy indicating whether the market was in a low (high) subsidy market. This allows for non-linearities in the effect of the subsidy per kg. For other measures of heterogeneity, we run specifications similar to Equation 7, conditioning on the desired dimension of heterogeneity.

[25]Because the ICC of price within a trader in a given market-day is high (0.9), in practice there is little variation in the prices entering into this average.

Table 1 presents the main results of the pass-through experiment. In Column 1, we see that pass-through is 22.4%, significantly different from zero at the 1% level and measured with a high degree of precision. Column 2 presents pass-through rates for low and high cost reduction treatments separately. The pass-through rates for each group are almost identical. This constant empirical pass-through rate will help inform the functional form assumptions in the following section on demand estimation.

We explore heterogeneity by the number of traders in the market.[26] Figure 3 presents these results, which show little evidence of meaningful heterogeneity. Estimates of pass-through rates are fairly tightly centered around the overall estimate of 22% and no clear pattern is seen with the number of traders. To gain statistical power, the bottom two measures show the sample pooled into below- and above-median number of traders; again, point estimates are not statistically significantly different and are in fact remarkably close in magnitude. That pass-through does not vary with number of traders is consistent with, though not definitive evidence of, collusion (see Equation 5).

We further explore in Figure 4 other dimensions of heterogeneity that could matter for how traders compete.[27] First, we measure whether pass-through is different for markets on and off paved (tarmac) roads, which serves as a proxy for market geographic isolation. We find no evidence of heterogeneity by this measure. Next, we explore whether a higher intensity of explicit collusion predicts lower pass-through rates, measured by the number of market-days within a market in which traders have explicitly admitted to collusion.[28] The point estimates suggest that pass-through is similar across these markets, and the differences are not statistically significant.

5.2 Demand Estimation

As described in Section 3, in order to draw inference in our simplified model about the level of competition from the observed pass-through, one must first understand the curvature of demand. To do so, we use the demand experiment to estimate how a household's quantity of maize demanded varies with price. We plot the estimated treatment effects by randomized

[26]This is the main source of heterogeneity pre-specified in a design registry submitted prior to the beginning of the experiment. The number of traders is defined as the average number of traders observed in the market over the course of the experiment. In order to remove any increases in the number of traders driven by the entry experiment, this figure uses the average of the predicted number of traders each week, based on market and week fixed effects.

[27]These were not included in the design registry.

[28]We construct, for each market, a count of the number of market-days in which at least one trader admitted to discussing (agreeing on) prices with other traders. We then divide the sample into markets above and below the median of this measure.

price change in Figure 5. Consumers were given the option to choose a new quantity to purchase at the new price. In the left figure, we plot the treatment effects on the fraction of consumers that changed quantities once offered the discount. This fraction is increasing in the size of the price change and reaches nearly 30% at the highest discount point. These changes translate into a strong relationship between price and quantity changes, as shown in the right figure.

Many common demand functional forms impose curvature and thus would divorce inference about how traders compete from the data. Instead, we will estimate demand curvature. Ideally we would estimate demand non-parametrically, but because household demand is so heterogeneous and demand curvature depends on a function of the second derivative of demand, inference based on non-parametric demand estimation is highly imprecise. We thus impose a flexible class of demand functions that nests several commonly used functional forms, while leveraging the panel structure of the experiment to maximize statistical power in the presence of demand heterogeneity. We use the design feature that customers were approached after agreeing on an initial price and quantity combination, so that we observe customers making two choices with an exogenous price difference. Let t index the "experimental period" – i.e., whether the customer has yet to be approached ($t = 0$) or already been offered a subsidy level ($t = 1$). This allows us to capture the considerable household demand heterogeneity while still estimating demand parameters precisely. We embed this household heterogeneity within a general Bulow-Pfleiderer class of demand functions. Household i's demand q_{imt} is:

$$q_{imt}(P_{imt}) = \begin{cases} \left(\frac{a-P_{imt}}{b_i}\right)^{\frac{1}{\delta}} \eta_{imt} & if \;\; P_{imt} \leq a \\ 0 & if \;\; P_{imt} > a \end{cases} \tag{8}$$

where $a, b_i, \delta, \eta_{imt} > 0$.

a is the choke price such that households are indifferent between purchasing or not. b_i captures persistent (within the few minutes the experiment lasts) household heterogeneity while η_{imt} is a high-frequency demand shock. Here the price faced (P_{imt}) varies by customer, even within a market, because the experiment varies the size of the discount. Given individual demand $q_{imt}(P_{mt})$, market demand is $Q_{mt}(P_{mt}) = \sum_{i \in \mathcal{I}_{mw}} q_{imt}(P_{mt})$.

We choose this particular class of demand functions for its flexibility, tractability, and empirical foundation. First, this demand structure is flexible, nesting many of the functional forms common to the development and trade literature, including linear demand and quadratic demand.

Second, this class of demand functions is tractable, producing a constant elasticity of the slope of inverse demand with respect to quantity (E) (Bulow and Pfleiderer, 1983).[29] Recall that in the simple model described in section 3, the pass-through rate is determined by the profit weight ω, potentially the number of traders, and the slope of inverse demand E. While in theory E can vary with quantities, this is a second order term for which it is already difficult to get precision on a single estimate using the full pooled data (as we will show below); attempting to further estimate E at different quantity levels would be even more challenging. Under the Bulow-Pfleiderer class of demand functions, E is constant with respect to q. To see this, note that the inverse market demand function is:

$$P_{mt} = a - \kappa_{mt} Q_{mt}^{\delta} \tag{9}$$

where constant κ_{mt} depends on the household heterogeneity (b_i) and time shocks (η_{imt}) in market m. In this case, the elasticity of the slope of inverse demand, $E_{mt} \equiv \left\{ \frac{Q_{mt}}{\frac{\partial P_{mt}}{\partial Q_{mt}}} \right\} \left\{ \frac{\partial \frac{\partial P_{mt}}{\partial Q_{mt}}}{\partial Q_{mt}} \right\}$ reduces to $\delta - 1$. Therefore, Equation 4 simplifies to:

$$\rho_{mw} \equiv \frac{\partial P_{mw}}{\partial c_{mw}} = \left\{ 1 + \frac{\delta}{N_{mw}} (1 + \omega(N_{mw} - 1)) \right\}^{-1} \tag{10}$$

with our specific models of competition reducing to:

$$\rho_{mw} = \begin{cases} \frac{N_{mw}}{N_{mw}+\delta} & \text{when Cournot competitive} \\ \frac{1}{1+\delta} & \text{when collusive.} \end{cases} \tag{11}$$

Third, this class of demand functions has a strong empirical foundation. The experimental design includes variation intentionally designed to test this empirical fit. As shown by Equation 10, because E is constant across q, this class of demand functions predicts a constant pass-through rate for a given ω, independent of the size of the cost shock (were E not constant in q, cost shocks of different sizes – by driving different levels of optimal quantity sold – would induce differential changes in E, which would in turn produce different pass-

[29]This property holds both for this specification of individual and market demand. Atkin and Donaldson (2015) also rely on this property in estimating markups.

through rates). By offering two different levels of the cost shock, we are able to test for this prediction of constant pass-through. Because markets are randomized into receiving the low vs. high subsidy rate, the only difference in these two sets of markets, on average, should be the level of the cost shock. Under the Bulow-Pfleiderer class of demand functions, we should therefore expect to see identical pass-through rates for these two markets. This is exactly what we see in Column 2 of Table 1, which suggests remarkably similar pass-through rates for the two levels of cost reduction. This lends empirical support to this choice of demand class.

Our focus on demand functions with E constant in q motivates our specification of consumer heterogeneity. We place heterogeneity in the b_i term while keeping a the same for all consumers.[30] This preserves constant E – matching the high vs. low cost shock pass-through results and providing us with the benefits of tractability when estimating a higher-order term – but comes at the expense of modeling an endogenous extensive margin.[31] Given the potential importance of extensive margin responses to price, in Section 6 we add heterogeneity in a and discuss the functional form we impose.[32]

5.2.1 Estimation, Identification, and Results

To estimate the demand model, we implement a log transformation of Equation 8 and take the first difference within customer:

$$log(q_{im1}) - log(q_{im0}) = \frac{1}{\delta}\left(log(a - P_{im1}) - log(a - P_{im0})\right) + \left(log(\eta_{im1}) - log(\eta_{im0})\right) \tag{12}$$

where $P_{im1} - P_{im0}$ is the subsidy amount.

Any endogeneity in prices due to persistent demand conditions is accounted for by the panel nature of this specification. We further use the randomized reduction in the price paid by consumers from the demand experiment as an instrument for the price term ($log(a -$

[30] We also impose constant δ across consumers. Because δ relates to a higher-order term in demand, we lack the variation to estimate heterogeneous δ with any precision.

[31] Variation in a, the choke price, would drive variation in which consumers enter the market at different prices. Constant a implies either full or no endogenous participation. The number of consumers may still vary for exogenous reasons, but given our functional form, would not affect inference on the model of competition.

[32] Another approach to integrating an extensive margin would have been to model market choice and quantity demand jointly in a discrete-continuous demand system, where the discrete and continuous choices are linked via Roy's Identity. While point estimates from this model lead to similar conclusions about the model of competition, the standard errors are large. Furthermore, given our data, summarizing the discrete choice of where to buy requires considerable dimension reduction, such as through a logit functional form assumption. This assumption puts strong restrictions on demand curvature that we seek to avoid.

$P_{im1}) - log(a - P_{im0}))$ to address any remaining endogeneity (e.g., in high-frequency demand shocks, η_{imt}) as we identify the model's parameters.

We run the analysis with 936 observations. We estimate the vector of parameters $\mathbf{\Theta} = (a, \delta)'$ in Equation 12 using generalized methods of moments. We construct our IV sample moments as the vector of 9 dummy variables for each positive discount level times the residual.[33] We minimize the GMM objective function using the optimal weighting matrix from two-step estimation.

Results are presented in the top panel of Table 2, which show the point estimates and 95% confidence intervals. Note that the confidence interval on δ is wide (for example, we cannot rule out very curved inverse demand of $\delta = 6.43$). This is because δ, which represents the elasticity of the slope of inverse demand (plus one), is a higher order object which we are underpowered to measure with great precision, even with over 900 observations from the demand experiment. That said, we have enough precision to rule out some standard demand functional forms, including linear demand ($\delta = 1$), Moreover, this degree of precision is sufficient for our purposes. As we will see in the next subsection, from the point estimate on δ, we can predict the level of pass-through that one should expect under various models of competition; we will find the prediction of one model to line up closely with what is observed empirically. Further, even at the bounds of our estimate of δ, we can still reject that what we see empirically is consistent with other common models of competition.

5.3 Model of Competition

First, we demonstrate that the observed pass-through is very close to the collusive model prediction evaluated at a demand curvature given by the parameter point estimates. Given the point estimate on δ of 4.1, we use Equation 10 to estimate the average pass-through rate one should expect to observe in the experiment under various models of competition. If markets are Cournot competitive ($\omega = 0$), we should observe pass-through rates that vary with the number of traders: $\rho = \frac{N}{N+4.1}$.[34] Given the distribution of number of traders in each market in our sample, the expected pass-through rate if markets are Cournot competitive is 46%. On the other hand, if markets are collusive ($\omega = 1$), we should expect to observe 20%

[33] We drop the consumers offered no discount as they have no within-consumer price variation and do not make a new quantity choice.

[34] This would predict that pass-through would be increasing in the number of traders. Note that we already saw in Section 5.1 that pass-through did not vary with the number of traders in way that is consistent with this predicted pattern.

pass-through.[35]

Figure 6 displays the bootstrapped distribution of ρ.[36] We see that the mass of the distribution of ρ is concentrated near the predicted pass-through of 20% under collusion. The dotted lines, which identify the 95% confidence interval, clearly reject a ρ consistent with that predicted under a model of Cournot competition (and Bertrand competition).

This exercise does not take into account the fact that δ is estimated imprecisely. To account for this imprecision, we generate a bootstrapped distribution of ω by estimating Equation 10 with 1,000 bootstrapped estimates of ρ and δ.[37] The top graph in Figure 7 presents this distribution, overlaid with the benchmark values of ω under Cournot competition and collusion. We plot in red the value of ω predicted by the point estimates on ρ and δ. The point estimate of ω is 0.78, with a 95% confidence interval of $(0.05, 7.48)$, which is quite close to – and statistically indistinguishable from – the model benchmark of $\omega = 1$ under perfect collusion. Moreover, while the collusive market benchmark of $\omega = 1$ lies well within the 95% confidence interval, the levels of ω predicted by a Cournot model and perfectly competitive model lie outside these bounds. We are therefore able to reject them with 95% confidence.[38]

Under this simple model, the observed pass-through rate is therefore consistent with an underlying market structure in which traders act as if they maximize joint profits.

6 Estimating Form of Competition in the General Empirical Model

In Section 3 we started with a general model before imposing two supply-side assumptions: trader symmetry and constant marginal costs. We also made a simplifying assumption in the demand model, placing heterogeneity across consumers in b_i, rather than in a, which restricts adjustment on the extensive margin. The value of these simplifying assumptions is that they allow us to reduce each experiment to identifying a single parameter; this one-to-one matching of experiment to theory makes transparent the link between experimental results and the identification of the model of competition. Using this approach, Section 5

[35]While not nested in our model, if traders are Bertrand competitors, we should observe 100% pass-through, at least in this simple model.

[36]The distribution was constructed using 1,000 block bootstrapped samples where blocks are defined by market x 4-week-blocks (the level at which treatment was randomized). There are 180 such clusters from 60 markets.

[37]We take the expectation of Equation 10 over our markets with heterogeneous numbers of traders and estimate ω to make Equation 10 hold in expectation.

[38]We are able to reject a Cournot competitive model because the confidence interval around δ, however large, does exclude the extreme curvature necessary to justify such low pass-through under a Cournot model. To achieve a predicted ρ of 22% under a Cournot model, we would have required a δ of about 12.

estimated the profit weight and found evidence consistent with joint profit maximization.

To test whether our conclusions are robust to these simplifying assumptions, we now relax the supply-side assumptions and extend our demand model to accommodate further household heterogeneity and an extensive margin. We view this more general model as complementary as it relaxes assumptions but with a less direct link between experimental results and inference on the model of competition, albeit still using experimental variation for identification of all parameters.

6.1 General Demand Model

We start with the general demand model as estimated demand will be an input into our supply model. We continue with the model in Section 5.2 but add further consumer heterogeneity by letting a, the choke price, be consumer-specific:[39]

$$q_{imt}(P_{imt}) = \begin{cases} \left(\frac{a_i - P_{imt}}{b_i}\right)^{\frac{1}{\delta}} \eta_{imt} & if \;\; P_{imt} \leq a_i \\ 0 & if \;\; P_{imt} > a_i \end{cases} \tag{13}$$

where $a_i, b_i, \delta, \eta_{imt} > 0.$

This heterogeneity makes the model more flexible and introduces a heterogeneous household extensive margin. If the price is above a household's a_i, then the household will not purchase any maize at the market that week. This heterogeneity, though, proves challenging to identify even with the panel structure of the demand experiment. We thus impose a functional form on the heterogeneity, assuming $a_i \sim N(\mu_a, \sigma_a^2)$. This heterogeneity permits more flexible market demand changes for a price change.[40]

Further, because the demand experiment reaches consumers only after they chose to visit the market and agree to a price-quantity bundle, we supplement our demand experiment results with additional data moments from the cost shock experiment to bring in more information about consumers' choices related to the extensive margin.[41] Before specifying the moments, we analyze the effects of the cost shock on market-level transacted quantities.

[39] To the extent that some consumers may make multiple transactions, we allow a to vary by transaction.

[40] While individual demand remains in the Bulow-Pfleiderer class of demand functions, market demand is no longer in the class and we will no longer rely on the sufficient statistics formula.

[41] The variation in the demand experiment alone is technically sufficient to identify μ_a and σ_a, the parameters of heterogeneity in a, as a_i affects not just whether to buy anything but also how much; however, because the additional moments better target extensive margin choices, we present them as our main specification. Note, though, that we get very similar results when relying only on the demand experiment moments.

We estimate reduced form regressions, using market-week level versions of Equations 6 and 7 but replacing the dependent variable of price with four quantity measures: number of transactions, transaction rate,[42] kgs per transaction, and total kgs. We summarize the results in Table 3.

We find a large quantity response to the cost shock. For a decrease in marginal cost of 1 Ksh/kg, we estimate an increase of 846 kgs in a market/day. The effect is driven almost entirely by the extensive margin, as we see an increase of 11 transactions. The effect on the intensive margin, which combines increased demand from a fixed set of consumers and compositional changes in the set of consumers transacting, is much smaller and not statistically significant. To convert these estimates to elasticities, we estimate:

$$Q_{mw} = \beta P_{mw} + \boldsymbol{\gamma_w} + \boldsymbol{\zeta_m} + \epsilon_{mw} \tag{14}$$

and instrument for market price with dummy variables for being in the low cost shock treatment or the high cost shock treatment. These two-stage least squares estimates, in columns (3), (5), (7), and (9) of Table 3, show steep demand curves. The implied elasticities when evaluated at the mean price and mean quantity under the low cost shock – the median of our three experimental groups – are listed at the bottom of the table. For total quantity ("Kgs") we estimate an elasticity of -20.4, while we estimate an extensive margin ("Num Trans") elasticity of -19.0.

It is worth noting that these estimates do not necessarily imply large consumption elasticities. The object estimated here is a market residual demand elasticity – the percentage increase in quantity that a market's traders can expect to sell for each percentage decrease in price – and is the relevant elasticity for characterizing traders' strategic incentives. This may be distinct from the increase in quantity that a consumer would consume if she faced lower prices universally. We explore the potential explanations for the large market residual demand elasticity in Appendix H.

Moreover, these simple specifications assume linear demand. However, Column (2), which shows the reduced form effect of each treatment on the number of transactions, reveals that doubling the cost reduction from low to high produces only a slightly greater number of transactions. In fact, if we calculate the elasticities piecewise, we find that the low cost shock versus control comparison yields an estimated elasticity of -28.1, while the low cost

[42] We observe the number of transactions for each market-day. We convert these to a transaction rate by dividing by the maximum daily number of transactions observed in sample for each market. Estimates of the profit weight in Section 6.2 are robust to increasing the market size by at least a factor of 2.

shock versus high cost shock comparison yields an elasticity of just -3.3. This suggests that demand may be highly non-linear (or in our demand model, a large fraction of potential consumers have a_i near market prices). Unfortunately, the cost experiment alone has insufficient variation to estimate a non-linear model; this motivates our combination of the cost shock experiment variation with the demand experiment's variation, with 9 exogenous price points, in estimating demand with a flexible functional form.

We thus add six moments from the cost experiment to our demand estimation: the mean market-week transaction rate and kgs per transaction in each experimental arm: control, low cost shock, high cost shock. We include the experiment's effect on the mean market-week transaction rate to capture extensive margin changes. And given that the consumers induced by the lower prices to purchase may be different from the consumers who show up even at high prices, we also include the moments related to kgs per transaction. We keep separate moments for the low and high cost shocks to capture the non-linearities evident in Table 3. The sample moment values are in Table F.1.

Using these moments and the high-powered demand experiment IV moments, we estimate demand using three-step GMM with an optimal weighting matrix and bootstrapped standard errors. We report the estimates in the bottom panel of Table 2. We estimate δ to be 4.21, close to the estimate from our simpler demand model. For the distribution of a_i, we estimate a mean of 29.15 and a standard deviation of 2.87. This tight distribution is consistent with the large observed response in transaction rates to the cost shocks. The estimated demand model generates a mean demand elasticity, at market prices and quantities, of -3.2. These elasticity estimates are lower in magnitude than the implied elasticities from the linear model in Table 3, though they are consistent with the estimate from the low cost shock vs. high cost shock comparison. The estimated demand model also yields declining kgs per transaction – driven by lower demand from marginal consumers – which matches the (noisy) results on kgs per transaction from the cost experiment variation.

6.2 General Supply Model

Turning to our supply model, we now relax the previous assumptions on trader symmetry and constant marginal costs. To do so, we estimate our general supply model from Equation 2 with a functional form for total cost that allows marginal costs to vary on several dimensions, including with respect to quantities. We specify trader j's total costs in week w as:

$$C_{jw} = \frac{1}{2}\gamma q_{jw}^2 + \sum_{m \in \mathcal{M}_{jw}} (FC_{jmw} + (c_m + c_w + c_j + \Delta c_{mw} + c_{jmw})q_{jmw})$$

where Δc_{mw} is the experimental cost shock, q_{jw} is j's total weekly quantity, and FC_{jmw} is j's fixed cost from trading in market m in week w. By modeling costs as depending quadratically on the total quantity sold in a week, we allow for increasing (or decreasing) marginal costs in the quantity sold in a given market-day, as well as the quantity sold throughout the week. Adding potential cost interdependencies across the week is important because many traders source maize on a weekly basis; we therefore allow for the possibility that marginal costs are increasing with quantities, due to, for example, the opportunity cost of not selling at another market that week or the cost of having to source from new farmers to fulfill demand at other markets that week. Our cost function also allows marginal costs to vary across markets (c_m) and weeks (c_w). This flexibility is important as certain markets may be more accessible or certain weeks may feature higher sourcing costs. Finally, we introduce heterogeneity across traders, allowing traders to face systematic differences in marginal costs (c_j). The remaining cost term, c_{jmw}, represents trader j's marginal cost shock in week w in market m (note this is distinct from the experimental cost shock, Δc_{mw}).

Plugging this cost function into Equation 2 and taking the first order condition with respect to quantities, we estimate the following supply model:

$$(15) \qquad P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} = -\omega \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

We now have two primary parameters remaining to be estimated: ω, the profit weight equal to 0 under Cournot competition and 1 under joint profit maximization, and γ, the rate at which marginal costs increase (or decrease) with quantities.[43] We group components without unknown parameters on the left-hand-side, noting that the right-hand-side is now linear in the parameters ω and γ, which facilitates their estimation with linear model techniques. The cost shock c_{jmw} is our structural error.

Because other traders' quantities ($\sum_{k \neq j} q_{kmw}$) and own weekly quantity (q_{jw}) are determined in equilibrium, they are likely correlated with c_{jmw}. Therefore, we will rely on instruments to identify ω and γ. To maintain the close connection with the field experiments, we will construct our instruments using experimental variation only. Because treatment status was determined through experimental randomization, market-block treatments are orthogonal to trader type (specifically, c_{jmw}).

The first set of instruments are whether the market-week was randomized to have a low

[43]We first estimate $\frac{\partial P_{mw}}{\partial q_{jmw}}$ using our demand estimates. Appendix F offers details on model estimation, including how we estimate $\frac{\partial P_{mw}}{\partial q_{jmw}}$ for each market-week.

cost shock, a high cost shock, or no cost shock. While the cost shock (Δc_{mw}) is directly in the first-order condition we estimate, it has a known coefficient. We can thus rely on the (market-level) cost shock as an exogenous shifter of other traders' quantities as well as where the traders are on the demand curve ($\frac{\partial P_{mw}}{\partial q_{jmw}}$).

We choose the second set of instruments to target identification of non-constant marginal costs. Simultaneously identifying the model of competition and non-constant marginal cost is difficult and often depends on an instrument that rotates demand (Bresnahan, 1982). We take a different strategy by looking for exogenous variation in a trader's total quantity sold in other markets that affects the focal market only through the cost function. In our setting, we exploit the fact that some traders operate in multiple in-sample markets and thus their exposure to cost shocks in *other* markets exogenously changes over time.[44] The experimental status of a multi-market trader j's *other* markets shifts trader j's total weekly quantity sold, which affects trader j's quantity supplied in the focal market through an increase (or decrease) in marginal cost, as determined by the size of γ. We thus construct the following instruments: the fraction of trader j's in-sample markets in week w that have a low cost shock or a high cost shock. To increase power and generate additional variation in quantities sold that week, we add more instruments to capture variation coming from the entry offer experiment. In that experiment, which we will discuss further in Section 7, potential entrants received low, medium, or high entry subsidies. The size of the subsidy predicts the likelihood of taking up the offer and entering a new market, which affects total quantity sold in a week. We thus use whether trader j randomly has a low, medium, or high entry offer (to another market) as additional instruments to identify non-constant marginal costs. We also add an instrument for the fraction of j's markets for which another trader has an entry offer.

Given these instruments, we run two-stage least squares to generate point estimates for ω and γ and construct confidence intervals by bootstrapping the entire demand and supply estimation. We present our estimates in the middle panel of Table 2 and the bootstrap distribution of ω in the bottom graph of Figure 7. We estimate ω to be 1.07, very close to the benchmark value of ω under joint profit maximization, with a 95% confidence interval of $(0.20, 3.09)$. Thus, we arrive at similar conclusions about the model of competition based on our more general model.

We can also directly test one of the assumptions of the sufficient statistics model –

[44] Though being a multi-market trader is unlikely to be random, by including trader fixed effects in our marginal cost specification, we isolate variation coming from the rotating experimental schedule over time.

constant marginal cost. Our estimate of γ, the marginal cost slope, is 0.0006 Ksh/kg, and 0 is well within a fairly tight 95% confidence interval of $(-0.0006, 0.0016)$. This point estimate is small, implying that a 1 standard deviation increase in weekly (in-sample) quantity sold (2300 kgs) corresponds to a cost increase of just 1.73 Ksh/kg. This pales in comparison to the heterogeneity in trader-market-week marginal cost intercepts – marginal cost for the first kg – where we estimate a standard deviation of 10.84 Ksh/kg.

In addition to the above approach, which tests whether ω coincides with well-defined models of competition, we can also implement a non-nested test of Cournot competition and joint profit maximization that does not treat ω as a structural parameter (Bresnahan, 1987; Villas-Boas, 2007). This test uses the exact same identifying variation as in the prior specification, but flips the approach. Rather than using the fact that the experimental treatment status is orthogonal to trader cost type (due to randomization) and then identifying the model of competition, it instead assumes a particular model of competition and then tests whether such independence holds, as it should under the correct model (Berry and Haile, 2014; Backus et al., 2019a). Specifically, we modify Equation 15, imposing either $\omega = 0$ (Cournot) or $\omega = 1$ (joint profit maximization) and inserting the cost shock Δc_{mw} on the right-hand-side.[45] Under the correct model of competition, the randomized cost shock and trader cost type should be uncorrelated and thus the coefficient on the cost shock (π) should be zero.

$$(16)\quad P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} + \omega \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} = \pi \Delta c_{mw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

Forming test statistics from the 1,000 bootstrap iterations, we reject $\pi = 0$ under Cournot competition, but fail to reject $\pi = 0$ under joint profit maximization. Results from this alternative approach are therefore consistent with our conclusions from the nested test. We provide more details in Appendix B.

6.3 Trader Markups

We now turn to the implications of joint profit maximization for trader markups and variable profits.[46] Given within-market variation across traders in quantities transacted,

[45] An added benefit is that this specification allows for joint estimation of γ and testing of the model of competition.

[46] Note these estimates depend on the shape of demand and the cost function. Unless we interpret ω structurally, cost estimates only have meaning in the context of specific model. Given the evidence presented

our homogeneous goods model implies considerable cost – and thus markup – variation. We estimate a median markup of 39% and a mean of 48%, where we express markups as $\frac{P_{mw}-MC_{jmw}}{P_{mw}}$ with MC_{jmw} being the marginal cost at the equilibrium quantity. There is wide dispersion in markups, with an estimated standard deviation of 35%.

Do these markups seem sensible? One check is to compare survey data on prices received by producers to those paid by final consumers. While we lack data on farmer prices in this study, a concurrent study conducted by Bergquist and McIntosh (2019) collected prices from local markets in neighboring eastern Uganda during the same four month period as this study. As much of the maize coming into western Kenya during the lean season comes from eastern Uganda,[47] prices observed in rural Uganda markets during this period approximate the relevant producer price.[48] Comparing this price with the mean consumer price observed in our data, we can generate a back-of-the-envelope markup estimate, which we find to be 45%, quite close to the median markup of 39% estimated from our model.

These large markups lead to large estimates of variable profits per market-day.[49] Variable profits are highly skewed with some traders earning very high variable profits. The median trader-market-week generates 3,400 Ksh while the mean generates 14,000 Ksh. These estimates indicate that traders charge prices well above their marginal costs. But whether traders actually make high total profits depends on fixed costs. We will use entry choices to learn about the magnitude of fixed costs in Section 7 and thus defer our complete discussion of welfare until Section 8.

7 Entry

Given that markets look fairly collusive, one natural policy response in the absence of antitrust enforcement is to encourage greater entry, especially among traders who might be willing to compete. There are several policies that could potentially encourage entry, such

above, we therefore assume joint profit maximization ($\omega = 1$) and re-estimate the supply model to back out the parameters of the cost function. We show the new estimate of γ in the bottom panel of Table 2. Unsurprisingly given our estimated ω is so close to 1, the estimate of γ is nearly unchanged at 0.0007 Ksh/kg.

[47]Uganda, known as the breadbasket of East Africa, experiences a harvest that occurs about four months earlier than that in western Kenya. Therefore, much of the maize available in our Kenyan study market during the four-month lean season in which this study is run is supplied by regions in eastern Uganda.

[48]While the markets studied in Bergquist and McIntosh (2019) are quite rural, these are not quite farmgate prices and therefore these prices are likely to be, if anything, slightly higher than the price eastern Ugandan farmers receive at farmgate. The estimates produced by this back-of-the-envelope exercise are therefore likely an underestimate of markups.

[49]For multi-market traders, we calculate weekly profits and divide by the number of in-sample markets to get a per-market estimate.

as offering lines of credit to potential new traders to rent long-haul trucks and disseminating information about profitable markets more broadly. But whether an entrant will introduce competition or collude is unclear. This is what we test in the third experiment, in which we randomly offer traders incentives to enter new markets.

Understanding entry is also relevant for evaluating the welfare implications of collusion. If low entry rates allow collusion to persist, then fixed costs of entry might be sufficiently large to dissipate high levels of variable profits. But because fixed costs do not affect the maximization of variable profits, the cost shock experiment tells us little about the distribution of fixed costs. Instead, we identify fixed costs by turning to the entry experiment that shifts how traders trade off fixed costs and variable profits.

7.1 The Cost of Entry

Because the offer amount is randomized, we can use traders' willingness to accept the offer as a measure of willingness to enter new markets. Table 4 presents take-up at each subsidy level (take-up defined as ever accepting any of the four market-day offers). Sensibly, we see that take-up increases in the size of the subsidy: take-up is 12% for the low offer (5,000 Ksh, or USD \$49), 28% for the medium offer (10,000 Ksh, or USD \$99), and 42% for the high offer (15,000 Ksh, or USD \$148). The fact that take-up is far from universal, even with the high offer that exceeds mean variable profits, suggests that the cost of entry appears to be high in this setting.[50]

We first explore heterogeneity in willingness-to-enter by a few key variables pre-specified in the design registry. While these results are merely correlational, and therefore cannot be interpreted through a strictly causal lens, they do point to some potential barriers to entry. To explore this heterogeneity, we estimate the following specification for the pool of 180 potential entrants:

$$\begin{aligned} T_{jm} &= \alpha + \beta X_{jm} + \epsilon_{jm} \quad (i) \\ T_{jm} &= \alpha + \beta X_{jm} + \boldsymbol{\zeta_m} + \epsilon_{jm} \quad (ii) \end{aligned} \tag{17}$$

in which T_{jm} is a indicator representing whether trader j ever took up an offer to enter his assigned market m. X_{jm} is the variable by which we explore heterogeneity. In specification

[50] Low take-up could also be due to trader mistrust of the offer. However, Innovations for Poverty Action (IPA), the implementing partner, had been conducting surveys with traders in the region for almost three years at the time of the experiment and therefore was well-known by many of these traders. As a result, when asked, fewer than 5% of traders who did not take up the offer cite trust issues as the explanation.

(ii), we control for market fixed effects ($\boldsymbol{\zeta_m}$), such that we only look at differential take-up of the entry offer *within* the same market. We do this to remove some of the endogeneity that might influence the composition of the pool of potential entrants. Because there were a few traders who were given multiple offers (though never for the same four-week block), we cluster standard errors by trader in both regressions.

Figure 8 displays the results. As presented earlier, a larger subsidy increases take-up. Longer distances to travel are also sensibly correlated with lower take-up; when comparing the effect of distance on take-up to that of the offer amount, we estimate that an additional 50km in distance is roughly equivalent to a drop of $46 USD in the offer amount.[51] Having contacts in the entry market is correlated with higher take-up (albeit not quite significantly). The point estimate suggests that the value of having contacts is equivalent to an increase in the offer amount of $36. Being a large firm (above median profits) is also correlated with higher take-up. The effect is substantial: having above median profits is equivalent to offering an additional $52. These results on contacts and firm size are consistent with the existence of barriers to entry in the form of requiring business networks and access to working capital to enter new markets. Interestingly, ethnic similarity between potential entrants and incumbents does not appear to have any correlation with the entrant's willingness to enter.[52]

Because the offer was made to three different traders per market, this offer generates a strong instrument for entry (despite the low take-up per trader). 53% of all markets had at least one day (out of four) with entry. 38% of all market-days had entry, 26% of which had more than one entrant. In total, an average entry market had an additional 0.6 traders present, an increase of 16% over the mean market size and 21% over the median.

7.2 The Effect of Entry on Price

We turn now to the effect of entry on prices. To measure the reduced form effect of the offer, we estimate:

$$P_{jmw} = \alpha + \beta EntryMarket_{mw} + \boldsymbol{\gamma_w} + \boldsymbol{\zeta_m} + \epsilon_{jmw} \tag{18}$$

[51] The precision of the distance effect drops when including market fixed effects; this is likely because comparing variation in distance to the same market removes much of the total variation in distance.

[52] This is perhaps surprising, given recent work from the region documenting the important role ethnic divisions can play in discouraging productivity among workers (Hjort, 2014) and integration across markets (Robinson, 2016). However, it is consistent with economic lab games from Kenya that fail to find evidence of co-ethnic bias, instead suggesting that observed ethnic divisions may be caused by mechanisms other than simple ethnic preferences (Berge et al., 2015).

where P_{jmw} is the average price per kg charged by trader j in market m in week w, $EntryMarket_{mw}$ is a dummy for whether market m is in an entry market in week w, and $\boldsymbol{\gamma_w}$ and $\boldsymbol{\zeta_m}$ are week and market fixed effects, respectively. Standard errors are clustered at level of market x four-week block, the level of randomization. Observations are weighted by the inverse of the number of traders in each market to give each market equal weight. The sample includes traders in market-days corresponding to either the entry treatment or control period (that is, cost shock treatment periods are omitted). Under this specification, the coefficient of interest is β, which yields the price reduction observed in the entry offer market.

We also run an IV specification to determine the effect of entry on prices:

$$P_{jmw} = \alpha + \beta NumEntrants_{mw} + \boldsymbol{\gamma_w} + \boldsymbol{\zeta_m} + \epsilon_{jmw} \tag{19}$$

in which $NumEntrants_{mw}$ represents the number of entrants in the market that day, for which we instrument with the $EntryMarket_{mw}$ dummy. Table 5 presents these results. We see a strong first-stage effect on the number of entrants (Column 1), while the number of incumbents does not change (Column 2). Reduced form effects are small and marginally significant, with only a 0.18 Ksh (or 0.6% of the mean) drop in prices (Column 3). Column 4 presents the result of using treatment status as an instrument for the number of entrants. We see that the entry of one trader reduces prices by 0.28 Ksh (or 1.0% of the mean), with a p-value of 0.077.

7.3 The Effect of Entry on Competition

What does the small price decrease tell us about how the underlying competitive environment (summarized by ω) has changed? We have not directly modeled how ω is determined, but the introduction of new trader to a market could affect the stability of a collusive arrangement. We explore this using the entry offer experiment by estimating whether ω changes with entry.

Let ω_n be the profit weight if there is no entry and ω_e be the profit weight with entry of new traders. We return to our general supply model and let ω depend on entry:

(20)

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} = -\omega_n (1 - Entry_{mw}) \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} - \omega_e Entry_{mw} \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw}$$
$$+ \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

where $Entry_{mw}$ is an indicator for whether market m has realized entry in week w.[53] We have already in prior sections estimated ω_n to be consistent with joint profit maximization, so we now impose $\omega_n = 1$ and leave ω_e to be estimated.

Realized entry likely depends on market and potential entrant characteristics. We thus instrument for entry with an indicator for whether market m in week w is part of the entry offer experiment. Because of the experimental randomization, this instrument is orthogonal to market characteristics. We restrict attention to incumbents (note that even if the market is randomized into treatment, the cost shocks, c_{jmw}, of the entrants themselves may still be correlated with treatment as entrants may have different costs from non-entrants or incumbents).[54] Our identification assumption then is that incumbents' cost shocks – which do not depend on market outcomes – are orthogonal to whether the market is an entry offer market this week.

We present our estimate of ω_e, with a bootstrapped confidence interval, in the top panel of Table 6. We estimate ω_e to be 0.66 with a 95% confidence interval of $(0.10, 0.96)$. The point estimate falls in the middle of Cournot and joint profit maximization, and we can reject Cournot ($\omega = 0$) and joint profit maximization ($\omega = 1$). This indicates that that entry may change how traders compete.

We cannot formally distinguish whether the estimate of $\omega_e = 0.66$ reflects a yet-to-be-considered form of competition in all markets (e.g., collusion at a quantity above the perfect collusive quantity) or rather an average treatment effect arrived at from a subset of markets switching fully from joint profit maximization to Cournot competition.[55] However, we can generate suggestive evidence on this point by examining whether ω_e varies according to the pre-specified sources of heterogeneity: number of pre-existing contacts, trader size, and trader ethnicity.

[53]We estimate profit weights that are constant within market-week. An alternative would be to let incumbent traders place different weights on other incumbents' profits and entrants' profits. We cannot separately identify these two models though our estimates lead to similar qualitative conclusions.

[54]We still keep entrants' quantities as part of the sum of other traders' quantities sold.

[55]And unless we commit to a structural interpretation for ω, a value of 0.66 has no specific meaning.

While we find no significant heterogeneity based trader ethnicity and noisy heterogeneity based on trader size (see Appendix G), we do find suggestive evidence of heterogeneity by whether the entrant has contacts in the market. 32% of potential entrants have contacts in the entry market, and these potential entrants may find it easier to integrate into pre-existing collusive arrangements. Along these lines, recall from Figure 8 that the offer take-up rate is higher for potential entrants with contacts than those without. We thus estimate whether entrants without contacts have differential effects, where we instrument for entrant type (whether he has contacts) with the type of the potential entrant that (randomly) received the high subsidy offer. We present the estimates in the second panel of Table 6. We estimate that markets with entrants that have contacts have $\omega_e = 0.95$, quite close to the collusive benchmark of $\omega = 1$, with a 95% confidence interval of $(0.15, 1.53)$. Thus, entrants with contacts appear to integrate into the lack of competition already present in the markets. The story is different for entrants without contacts. Markets with these entrants have estimated $\omega_e = 0.44$, with a 95% confidence interval of $(-0.48, 0.96)$. For these markets, we can reject joint profit maximization but cannot reject Cournot competition.

These results suggest that entry's effectiveness in inducing more competition depends on the entrant type.[56] Policy-makers interested in increasing competition may therefore prefer policy instruments that target entrants without contacts. However, this may be challenging to achieve, as the lower offer take-up rates for entrants without contacts suggest these individuals are less willing to enter. This may be because, given that they induce more competition, these entrants likely earn lower profits than those able to facilitate join profit maximization.

But to draw any definitive conclusions on profit levels – of either incumbents or entrants – we must first estimate traders' fixed costs. While fixed costs are not directly observable in the data, they are key determinants of traders' entry decisions, which we do observe. We therefore estimate a model of traders' entry decisions to back out a fixed cost distribution and conduct welfare analysis. As with our earlier demand and supply models, we will focus on using exogenous variation to identify the model parameters, this time using the entry experiment.

[56] Whether a trader has contacts is not necessarily a fixed characteristic so it is possible that entrants that plan to visit a market well beyond a four-week period may find it worthwhile to form relationships that facilitate collusion.

7.4 Entry Model

We model potential entrant j as choosing to enter market m in week w if the variable profits from entering exceed the fixed cost (net of any experimental subsidy):

$$Entry_{jmw} = \begin{cases} 0 & if \ \ \pi^V_{jmw}(MC^0_{jmw}, \omega) < FC_{jmw} - EntrySubsidy_{jmw} \\ 1 & if \ \ \pi^V_{jmw}(MC^0_{jmw}, \omega) \geq FC_{jmw} - EntrySubsidy_{jmw} \end{cases} \tag{21}$$

where π^V_{jmw} and FC_{jmw} are, respectively, trader j's variable profits and fixed costs from entering market m in week w, and $EntrySubsidy_{jmw}$ is the randomized subsidy from the entry experiment. We include MC^0_{jmw}, the marginal cost intercept $(c_j + c_m + c_w + c_{jmw})$, and ω as arguments to variable profits to highlight that entry decisions vary across traders based on heterogeneous fixed costs, heterogeneous marginal cost functions, randomized subsidy levels, and whether the trader changes the market's model of competition. For potential entrants with contacts in market m, we assume $\omega = 1$, and for potential entrants without connections, we assume $\omega = 0$.

The model clarifies the link between the entry experiment subsidy variation and observed entry outcomes. But it also demonstrates why the average treatment effect of an entry offer on entry decisions, or market outcomes conditional on entry, fails to identify the distribution of fixed costs. First, entry decisions depend on multiple factors. We address this by jointly modeling fixed and marginal cost heterogeneity. Second, the average treatment effect does not trace out the full cost distribution, which is important for assessing surplus for infra-marginal traders. This necessitates specifying a functional form for the marginal and fixed cost distributions. We assume that they are jointly lognormal:

$$\begin{pmatrix} MC^0_{jmw} \\ FC_{jmw} \end{pmatrix} \sim logN \left(\begin{pmatrix} \mu_{MC} \\ \mu_{FC} \end{pmatrix}, \begin{pmatrix} \sigma^2_{MC} & \rho_{MCFC}\sigma_{MC}\sigma_{FC} \\ \rho_{MCFC}\sigma_{MC}\sigma_{FC} & \sigma^2_{FC} \end{pmatrix} \right).$$

This distributional assumption leaves us with 5 parameters to estimate: μ_{MC}, μ_{FC}, σ_{MC}, σ_{FC}, ρ_{MCFC}. We estimate the model via simulated method of moments with an optimal weighting matrix. As moments, we use entry probabilities by size of experimental subsidy and mean marginal cost intercepts times entry, by size of experimental subsidy. To estimate this second set of moments, we use our supply model (Equation 20) with estimated γ (Table 2) and an ω of 0 or 1, depending on whether the entrant has contacts, to back out marginal cost intercept estimates. For a candidate set of parameters, we draw potential entrants' marginal and fixed costs and then solve for the new market equilibrium under entry. We then compare variable profits, under entry, to fixed costs less the randomized entry subsidy to determine whether the potential entrant would choose to enter. We form the model moments from these simulated entry decisions. We use importance sampling to limit computational burden.

While our model contains distributional assumptions, the experimental moments prove highly valuable in identifying the cost distribution parameters. The randomization of the subsidy amount across ex-ante identical potential entrants offers exogenous variation in net fixed cost that helps us trace out the shape of the fixed cost distribution. The marginal cost moments, which rely on our supply model estimates from Section 6, are important because they help describe the relative importance of variation in marginal vs. fixed costs in explaining entry decisions. Mean estimated marginal cost conditional on entry is similar across the three subsidy levels and, in fact, lowest for the high subsidy group (see the top panel of Table 7). If marginal cost variation explained most entry choices, then we would expect that the low subsidy group would have the lowest marginal cost, conditional on choosing to enter. That we see little such variation implies that fixed costs drive most of the entry decisions.

Our entry model estimates, with standard errors, are in the bottom panel of Table 7. The estimated parameters imply that, for the pool of potential entrants, mean marginal cost (intercepts) and fixed costs are about 28 Ksh and 46,000 Ksh, respectively, with a small positive correlation (0.14) between marginal and fixed costs. These means are unconditional and much higher than means conditional on entry. We estimate that entrants, in the absence of a subsidy, would have mean marginal cost intercepts of 19 Ksh and mean fixed costs of 17,000 Ksh. Fixed costs are high relative to variable profits and thus a meaningful consideration in welfare analysis. Our estimates are relatively imprecise, though as we will see in the next section, we still have enough precision to make meaningful welfare statements.

8 Welfare and Counterfactuals

We now turn to estimating welfare and the division of surplus. With our estimates of demand and sellers' marginal costs, we only lack estimates of incumbent traders' fixed costs. In Section 7 we estimated the fixed cost distribution for potential entrants. Applying this same fixed cost distribution to incumbents would ignore the likelihood that incumbents are not a random sample of potential entrants. We therefore make two refinements in estimating incumbents' fixed costs. First, all incumbents chose to enter, which means that variable profits must exceed fixed cost. Second, because marginal and fixed costs are correlated, we condition the fixed cost distribution on estimated marginal costs for incumbents. With these refinements, we estimate a trader's mean total profits in each market-week by integrating over possible fixed cost realizations.

We estimate that the mean (median) daily fixed cost for incumbents is roughly 4,200 (1,800) Ksh. For many incumbents, this cuts substantially into variable profits; for the median trader, fixed costs dissipate 71% of variable profits, leaving him with an estimated total daily profit of 1,200 Ksh. The median trader thus keeps 12% of revenues as profit.

We also calculate consumer surplus using our estimated demand system. We find that consumer surplus makes up only 18% of the total surplus remaining after accounting for deadweight loss, while traders reap 82%. While the standard errors on our entry model parameters are large, the 95% confidence interval for the consumers' fraction of surplus is (15%, 55%) and thus we can reject that consumers capture more than 55% of all surplus. In total, we estimate mean surplus per market-day as 64,000 Ksh (almost $605).

We estimate that consumers capture only a small fraction of total surplus, and yet the typical trader keeps a fairly small fraction of revenue as profit. Underlying this, we find considerable heterogeneity in trader margins, as a long tail of traders sells very large quantities at high markups. Mean total daily profits are 13,000 Ksh (more than ten times the median) and 8% of traders see daily profits above 40,000 Ksh. The mean total profits, as a fraction of revenue, is 25%.[57] Further, because these higher markup traders tend to sell larger quantities, we find that the quantity-weighted mean total profits, as a fraction of revenue, is 45%. This heterogeneity implies that while the typical trader is not making large profits, the typical consumer is being served by a trader who is. Competition policy is therefore likely to be most successful if it is targeted, focusing on markets with these large, high-margin traders.

8.1 Counterfactuals

Even if competition policy could target the most profitable colluding traders and induce more competition, the gains to consumer surplus depend on trader heterogeneity and the shape of demand. We now explore these potential gains by conducting counterfactual exer-

[57] Our estimates line up quite closely with existing estimates of profit margins collected from survey data with agricultural traders. For example, Fafchamps et al. (2005) find average margins of 11% and median margins of 8% in Benin, average margins of 27% and median margins of 11% in Madagascar, and average margins of 37% and median margins of 27% in Malawi. Other estimates of average margins range from 5-34% (see Dessalegn and Shaffer (1998) for estimates from Ethiopia; Minten and Kyle (1999) for the Democratic Republic of Congo; Gabre-Madhin (2001) for Ethiopia; and Fafchamps and Gabre-Madhin (2006) for Benin and Malawi).

cises where we vary the level of competition (by varying ω). For each value of ω, we solve for a new quantity-setting equilibrium in each market-week; we start by keeping the set of traders fixed so that we can explore how variable surplus changes with competition. We remain agnostic about the actual policy that would induce more competition but rather focus on quantifying the potential gains.

Figure 9 shows the division of surplus as we move between Cournot competition and joint profit maximization. For joint profit maximization, which we estimate as describing current market conduct, consumer surplus is only 14% of total potential surplus, while traders' profits constitute 50%. The remaining 36% is deadweight loss from missing transactions that would have occurred had sellers priced at cost.[58] As we decrease ω to Cournot competition, consumer surplus rises while trader profits and deadweight loss fall. Under Cournot competition, the other well-defined form of conduct, we estimate that consumer surplus would increase to 42%, traders would capture 37% in profits, and deadweight loss would be 20%.[59] In terms of absolute magnitudes, this would represent an average increase of 36,000 Ksh ($333) in consumer surplus per market-day, and average decreases of 16,000 Ksh ($152) and 19,000 Ksh ($181) in trader profits and deadweight loss, respectively, per market-day.

The previous counterfactual ignores a potentially important margin – exit. If competition increases, then variable profits might fall by enough that some traders might exit (or fail to enter), which could potentially undo some of the gains from competition. We thus conduct a second counterfactual exercise where we impose Cournot competition but allow traders to exit if total profits are negative.[60] We find similar results to the counterfactual without exit. Consumer surplus is 6% lower and trader profits are 7% higher under exit, but these differences are small relative to the variation from the model of competition.

9 Conclusion

Policymakers have long speculated that agricultural traders in Africa exert market power, paying below-competitive prices to farmers and charging above-competitive prices to consumers. However, limited trader record keeping and difficulties in identifying clean shocks to traders' operating costs have challenged the ability of previous work to provide clear evidence on the nature of competition in this sector. In this paper, we present some of the first

[58]These estimates differ slightly from the baseline welfare statements above because our counterfactual analysis includes a model of consumers arriving to the market. See Appendix F for details.

[59]If traders priced at cost, consumer surplus would constitute 80% of the total. Traders would still earn profits because the marginal cost curve is upward-sloping.

[60]In some markets there could be multiple equilibria in terms of which traders exit. We solve for the equilibrium in which the traders whose profits would be most negative are the ones that exit.

experimental evidence on the topic. We implement trader cost-shock and demand subsidy experiments to estimate how traders compete within a model of supply and demand. We find evidence of a high degree of intermediary market power. Welfare analysis suggests that consumers enjoy only 18% of total surplus from these transactions, while intermediaries reap the rest. If traders priced at cost, total surplus would increase by 56%.

Given the high degree of market power observed, policymakers may be interested in pursuing policies that explicitly target enhanced competition among intermediaries. Our estimated counterfactuals indicate increased competition would yield large gains to consumers and improve market efficiency. However, antitrust regulation of traders would likely be difficult to implement in an environment of low state capacity, and direct state intervention into the market to supplant the private sector may create additional problems, as seen during the largely disappointing experience with state-run markets following independence. Policies that encourage greater market entry may be more a feasible response. In an additional experiment, we generate exogenous entry by offering traders subsidies to enter specific, randomly-selected markets in which they have never worked before. We then estimate whether entry increases competition. We find that each additional trader entering the market reduces prices by only about 1%. Estimates suggest that traders with contacts are able to easily collude with incumbents and that those without contacts – though better able to encourage increased competition – are less likely to take up the entry offer in the first place. Thus, a broad policy of encouraging entry may find it challenging to generate the type of entry that increases competition.

Identifying mechanisms that increase competition is therefore an open challenge, given that collusive agreements seem flexible in incorporating the types of traders most likely to enter markets. New technologies, such as mobile marketplaces, hold some promise here. On these platforms, a larger pool of sellers interacts more anonymously, making coordination on price more difficult. Further, buyers can access a variety of sellers, rather than just those close to home. However, technological solutions must still address the real-world constraints of high transportation costs, limited trust, and other barriers that discourage exchange between new parties. The power of these technologies, as well as that of other potential mechanisms for expanding competition in these markets more broadly, is a ripe area for future research.

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Tables and Figures

Figure 1: **Maize prices in study markets**. Gray lines show the price for each market over the 12-week study period. The black line shows the average price across markets.

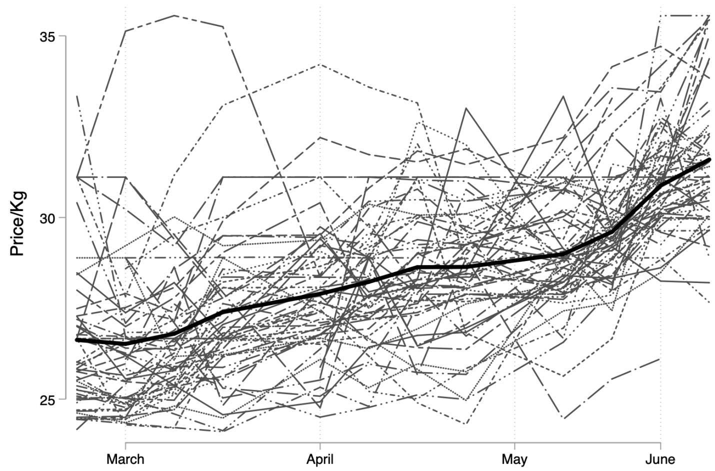
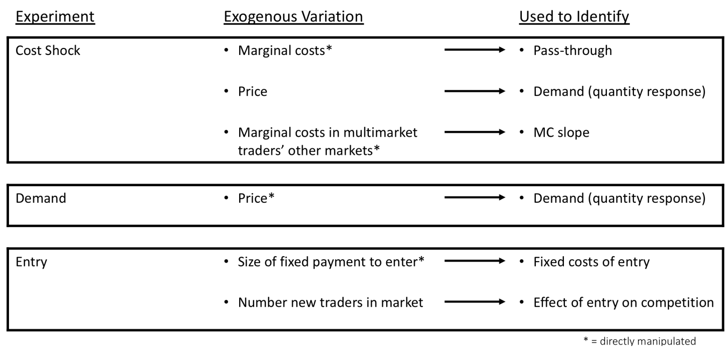
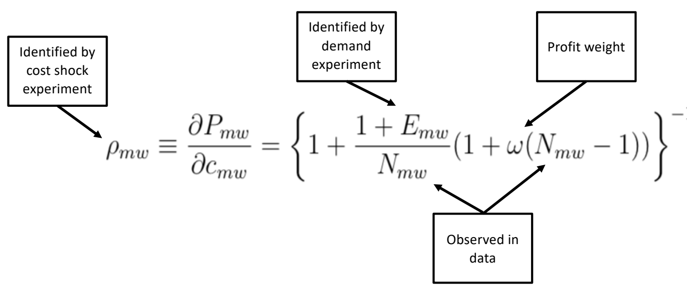


Figure 2: **Experimental design**. Panel A lays out the experiments, the exogenous variation driven by each experiment, and the objects identified by said exogenous variation. Panel B and Panel C lay out the main identifying equations for the simple and general model, respectively, and how each component is identified.

Panel A: **Experiments**



Panel B: **Simple model**



Panel C: **General model**

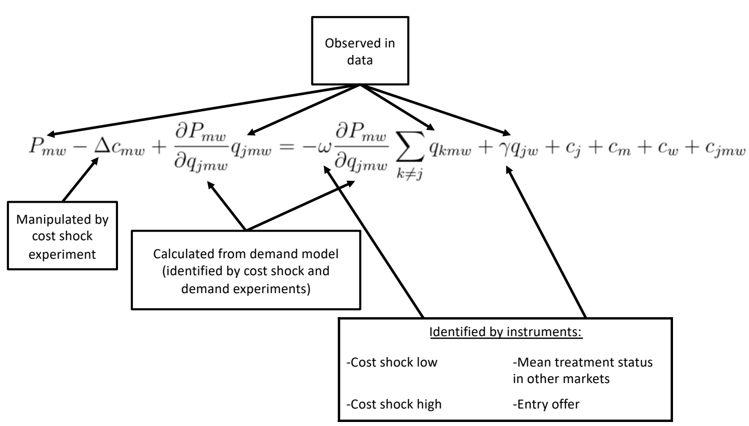


Figure 3: **Pass-through by market size**. Pass-through as estimated in markets of each size (bars represent the 95% confidence interval). The average for the full sample is 22% (dotted line). The bottom two estimates show pooled results, grouped into above/below median size.

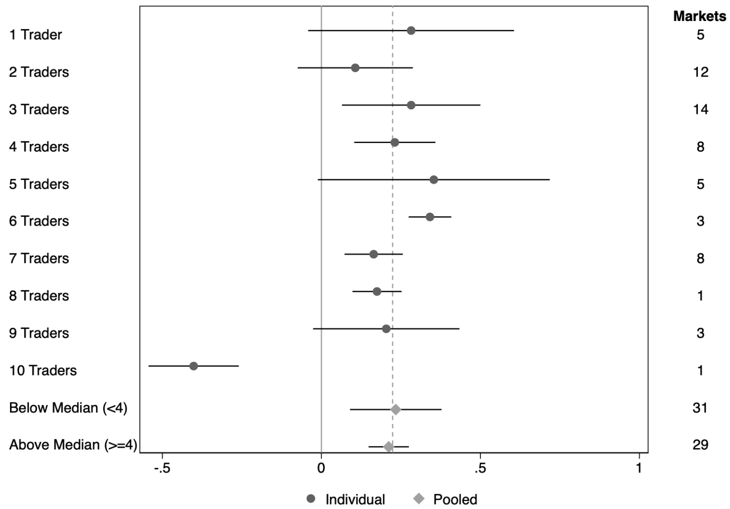


Figure 4: **Pass-through by various factors**. Pass-through as estimated in markets in each category (bars represent the 95% confidence interval). The average for the full sample is 22% (dotted line). Categories are: above/below median number of traders; on/off tarmac roads; above/below median number of days in which at least one trader reports discussing prices with other traders; above/below the median number of days in which at least one trader reports a price agreement.

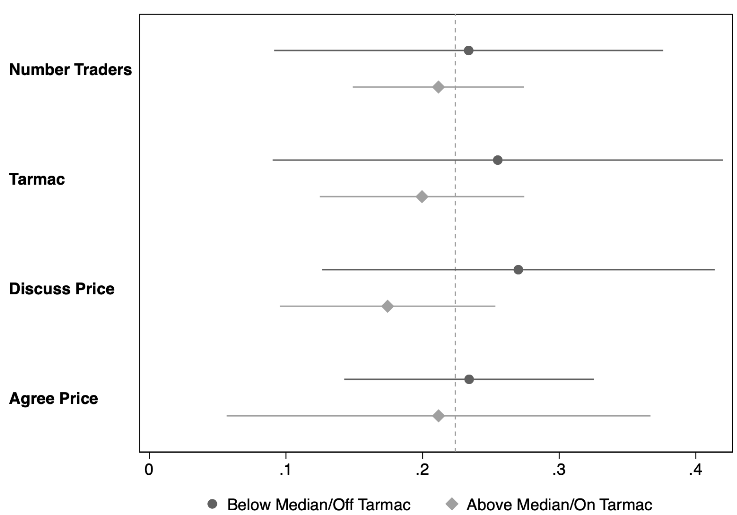


Figure 5: **Demand experiment quantity effects**. The figures show treatment effects on the fraction of consumers that choose a new quantity (left) and change in quantity transacted (right) by level of price change. Quantity changes are expressed in levels and are means that include consumers making no change. Price changes (Ksh/90kg bag) are determined by the randomized subsidy, which can take up to 10 values: $\{0, -25, -50, -100, -150, -200, -250, -300, -350, -400\}$ Ksh/90kg bag. The x-axis on the plots are price changes per kg. The mean initial quantity and price are 65kg and 32 Ksh/kg, respectively. The 95% confidence interval is shown around each point estimate.

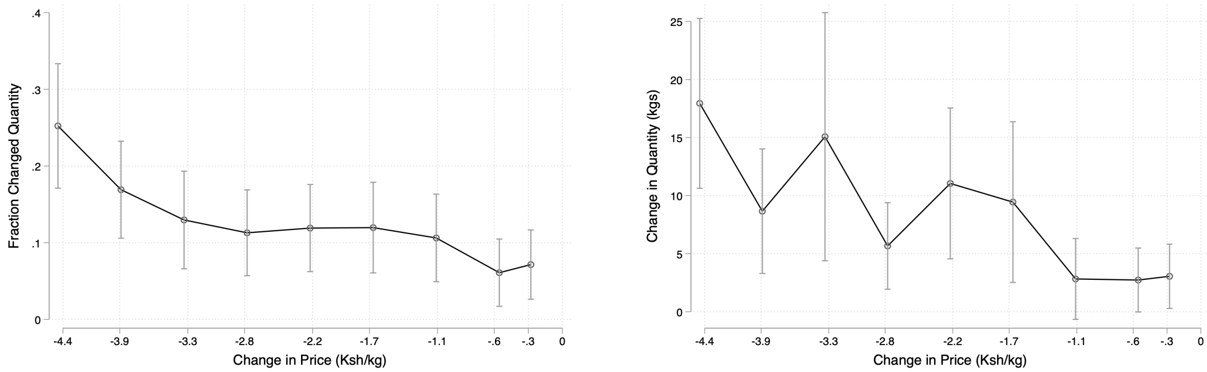


Figure 6: **Predicted pass-through under three simple models**. Given our demand curvature estimate, we predict within our simple model that one would have observed 100% pass-through in a Bertrand competitive market, 46% pass-through in a Cournot competitive market, and 20% pass-through in a collusive market environment. The distribution of empirical pass-through, calculated for 1,000 bootstrapped samples, is shown in gray. The point estimate and 95% confidence interval are shown in red.

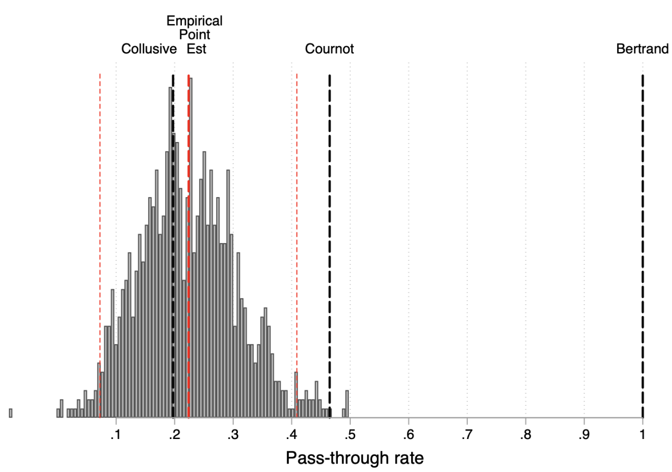


Figure 7: **Profit weight (omega) estimates – simple and general models**. The top figure shows the bootstrapped estimated ω distribution for the simple model while the bottom panel is for the general model. Bootstrap estimates of ω come from 1,000 bootstrapped estimates of ρ and δ plugged into Equation 10 for the simple model and full demand and supply bootstrap for the general model. Recall that $\omega = 0$ if Cournot competitive and $\omega = 1$ if perfectly collusive. The point estimate and 95% confidence interval are shown in red.

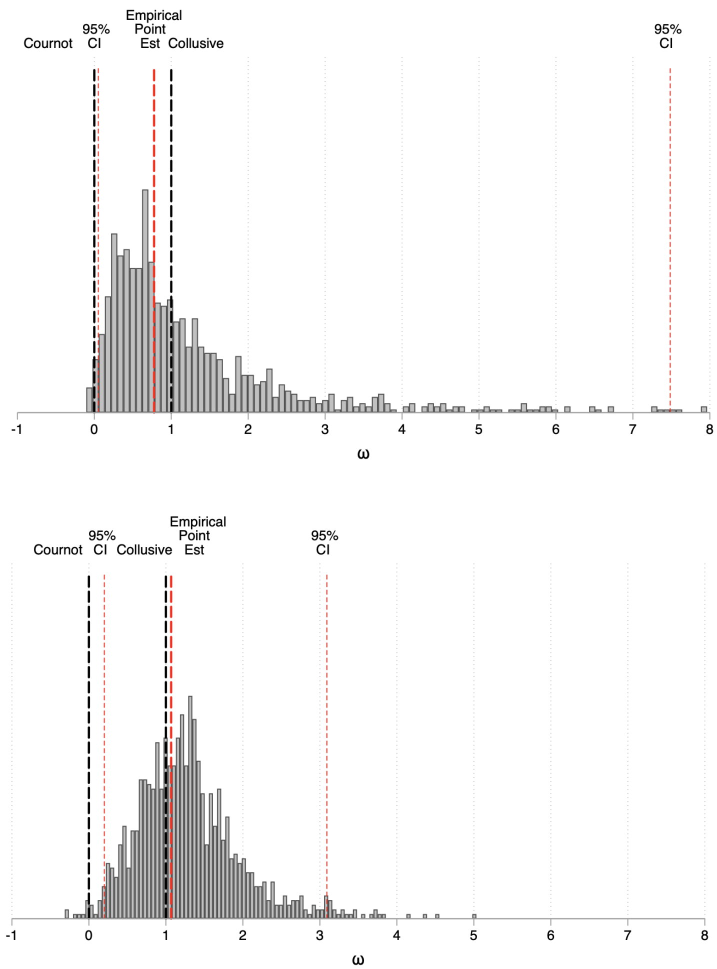


Figure 8: **Heterogeneity in willingness-to-enter**. Take-up of the entry offer regressed on various measures of heterogeneity (alternately without and with market fixed effects; the latter compares only traders offered to attend the same market). The coefficient and 95% confidence interval is plotted.

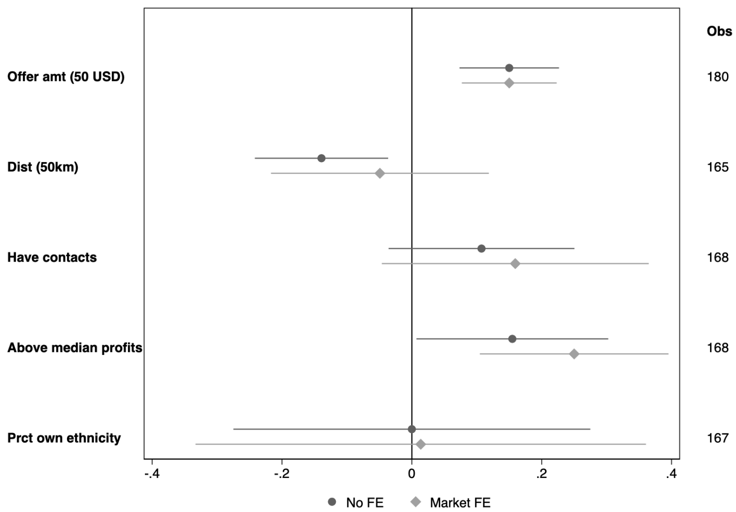


Figure 9: **Welfare counterfactuals**. Counterfactual division of welfare is shown for the market-weeks in the cost shock experiment and the control group. The estimated division of surplus under joint profit maximization is shown at the far right vertical dotted line, suggesting that trader surplus (TS) is 50% of total surplus, while consumer surplus (CS) is only 14% and deadweight loss (DWL) is 36%. Movements to the left represent increases in competition. The dotted vertical line at "Cournot" indicates how this division would be altered if the market operated under Cournot competition.

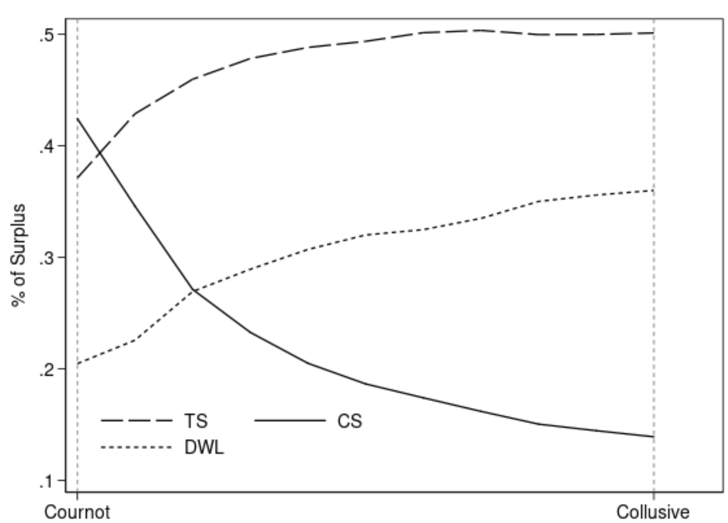


Table 1: **Pass-through.** Price regressed on "Cost Change," the level of cost reduction per kg offered in market m on week w. "Cost Change" is the *negative* value of the marginal cost subsidy in cost shock treatment markets and zero elsewhere, such that "Cost Change"={0 Ksh, -200 Ksh, -400 Ksh}) or {0 USD, -1.98 USD, -3.96 USD}. Week and market fixed effects are included to improve precision. The sample includes traders in market-days in which the market was in either the cost shock treatment or control period – market days assigned to the entry treatment are omitted. Under this specification, the coefficient on the cost reduction term yields the pass-through rate, or $\frac{\partial P}{\partial c}$. The first column shows the overall pass-through rate of 22%. The second column shows pass-through rates separately by "low" and "high" offers.

	(1) Price	(2) Price
Cost Change	0.224 (0.0434)	
Cost Change - Low		0.219 (0.0538)
Cost Change - High		0.228 (0.0618)
Mean Dep Var	28.92	28.92
N	1860	1860
Market FE	Yes	Yes
Week FE	Yes	Yes

Table 2: **Model Estimates.** The point estimates and the 95% confidence intervals for the estimated parameters of the simple and general models are displayed. The final panel is the point estimate for the marginal cost slope under the general model where we impose $\omega = 1$. This is the relevant cost parameter for markup and welfare analysis, which we implement given that traders maximize joint profits. The 95% confidence interval on γ in the model with $\omega = 1$ imposed does not account for the testing step. For a specification that allows for joint estimation of γ and testing of the model of competition, see Appendix B.

	Parameter Estimate	**95% CI Lower Bound**	**95% CI Upper Bound**
Simple Model			
a	42.76	41.56	43.96
δ	4.07	1.71	6.43
ω	0.78	0.05	7.48
General Model			
μ_a	29.15	28.84	52.19
σ_a	2.87	2.48	60.00
δ	4.21	0.70	9.64
ω	1.07	0.20	3.09
γ	0.0006	-0.0006	0.0016
General Model, $\omega = 1$			
γ	0.0007	0.0001	0.0031

Table 3: **Quantity Effects.** Columns (1), (2), (4), (6), and (8) present reduced form results while the remaining columns are results from two-stage least squares with low and high cost reduction dummy variables as instruments. “Num Trans” is the number of transactions observed in any week for a given market. “Trans Rate” is the number of transactions divided by the maximum number of transactions observed in any week for a given market. “Kgs/Trans” is the average kgs per transaction in any week for a given market. “Kgs” is the total kgs sold in any week for a given market. “Cost Change” is the change in cost (Ksh/kg) from treatment: -2.22 in the low cost shock treatment and -4.44 in the high cost treatment. “Low Treatment” and “High Treatment” are dummy variables for whether the market-week is in the low cost shock and high cost shock treatments, respectively. Elasticities are evaluated at the mean price and mean quantity under the low cost shock (the median of the three experimental groups).

	(1) Num Trans	(2) Num Trans	(3) Num Trans	(4) Trans Rate	(5) Trans Rate	(6) Kgs/Trans	(7) Kgs/Trans	(8) Kgs	(9) Kgs
Cost Change	-11.05			-0.0871		3.143		-845.6	
	(1.908)			(0.00931)		(4.111)		(163.5)	
Low Treatment		33.43							
		(5.522)							
High Treatment		36.49							
		(10.94)							
Price			-51.64		-0.405		14.68		-4043.7
			(15.48)		(0.0929)		(18.62)		(1034.8)
Mean Dep Var	60.92	60.92	60.92	0.603	0.603	87.79	87.79	4809.5	4809.5
Elasticity			-19.0		-16.0		6.0		-20.4
N	454	454	454	454	454	454	454	454	454
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 4: **Take-up of Entry Offers.** Offers ranged from 5,000-15,000 Kenyan shillings ($49-148 USD). "Take-up" = 1 if the trader *ever* took up an offer during any of the four weeks for which the offer was available.

	Offer Amount		Take-up	Obs
	Ksh	*USD*	Rate	
Low Offer	5,000	49	0.12	60
Medium Offer	10,000	99	0.28	60
High Offer	15,000	148	0.42	60

Table 5: **Effect of Entry on Prices.** The variable "Entry Market" is a dummy for treatment status in the entry experiment. "Number Entrants" is the number of traders present in the market on that day who were offered a subsidy to enter. "Number Incumbents" is the number of traders presented in the market on that day who were not offered a subsidy to enter. Column 1 presents the first stage effect of treatment on the number of entrants. Column 2 presents the effect of the treatment on the number of incumbents. Column 3 presents the reduced form effect of treatment on price. Column 4 presents the effect of the number of entrants on the price, instrumenting for the number of entrants with treatment.

	(1)	(2)	(3)	(4)
	Number Entrants	Number Incumbents	Price	Price
Entry Market	0.636	-0.0574	-0.180	
	(0.0601)	(0.131)	(0.106)	
Number Entrants				-0.283
				(0.160)
F-Stat FS				111.9
Mean Dep Var	0.303	4.045	29.04	29.04
N	1776	1776	1776	1776
Market FE	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes

Table 6: **Effect of Entry on Competition.** The top panel presents the estimate of the profit weight ω when a trader enters in the entry experiment. Markets that do not receive entry keep $\omega = 1$. The second and third columns show the bounds of the 95% confidence interval, calculated with 1,000 bootstrap iterations. The bottom panel shows separate profit weights depending on whether the entrant has contacts in the market. Of the potential entrants in the entry experiment, 32% have contacts in the targeted market.

Group	**Parameter Estimate**	**95% CI LB**	**95% CI UB**
Pooled model			
ω_e All Entrants	0.66	0.10	0.96
Heterogeneous by contacts			
ω_e^{with} Entrants with contacts (32%)	0.95	0.15	1.53
$\omega_e^{without}$ Entrants without contacts (68%)	0.44	-0.48	0.96

Table 7: **Entry Model Moments and Estimates.** The top panel presents the entry model moments and their sample values. The first six rows are the moments while the last three rows show the marginal cost intercepts conditional on entry implied by the moments. Note we use the unconditional moments in estimation but also present the conditional values here for clarity. The bottom panel presents the parameter estimates, with standard errors estimated from 1,000 bootstrap iterations. The parameters describe the lognormal multivariate distribution of marginal and fixed costs. Entry take-up rate is calculated weekly such that each potential entrant contributes four observations.

Model Moments	
Description	Estimate
Weekly takeup rate - high offer	0.2982
Weekly takeup rate - medium offer	0.1789
Weekly takeup rate - low offer	0.0596
Entry * marginal cost intercept - high offer	3.76
Entry * marginal cost intercept - medium offer	2.59
Entry * marginal cost intercept - low offer	0.86
Marginal cost intercept - high offer	12.61
Marginal cost intercept - medium offer	14.49
Marginal cost intercept - low offer	14.43

Model Estimates		
Parameter	Estimate	Standard Error
μ_{MC}	3.31	1.87
μ_{FC}	10.28	4.79
σ_{MC}	0.25	0.97
σ_{FC}	0.94	1.13
ρ_{MCFC}	0.14	0.73

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A Appendix: Maize Value Chains and Trader and Consumer Characteristics

Figure A.1 displays the maize output market chain in western Kenya. Data for the percentage breakdown in sourcing and sale location was collected in a four-round panel survey conducted with over 300 regional traders in the area from 2013-2014 (averages displayed).

Regional traders, the subjects of this study, are responsible for large-scale aggregation, storage, and transportation. They report purchasing 50% of their maize from small and medium farmers (selling less than 5 tons), 16% from large farmers, and 33% from other traders. About half of the purchases from farmers use a local assembler or broker. Brokers are often slightly wealthier members of rural communities (and are often farmers themselves) who identify other farmers in their villages who are ready to sell. They either purchase from fellow farmers, bulk, and sell to the regional trader or, for a commission, they simply identify farmers who are willing to sell. Either way, they are small scale, often work only seasonally, and typically lack the working capital to do large-scale aggregation, long-run storage, or transport of any distance.

Traders tend to own a warehouse in a market center and either rent or own a truck which they use to purchase maize, bring it back to their warehouse for sorting, drying, and re-packaging, and then carry onward to their destination of sale. In our sample, 64% of sales take place in open-air markets in rural communities. There, 66% of traders' customers are individual households, while the rest are primarily village retailers. Traders also sell about 16% of their inventories to millers, who mill maize into flour for sale to supermarkets and other stores that serve urban consumers. They sell another 16% to other traders, who sell in other areas of Kenya or eastern Uganda. A very small portion of sales – about 2% – is sold to restaurants, schools, and other institutions. Finally, about 2% is sold to the Kenyan National Cereals and Produce Board, the former state maize marketing board that still has limited involvement in the market by purchasing, storing, and selling small reserves of maize with a goal of stabilizing prices.

Table A.1 presents summary statistics for traders in the sample. Figure A.2 displayed the average number of traders per market. The number of traders is calculated as the average number of traders present in the market during 12 weeks of the study period, as predicted by week and market fixed effects (that is, any increase in number of traders due to the entry experiment is omitted).

Table A.2 presents summary statistics for consumers served by traders in the sample. This data is drawn from a phone survey with 165 consumers randomly selected from the demand experiment sample. This survey was conducted in July and August 2016 immediately following data collection for the main experiment.

Figure A.1: **Maize value chain in study area**.

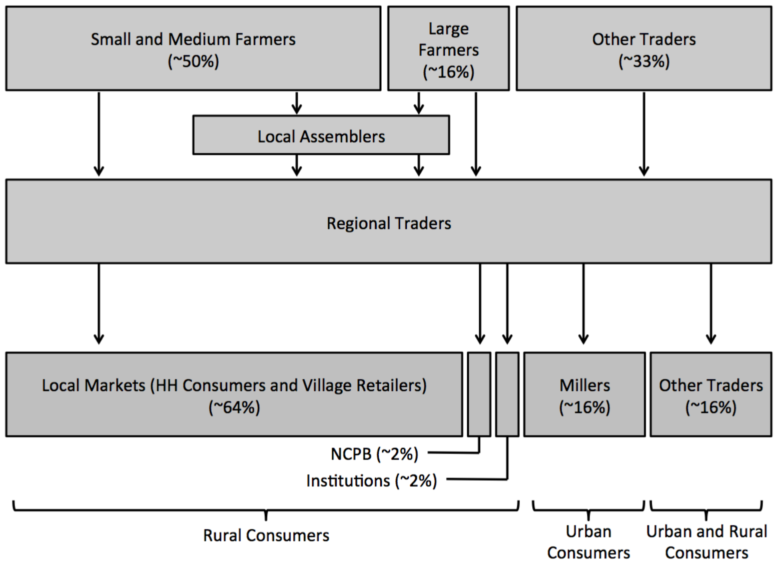


Figure A.2: **Number of traders per market**

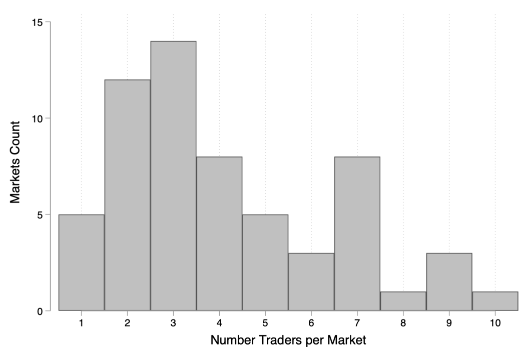


Table A.1: **Trader summary statistics.**

	Mean	Std. Dev.	Obs
Education and Business Characteristics			
Complete primary	0.78	0.42	2,728
Complete secondary	0.33	0.47	2,728
Percent corrrect Ravens	0.49	0.22	2,681
Review financial stregth monthly+	0.62	0.49	2,728
Keep written records	0.58	0.49	2,728
Any employees	0.37	0.48	2,728
Number employees	1.04	1.98	2,728
Own lorry	0.35	0.48	2,992
Market Experience			
Work in this market most weeks	0.96	0.20	2,934
New trader	0.01	0.11	2,934
Worked with all before	0.77	0.42	3,008
Know other traders well	0.68	0.47	2,549
Know other traders well or somewhat well	0.94	0.23	2,549
Single sample market trader	0.83	0.37	465
Number sample markets visited	1.29	0.77	465
Collusion Reports			
Self-report discuss price	0.38	0.49	2,549
Someone in market report discuss price	0.80	0.40	2,777
Percent traders with whom discuss price	0.77	0.28	976
Self-report agree price	0.30	0.46	2,549
Someone in market report agree price	0.72	0.45	2,777
Percent traders with whom agree price	0.77	0.28	777

Table A.2: **Consumer summary statistics.** "Number markets" is the number of markets at which the consumer typically buys maize. "Buys at least once a week" presents the percent of consumers who report buying maize at last once a week. "Search" is the percent of consumers who report approaching multiple traders before deciding from whom to buy. "Same trader" is the percent of consumers that always buy from the same trader.

	Mean	Median	SD
Number markets	1.57	1.00	1.05
Buys at least once a week	0.87		
Search	0.48		
Same trader	0.61		

A.1 External Validity

Maize is a distinctly important crop in Kenya, accounting for over a third of average gross caloric intake and about 9% of annual household expenditure (Argent and Begazo, 2015). However, it is by no means unique in its market set-up, especially with regard to the physical layout of markets. The markets in which this study operates are not exclusive to maize, but rather sell a wide-variety of crops, including beans, potatoes, cabbage, tomatoes, onions, peppers, bananas, etc. For almost all crops, sellers are located immediately adjacent to each other, facilitating easy search (and potentially easy collusion). One important distinction is that while maize traders tend to exclusively sell maize, sellers of fruits and vegetables often sell several types of produce at once. Further, maize traders tend to have larger firm sizes and conduct trade across longer distances, while many produce sellers are smaller, more locally-based retail vendors.

B Appendix: Non-Nested Tests of Joint Profit Maximization and Cournot Competition

As we described in Section 3, we use the profit weight model primarily to test between joint profit maximization and Cournot competition. We now turn to a non-nested test of these forms of competition, where we follow the logic of Berry and Haile (2014) and the application of Backus et al. (2019a). We refer to the results in Section 6 and provide more detail here.

Our identifying logic in Section 6 was that the experimental cost subsidy should be orthogonal to traders' cost type (pre-subsidy). We employ similar logic here. Under the null hypothesis that a specific model describes conduct, we can construct traders' first-order conditions. If the model is correctly specified, then the experimental cost subsidy, except for its direct effect of lowering costs by a known amount, should be orthogonal to traders' cost type and thus should be orthogonal to traders' implied marginal benefit.[61]

Specifically, we return to Equation 15 and plug in $\omega = 0$ for the null hypothesis of Cournot competition and $\omega = 1$ for the null hypothesis of joint profit maximization. For the null hypothesis of Cournot competition, we estimate:

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} = \pi \Delta c_{mw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw} \tag{22}$$

where we continue to use instruments because q_{jw} is endogenous but we omit the cost subsidy instruments.[62] We then test whether $\pi = 0$ where we form test statistics using our 1,000 bootstrap iterations. Our p-value on this test is 0.006 such that we reject Cournot competition.

For the null hypothesis of joint profit maximization, we estimate:

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} + \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} = \pi \Delta c_{mw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw} \tag{23}$$

with the same instruments. We then test whether $\pi = 0$ where we form test statistics using our 1,000 bootstrap iterations. Our p-value on this test is 0.164 such that we fail to reject joint profit maximization.

If we impose constant marginal costs and estimate via OLS, our p-values on Cournot and joint profit maximization are 0.006 and 0.180, respectively.

[61] We refer to marginal "benefit" instead of marginal revenue because under joint profit maximization the trader is not just considering his own revenues.

[62] To construct a single test, we combine the low and high cost shock treatment indicators into a single regressor: Δc_{mw}. Results are similar if we include both treatments separately.

C Appendix: Evaluating Model Assumptions

C.1 Static Model

This appendix presents the empirical basis for the decision to model a static equilibrium. Because maize is in theory a storable commodity, an alternative would be to model demand as dynamic, with prices and quantities purchased in one week affecting those bought in the next. However, empirically, consumer stockpiling is quite limited. The modal consumer purchases maize every week from her local weekly market (see Table A.2) and buys only the small amount necessary for weekly consumption (the median household consumer buys 7 kg and the median vendor buys one 90-kg bag). These weekly purchases occur against the backdrop of a 19% increase in price over the course of the lean season. If consumers were stockpiling, one would expect large purchases early in the season, when prices are low, and limited purchases later in the season, when prices are high. This is not what we observe. Related work in the region suggests that credit constraints limit households' ability to arbitrage these price fluctuations (Burke et al., 2019).

The randomized order of treatment periods allows us to go one step further and explicitly test the validity of this assumption. If inter-temporal dynamics are at play and consumers are stockpiling maize when prices drop during the pass-through experiment, one would expect a lower quantity of maize to be sold in the period following the removal of the subsidy, as consumers have stockpiled the period before. To test for this, we regress the total quantity sold in a given market-day on the previous period's treatment status (controlling for current treatment status). Column 1 of Table C.1 presents the results for the full sample. We see that having been a cost shock market in the previous 4-week block does not affect the prices, quantities sold, or number of customers in the following block. The point estimate is small in magnitude and far from statistically significant. In order to confirm that this null finding is not merely the result of low power (perhaps due to a quickly petering out stockpiling effect over the course of the 4-week block), Column 2 restricts the sample to the week immediately following the switch of treatment status, a period in which one should expect the stockpiling effect to be most concentrated. We continue to see no evidence of a stockpiling effect here (in fact, the point estimate on quantities becomes positive, though standard errors also increase substantially with this reduced sample). Given limited evidence of consumer stockpiling, we model demand as static and therefore decisions regarding prices and quantities as separable across market-days.

It is possible that the lack of effect on total quantities is the result of two competing effects canceling each other: out new customers, as they learn that the price is lower, and less demand from existing customers, as they stockpiled maize. To check for this, Columns 3 and 4 run similar specifications with the number of customers as the outcome variable. Again, we see no effects of the previous period's treatment status, suggesting this alternative explanation is not at play, and again adding confidence to the static model.

Finally, Columns 5 and 6 check for dynamic pricing, running the same specification with price as the outcome. Again we see no significant effects.

Table C.1: **Effect of Previous Treatment Status on Outcomes in Current Period.** Outcome variables as a function of previous treatment status, controlling for current treatment status. Outcome variables are log quantity sold (Columns 1 and 2), log number of transactions (Columns 3 and 4), and log price. "Cost Shock Previous" is a dummy for whether the market was in a cost shock treatment market in the previous period. Columns 1, 3, and 5 present results for the full sample. Columns 2, 4, and 6 present results for the first week of the block, when one would expect to see most concentrated dynamic effects, if existent.

	Ln Kgs	Ln Kgs	Ln Num Customers	Ln Num Customers	Ln Price	Ln Price
Cost Shock Previous	-0.0131	0.199	-0.0315	0.0539	-0.00316	-0.0108
	(0.157)	(0.213)	(0.0611)	(0.0939)	(0.00513)	(0.00657)
Mean DV	7.369	7.273	2.103	2.094	3.390	3.363
N	2191	541	2047	497	2029	495
Sample	Full Block	W1 Only	Full Block	W1 Only	Full Block	W1 Only
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes

It is also worth noting that storage is also quite limited among traders. Burke et al. (2019) find that "in a panel survey of local traders, we record data on the timing of their marketing activities and storage behavior, but find little evidence of long-run storage." In that data, collected with traders from the same region, only 31% of traders report doing any storage. Those that do on average store about 30% of the bags they buy and only for extremely short periods of time (on average, 4 days, among those that store). Only 1.2% of traders store maize for more than a week. For the rest of the traders, our general supply model accommodates such arbitrage as the cost function covers all sales in the week.

C.2 Product Differentiation

Staple food commodities are often pointed to as the textbook example of a homogenous goods. However, we take seriously the concern that this assumption could be wrong and that there could be quality differences across sellers, which would result in product differentiation. We therefore collect detailed quality estimates. Note that the use of grain standards in Kenya is restricted to the most formal settings of large millers and the National Cereals and Produce Board. Regional traders typically do not know the official grade of their maize, and consumers do not use grades to describe or evaluate quality. Instead, traders and consumers assess quality of maize based on several readily observable characteristics: coloration, grain size, grain intactness, presence of foreign matter, and presence of weevil infestations. Therefore, we measure quality according to the these standards, which are those relevant to the market actors in question. Enumerators were trained to grade quality on a scale from 1 (lowest quality) to 4 (highest quality) according to the following rubric, which was developed with the guidance of several traders in the pilot: 4=Excellent [no pest, no foreign matter, no broken grain, no discoloration, sizable grain]; 3=Good [barely infested, <5% foreign matter (e.g., maize cobs, dust, sand etc.), <5% broken grain, <5% discolored]; 2=Fair [infested, 5%-25% foreign matter, 5%-25% broken grain, 5%-25% discolored]; 1=Poor [infested, >25% foreign matter, >25% broken grain, >25% discolored].[63]

There is no variation in quality offered by a single trader to his customers in the same market-day. In fact, it is common for traders to mix bags they have purchased of different quality prior to arrival at the market with the explicit goal of offering a uniform quality level.[64] We therefore collect only one measurement of quality for each trader in each market-day. Across traders in the same market day we observe little variation in quality, as measured on a scale of 1-4 (97% of all maize receiving a rating of 2 or 3). Moreover, as shown in Column 1 of Table C.2, prices are not statistically different across the (limited) variation seen in quality.

The other salient dimension on which products might be differentiated is the availability of credit (while not strictly a dimension of the physical product, the ability to buy on credit is dimension of the transaction). However, credit does not appear to be a salient factor in these primarily "cash-and-carry" spot markets; over 95% of transactions are conducted in cash. That said, it may be that the *availability* of credit matters to a minority of customers; when asked how customers decide on which trader from whom to buy, 34% cite the availability of credit when needed, so it does appear that a slightly larger percent of customers value the possibility of obtaining a line of credit in periods when they are in need. Moreover, while we do see small price differences for purchases on credit, this relationship disappears when controlling for other features of the transaction.[65]

Locational differences, combined with search costs, could also be a basis for product differentiation. However, within a given market, search costs for consumers are negligible in this setting, as traders sell in trucks parked immediately next to each other or in stores located immediately adjacent to each other

Reflective of this limited variation in product characteristics (e.g. quality, credit, etc.), we see little variation in prices. The coefficient of variation in prices offered by the same trader, same day is 3.1%, while the coefficient of variation in the average price of traders in the same market, same day is 5.1%.

Therefore, the weight of evidence appears to suggest that maize sold in these markets is a relatively homogenous good.

Finally, it is worth noting that even if differentiation were driving the low pass-through we estimate, much of the paper's framework and conclusions would still be relevant. We would still interpret the low pass-through, given the same demand estimates, as evidence that the

[63]No formal tools were used to measure precise percentages; rather, enumerators were trained to take a handful of maize in their palm and count the kernels that matched each description. While this involves some imprecision, it is nearly identical to the process by which consumers judge quality – that is, by feel, sight, etc. – and therefore captures well the information available to consumers, which is the pertinent metric. Enumerator training on grading included practice evaluating the quality level of real samples of maize.

[64]Incentives to maintain a uniform average quality could be driven by consumer preferences or by a desire to not deviate from the average quality offered by other traders.

[65]Unexpectedly, the relationship between credit and price seen in Column 2 is negative, but this may be driven by omitted variables such as transaction size and consumer identity. After controlling for these factors in Column 3, there is no significant difference in price charged for credit transactions (and the coefficient is now sensibly positive, albeit very small in magnitude).

trader is capturing most of the change in surplus from the cost decrease.[66] Our demand model includes customer fixed effects, which could represent customer heterogeneous preferences or some additional utility the customer derives from buying from that trader, especially because the demand experiment occurs after the customer has chosen a trader. The main adjustment would be altering the supply side to determine how heterogeneous consumers sort to different traders. Thus, while the source of market power would be different, it would still likely lead to traders capturing most surplus gains from cost decreases.

Table C.2: **Product Differentiation.** Data drawn from trader price surveys, broken out by transaction (there are almost 40,000 transactions observed in the full dataset). Market-day fixed effects are employed to compare difference in transaction characteristics only within the same market-day. Quality is ranked on a scale from 1(=lowest quality) to 4(=highest quality). Credit is a dummy for whether the transaction was conducted on credit. Other controls refer to the size of the transaction and the identity of the customer (household vs. village retailer). All standard errors are clustered at the trader x date level.

	(1) Ln Price	(2) Ln Price	(3) Ln Price
Quality (1-4, 4=best)	0.000450		0.00156
	(0.00212)		(0.00180)
Credit		-0.0177	-0.000767
		(0.00273)	(0.00276)
Mean Dep Var	3.366	3.366	3.366
N	39598	39667	39598
Market-day FE	Yes	Yes	Yes
Other Controls	No	No	Yes

C.3 Price Discrimination

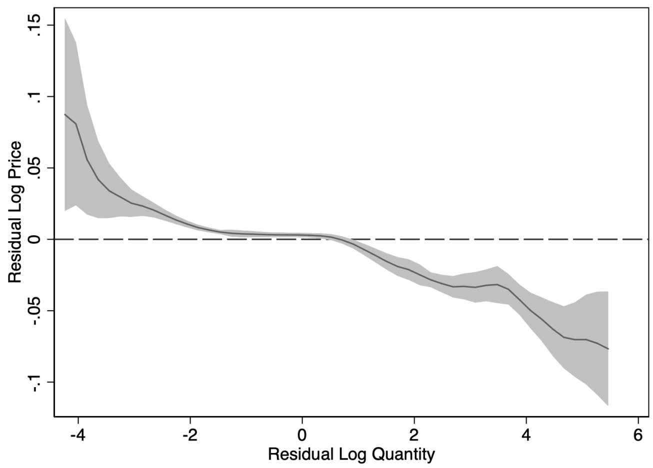
Empirically, we see little variation in the price that a given trader offers his customers through the day; the intra-cluster correlation of these prices is 0.9. While there is no official posted price to ensure that prices are equivalent across customers, negotiations between traders and customers occur in public (often in front of the trader's truck or store, where other customers are typically lined up to purchase). This likely limits traders' ability to engage in dramatic price discrimination. However, traders may be able to engage in some small and imperfect price discrimination using tools such as bulk quantity discounts, as documented in recent work by Attanasio and Pastorino (2015).[67] To explore whether there is evidence of such nonlinear pricing schemes in our setting, we utilize transaction-level data (totaling 39,667 transactions) and explore the covariance of price and quantity of maize sold by the

[66]Note that any costs the trader incurred to supply a differentiated product are already incorporated via heterogeneous marginal cost or are fixed costs that do not change in response to the cost decrease.

[67]Attanasio and Pastorino (2015) find that sellers of food staples in Mexico are able to exert market power to discriminate across customers with different levels of willingness (and ability) to pay. Sellers in their setting offer nonlinear pricing schemes using bulk discounts.

same trader to his customers in a given market-day. Figure C.1 presents this relationship, plotting a kernel-weighted local polynomial regression of log price on log quantity, both demeaned by trader x market-day fixed effects. While the relationship is relatively flat in the middle of the distribution, we see that customers at the lower end of the quantity distribution are paying more per kg, while those at the higher end are paying less per kg. The 95% confidence interval area, delineated in grey, suggests that these bulk discounts are particularly prominent at very large quantities. The effect sizes are relatively small, with the bulk of overall variation of price lying within a band of about +/-1%; however, they do suggest that traders possess some limited ability to use nonlinear pricing to price discriminate. Note that any ability to price discriminate is prima facie evidence of market power.

Figure C.1: **Quantity discounts**. Within trader x market-day residuals of transaction-level log price/kg and quantity/kg. N=39,667. Grey area represents the 95% confidence interval.



D Appendix: Constant Marginal Costs

A key assumption of the model underpinning the simple model is that of constant marginal costs. In this appendix, we present direct empirical evidence, beyond the more general model, supporting this assumption.

This evidence suggests that the assumption of constant marginal cost is in fact a fairly good fit for the empirical setting. Agricultural intermediation is an industry for which the majority of variable costs – the purchase price of the inventory, the cost of casual laborers' time for loading and off-loading, etc. – appear to be fairly constant with respect to quantities. While there may be a discontinuous increase in marginal cost when capacity constraints are hit (for example, if a trader sells more than the capacity of his truck and would need to bring a second truck to sell an additional bag), empirically this constraint is rarely binding, as only 7% of traders in the sample sell out of the full amount of maize they have brought to the market that day. Consistent with this, a detailed investigation of trader expenses across three countries finds that traders appear to face fairly constant costs across these settings (Fafchamps et al., 2005).

This is concordant with the estimates from our general model. Our estimate of γ, the marginal cost slope, is 0.0006 Ksh/kg, and 0 is well within a fairly tight 95% confidence interval of $(-0.0006, 0.0016)$. This point estimate is small, implying that a 1 standard deviation increase in weekly (in-sample) quantity sold (2300 kgs) corresponds to a cost increase of just 1.73 Ksh/kg. This is small compared to the heterogeneity in trader-market-week marginal cost intercepts – marginal cost for the first kg – where we estimate a standard deviation of 10.84 Ksh/kg. Thus, given this auxiliary cost data plus the structural model estimates, we consider constant marginal costs as a reasonable approximation of the empirical setting in which this experiment takes place.

E Appendix: Sample Selection and Experimental Schedule

The sample of markets in this study is drawn from six counties in Western Kenya. These counties encompass most of the (Kenyan) area within a 50km radius from the town of Bungoma, Kenya, the site of the research hub for this study. A listing exercise was conducted with the Director of Trade in each county to get a comprehensive list of all markets in the county. We excluded markets that were reported to not have any maize traders typically present. These represent some of the smallest rural markets, which have only maize retailers, who in turn purchase their maize from traders in larger markets. Major urban markets in the town centers were also excluded since the primary focus of this study is on the rural markets frequented by rural consumers.[68]

The exercise yielded 154 potential markets for inclusion. From this sample, 60 markets were selected in the following stratified manner: 40 markets were selected from within a radius of 50 km of Bungoma town and 20 markets were selected from outside this radius.[69] We administered a pre-experiment survey to this group of 60 selected markets in which we verified information provided by the Director of Trade and recorded the number of traders typically in the market.[70] In a large number of these markets, it was found that the information provided by the Director of Trade was inaccurate.[71] Markets that were deemed ineligible upon visit were then replaced with market from their same stratum.[72] Newly selected markets were then visited in an identical verification exercise. This process was continued until 60 markets had been selected for inclusion in the sample.

Figure E.1 presents the experimental schedule. The 60 markets in our sample are randomly assigned one of six possible schedules, in order to yield randomized ordering of treatment statuses. There are therefore 10 markets in each schedule. This allows the inclusion of market and week fixed effects in every analysis. There is therefore a total of 720 market days in our sample, clustered into 180 market x four-week block cluster (standard errors in all specifications are clustered at this market x four-week block level). The demand experiment is run in a quarter of the markets during each week break in between each treatment status. Each market therefore receives the demand experiment once.

[68]These markets represented only 2% of the total markets listed.

[69]The 40 markets within 50km of Bungoma were selected randomly. This randomization was stratified to include 25 markets from which we had valuable historical data from pilot work, while the remaining 15 markets were new to the sample. The 20 markets located more than 50km from Bungoma were selected according to a non-random algorithm in order to minimize confounding effects due to spillovers and get a larger geographic distribution of markets. For each market, the distance to the nearest market in the pool (the 40 selected markets within 50km of Bungoma as well as any remaining markets in this outer circle pool) was calculated and then the market with the shortest distance was dropped.

[70]Each trader present in the market during this verification exercise was asked "How many maize traders are typically present in this market on an average market day from March to July?" Answers were averaged across all traders to yield a single measure of the number of traders typically present in the market.

[71]The most common issue being that the market was so small as to not have any traders.

[72]That is, markets from the first stratum forming the area within 50 km of Bungoma were replaced with another randomly selected market from this stratum. Markets from the outer stratum of 20 markets were replaced with the next further market, according to the algorithm determining selection in this stratum.

Figure E.1: **Experimental schedule.**

<table>
<tr><th></th><th>Schedule 1</th><th>Schedule 2</th><th>Schedule 3</th><th>Schedule 4</th><th>Schedule 5</th><th>Schedule 6</th></tr>
<tr><td>Week 1</td><td colspan="6">Demand Experiment in 1/4 of markets</td></tr>
<tr><td>Week 2</td><td rowspan="4">Pass Through</td><td rowspan="4">Control</td><td rowspan="4">Entry</td><td rowspan="4">Pass Through</td><td rowspan="4">Control</td><td rowspan="4">Entry</td></tr>
<tr><td>Week 3</td></tr>
<tr><td>Week 4</td></tr>
<tr><td>Week 5</td></tr>
<tr><td>Week 6</td><td colspan="6">Demand Experiment in 1/4 of markets</td></tr>
<tr><td>Week 7</td><td rowspan="4">Entry</td><td rowspan="4">Pass Through</td><td rowspan="4">Control</td><td rowspan="4">Control</td><td rowspan="4">Entry</td><td rowspan="4">Pass Through</td></tr>
<tr><td>Week 8</td></tr>
<tr><td>Week 9</td></tr>
<tr><td>Week 10</td></tr>
<tr><td>Week 11</td><td colspan="6">Demand Experiment in 1/4 of markets</td></tr>
<tr><td>Week 12</td><td rowspan="4">Control</td><td rowspan="4">Entry</td><td rowspan="4">Pass Through</td><td rowspan="4">Entry</td><td rowspan="4">Pass Through</td><td rowspan="4">Control</td></tr>
<tr><td>Week 13</td></tr>
<tr><td>Week 14</td></tr>
<tr><td>Week 15</td></tr>
<tr><td>Week 16</td><td colspan="6">Demand Experiment in 1/4 of markets</td></tr>
</table>

F Appendix: Estimation Details

In this appendix we provide estimation details for the empirical models in the main text. We start by specifying all model equations before providing estimation details for each component. Our general model is:

$$q_{imt}(P_{imt}) = \begin{cases} \left(\frac{a_i - P_{imt}}{b_i}\right)^{\frac{1}{\delta}} \eta_{imt} & if \;\; P_{imt} \leq a_i \\ 0 & if \;\; P_{imt} > a_i \end{cases} \tag{24}$$

$$a_i \sim N(\mu_a, \sigma_a^2) \tag{25}$$

$$Q_{mw}(P_{mw}) = \sum_{i \in \mathcal{I}_{mw}} q_{imw}(P_{mw}) \tag{26}$$

(27)

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} = -\omega(Entry, Contacts) \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

$$\omega(Entry, Contacts) = \begin{cases} \omega_n & if \;\; \text{No Entry} \\ \omega_e^{with} & if \;\; \text{Entry by Trader with Contacts} \\ \omega_e^{without} & if \;\; \text{Entry by Trader without Contacts} \end{cases} \tag{28}$$

$$Entry_{jmw} = \begin{cases} 0 & if \;\; \pi^V_{jmw}(MC^0_{jmw}, \omega) < FC_{jmw} - EntrySubsidy_{jmw} \\ 1 & if \;\; \pi^V_{jmw}(MC^0_{jmw}, \omega) \geq FC_{jmw} - EntrySubsidy_{jmw} \end{cases} \tag{29}$$

$$\begin{pmatrix} MC^0_{jmw} \\ FC_{jmw} \end{pmatrix} \sim logN \left(\begin{pmatrix} \mu_{MC} \\ \mu_{FC} \end{pmatrix}, \begin{pmatrix} \sigma^2_{MC} & \rho_{MCFC}\sigma_{MC}\sigma_{FC} \\ \rho_{MCFC}\sigma_{MC}\sigma_{FC} & \sigma^2_{FC} \end{pmatrix} \right). \tag{30}$$

Equations 24 and 26 are household and market demand, respectively. Equation 27 describes the supply side and nests Cournot and collusion. Equation 29 determines trader entry and Equation 28 lets the form of competition change with entry (ω_e indicates entry, ω_n indicates no entry). Equations 25 and 30 impose distributional assumptions on some of the unobserved heterogeneity. This gives us the following parameters:

Parameter	Description
δ	Curvature of individual demand
μ_a	Mean of demand intercept
σ_a	Standard deviation of demand intercept
b_i	Heterogeneity in price coefficient
γ	Marginal cost slope
c_j	Trader-specific marginal cost
c_m	Market-specific marginal cost
c_w	Week-specific marginal cost
ω_n	Profit weight if no entry (baseline equilibrium)
ω_e^{with}	Profit weight if entry by connected entrant
$\omega_e^{without}$	Profit weight if entry by unconnected entrant
μ_{MC}	Mean marginal cost intercept
μ_{FC}	Mean fixed cost
σ_{MC}	Standard deviation of marginal cost intercepts
σ_{FC}	Standard deviation of fixed costs
ρ_{MCFC}	Correlation between marginal cost intercept and fixed cost

Because we estimate model separately, we list each set of moments below depending on the equation.

F.1 Estimating Demand

We take the log of Equation 24 and then take first differences within consumer (where the two time periods are before the offered subsidy and after). This gives us the following equation to estimate:

$$(31)\quad log(q_{im1}) - log(q_{im0}) = \frac{1}{\delta}\left(log(a_i - P_{im1}) - log(a_i - P_{im0})\right) + \left(log(\eta_{im1}) - log(\eta_{im0})\right)$$

where $d_i \equiv P_{im1} - P_{im0}$ is the subsidy (or discount) amount. d_i takes on 10 values, but because consumers randomized to the zero subsidy treatment were not offered a chance to change transacted quantity, we drop this group such that the remaining sample has 9 different subsidy values.

Our first set of moments is $E(\mathbb{1}\{d_i = d\}\,(log(\eta_{im1}) - log(\eta_{im0})))$ for the 9 different values of d. Our second set of moments draws from the control and cost shock markets. Let t_{mw} be the transaction rate in market m in week w and q_{mw} be the mean quantity (kgs) per transaction.[73] Let t^c, t^{low}, and t^{high} be the sample mean transaction rates for control, low cost shock, and high cost shock market-weeks, respectively, with analogous notation for mean

[73] We construct t_{mw} from the number of observed transactions, dividing by the maximum number of transactions observed in that same market in a single week during the 12-week experimental period. Our results are robust to increasing this denominator by at least a factor of 2.

quantity per transaction. Further, let P^c, P^{low}, and P^{high} be the sample mean prices in these three treatment arms.[74] Then we construct the following six moments:

- $E(\mathbb{1}\{a_i > P^c\} - t^c) = 0$
- $E(\mathbb{1}\{a_i > P^{low}\} - t^{low}) = 0$
- $E(\mathbb{1}\{a_i > P^{high}\} - t^{high}) = 0$
- $E(\mathbb{1}\{a_i > P^c\}\left(\frac{a_i - P^c}{b_i}\right)^{\frac{1}{\delta}}\eta_{imt} - q^c) = 0$
- $E(\mathbb{1}\{a_i > P^{low}\}\left(\frac{a_i - P^{low}}{b_i}\right)^{\frac{1}{\delta}}\eta_{imt} - q^{low}) = 0$
- $E(\mathbb{1}\{a_i > P^{high}\}\left(\frac{a_i - P^{high}}{b_i}\right)^{\frac{1}{\delta}}\eta_{imt} - q^{high}) = 0$

The sample values of these moments are:

Table F.1: **Quantity Moments from Cost Experiment.** Transaction rate is the number of transactions divided by the maximum number of transactions observed in any week for a given market. Kgs/Transaction is the average kgs per transaction in any week for a given market.

	(1) Transaction Rate	(2) Kgs/Transaction
Low Cost Reduction Treatment	0.267	-32.63
	(0.0258)	(11.26)
High Cost Reduction Treatment	0.284	-14.68
	(0.0547)	(18.38)
Constant	0.241	72.25
	(0.0576)	(39.17)
Mean Dep Var	0.589	98.27

We use these 15 moments in estimating δ, μ_a, and σ_a via method of simulated moments where we simulate from the normal distribution of a_i. We use a three-step (iterated) procedure with an estimated optimal weighting matrix and use an analytical gradient to speed up computation. We do not estimate b_i directly, as it drops out of our first difference specification. But the distribution of b_i, or more specifically, $b_i^{-\frac{1}{\delta}}\eta_{imt}$, is necessary for calculating the model predicted moments and for subsequent analysis. Given estimates of δ, μ_a, and σ_a, we estimate the distribution of $b_i^{-\frac{1}{\delta}}\eta_{imt}$ with the following procedure:

1. Draw a transaction (quantity-price pair) from the set of transactions in control, low cost shock, and high cost shock market-weeks.
2. Draw a_i from $N(\hat{\mu_a}, \hat{\sigma_a^2})$, which we have assumed is independent across consumers.

[74] If we use the full vector of market-week prices in sample we get similar results.

3. Compute $b_i^{-\frac{1}{\delta}}\eta_{imt}$ to rationalize the chosen quantity-price pair.

4. Repeat until have sampled all transactions from the data.

To construct 95% confidence intervals, we run 1,000 bootstrap iterations where we draw two iterations in each sample. First, we resample (with replacement) a set of market-blocks from the control, low cost shock, and high cost shock markets, to recompute the last 6 sample moments. Then we resample (with replacement) the set of consumers from the demand experiment.

With estimates of individual demand, we can estimate market demand. The key object to estimate is $\mathcal{I}_{mw}$, the number of consumers per market-week.[75] We use our demand estimates to simulate consumers and predict their demand, given the observed market-week price. We draw consumers until in aggregate their predicted demand matches Q_{mw}, the observed quantity transacted in the market-week.

We then estimate $\frac{\partial Q_{mw}}{\partial P_{mw}}$ for each market-week. Given the functional form for demand,

$$\frac{\partial Q_{mw}}{\partial P_{mw}} = \sum_{i\in\mathcal{I}_{mw}} \frac{-1}{\delta(a_i - P_{mw})}\left(\frac{a_i - P^{mw}}{b_i}\right)^{\frac{1}{\delta}}\eta_{imt} = \sum_{i\in\mathcal{I}_{mw}} \frac{-1}{\delta(a_i - P_{mw})} q_{imw} \tag{32}$$

where in the last step we plug in transacted quantities from the data.

The functional form also lets us calculate consumer surplus:

$$CS_{mw} = \sum_{i\in\mathcal{I}_{mw}} \frac{\delta}{1+\delta}(a_i - P_{mw})q_{imw} \tag{33}$$

F.2 Estimating Supply, without Entry

On the supply side, we estimate:

$$P_{mw} - \Delta c_{mw} + \frac{\partial \hat{P}_{mw}}{\partial q_{jmw}} q_{jmw} = -\omega_n \frac{\partial \hat{P}_{mw}}{\partial q_{jmw}} \sum_{k\neq j} q_{kmw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw} \tag{34}$$

with two-stage least squares. The only differences between 27 and 34 is here we plug in estimated inverse demand derivatives and given there is no entry in our main supply model, we specify $\omega(Entry, Contacts) = \omega_n$.

In addition to the sets of trader, market, and week fixed effects, we have 8 excluded instruments:

- an indicator for whether the market is in low cost shock treatment (1 moment)

[75] One option would be to use the maximum number of transactions observed in that same market in a single week during the 12-week experimental period, as we did above in constructing our moments. But because the number of consumers per market-week is often not too big, our estimate may be far from the observed data if, say, consumers in a market-week draw particularly high values of a_i. To generate more precise estimates, we therefore incorporate observed transacted quantities in each market-week.

- an indicator for whether the market is in high cost shock treatment (1 moment)
- the fraction of a trader's markets in each treatment group (low cost shock, high cost shock, entry) (3 moments)
- indicators for whether the trader has a low, medium, or high subsidy to enter *a different* market (3 moments)

Our moment conditions are $E(IV * c_{jmw} = 0|j, m, w)$ for each of the 8 instruments. The first two instruments are orthogonal to cost type, even unconditionally, based on the experimental randomization. The last 6 require conditioning on the trader.

We estimate standard errors and confidence intervals with 1,000 bootstrap iterations where we resample (with replacement) at the market-block level, which was the level of experimental randomization. The sampling is the same as the demand estimates, and we bootstrap demand and supply jointly.

After testing whether $\omega_n = 0$ or 1, and finding that $\omega_n = 1$, we impose $\omega_n = 1$ and re-estimate Equation 34. We use these re-estimated cost parameters when calculating markups and profits and in all subsequent analysis.

F.3 Estimating Supply, with Entry

We then estimate whether entry changes how traders compete. We start by estimating via two-stage least squares a pooled entry effect: ω_e:

(35)

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} + (1 - Entry_{mw}) \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} = -\omega_e Entry_{mw} \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

where we imposed $\omega = 1$ if no entry occurs. We add an extra instrument: whether the market-week is in the entry experiment. We construct our regressors using all of the traders, but we only include non-entrants as observations in estimation.[76]

We then examine heterogeneity in entry effects based on entrants with and without connections to traders in the market. Using two-stage least squares, we estimate:

(36)

$$P_{mw} - \Delta c_{mw} + \frac{\partial P_{mw}}{\partial q_{jmw}} q_{jmw} + (1 - Entry_{mw}) \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} = -\omega_e^{with} EntryWith_{mw} \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} - \omega_e^{without} EntryWithout_{mw} \frac{\partial P_{mw}}{\partial q_{jmw}} \sum_{k \neq j} q_{kmw} + \gamma q_{jw} + c_j + c_m + c_w + c_{jmw}$$

[76]We re-estimate the cost parameters – for use only in testing ω_e – rather than using the estimates from above, as the sample of traders is somewhat different now that entry treatment markets are in the sample.

where "With" indicates entry by a trader with connections and "Without" indicates entry by a trader without connections. We add one more instrument: whether the potential entrant who received the high entry subsidy has connections in the market.

For both models, we estimate standard errors and confidence intervals with 1,000 bootstrap iterations.

F.4 Estimating Entry

Let $MC^0_{jmw} = c_j + c_m + c_w + c_{jmw}$ be trader j's marginal cost intercept in market m in week w. We estimate Equation 29 using method of simulated moments where we draw marginal and fixed costs according to Equation 30 (evaluated at candidate parameter values), calculate variable profits if the potential entrant were to enter, and then use the model to determine whether the potential entrant would actually enter.

Estimating variable profits involves finding a new market equilibrium in quantity choices. For candidate quantity choices, we estimate the market price using estimated inverse demand, and we solve for an equilibrium where all traders choosing positive quantities have their first-order conditions hold. We estimated incumbents' costs above, which allows us to search for a new equilibrium.

Let $Takeup^{Low}$, $Takeup^{Med}$, and $Takeup^{High}$ be the sample entry take-up rates for the potential entrants receiving low, medium, and high subsidies, respectively. Let $EntryMC^{Low}$, $EntryMC^{Med}$, and $EntryMC^{High}$ be the estimated marginal cost intercepts time entry for the potential entrants receiving low, medium, and high subsidies, respectively.[77] Let $PredTakeup^{Low}$, $PredTakeup^{Med}$, $PredTakeup^{High}$, $PredEntryMC^{Low}$, $PredEntryMC^{Med}$, and $PredEntryMC^{High}$ be the model predictions for the same objects.

We specify 6 moments:

- $E(PredTakeup^{Low} - Takeup^{Low}) = 0$
- $E(PredTakeup^{Med} - Takeup^{Med}) = 0$
- $E(PredTakeup^{High} - Takeup^{High}) = 0$
- $E(PredEntryMC^{Low} - EntryMC^{Low}) = 0$
- $E(PredEntryMC^{Med} - EntryMC^{Med}) = 0$
- $E(PredEntryMC^{High} - EntryMC^{High}) = 0$

We estimate via method of simulated moments, where we estimate in two steps to use an estimated optimal weighting matrix. To ease the computational burden of needing to solve for a new equilibrium for every market-week for every set of candidate parameters, we use importance sampling for simulating the marginal cost distribution.[78] We use $\mu = 3$ and

[77] As we estimate marginal cost intercepts above, we can only estimate them for actual entrants. Thus, by interacting with entry, we only need estimates for entrants' marginal costs.

[78] Fixed cost draws do not alter the quantity-setting equilibrium, conditional on entry, so we simulate in two steps. First we simulate marginal costs using importance sampling. Then we simulate from the fixed cost distribution, conditional on each marginal cost draw.

$\sigma = 1$ as parameters for the importance sampling distribution. We simulate 25 marginal cost draws per potential entrant-market-week. For certain starting values we run into estimates of a degenerate distribution, a common problem in importance sampling that introduces considerable simulation error (Ackerberg, 2009). We impose a lower bound on the parameters of 0.25 and avoid degeneracy.[79]

We estimate standard errors with 1,000 bootstrap iterations.

F.5 Counterfactuals

In counterfactuals, we alter the form of competition, and recompute equilibria in quantity choices in each market-week. For candidate quantity choices, we estimate the market price using estimated inverse demand, and we solve for an equilibrium where all traders choosing positive quantities have their first-order conditions hold. In the counterfactual with exit, if a trader is making negative total profits, we remove him from the market and compute a new equilibrium. We iterate until no remaining trader makes negative total profits. If multiple traders make negative total profits in a given equilibrium, we remove the trader making the largest losses and recompute the equilibrium.

[79] Results are similar for other lower bounds.

G Appendix: Heterogeneity in Entry Effects

This appendix explores pre-specified dimensions of heterogeneity in the effect of entry on competition. Specifically, we pre-specified three dimension of heterogeneity: whether the entrant has contacts in the market, whether the entrant is large (above median profits), and whether the entrant's ethnicity matches that of the majority of traders in the market.

We already explored how the form of competition varies with whether the entrant has contacts in the market (Table 6). Here we report the results from the other two pre-specified sources of heterogeneity: entrant size and entrant ethnicity. We see that our estimates to too imprecise to make strong conclusions on heterogeneity based on either dimension.

Table G.1: **Effect of Entry on Competition.** The tables shows separate profit weights depending on (1) whether the entrant has above or below median profits and (2) whether the entrant's ethnicity corresponds to the majority ethnicity among traders in the market. The second and third columns show the bounds of the 95% confidence interval, calculated with 1,000 bootstrap iterations.

Group	Parameter Estimate	95% CI LB	95% CI UB
Heterogeneous by profits			
ω_e^{above} Entrants with above median profits	0.52	-0.28	1.38
ω_e^{below} Entrants with below median profits	0.98	0.20	1.68
Heterogeneous by ethnicity			
ω_e^{maj} Entrants in ethnic majority	0.58	-0.09	1.23
ω_e^{min} Entrants in ethnic minority	1.29	-3.81	12.66

H Appendix: Quantity Effects in Cost Shock Experiment

We have explored several candidate explanations of the large demand response to the cost shock experiment. First, we consider the question of data quality. One concern might be that if busy markets led enumerators to potentially miss some transactions, and subsidized traders had an incentive to have their transactions recorded, this could generate a large extensive margin elasticity. We believe our data is robust to this concern. Enumerators were stationed at the trader's location of sale, visually monitoring each transaction. While we cannot rule out that enumerators may have missed some transactions during busy market days, we intentionally allocated more enumerators to markets that were particularly busy, with the explicit aim of minimizing this.

A second possibility is storage, which might mean that *market* residual demand is substantially more elastic than *consumption* demand. However, as addressed in Appendix C.1, there is limited evidence of meaningful storage in our setting.

A third possibility is that consumers are substituting across markets. While our survey indicates that most consumers are "captured" by the local market, it is possible that some consumers substitute from nearby markets outside our sample (control markets in our study were intentionally spread out sufficiently to avoid spillover concerns) and that this contributes to the increase in quantities observed in treatment markets. We find somewhat greater evidence in support of this explanation. We take the census of all markets in the six counties in which our study was run (this is the sample frame from which we initially randomly sampled markets for inclusion in our experiment). We identify, for each market in our sample, the number of "potential substitute" markets by counting the number of other markets occurring on the same day of the week within a variety of radii surrounding the market. We then test whether the increase in quantities observed in the experiment is concentrated in markets with a greater number of neighboring markets, as one would expect if substitution were at play. We also look specifically at the effect of having large neighbor markets, as these markets may contain a larger number of consumers who may switch into the study market and prompt a large quantity response.

Table H.1: **Quantity Effects by Number of Neighbors.** "Kgs" is the total kg sold in any week for a given market. "Cost Change" ("CC") is the change in cost (Ksh/kg) from treatment: -2.22 in the low cost shock treatment and -4.44 in the high cost treatment. "Num Neigh Xkm" is the number of markets within X km that have the same market-day and "Num Large Neigh Xkm" is the number of such markets that are large according to the market census.

	(1) Kgs	(2) Kgs	(3) Kgs	(4) Kgs	(5) Kgs	(6) Kgs	(7) Kgs
Cost Change	-846.8 (160.5)	-615.2 (187.8)	-681.8 (127.1)	-670.4 (139.6)	-676.3 (120.2)	-665.0 (131.0)	-663.2 (119.2)
CC x Num Neigh 10km		-349.6 (301.0)					
CC x Num Large Neigh 10km			-1143.6 (453.6)				
CC x Num Neigh 5km				-953.6 (552.6)			
CC x Num Large Neigh 5km					-2527.0 (557.5)		
CC x Num Neigh 3km						-1687.8 (865.3)	
CC x Num Large Neigh 3km							-3850.6 (132.4)
Mean Dep Var	4627.9	4627.9	4627.9	4627.9	4627.9	4627.9	4627.9
N	474	474	474	474	474	474	474
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Results indicate that the quantity response seems to be related to the number of potential substitute neighboring markets, and that this is driven mostly by having *large* neighbors. We also see that the coefficients increase in magnitude as we focus on geographically closer neighbors. We see very similar results when we look at the number of transactions:

Table H.2: **Transactions Effects by Market's Neighbors.** "Trans" is the total number of transactions in any week for a given market. "Cost Change" ("CC") is the change in cost (Ksh/kg) from treatment: -2.22 in the low cost shock treatment and -4.44 in the high cost treatment. "Num Neigh Xkm" is the number of markets within X km that have the same market-day and "Num Large Neigh Xkm" is the number of such markets that are large according to the market census.

	(1) Trans	(2) Trans	(3) Trans	(4) Trans	(5) Trans	(6) Trans	(7) Trans
Cost Change	-10.80	-11.50	-9.907	-10.16	-9.953	-10.32	-10.13
	(1.838)	(2.571)	(1.949)	(2.132)	(1.877)	(1.998)	(1.885)
CC x Num Neigh 10km		1.051					
		(2.162)					
CC x Num Large Neigh 10km			-6.194				
			(3.256)				
CC x Num Neigh 5km				-3.473			
				(4.168)			
CC x Num Large Neigh 5km					-12.57		
					(3.994)		
CC x Num Neigh 3km						-4.465	
						(4.806)	
CC x Num Large Neigh 3km							-14.16
							(6.485)
Mean Dep Var	58.73	58.73	58.73	58.73	58.73	58.73	58.73
N	474	474	474	474	474	474	474
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

In terms of magnitude, how much of the total quantity response is explained by this effect? We do a simple back-of-the-envelope calculation based on these regression results. Taking the 5-km radius results (a reasonable travel distance in this setting), we see in Table H.1 Col. 5 that markets with no neighbors see a 676 kg increase in quantity transacted per one unit change in the cost reduction, and this effect increases by 2527 kg for each same-day large neighbor. The mean market has 0.068 same-day large neighbors within 5 km. Thus, we can calculate the total treatment effect as $-676 - 2527 * 0.068 = -848$ and the same-day large neighbor component as $-2527 * 0.068 = -172$, or 20% of the total treatment effect.[80]. Of course, this specification by no means captures all forms of substitution, so we view these back-of-the-envelope calculations as merely suggestive that substitution from large, same-day neighboring markets explains a relevant portion of the demand elasticity, but not necessarily

[80]If we focus on the all neighboring markets specification in Col. 4, we see again that the effects are primarily driven by the large markets. The mean market has 0.169 same-day neighbors (of any size) within 5 km. If we multiply the regression coefficient in Col. 4 by the mean number of neighbors within 5 km, we get -161; if we multiply the regression coefficient in Col. 5 by the mean number of large neighbors within 5 km, we get -172. Thus, the effect of seems driven entirely by large neighbors.

all of it.

To the extent that this substitution explains at least some of the large demand response, our inference on the form of conduct appears robust. The market residual demand elasticity is what we need to identify the degree of competition in the market. We are estimating traders' optimal response to cost shocks in their market, and this is dictated by the market residual demand curve they face. As long as the estimated demand curve reflects the change in a trader's quantity transacted from lowering price, regardless of where these consumers are coming from, we can model traders' incentives and thus use their choices to infer the degree of competition.

However, cross-market substitution could potentially create spillovers that contaminate the control group and bias our estimates of pass-through. We think this is unlikely for several reasons. First, our study sample includes only a random 60 markets from among the full census of 225 markets in the study counties, and therefore most of the markets from which additional customers are drawn are markets that are not in our study, rather than control markets. Moreover, study markets were intentionally chosen to be spread out from each other to mitigate this issue. Indeed, just 22% of neighboring markets (within a 5km radius) are in the study sample. This is particularly the case for large markets, from which the above evidence suggests most of these substituting customers are drawn. Of the 38 large markets in our study area, only two are within a 5km radius of any other same-day study market. Second, we find that while the quantity effects depend in part on neighboring same-day markets, there is minimal evidence that pass-through rates do (see Table H.3). [81]

[81] We can also directly control for any potential spillover effects in a Miguel-Kremer (2004) style specification, and we continue to find pass-through rates of around 22%.

Table H.3: **Price Effects by Market's Neighbors.** "Cost Change" ("CC") is the change in cost (Ksh/kg) from treatment: -2.22 in the low cost shock treatment and -4.44 in the high cost treatment. "Num Neigh Xkm" is the number of markets within X km that have the same market-day and "Num Large Neigh Xkm" is the number of such markets that are large according to the market census.

	(1) Price	(2) Price	(3) Price	(4) Price	(5) Price	(6) Price	(7) Price
Cost Change	0.224 (0.0434)	0.225 (0.0499)	0.210 (0.0468)	0.221 (0.0511)	0.220 (0.0451)	0.224 (0.0481)	0.229 (0.0459)
CC x Num Neigh 10km		-0.00167 (0.0362)					
CC x Num Large Neigh 10km			0.0910 (0.0627)				
CC x Num Neigh 5km				0.0128 (0.0633)			
CC x Num Large Neigh 5km					0.0508 (0.0931)		
CC x Num Neigh 3km						-0.00157 (0.0687)	
CC x Num Large Neigh 3km							-0.0954 (0.0526)
Mean Dep Var	28.92	28.92	28.92	28.92	28.92	28.92	28.92
N	1860	1860	1860	1860	1860	1860	1860
Market FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Third, as a robustness check, we verify that these large markets with same-day neighbors, which might be affected by neighboring markets' treatment status, do not affect our conclusions about the form of competition by dropping them from our analysis. In the simple model, the cost experiment only affects the inference about competition through the pass-through rate. In Table H.4 we see that the estimated pass-through rate is nearly identical when we use the full sample of markets, drop all large markets, or drop the two large markets that are within the 5km radius of any other study market with a common market-day ("donors"). We conduct a similar exercise for the general model. Recall that using the full sample, we have a point estimate on ω, the profit weight, of 1.07. If we drop the two large markets that are within the 5km radius of any other same-day study markets, we estimate a profit weight of 1.01. Our conclusions are therefore robust to restricting our sample to either markets that are unlikely to draw consumers away from other markets (Table H.3)) or to markets that are unlikely to lose consumers to other markets.

Table H.4: **Price Effects by Market's Same-Day Neighbors and Market Type.** "Cost Change" ("CC") is the change in cost (Ksh/kg) from treatment: -2.22 in the low cost shock treatment and -4.44 in the high cost treatment. The "No Large" sample excludes markets that are large according to the market census. "No Donors" excludes the two large markets that have a same market-day neighbor within 5km.

	(1) Price	(2) Price	(3) Price
Cost Change	0.224	0.231	0.232
	(0.0434)	(0.0517)	(0.0465)
Mean Dep Var	28.92	29.10	29.00
N	1860	1255	1743
Market FE	Yes	Yes	Yes
Week FE	Yes	Yes	Yes
Sample	All	No Large	No Donors

I Appendix: Entry Effects in Cost Shock Experiment

In using the cost shock experiment to infer how traders compete, a maintained assumption is that the set of traders does not endogenously change in response to the cost shock. However, it is possible that the shock introduces entry of new traders to take advantage of the subsidy. We investigate this possibility by estimating the effect of the cost shock on the number of entrants to the market:

Table I.1: **Entry Effects.** "Cost Reduction Market" is a dummy for treatment status in the cost-reduction experiment. "Number Entrants" is the number of traders present in the market on that day who had never worked in that market before.

	(1) Number Entrants
Cost Reduction Market	0.0338 (0.0175)
Mean Dep Var	0.0484
N	1860
Market FE	Yes
Week FE	Yes

We are able to classify entrants, even in the cost shock experiment, because the first time we encountered a trader during our study period, our survey asked the him about his past experience working in that market. We find a very small – albeit marginally significant – effect on the number of entrants. Note that – to the extent that we do see entry in the cost-shock experiment – this will mean we are less likely to infer collusion. To see this, imagine two scenarios. First, suppose that entry occurs but that conduct is unchanged. Having more traders in the market would weakly lower prices. We would thus expect the experimental pass-through estimate to be larger in magnitude than the estimate we would get were there no entry. This would bias us away from collusion. Second, suppose that entry occurs and that it changes conduct. This would likely be a change toward more competition, which would be a second reason for prices to fall. Again, the experiment pass-through estimate would be larger in magnitude than the estimate we would get were there no entry. As a final robustness check, we re-estimate our general model, but drop the traders that entered. This re-defines the moment condition to be that the cost shock is orthogonal to incumbents' cost residuals, and avoids any potential endogeneity of particularly low- or high-cost traders endogenously entering and causing our original moment condition to be violated. When we estimate this model, we get an estimate of ω of 1.09 (vs. 1.07 in the baseline specification). Thus, we do not believe that this small entry effect changes our conclusions.

J Appendix: Additional Details on the Experimental Design

This appendix provides additional detail on the experimental design of the three experiments.

J.1 Experiment 1: Trader Cost Shock Experiment

When introducing the subsidy, enumerators first asked the trader to describe some of the major costs that he faced in his business (traders in control market days were also asked these questions, to avoid confounding treatment with any priming effects). The subsidy was then framed as a reduction of these costs. At no point were traders told that the purpose of the subsidy was to see how much would be passed on to the prices they set for customers; rather they were told the research was interested generally in how "reductions in cost affect your business." To reduce the chance that traders viewed the subsidy as a gift, we explicitly stated the following in the script that described the subsidy: "We would wish to offer [X] Ksh for every bag you will be able to sell in this market today. This amount of [X] Ksh will offset the costs you incurred to get these bags to the market today. Remember, [X] Ksh is directed towards cutting costs and is NOT a personal gift or a promotion."

To ensure trader comprehension, enumerators then guided traders through a check for understanding. This included asking the trader if he understood the rules (99.8% of traders reported they did). To confirm this understanding, the enumerator then asked the trader to explain back to enumerators, in his own words, the meaning of the cost-reduction subsidy and the rules by which it operated. Using this method, enumerators confirmed that 99.6% of traders understood the rules (the most crucial of which were the size of the subsidy and the fact that it was tied to the number of bags sold). A similar procedure was used to ensure that traders understood the duration of the treatment, with traders describing back to enumerators in their own words how long the subsidy would run. 96.8% of traders were reported to have understood the duration of the intervention "well," 3.0% "somewhat well," and only 0.2% "not well." Finally, and most importantly, payments were set-up in a two-step procedure each day, with the explicit goal of building trader trust. Traders received the first payment early in the day (following the first hour of sales) and the second payment at the end of the day, following the completion of the day's sales. Payments were sent to traders' phone via M-Pesa (mobile money), so they received them in real time. This was designed (1) to build trader trust that they would, in fact, receive payment and (2) to ensure that traders experienced how the amount of the subsidy was calculated as a function of the number of bags sold, which is the key feature that traders must understand to align their incentives with the theory tested in the experiment. These features ensured that traders understood and trusted the structure of payments and their implied incentives for pricing.

We did not inform consumers about the subsidy. Rather, we left it to traders to determine how much information to share with consumers, as they would naturally.

We took several steps to prevent fraudulent sales. Most importantly, enumerators were stationed with each trader, monitoring each transaction, and were therefore able to observe cash and maize exchange hands. A random subset of customers was selected for additional monitoring, including questions on customer identity, the purpose of the purchase, etc. Managers stationed in the market were required to be present for this interview and approve any transactions above a certain size as subsidy-eligible.

The subsidy ran for four weeks. Exploratory interviews conducted with traders prior to the implementation of the experiment suggested that this matched well the duration at which traders face naturally occurring cost shocks, such that both traders and consumers would find this duration of shock to be commonplace and would respond naturally. Traders were specifically asked whether consumers would react badly if they lowered their prices during the subsidy and raised them in the future, following the removal of the subsidy. Traders stated this was not a major concern, as the duration of the shock matched other cost shocks during the season, such that would not find any resulting price shock to be unusual.

J.2 Experiment 2: Demand Experiment

In the demand experiment, customers were first allowed to approach traders and negotiate a price and quantity in a natural way before being approached by an enumerator to invite them to the demand experiment. If the customer consented, a random discount amount was drawn (using a randomization feature within SurveyCTO) and the customer was told that the price he had previously received from the trader would be reduced by that amount. The customer was then invited to select a new quantity he would like to purchase in light of this new price. Consumers were permitted to return home to collect any additional cash required to make their desired purchase, if needed. The price discount was given to the customer in the form of a mobile money or a cash transfer, and the customer paid the trader the originally negotiated price.

Table J.1 presents balance by baseline price and quantity demanded, by subsidy level. The p-value from an overall F-test is presented at the bottom of the table and demonstrates that we cannot reject that all coefficients are zero.

Table J.1: **Demand Balance.** Pre-subsidy pice is presented in Column 1, while pre-subsidy quantity is presented in Column 2. The p-value from an overall F-test is presented at the bottom.

	Price	Quantity
Subsidy Level 2	0.23	-27.58
	(0.53)	(17.36)
Subsidy Level 3	-1.16	0.57
	(0.54)	(17.68)
Subsidy Level 4	-0.32	-25.61
	(0.55)	(17.89)
Subsidy Level 5	-0.45	-19.92
	(0.55)	(17.89)
Subsidy Level 6	-0.30	-13.23
	(0.53)	(17.39)
Subsidy Level 7	-0.63	-3.30
	(0.53)	(17.28)
Subsidy Level 8	-0.90	-21.31
	(0.55)	(17.93)
Subsidy Level 9	-0.70	-0.00
	(0.52)	(17.08)
Subsidy Level 10	-0.57	-10.65
	(0.55)	(17.93)
Mean Dep Var	32.06	78.04
N	1361	1361
F-Test	.31	.62

Several checks were put into place to prevent consumers from making multiple visits until they received a larger subsidy. First, enumerators were stationed in the market for the full day and were trained to identify such returning customers. Second, prior to revealing the discount amount, enumerators recorded the name and phone number of the consumer, which could be used to check for previous subsidy assignment. Consumers would therefore have to give false names and phone numbers if they were to revisit with the goal of receiving a larger subsidy. Because the subsidy was delivered via mobile money to the phone number listed, this discouraged reporting of false phone numbers.

J.3 Experiment 3: Entry Experiment

In the entry experiment, traders who had never before worked in the treated market were offered subsidies to enter and attempt to sell there. The pool of traders eligible to receive the entry offers was drawn from the sample of traders interviewed in pilot work (traders from markets in the same region in Kenya) and the universe of all traders found during the market census activity. Small traders who did not own or regularly rent trucks were then excluded from the pool as pilot work showed that these traders categorically did not take up the offer. A phone survey was conducted with the remaining 187 traders to determine markets in which they had ever worked. For each of the 60 sample markets, we then identified the set of eligible traders who (1) had never before worked in that market and (2) did not work in other study markets that occur on the same day of the week in order to avoid inducing exit in our sample. The median market had 37 eligible traders, the minimum had 28, and the maximum had 56. From each of these sets, we then randomly selected the three traders who would receive the entry offers.

Because we did not want to overwhelm a single trader with too many offers, we only offered each trader one offer per 4-week block. Because this has cascading effects for the set of eligible traders for each market, we randomize the order in which markets were assigned traders from the remaining pool. In the first block, a few traders asked to be removed from the study (due to lack of interest in the subsidy and therefore unwillingness to answer surveys). When these traders were scheduled to receive an offer in a subsequent block, they were then replaced and the offer was given to a new, unassigned trader from the same pool.