Policies aimed at raising agricultural productivity have been a centerpiece in the fight against global poverty. Their impacts are often measured using field or quasi-experiments that provide strong causal identification, but may be too small-scale to capture the general equilibrium (GE) effects that emerge once the policy is scaled up to a broader segment of the population. We propose a new approach for quantifying large-scale GE policy counterfactuals that can both complement and be informed by evidence from field and quasi-experiments in agricultural settings. To this end, we develop a quantitative model of farm production, consumption and trading that captures important features of this setting, and propose a new solution method that relies on rich but widely available microdata. We showcase our approach in the context of a subsidy for modern inputs in Uganda, using administrative data for model calibration and variation from field and quasi-experiments for parameter estimation. We find that both the average and distributional impacts of the subsidy differ meaningfully when comparing a local intervention to one at scale, even for the same sample of farmers, and quantify the underlying mechanisms. We further document new insights on how the sign and extent of GE forces differ as a function of saturation rates at different geographical scales, and on the importance of capturing a granular economic geography for counterfactual analysis. Finally, we discuss practical considerations for combining our toolkit with evidence from field and quasi-experiments.
1 Introduction

Roughly two thirds of the world’s population living below the poverty line work in agriculture (Castaneda et al., 2016). In this context, interventions aimed at improving agricultural productivity, such as programs providing access, information, training or subsidies for modern production techniques and inputs, have played a prominent role in the global fight against poverty.\(^1\) To inform these policies using rigorous evidence, much of the recent literature studies local interventions with variation in policy exposure across households or local markets generated by randomized control trials (RCTs) or natural experiments. While rightly credited for revolutionizing the field of development economics, experiments and quasi-experiments often face the well-known limitation that their estimates may not speak to the broader general equilibrium (GE) effects that emerge once policies are scaled up to a larger segment of the population. At the same time, an earlier literature in agriculture and development, employing computable general equilibrium (CGE) analysis to quantify GE implications, often relies on less well-identified moments for parameter estimation and largely abstracts from modeling the granular economic geography of farm-level production, consumption and trade costs that underlies the propagation of shocks and their incidence in GE.\(^2\)

To make progress on these challenges, we propose a new methodology for quantifying large-scale policy counterfactuals that can both complement evidence from local interventions and be informed by it. In doing so, we contribute to two recent strands of the literature. First, similar to recent work by e.g. Buera, Kaboski, and Shin (2017), Lagakos, Mobarak, and Waugh (2021), and Gollin, Hansen, and Wingender (2021), our analysis combines a structural model with evidence from RCTs or quasi-experiments to quantify GE counterfactuals that are frequently outside the scope of reduced-form estimation. Second, we build on recent methodological contributions in international trade and economic geography (e.g. Sotelo (2020), Fajgelbaum and Redding (2018), Costinot and Donaldson (2016)) and develop a quantitative model of farm-level production and trading that captures several important features that we document in this setting. These include additive trade costs, non-homothetic preferences, technology choice in crop production and homogeneous agricultural goods, where trade flows are not assumed ex ante positive between all origin-destination pairs.

After laying out the model, we propose a new solution method for counterfactuals in this environment that relies on rich but widely available microdata on household location, production and consumption across the economy. We first show that we can use information on trade costs between and within markets in combination with data on household-level expenditure shares and agricultural production quantities to set up a price discovery problem. This entails solving for equilibrium farm-gate prices and trade flows that rationalize the observed consumption and

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\(^{1}\)See e.g. Caldwell et al. (2019) for a review of recent impact evaluations in this space.

\(^{2}\)See e.g. de Janvry and Sadoulet (1995) for a review. This literature has also been referred to as “multi-market” analysis, as the impact of shocks is traced across multiple output and factor markets in the economy. See further discussion of related literature below.
production decisions given a graph of trade costs connecting households and markets. In turn, with knowledge of farm-gate prices and trade costs, we can express farm-level excess demand functions in terms of counterfactual prices and changes in farm productivities (along with expenditure shares and production in the original equilibrium). We then use these excess demand functions and the no-arbitrage conditions to form a system of equations that we can solve for the counterfactual equilibrium.

This approach has several advantages. First, we are able to solve the model without imposing structural gravity (Head and Mayer, 2014) and without introducing stark new data requirements (such as observing the full set of pre-existing market prices). Second, our solution method ensures that the economy is in equilibrium before solving for counterfactuals: the household prices we obtain from the price discovery are by construction consistent with the calibrated trade costs and the consumption and production decisions we observe in the data. Finally, from a computational perspective, our solution method is capable of handling high-dimensional GE counterfactuals at the level of individual households who populate the macroeconomy. This allows us to match the unit of observation often used in experiments (individual households), as well as to speak to distributional effects at this granular level.

We showcase our approach by evaluating the local vs. at-scale implications of a subsidy for modern inputs (chemical fertilizers and hybrid seed varieties) in Uganda. Drawing on the strengths of experiments for identification, we estimate the model’s key demand and supply elasticities using exogenous variation in consumer and producer prices from existing RCTs (Bergquist and Dinerstein, 2020, Carter et al., 2020). On the supply side, we also make use of a natural experiment that exploits changes in crops’ world market prices that propagate differently to local markets as a function of (additive) trade costs to the nearest border crossing. To calibrate cross-market trade costs, we make use of estimates from Bergquist et al. (2022), using market and trader survey microdata to provide information on market-to-market trade flows and crop prices at origin and destination across crops. To calibrate within-market trade costs between farmers and their local markets, we use observed gaps in the Ugandan National Panel Survey (UNPS) between farm-gate prices and local markets in combination with knowledge of farmer-level trade flows to and from the markets. Finally, we use Ugandan administrative data on household location, production and consumption to calibrate the model to the roughly 4.5 million households who populate the country.

We then use the calibrated model to conduct counterfactual analyses. We first study how the average and distributional effects of agricultural policies differ between a local intervention and one implemented at scale. We run two types of counterfactuals for each of the roughly 4,500 rural parishes in Uganda. In each parish, we randomly select 2.5 percent of the local population (a sample of roughly 100,000 households nationwide, or 25 per parish). We first solve for counterfactual changes in household welfare due to an intervention that targets a 75% cost subsidy for

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3For example, Sotelo (2020) uses province-level crop unit values from agricultural surveys to calibrate and solve the model, but these price data are not, in general, model-consistent given calibrated trade costs.
modern inputs only at each of these local treatment groups, keeping the rest of Uganda unexposed (akin to implementing roughly 4,500 separate RCTs). We then compare these local effects to the welfare changes experienced by the same sample of households under an intervention that scales the subsidy policy to all rural households in Uganda.

Pooling all local randomized interventions, we find that the average effect of the subsidy at small scale is a 4.4 percentage point increase in household real income. This is driven almost entirely by farmers saving on costs for the subsidized inputs, while output and other input prices remain largely unaffected. However, at scale we find that the welfare effect – for the same sample of farmers receiving the same intervention as in the local experiment – changes by as much as + or -5 percentage points. This is large relative to the local treatment effect: more than 80 percent of households experience a change greater than +/-10 percent of their local effect, with over a third experiencing a greater than 50 percent change. On average, the at-scale intervention produces a smaller welfare effect by about 20 percent (a 3.6 percentage point gain vs. a 4.4 gain in the local intervention). However, not all households are worse off at scale: about 20 percent of all the 100k sample households experience at-scale effects that exceed their gains from the local intervention.

The distributional implications underlying these differences turn out to be key. The local intervention is highly regressive: land-rich farmers experience an 8 percentage point gain, while land-poor farmers experience only a 2.5 percentage point gain. In contrast, we find that the scaled-up intervention is significantly less regressive, as land-poor farmers do better at scale (experiencing a 4 percentage point gain) while the gains of land-rich farmers drop from 8 to 6 percentage points. We investigate the mechanisms underlying these differences between the local and at-scale interventions, exploring effects on both nominal incomes and the consumer price index. On the income side, because land-rich households use modern inputs more intensively before the intervention, the income gains from the local subsidy (where output and factor prices are mostly unaffected) are concentrated among this group. However, at scale, we find that land-poor households’ incomes increase by about 1.5 percentage points relative to the local intervention, whereas the effect is 2 percentage points in the opposite direction for land-rich households. This asymmetry is driven by GE effects that decrease the price of modern input-intensive crops and increase the price of local labor. The resulting reduction in land revenues and increase in labor compensation benefit households with higher initial reliance on wage labor relative to land-rich households. The impact of price index changes are more muted. We find that food prices decline and manufacturing prices increase on average. While land-poor households spend a larger share of their expenditure on food overall (and thus have the potential to benefit differentially from the decline in food prices), in practice, we find that prices drop the most for crops that land-rich households spend more on compared to the poor within food consumption. The price index implications are therefore small, both on average and distributionally, relative to the role played by nominal income changes. Finally, we document that differences at scale are most pronounced among crops and farmers with higher initial usage of
modern inputs (where both partial equilibrium gains and GE effects on local prices are higher on average) and among more remote regions, where local market prices are less pinned down by trade with border crossings or nearby cities.

We also use our framework to provide insights relevant for experimental approaches to estimating GE effects. A growing literature employs “randomized saturation designs”, which randomize not only treatment across individuals, but also the saturation rate (share of individuals treated) across geographic areas (“clusters”), to elicit GE effects with experimental variation (e.g. Baird et al. (2011), Burke et al. (2019)), Egger et al. (2022)). Due to constraints on statistical power and feasibility of implementation, such designs often limit the comparison to two discrete levels of saturation, implemented within clusters (typically villages or groups of villages). In order to identify the impact of policies at scale (e.g. at 100% national saturation), one must thus typically extrapolate from these two points of saturation, subject to two important assumptions: i) that GE forces are both monotonic and roughly linear with respect to changes in the saturation rate; and ii) that the GE forces experienced at the level of local clusters are representative of the effects of saturation at a broader geographical scale (e.g. nationwide). We provide evidence on the plausibility of these two assumptions by exploring how welfare implications evolve as a function of saturation rates at different geographical scales.

We find both reassuring and more cautionary evidence. On the positive side, the results point to GE effects that are monotonic and roughly linear as a function of the nationwide saturation rate. We test this by starting with the local intervention that treats 2.5% of farmers in each parish. We then estimate how that original sample of roughly 100,000 treated farmers fare when the program is also sequentially scaled up in steps of 10% of the remaining rural Ugandan population (randomly chosen), up to 100% saturation. We find that the average gains to the initially treated farmers in the local interventions decline close to linearly as a function of scale-up to the rest of the country. This evidence provides some reassurance about the lessons that can be drawn from designs relying on just two discrete saturation rates.

Our results also suggest some caution about these designs. Because it is nearly impossible to randomize nationwide saturation rates, experiments typically randomize saturation rates at some lower, sub-national level. We find that the geographical scale of saturation meaningfully changes conclusions about both the average and distributional effects of the policy. In our setting, we find that increases in saturation at the national level decrease the average rural welfare gain; however, when we implement the same counterfactuals in steps of 10% of the population, but instead within subcounties (a large but feasible unit for randomization saturation), we find no change in average welfare gains even at 100% saturation within the subcounty. Underlying this, we find that land-poor farmers gain significantly more as a function of sublocation vs. national saturation, while land-rich farmers lose less. These findings suggest caution when extrapolating from GE effects observed in designs that randomize saturation within smaller geo-

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4Uganda is made up of roughly 800 subcounties. We thus (conservatively) choose regions on the larger side of common definitions of “clusters”.

graphic units to the effects that would be observed at a broader scale of rollout, both on average and related to distributional effects.

We conduct two additional exercises to investigate the role of the granular economic geography embraced by our model, comparing our results to those using more standard existing approaches in the literature. In a first comparison, we evaluate the welfare impact at scale in a setting without trading frictions – as if all households were selling into one integrated domestic market. Modeling a single market has been standard in an earlier literature using CGE models, as well as in a more recent literature in macroeconomics on quantifying the aggregation of local shocks if they were to occur to all agents in the economy (e.g. Buera et al. (2017), Sraer and Thesmar (2018), Fujimoto et al. (2019)). In a second comparison, we allow for trade costs, but instead consider the common workhorse structure of quantitative trade models (e.g., Costinot and Rodriguez-Clare (2014) and Baqae and Farhi (2019)), featuring iceberg (ad valorem) trade costs and structural gravity with differentiated varieties – implying that all origin-destination market pairs engage in bilateral trade flows unless the costs of doing so are prohibitive (and thus are unaffected by policy changes).\(^5\) In both cases, we find meaningful differences in the average and distributional implications of the subsidy at scale compared to our framework, and discuss the mechanisms that are missed when imposing coarser assumptions about the economic geography.

We also explore the sensitivity of our main results across different modeling assumptions. We explore sensitivity across parameter ranges that deviate from our preferred estimates on both the supply and demand sides. While results do not vary strongly across alternative demand-side elasticities in our application, the magnitude of the GE adjustments are sensitive to the estimated supply elasticities. This highlights the important role that RCTs and well-identified natural experiments can play in cleanly identifying key model parameters in a given policy environment.

We conclude by briefly discussing a set of practical considerations for combining our toolkit with evidence based on fieldwork or quasi-experiments. The remainder of the paper is structured as follows. Section 2 develops the model and solution method. Section 3 describes the Ugandan data. Section 4 presents the calibration and parameter estimation. Section 5 presents the counterfactual analysis. Section 6 provides a brief discussion of practical considerations when combining our toolkit with evidence based on fieldwork or quasi-experiments. Section 7 concludes.

\(^5\)Another important difference is in the question studied: whereas standard quantitative trade models aim to measure the aggregate welfare effect of a shock, we are interested in linking the quantitative analysis to the outcomes typically studied in impact evaluations: the average and distributional effects among individual farmers or households. Given our model economy is efficient and we ignore the cost of the subsidy in our application, the aggregate effect –to a first order approximation– would equal the savings on import costs for modern intermediates.
2 Model and Solution Method

We develop a rich but tractable GE model that is able to capture the granular economic geography of household location, production, consumption and trading that one can observe in administrative microdata, as well as a number of stylized facts that we document in the Ugandan data (see Appendix 1). These features deviate from the workhorse structure of quantitative trade and economic geography models: i) the vast majority of local markets do not trade with one another, pointing to a limited degree of product differentiation within crops; ii) preferences are non-homothetic, with falling expenditure shares on food consumption as incomes rise; iii) trade costs from farmers to local markets and between local markets appear to be additive (charged per unit of weight) rather than ad valorem; and iv) the adoption of modern inputs, such as chemical fertilizer or hybrid seeds, changes the relative cost shares of traditional inputs, land and labor.

In line with these facts, our model features heterogeneous producers and consumers who interact across a realistic geography. The economy is populated by farmers who are endowed with land of heterogeneous suitability for different crops that are homogeneous. Farmers trade both labor and crops in their nearest local market. These local markets are connected with all other markets and the rest of the world by a graph based on existing transport infrastructure. Our model allows trade costs between farmers and markets and between markets to have both an additive and an ad valorem (iceberg) component. Farmers are also allowed to choose between different production technologies, where the adoption of modern inputs may affect the production function with respect to traditional inputs. Preferences are non-homothetic, such that GE price changes in agriculture can affect initially richer or poorer households asymmetrically through the price index.

Environment

We lay out the theory in general notation and will then specify particular functional forms (under “Parametrization” below) on the demand and supply sides that we use in our application. There are two kinds of agents, farmers (indexed by $i$) and urban households (indexed by $h$), and two kinds of markets, villages (indexed by $v$) and urban centers indexed by $u$). There is also an agent that we call Foreign (denoted by $F$) which stands for the rest of the world. In general, each of these nodes (agents or markets) in the economy is indexed by $o$ (origin) or $d$ (destination) when dealing with the trade network, and with $j$ (agents, who may be households $i$ or $h$ or Foreign $F$) or $m$ (market) when dealing with agent behavior or market clearing conditions, respectively.

These nodes trade in outputs ($k \in K$) and inputs ($n \in N$). There are two kinds of outputs, agricultural goods ($k \in K_A$) and a manufacturing good ($k = M$), and two kinds of tradable inputs, intermediate goods ($n \in N_I$) and labor ($n = L$). We use $g$ as a generic index that encompasses both outputs and inputs, hence $g \in \mathcal{G} = K \cup N$.

Farmers own land and labor in quantities $Z_i$ and $L_i$, and they produce agricultural goods (or
crops) using their own land (i.e., land is not tradable) as well as labor and intermediate goods.\footnote{We model land as not tradable in line with empirical evidence showing that land markets in Africa are generally thin, with sparse rental markets and in some cases “almost non-existent” transactions (Acampora et al., 2022) (see also e.g. Deininger et al. (2008); Holden et al. (2010)).}

Urban households own labor in quantity $L_h$ and produce the manufacturing good using labor. Intermediate goods are imported from Foreign.

Let $O_g(d)$ denote the set of origin nodes from where node $d$ (which could be an agent or a market) can buy good $g$ and $D_g(o)$ denote the set of destination nodes which can buy good $g$ from node $o$. Trade in good $g$ from $o$ to $d$ only happens if $o$ and $d$ are directly connected on the graph, i.e., $o \in O_g(d)$ or, equivalently, $d \in D_g(o)$. We assume that a farmer $i$ can trade only with the village $v$ in which she is located, that is, $O_g(i) = D_g(i) = \{v\}$ for all $g$. Similarly, an urban household $h$ can trade only with the urban center $u$ in which it is located, that is, $O_g(h) = D_g(h) = \{u\}$ for all $g$. Further, while each village can consist of multiple farmers, each urban center consists of one representative household. Labor is not tradable across markets, i.e., $m' \notin O_L(m)$ for all markets $m \neq m'$.\footnote{While we could introduce migration – e.g. along the lines of recent work on quantitative spatial equilibrium models (e.g., Allen and Arkolakis (2014)) – we abstract from this margin of adjustment in our setting, as meaningful migration responses have not been found empirically in the context of the typical agricultural policy experiments (so limited, in fact, that few studies even attempt to measure effects on migration; e.g. Huntington and Shenoy (2021) find no impact). More broadly, shocks to agricultural productivity due to rainfall or extreme weather have been shown to have small and statistically insignificant effects on migration (e.g. Emerick and Burke (2016); Emerick (2018)). For applications of our approach to larger long-term shocks that might drive migration and structural adjustment, cross-market labor mobility could be readily incorporated in addition to the within-market commuting we focus on here.}

Let $p_{j,g}$ denote the price at which agent $j$ buys or sells good $g$, and let $p_{m,g}$ denote the price at which good $g$ is bought or sold at market $m$. Trade in good $g$ from $o$ to $d \in D_g(o)$ is subject to iceberg and additive trade costs. Iceberg trade costs are $\tau_{od,g}$ and additive trade costs are $t_{od,g}$ in units of a “transportation good.” We use index $T$ for this transportation good and assume that it is produced by Foreign at price $p_{F,T}$, and further assume that there are no trade costs for this good, so that all agents can access this good at price $p_{F,T}$. Thus, for example, if a farmer buys good $g$ from her village $v$, her farm-gate price is $p_{i,g} = \tau_{vi,g}(p_{v,g} + p_{F,T} t_{vi,g})$. We take this “transportation good” as the numeraire and so we set $p_{F,T} = 1$.\footnote{This implies that the policies we study do not lead to additional GE effects through changing (endogenous) transportation costs in the country.}

Finally, we assume that our economy is “small” in the sense that Foreign is willing to buy from or supply to it any amount of any good $g$ at exogenous prices $p_{F,g}^\star$.

### Preferences

Agent $j$ has an indirect utility function $V_j(\{a_{j,k} p_{j,k}\}, I_j)$, where $I_j$ denotes income and $a_{j,k}$ and $p_{j,k}$ denote taste shifters and prices of goods $k \in K$ for agent $j$. Let $\xi_{j,k}$ denote the expenditure share of agent $j$ on good $k$ and let $\varphi_{j,k}$ denote the corresponding expenditure share function.
Roy’s identity implies that

\[ \xi_{j,k} = \varphi_{j,k} \left( \{a_{j,k'}p_{j,k'}\}_{k'}, I_j \right) \equiv -\frac{\partial \ln V_j \left( \{a_{j,k'}p_{j,k'}\}_{k'}, I_j \right)}{\partial \ln p_{j,k}} \frac{\partial \ln p_{j,k}}{\partial \ln V_j \left( \{a_{j,k'}p_{j,k'}\}_{k'}, I_j \right)} \].

Further, letting \( \varphi_j \left( \{a_{j,k}p_{j,k}\}_{k}, I_j \right) \equiv \{ \varphi_{j,k} \left( \{a_{j,k'}p_{j,k'}\}_{k'}, I_j \right) \}_{k} \), we assume that \( \varphi_j (\bullet) \) is invertible (up to a normalization for prices) so that one can obtain \( \{a_{j,k}p_{j,k}\}_{k} \) as a function of expenditure shares and income,

\[ \{a_{j,k}p_{j,k}\}_{k} = \varphi_j^{-1} \left( \{\xi_{j,k}\}_{k}, I_j \right). \] (1)

This assumption, and the equivalent assumption for technology made below, will be important for the solution method and counterfactual analysis.

**Technology**

We start with farmers and then describe urban households. A farmer can produce good \( k \in K_A \) with \( \omega \in \Omega \) techniques. For farmer \( i \) and good \( k \), technique \( \omega \) uses inputs \( n \in N \) in a Cobb-Douglas production function with shares \( \alpha_{i,n,k,\omega} \), with \( 1 - \sum_n \alpha_{i,n,k,\omega} > 0 \) the share of land. The function defining land returns for farmer \( i \) is given by

\[ Y_i \left( \{v_{i,k,\omega}\}_{k,\omega} \right) \equiv \max_{\{Z_{i,k,\omega}\}_{k,\omega}} \sum_{k,\omega} v_{i,k,\omega} Z_{i,k,\omega} \quad \text{s.t.} \quad f_i(\{Z_{i,k,\omega}\}_{k,\omega}) \leq Z_i, \]

where

\[ v_{i,k,\omega} \equiv \left( \frac{b_{i,k,\omega}p_{i,k}}{\prod_n p_{i,n}^{\alpha_{i,n,k,\omega}}} \right)^{\frac{1}{\sum_n \alpha_{i,n,k,\omega}}} \] (2)

and \( b_{i,k,\omega} \) are technology shifters. Here \( Z_{i,k,\omega} \) can be understood as the effective units of land allocated to producing agricultural good \( k \) with technique \( \omega \), and \( v_{i,k,\omega} \) can be seen as the return to a unit of effective land allocated to crop \( k \) with technique \( \omega \). We assume that \( f_i (\bullet) \) is strictly quasiconvex so that the maximization problem has unique solution.

Consistent with the stylized fact that the adoption of modern technologies can change the relative cost shares of labor and land, we allow input shares to vary not only across crops but also across techniques within crops. When we turn to the model calibration in Section 4, we will allow input shares to differ across Ugandan regions, and we will allow for only two techniques: traditional, \( \omega = 0 \), and modern, \( \omega = 1 \). We will map these two techniques to data in terms of observed use of modern intermediates (such as chemical fertilizer or hybrid seeds) in production: the traditional technique makes use of land and labor (with \( \alpha_{k,0} = 0 \)), whereas the modern technique adopts the use of intermediates (with \( \alpha_{k,1} > 0 \)). Thus, the choice of a modern technique will increase the importance of intermediates and decrease the importance of land or labor.
Let $\pi_{i,k,\omega} \equiv \frac{v_{i,k,\omega}Z_{i,k,\omega}}{\sum_{k',\omega'} v_{i,k',\omega'}Z_{i,k',\omega'}}$ denote the share of land returns coming from production of crop $k$ with technique $\omega$ and let $\psi_{i,k,\omega}(\bullet)$ denote the corresponding share function. An envelope result implies that

$$\pi_{i,k,\omega} = \psi_{i,k,\omega}(\{v_{i,k',\omega'}\}_{k',\omega'}) = \frac{\partial \ln Y_i(\{v_{i,k',\omega'}\}_{k',\omega'})}{\partial \ln v_{i,k,\omega}}.$$ 

In turn, demand for input $n$ (in value) as a ratio of land returns is

$$\phi_{i,n}(\{v_{i,k',\omega'}\}_{k',\omega'}) = \sum_{k,\omega'} (\alpha_{i,n,k,\omega} - \sum_{n'} \alpha_{i,n',k,\omega}) \psi_{i,k,\omega}(\{v_{i,k',\omega'}\}_{k',\omega'}).$$

Finally, letting $\psi_i(\{v_{i,k,\omega}\}_{k,\omega}) \equiv \{\psi_{i,k,\omega}(\{v_{i,k',\omega'}\}_{k',\omega'})\}_{k,\omega}$, we assume that $\psi_i(\bullet)$ is invertible (up to a normalization for prices) so that one can obtain $\{v_{i,k,\omega}\}_{k,\omega}$ as

$$\{v_{i,k,\omega}\}_{k,\omega} = \psi_i^{-1}(\{\pi_{i,k,\omega}\}_{k,\omega}).$$

Now we turn to urban households. These households produce the manufacturing good. We keep their technology simple by assuming that production is linear in labor, so that the quantity of the manufacturing good produced is given by $b_{h,M}L_h$. Given that labor supply is perfectly inelastic, we can then simply treat $y_{h,M} \equiv b_{h,M}L_h$ as the urban households’ endowment of the manufacturing good.

**Equilibrium**

In equilibrium, rural and urban households maximize utility taking prices as given, prices respect no-arbitrage conditions given trade costs, and all markets clear. We assume that markets are competitive, with all frictions in goods and factor markets operating through within- and cross-market trade costs (including local labor markets as well as other inputs and output).\footnote{The presence of additive trade costs also implies that pass-through is not log linear. This leads to richer comparative statics than in models with only iceberg trade costs and perfect competition or even monopolistic competition with fixed markups.}

To formalize the definition of equilibrium, let $\chi_{j,g}(\{a_{j,k}p_{j,k}\}_{k}, \{v_{j,k,\omega}\}_{k,\omega}, I_j)$ be the excess demand (in value) of agent $j$ for good $g$ given prices of outputs and inputs, and let $\chi_{F,g}(\{p_{F,k}\}_g)$ be the corresponding excess demand function for Foreign. The equilibrium is a set of prices, $\{p_{j,g}\}$ and $\{p_{m,g}\}$, and trade flows (in quantities), $\{x_{od,g}\}$, such that excess demand is equal to the difference between purchases and sales for each agent $j$ and good $g$,

$$\chi_{j,g}(\{a_{j,k}p_{j,k}\}_{k}, \{v_{j,k,\omega}\}_{k,\omega}, I_j) = p_{j,g} \left( \sum_{o \in O_{g}(j)} x_{o,j,g} - \sum_{d \in D_{g}(j)} x_{j,d,g} \right) \quad \forall j \neq F, g.$$  

(4)
\[ \chi_{F,g}\left(\{p_{F,g}\}_{g}\right) = p_{F,g}\left(\sum_{o \in O_g(F)} x_{oF,g} - \sum_{d \in D_g(F)} x_{Fd,g}\right) \quad \forall g, \] (5)

markets clear,
\[ \sum_{d \in D_g(m)} x_{md,g} = \sum_{o \in O_g(m)} x_{om,g} \quad \forall m, g, \] (6)

and no-arbitrage conditions hold,
\[ \tau_{od,g}(p_{o,g} + t_{od,g}) \geq p_{d,g} \quad \forall d \in D_g(o), g, \] (7)

with farmer \( j \)'s income equal to the sum of land returns and wages,
\[ I_j = Y_j\left(\{v_{j,k,\omega}\}_{k,\omega}\right) + p_{j,L}L_i \] (8)

urban households' income given by
\[ I_h = p_{h,M}y_{h,M} \] (9)

and \( v_{i,k,\omega} \) given by equation 2. Here the symbol \( \perp \) between a weak inequality and a variable indicates that the weak inequality holds as equality if the variable is strictly positive. For example, if farmer \( i \) sells good \( k \) to market \( v \) then \( x_{iv,k} > 0 \) and we must have \( p_{v,k} = \tau_{iv,k}(p_{i,k} + t_{iv,k}) \), while the converse implies that if \( p_{v,k} > \tau_{iv,k}(p_{i,k} + t_{iv,k}) \), then \( x_{iv,k} = 0 \). The excess demand functions \( \chi_{j,g} \) for farmers, urban households and Foreign are determined by the results in the previous subsections, and can be found in Appendix 2.

The equilibrium conditions across all crops, labor, the intermediate good and the manufacturing good imply that there is trade balance, which is given by the condition that Foreign runs a deficit in goods that is paid for by the economy’s total expenditure on trade costs (which is an income to Foreign).

**Solution of Counterfactuals**

Many policies in the agricultural sector can be classified as shocks to preferences (e.g. a program to inform households about the nutritional content of different foods), technology (e.g. a new seed variety), or trade costs (e.g. a road building initiative). We are therefore interested in computing the effect of these shocks, which using hat notation (i.e., \( \hat{x} = x' / x \)), are given by \( \{\hat{a}_{j,k}\}, \{\hat{b}_{j,k,\tau}\}, \) and \( \{\hat{\tau}_{od,k}, \hat{t}_{od,k}\} \). In the counterfactual equilibrium, equations 4-7 can be written as

\[ \chi_{j,g}\left(\{\hat{a}_{j,k}\}, \{\hat{b}_{j,k,\tau}\}, \{\hat{\tau}_{od,k}, \hat{t}_{od,k}\}\right) \]  

\[ = p'_{j,g}\left(\sum_{o \in O_g(j)} x'_{oj,g} - \sum_{d \in D_g(j)} x'_{jd,g}\right) \quad \forall j, g, \] (10)
\[ \chi_{F_g} \left( \{p_{F,g}\}_g \right) = p_{F_g} \left( \sum_{o \in \mathcal{O}_g(F)} x'_{oF_g} - \sum_{d \in \mathcal{D}_g(F)} x'_{Fd,g} \right) \forall g, \]  

\[ \sum_{d \in \mathcal{D}_g(m)} x'_{md,g} = \sum_{o \in \mathcal{O}_g(m)} x'_{om,g} \forall m, g, \]  

\[ \tau_{od,g} \left( p_{o,g} + t_{od,g} \right) p_{o,g} \geq p_{d,g} \perp x'_{od,g} \forall d \in \mathcal{D}_g(o), g, \]  

with

\[ \hat{I}_j = Y_j \left( \{ \hat{v}_{j,k,\omega} \hat{p}_{i,k} \} \right) \left( \{ \pi_{j,k',\omega'} \} \right)_{k' \in \mathcal{K}_A, \omega} / I_j + \lambda_i, L \hat{p}_{i,L}, \]  

\[ \hat{I}_h = \hat{p}_{h,M}, \]  

and

\[ \hat{v}_{i,k,\omega} = \left( \prod_{n} \hat{p}_{i,n,\omega} \right) \frac{1}{\sum_{n} \hat{p}_{i,n,\omega}} \forall i \in \mathcal{I}, k \in \mathcal{K}_A, n \in \mathcal{N}, \omega \in \Omega. \]  

The term on the LHS of equations 10 and 11 along with equations 14-16 are in terms of hat changes, as in exact-hat algebra, but the RHS of equations 10 and 11 as well as equations 12 and 13 are in terms of counterfactual levels. This implies that in this system we have prices both in hat changes and counterfactual levels, \( \{ \hat{p}_{j,g}, \hat{p}_{m,g} \} \) and \( \{ p_{j,g}, p_{m,g} \} \), so we need the original prices \( \{ p_{j,g}, p_{m,g} \} \) to solve the system. We propose to recover these prices in a manner that is consistent with the model and the variables observed in microdata.

We observe expenditure shares for farmers and urban households, \( \{ \xi_{i,g}, \xi_{h,g} \} \), crop output levels for farmers \( \{ y_{i,k,\omega} \} \), output of manufacturing for urban households \( \{ y_{h,M} \} \), labor endowments of farmers, \( \{ \lambda_i \} \), cost shares of farmers \( \{ \alpha_{i,n,k,\omega} \} \), calibrated trade costs \( \{ \tau_{od,g} \} \), and prices at Foreign \( \{ p_{F,g} \} \). Let \( \mathcal{D} \equiv \{ \xi_{i,g}, \xi_{h,g}, y_{i,k,\omega}, y_{h,M}, \lambda_i, \alpha_{i,n,k,\omega}, \tau_{od,g}, t_{od,g}, p_{F,g} \} \).

First, we recast excess demand functions \( \chi_{j,g}(\cdot) \) as functions of data \( \mathcal{D} \) and prices \( \{ p_{j,g} \} \) for farmers, urban households and Foreign (see Appendix 3 for expressions). We then solve for prices \( \{ p_{j,g}, p_{m,g} \} \) in the initial equilibrium as a solution to the following system of equations:

\[ \chi_{j,g} \left( \{ p_{j,g}' \}_g ; \mathcal{D} \right) = p_{j,g} \left( \sum_{o \in \mathcal{O}_g(j)} x_{oj,g} - \sum_{d \in \mathcal{D}_g(j)} x_{jd,g} \right) \forall j, g, \]  

\[ \sum_{d \in \mathcal{D}_g(m)} x_{md,g} = \sum_{o \in \mathcal{O}_g(m)} x_{om,g} \forall m, g, \]  

\[ \tau_{od,g} \left( p_{o,g} + t_{od,g} \right) p_{o,g} \geq p_{d,g} \perp x_{od,g} \forall d \in \mathcal{D}_g(o), g. \]  

In Appendix 3 we show how to transform the price discovery step into an equivalent problem of finding the equilibrium of an exchange economy that is integrated as a small open economy with the rest of the world. We then show that, if there are no additive trade costs, the goods in such an economy satisfy the connected substitutes condition in Berry et al. (2013) and hence there is a unique equilibrium in which all agents are directly or indirectly connected through
trade. This implies that there is a unique (connected) solution to the price discovery step. Although we can no longer establish uniqueness analytically if there are additive trade costs, our numerical analysis suggests that this is indeed the case in our context.10

Using prices \( \{p_{j,g}, p_{m,g}\} \) thus obtained, data \( D \) and shocks \( \{\hat{a}_{j,k}, \hat{b}_{j,k,\omega}, \hat{\tau}_{od,k}, \hat{i}_{od,k}\} \) to the initial equilibrium, we evaluate the excess demand functions for all agents in the counterfactual equilibrium with income levels for farmers and urban households computed as

\[
I_i \left( \{p_{i,g}\}_g ; D \right) = \sum_{k,\omega} \left( 1 - \sum_n \alpha_{i,n,k,\omega} \right) p_{i,k} y_{i,k,\omega} + p_{i,L} L_i,
\]

and

\[
I_h \left( \{p_{h,g}\}_g ; D \right) = p_{h,M} y_{h,M},
\]

respectively, and land-rent shares across crops and technologies computed as

\[
\pi_{i,k,\omega} = \frac{(1 - \sum_n \alpha_{i,n,k,\omega}) p_{i,k} y_{i,k,\omega}}{(1 - \lambda_{i,L}) I_i}, \tag{20}
\]

where \( \lambda_{i,L} = \frac{p_{i,L}}{I_i} \) is the share of farmer’s total income coming from wage income. Finally, we can obtain counterfactual trade flows \( \{x_{od,g}'\} \) and prices \( \{p_{j,g}', p_{m,g}'\} \) as a solution to the system of equations 10-16.

**Parametrization**

In the final part, we impose specific functional forms that we use to calibrate the model for our application below. Motivated by the large differences across households in expenditure shares on food in the data, we assume non-homothetic preferences between the agricultural and manufacturing goods. In particular, we assume that upper tier preferences are Stone-Geary, so that households need to consume a minimum amount of the agricultural-good composite, \( \bar{C}_A \). In turn, agricultural goods are aggregated into a CES composite with elasticity of substitution \( \sigma \). The indirect utility function is then

\[
V_j \left( \{a_{j,k}p_{j,k}\}_k, I_j \right) = \frac{I_j - P_{j,A} \bar{C}_A}{\frac{P_{j,A}}{P_{j,M}}^{1-\xi}},
\]

with

\[
P_{j,A} = \left( \sum_{k \in K_A} (a_{j,k}p_{j,k})^{-(\sigma-1)} \right)^{-\frac{1}{\sigma-1}}.
\]

---

10We consider 100 different initial guesses for prices and find the same equilibrium in all cases.
This implies that

$$\xi_{j,k} = \varphi_{j,k} \left( \left\{ a_{j,k}P_{j,k} \right\}_k, I_j \right) = \left( \frac{a_{j,k}P_{j,k}}{P^{-\sigma-1}_{j,A}} \right) \left( \zeta + (1 - \zeta) \frac{P_{j,A}C_A}{I_j} \right)$$

for \( k \in K_A \) and \( \xi_{j,M} = (1 - \zeta) \left( 1 - \frac{P_{j,A}C_A}{I_j} \right) \).

On the production side we assume that

$$f_i \left( \left\{ Z_{i,k,\omega} \right\}_k \right) = \gamma^{-1} \left( \sum_k \left( \sum_{\omega} Z_{i,k,\omega}^{\kappa/(\kappa-1)} \right)^{\mu/\kappa} \right)^{\frac{\mu-1}{\mu}},$$

where \( \kappa \) and \( \mu \) are positive parameters and \( \gamma \) is some constant.\(^{11}\) We then have

$$Y_i \left( \left\{ v_{i,k,\omega} \right\}_{k,\omega} \right) = \gamma \left( \sum_k \left( \sum_{\omega} v_{i,k,\omega}^{\kappa/\mu} \right)^{\mu/\kappa} \right)^{1/\mu} Z_i,$$

and

$$\psi_{i,k,\omega} \left( \left\{ v_{i,k',\omega'} \right\}_{k',\omega'} \right) = \frac{v_{i,k,\omega}^{\kappa/\mu}}{\sum_{\omega'} v_{i,k,\omega'}^{\kappa/\mu}} \frac{\left( \sum_{\omega'} v_{i,k',\omega'}^{\kappa/\mu} \right)^{\mu/\kappa}}{\sum_{k'} \left( \sum_{\omega'} v_{i,k',\omega'}^{\kappa/\mu} \right)^{\mu/\kappa}}.$$

Here, \( \kappa \) is the supply elasticity that governs the substitution of allocating land across technology regimes within a given crop \( k \) (in the lower nest), while \( \mu \) is the supply elasticity that governs substitution of land allocations across crops in the upper nest.

### 3 Data

Our analysis makes use of six main datasets. Appendix 1 provides additional details and descriptive statistics.

**Uganda National Panel Survey (UNPS)**

The UNPS is a multi-topic household panel collected by the Ugandan Bureau of Statistics as part of the World Bank’s Living Standards Measurement Survey. The survey began as part of the 2005/2006 Ugandan National Household Survey (UNHS). Then starting in 2009/2010, the UNPS...
set out to track a nationally representative sample of 3,123 households located in 322 enumeration areas that had been surveyed by the UNHS in 2005/2006. The UNPS is now conducted annually. Each year, the UNPS interviews households twice, in visits six months apart, in order to accurately collect data on both of the two growing seasons in the country. In particular, the main dataset that we assembled contains 77 crops across roughly 100 districts and 500 parishes for the periods 2005, 2009, 2010, 2011 and 2013. It includes detailed information on agriculture, such as crop production, crop unit values, amount of land, amount of land allocated to each crop, labor and non-labor inputs used in each plot and technology used at the household-parcel-plot-season-year. Data on consumption of the household contains disaggregated information on expenditures, consumption quantities and unit values.

**Uganda Population and Housing Census 2002**

The Ugandan Census has been conducted roughly every ten years since 1948. Collected by the Ugandan Bureau of Statistics, it is the major source of demographic and socio-economic statistics in Uganda. Over the span of seven days, trained enumerators visited every household in Uganda and collected information on all individuals in the household. At the household level, the Census collects the location (down to the village level), the number of household members, the number of dependents, and ownership of basic assets. Then for each household member, the Census collects information on the individual’s sex, age, years of schooling obtained, literacy status, and source of livelihood, among other indicators. We have access to the microdata for the 100 percent sample of the 2002 Census.

**GIS Database and Border Prices**

We use several geo-referenced datasets. We use data on administrative boundaries and detailed information on the transportation network (covering both paved and non-paved feeder roads) from Uganda's Bureau of Statistics. We complement this database with geo-referenced information on crop suitability from the Food and Agricultural Organization (FAO) Global Agro-Ecological Zones (GAEZ) database. This dataset uses an agronomic model of crop production to convert data on terrain and soil conditions, rainfall, temperature and other agro-climatic conditions to calculate the potential production and yields of a variety of crops. We use this information as part of the projection from the UNPS sample to the Ugandan population at large. Finally, we use information on world crop prices at Uganda's border from the FAO statistics database.

**Survey Data on Cross-Market Trade Flows and Trade Costs**

The survey data collected by Bergquist et al. (2022) captures cross-market trade flows and can be used to calibrate between-market transportation costs. They collect trade flow data in a survey of maize and beans traders located in 260 markets across Uganda (while not nationally representative, these markets are spread throughout the country). Traders are asked to list the markets...
in which they purchased and sold each crop over the previous 12 months. This information can be used to limit the calibration of cross-market trade costs to market pairs between which there were positive trade flows over a given period. They complement this data with a panel survey, collected in each of the 260 markets every two weeks for three years (2015-2018), in which prices are measured for maize, beans, and other crops. A greater description of the data collection can be found in Bergquist et al. (2022).

**Demand Estimation**

To estimate the slope of the demand curve for crops in Sections 2 and 4, we bring to bear transaction-level microdata from maize markets in rural Kenya that was collected as part of an experiment in Bergquist and Dinerstein (2020). Though for our purposes these subjects would ideally be representatively drawn from the same area in which the at-scale policy will be implemented, rural areas across East Africa share many features, including crops grown, farming methods (mostly rain-fed agriculture), and overall levels of development. This is especially true for the rural area of western Kenya studied in Bergquist and Dinerstein (2020), which takes place 30km from the Ugandan border.

In their experiment, which took place in open-air maize markets, individual consumers who approached maize traders to make a purchase were offered a surprise discount—the size of which was randomized across ten possible amounts—in the price they would pay for any maize they wished to purchase that day. The value of the discount ranged from from roughly 0-15% of the baseline price and was randomized across customers within a given market-day. Using the subsidy as exogenous variation in consumer prices, the experiment measured resulting quantities purchased. We use these experimental data to estimate our key demand elasticity.

**Supply Estimation**

To estimate the key supply elasticity governing farmers’ choice of land allocation across modern or traditional planting technologies, we exploit experimental variation from Carter et al. (2020). In this RCT, randomly selected farmers in Mozambique were offered fertilizer and improved seeds at a subsidized price. Data collected on farmers’ use of modern technologies and output by plot allows estimation of the impact of changing input prices (instrumented by treatment) on land allocations across technologies. We complement this RCT with a natural experiment in the UNPS microdata that allows us to estimate the upper-tier supply elasticity in our model for substitution of land allocations across crops.

**4 Calibration and Parameter Estimation**

Building on the theory and data discussed in the previous sections, we calibrate the model to the Ugandan economy in two main steps. In the first step, we describe the estimation of the
demand and supply parameters ($\zeta$, $\sigma$, $\alpha_{k\omega}$, $\beta_{k\omega}$, $\gamma_{k\omega}$, $\kappa$ and $\mu$), and the calibration of trade frictions between individual households $i$ and their local markets $m$ ($t_{img}$) and across local markets ($t_{odg}$).\footnote{Consistent with the stylized facts in Appendix 1, we focus on additive trade costs here.} In the second step, we use the survey data on household expenditure shares across crops and sectors and crop quantities produced ($\xi_{ik}$ and $y_{ik\omega}$) from the UNPS panel data, and extrapolate this information to the Ugandan population at large. To this end, we use the microdata on household locations and their characteristics from the 100% sample of the Ugandan population census in 2002 described in Section 3.

Using the solution method of the previous section, this combination of parameter values and raw disaggregated information on pre-existing household consumption and production choices allows us to solve for unobserved farm-gate and market prices ($p_{ig}$ and $p_{mg}$) and household revenue shares ($\pi_{ik\omega}$) for the whole of Uganda. This, in turn, allows us to use exact hat algebra to solve for GE counterfactuals in Section 5.

**Demand Estimation**

To estimate the demand parameter, $\sigma$, that governs the elasticity of substitution between crops, we exploit the data and randomized demand experiment run in Bergquist and Dinerstein (2020). We run the following specification:

$$\log q_{ismd} = \alpha + \beta \log p_{ismd} + \theta_{smd} + \epsilon_{ismd},$$

regressing log quantity purchased by individual $i$ from seller $s$ in market $m$ on date $d$ on log price, instrumenting for price with the randomized subsidy amount. Because the subsidy was randomized across consumers buying from the same seller in the same market-day, we run specifications including either market x date fixed effects ($\theta_{md}$) or seller x market x date fixed effects ($\theta_{smd}$), presented in Columns 2 and 4 of Table 1, respectively. Both specifications yield estimates close to 1. We therefore calibrate our model with $\sigma = 1$. We also explore the sensitivity of the counterfactual analysis across a range of higher or lower values for $\sigma$ in Section 5.

To calibrate the demand parameter, $\zeta$, that governs non-homotheticity in food consumption, we use the following relationship that holds subject to utility maximization under Stone-Geary:

$$\frac{P_{iA}C_{A}}{I_i} = \frac{\xi_{iA} - \zeta}{(1 - \zeta)},$$

where the left-hand side is the share of household income spent on subsistence, and $\xi_{iA}$ is the observed share spent on total food consumption. We use the typical feature of these preferences that the share of income spent on subsistence approaches zero for the richest households, setting the left-hand side equal to zero, and calibrating $\xi_{A}$ with the average share of expenditure spent on total food consumption among the richest 5 percent of Ugandan households (which is close to 0.1 in the UNPS data). This yields an estimate of $\zeta = 0.1$, implying that the share spent
on subsistence is on average 38 percent across Ugandan households.

**Supply Estimation**

To estimate the cost share parameters of the production function, \( \alpha_{ink}\omega \), we take the median of the cost shares that we observe across households in the UNPS microdata by region of the country and appropriately weighted using sampling weights. Appendix Table A.9 presents the cost shares observed in production across the 9 major crops and the two technology regimes averaged across Ugandan regions.

To estimate the first key supply elasticity, \( \kappa \), that governs substitution across technologies within a given crop, we use the data and randomized variation from Carter et al. (2020) described in Section 3. We derive the following estimation equation from the previous section:

\[
\log \left( \frac{\pi_{1|kt}}{\pi_{0|kt}} \right) = - \left( \kappa \frac{\alpha_{input}^{ik1}}{\alpha_{land}^{ik1}} \right) \log InputPrice_{ikt} + \epsilon_{ikt}, \tag{21}
\]

where we have the relative land allocations of modern vs traditional production techniques within maize production on the left-hand side, and the log price of modern inputs on the right-hand side. The extent to which a price shock for modern inputs affects land allocations across production techniques will be a function of the supply elasticity in the lower nest, \( \kappa \), as well as the relative cost shares of these inputs and land in modern production, \( \alpha_{input}^{ik1} \) and \( \alpha_{land}^{ik1} \) respectively.

We construct a price index for modern inputs as the weighted average of prices of chemical fertilizer and hybrid seeds, with weights proportional to their relative cost shares. We then instrument this price with the subsidy treatment in Carter et al. (2020).\(^{13}\) Table 2 presents the estimation results across the first stage, reduced form and IV point estimates. For each, we report results both from a single post-treatment cross-section or using baseline and post-treatment panel data with round and community fixed effects.\(^{14}\) The IV point estimate in columns 5 and 6 is 0.83 and 0.85. Using the ratio of cost shares of land over fertilizer and hybrid seeds in Table A.9, this implies that \( \kappa = 2.5 \). We use this estimate of the lower (within-crop) nest elasticity as our baseline, and explore the sensitivity of the counterfactual analysis across a range of higher or lower values for \( \kappa \).

Turning to the upper-tier supply elasticity across crops, \( \mu \), we exploit a natural experiment in our Ugandan microdata.\(^{15}\) The estimation equation derived from the parametrization above is as follows:

\(^{13}\)Given these data record just one snapshot of production, where some farmers were allocating 100% of production to either modern or traditional techniques, we aggregate both left and right-hand sides to the level of 32 communities broken up by treatment status, summing land allocations on the left and taking average prices on the right. This is to avoid the assumption that those farmers could never make use of the other technology.

\(^{14}\)Carter et al. (2020) also explore the spillover effects of the subsidy on non-treated farmers along the personal networks of treated farmers. Those dynamic effects were not present in the first post-treatment round that we use for estimation here.

\(^{15}\)The experiment in Carter et al. (2020) did not induce changes in the allocation of land across crops that one could use for estimating \( \mu \).
\[
\log \left( \sum_{\omega} \pi_{\omega|kt} \prod_{n \in N} q_{ink\omega|kt}^{-1} \right)^{\frac{1}{\mu}} = \left( \frac{\mu - 1}{\mu} \right) \log \pi_{ikt} + \log \left( \sum_{\omega} \tau_{\omega|ikt}^{\frac{1}{\mu}} \right)^{\frac{1}{\mu}} + \log \frac{Y_t}{V_t} (22)
\]

The left-hand side of (22) is farmer \(i\)'s harvest quantities for crop \(k\) aggregated across both technology regimes \(\omega\) in survey year \(t\) (summed across both seasons) adjusted for the reported units of labor, modern intermediates and land used in production.\(^{16}\) The first term on the right-hand side, \(\log \pi_{ikt}\), is the land share for crop \(k\) used in producing the harvests on the left-hand. The final two terms capture both average and farmer-specific production shocks over time and across crops, which we capture by including crop-by-year fixed effects (\(\theta_{kt}\)), farmer-by-crop fixed effects (\(\phi_{ik}\)) and an error term \(\epsilon_{ikt}\).\(^{17}\) The regression coefficient of interest, \(\beta = \frac{\mu - 1}{\mu}\), is thus estimated using changes in land allocations within farmer-by-crop cells controlling for average changes by crop across farmers over time.\(^{18}\)

To estimate \(\mu\) convincingly, we require plausibly exogenous variation in land allocations (\(\log \pi_{ikt}\)) across crops over time by farmers that are not confounded with unobserved local productivity shocks. To this end, we make use of the fact that the unit cost nature of trade frictions documented in Section 3 implies that shocks to world market prices across crops \(k\) should lead to a larger reallocation of land shares for farmers closer to the border, as the percentage change in local producer prices is \(\Delta p_{\text{world}} / p_{\text{world},t0 + \text{bordercost}_i}\). We use shocks to world prices for coffee, as world coffee prices are both highly relevant (more than 90% of Ugandan coffee production is exported)\(^{19}\) and likely exogenous to domestic production (Uganda accounts for less than 2% of world coffee sales). We thus construct the instrument as the interaction of the log distance to the nearest border crossing for farmer \(i\), a dummy for whether crop \(k\) is coffee, and the log of the relative world price of coffee relative to the world price of the other eight crops. Note that the fixed effects \(\phi_{ik}\) and \(\theta_{kt}\) absorb all but the triple interaction term. The identifying assumption is that individual farmer productivity shocks in coffee production relative to other crops are not related to the direction of relative world price changes and distance to the border.

As documented in appendix Figure A.3, the relative world price of coffee dropped significantly over our sample period 2005-2013. All else equal, land shares used for coffee production

\(^{16}\)This represents an observable measure of land productivity for a crop \(k\) as the harvest amounts we observe for a given farmer under either of the two technology regimes are deflated by the inputs used across all plots of land allocated to crop \(k\).

\(^{17}\)Alternatively, to allow for region-specific shocks across crops over time, we also replace \(\theta_{kt}\) with region-by-crop-by-year fixed effects (\(\theta_{rkt}\)).

\(^{18}\)The advantage of writing the estimation equation as in (22) is that all terms are observable to us using the Ugandan microdata. In particular, using changes in land shares as the main regressor (instead of farm-gate prices with land shares on the left-hand side) provides us with a much more complete dataset for estimation given somewhat scant information about unit values. And while we observe changes in inputs to production by plot and farmer over time, we do not observe changes of the input factor prices (e.g. locality-specific changes to the prices of modern intermediates).

\(^{19}\)Among the 9 main crops we study in Uganda, only coffee falls into this category: the share of exports to production for coffee exceeds 90 percent in all years of our sample, whereas the sum of exports plus imports over domestic production is close to zero (below 4 percent) for the other crops.
should have thus fallen more strongly closer to the border. Panel A of Table 3, which presents the first-stage regression, documents that this is indeed the case: the negative point estimate on our instrument implies that negative relative world price changes for coffee decrease land allocation to coffee more for farmers closer to the border.\textsuperscript{20} Panel C presents the second-stage estimation of equation (22).\textsuperscript{21} We find statistically significant point estimates in the range of 0.45-0.75. Recall that this point estimate captures $\beta = \frac{\mu - 1}{\mu}$; this therefore implies estimates of $\mu$ in the range of 1.8-4. Reassuringly, these are close to existing estimates of this parameter reported in Sotelo (2020) ($\mu = 1.7$). To be conservative, we pick the low estimate of $\mu = 1.8$ as our baseline calibration.\textsuperscript{22} We also report estimation results across a range of alternative parameter assumptions in a number of additional robustness checks.

**Trading Frictions**

To calibrate trade frictions across local markets, we use results in Bergquist et al. (2022), which collects survey microdata on bilateral trade flows between Ugandan markets and origin and destination prices. Consistent with the stylized facts in Appendix 1, we estimate additive trade costs as a function of road distances between markets. Using only bilateral price gaps from market pairs during months in which they observe positive trade flows between the pair, in addition to information on the road distance between the markets from the transportation network database, we estimate the following specification:

$$t_{odkt} = (p_{dkt} - p_{okt}) = \alpha + \beta (RoadDistance_{od}) + \epsilon_{odkt}$$

where $t$ indexes survey rounds and the error term $\epsilon_{odkt}$ is clustered at the level of bilateral pairs $(od)$. $RoadDistance_{od}$ is measured in road kilometers traveled along the transportation network. We estimate a single function of trade costs with respect to road distances across all crops, so $t_{odk} = t_{od}$.\textsuperscript{23} The estimated trade cost for an additional road kilometer traveled between two markets is 1.2 Ugandan shillings (standard error 0.289), which implies a cost of about $0.5 per kilometer for one ton of shipments. This is consistent with additional survey data from Bergquist et al. (2022) documenting that fuel costs for a fully-loaded 5-ton is 0.3 Ugandan shillings per kg per km (standard error 0.024), which implies that fuel costs account for about 25% of total trade frictions. If we replace the specification above to be in logs on both left and right-hand sides, the

\textsuperscript{20}This relationship holds both before and after including region-by-crop-by-technology-by-time fixed effects, and when using all years of data (2005, 2009, 2010, 2011 and 2013) or just using long changes 2005-2013.

\textsuperscript{21}In Panel B, we report estimation results before adjusting farmer harvests ($y_{ikt}$) by inputs used in production in the denominator of the left-hand side. Judging from Panel B, it does not seem to be the case that OLS estimates are biased upward compared to IV estimation. If anything, the IV point estimates of harvest on land shares are somewhat larger than in OLS. This could suggest that unobserved idiosyncratic productivity shocks pose less of an omitted variable concern in this setting compared to potentially significant measurement error in the reported land shares allocated to different crops and across different technology regimes on individual farmer plots in the survey data.

\textsuperscript{22}This is conservative in terms of welfare impacts, and in terms of the difference between local-vs-at-scale effects.

\textsuperscript{23}We do so for power reasons. The dataset covers two crops, maize and beans. Including a crop-month FE in the regression above gives very similar results.
distance elasticity is .0258 (standard error 0.0057), which is close to existing recent evidence for within-country African trade flows by e.g. Atkin and Donaldson (2015).

To calibrate the local trading frictions between farmers and their local market \( t_{imkt} \), we implement a similar strategy, using gaps between selling farmers’ farm-gate prices and local market prices. Because we do not directly observe local market prices in the UNPS data, we infer price gaps as residuals from regressing farm-gate prices on a set of parish-crop-harvest time fixed effects \( \eta_{mkt} \), which serve as village prices, using the following specification:

\[
p_{imkt} = p_{mkt} - t_{imkt} = \eta_{mkt} - t_{imkt}
\]

where \( p_{imkt} \) is the farm-gate price of farmer \( i \) in parish \( m \) at year-month \( t \) and \( p_{mkt} \) is the market price (which is replaced in our estimation by fixed effect \( \eta_{mkt} \)). The residual is \( -t_{imkt} \), the negative of the local trade cost.

In the next step, we predict trade costs \( t_{imkt} \) for the full Ugandan population. Unlike the cross-market trade flow data, we do not have exact geo-locations for every household and local market as part of the UNPS database, meaning that we cannot project trade frictions as a function of distance traveled to the local market. Instead, we project these costs on a number of socio-demographic characteristics that we observe in both the UNPS panel and 100 percent Census data:

\[
\hat{t}_{imy} = \alpha + \beta \left( X'_{iy} \right) + \epsilon_{iky}
\]

where \( \hat{t}_{imt} \) are the average residuals from all transactions of farmer \( i \) on market \( m \) in year \( y \) with local traders at the farm-gate (again, we estimate a single function for all goods, so that \( t_{imk} = t_{im} \)). We discuss in detail the household characteristics included in the vector \( X'_{iy} \) as part of the next subsection (projection to population). The estimated average farmer trade friction to their local markets ranges between 23 at the 1st and 90 shilling at the 99th percentile, with an average of about 66 Ugandan shilling per kilogram, which amounts to roughly 8 percent of the average crop price.

We estimate the trading frictions farmers face when hiring or selling labor in the local market in a similar way as for crop trade costs. Farmers in the UNPS microdata report hired labor inputs in person-days and total expenditure, which we can use to construct daily wages at the farm-gate. First, we regress farm-gate wages on a set of parish-year-season fixed effects (our measure

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\[24\] In order to ensure we are capturing farm-gate prices, we make two sample restrictions, restricting attention to transactions that (i) involve selling to a "private trader in local market/village" (survey evidence from Bergquist et al. (2022) indicates that 64% of farmers who report selling to private traders sell their crops exclusively at the farm-gate); and (ii) for which no transport costs related to the sale are reported. To avoid noisy estimates of the parish-crop-harvest time fixed effects, we use observations with at least 10 transactions in each parish-crop-time cell after dropping the top/bottom 5% of unit values with each crop-year-season cell.

\[25\] Since the distribution of trade costs is therefore mechanically centered at zero, after predicting trade costs for the full Ugandan population (see the next step), we shift the distribution rightwards such that a farmer in the bottom 0.1 percentile faces trade costs of 1 Ugandan shilling.

\[26\] All regressions involving the Ugandan household microdata include appropriate weights using survey weights.
Then, we project the residuals on the same vector of socio-economic characteristics as above and shift the distribution rightwards such that trade costs at the 1st percentile amount to 1 shilling per day. On average, hiring farmers incur labor trading frictions of 248 shilling (or 10 US cents) per day for hiring a worker or around 5% of the daily wage for men. Lastly, we convert daily to annual labor trade costs by multiplying by 150 work days.

In the upper panel of table A.10, we show that predicted crop trade costs are significantly related to other measures of remoteness at the farmer level in the UNPS data. Column 1 documents that self-reported unit transport costs of selling at the local market (transactions not used in forming the prediction) represent on average 36% of predicted trade costs, broadly in line with the evidence on fuel costs above. Columns 2-5 show that trade costs are positively correlated with distances to various road types. In the bottom panel, we document the predicted trade costs for labor are predictive of whether a household outside our estimation sample hires any labor inputs.

**Extrapolation from Survey Data to Population**

To calibrate the model to the full set of local markets populating Uganda, we need household-level information on pre-existing production quantities and expenditure shares across crops and sectors for the full population, which is generally not available in microdata such as the census. Instead, we use the UNPS, which includes such detailed household-level information for a nationally representative sample of Ugandan households, to project these outcomes on a number of household and location characteristics that are also observed in the 100 percent sample microdata from the 2002 population census, to predict these outcomes for the full population. Outcomes of interest are total harvest by technology in each crop, land endowment, expenditure share on food and expenditures by crop. For each of these outcomes, the commonly observed household and location characteristics used in the predictions are as follows: age and education of head of household, number of dependents and household members, an asset ownership index, potential yield of a given location in the FAO/GAEZ database, dummies for subsistence farming and urban households, and district fixed, each interacted with year fixed effects. For this estimation, we employ Poisson pseudo-maximum likelihood (PPML), which has the nice property of preserving aggregates in the predicted data.

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27 Wages are computed from expenditure on the side of the hiring farmer (not the worker). Hence, we can treat those farm-gate wages inclusive of trade costs. Since hired labor is broken into person-days by men, women and children but expenditure on hired labor covers all three categories, we first drop observations with child labor present and we include the average daily wage of both male and female day laborers but control for the share of male workers in the first step. We use only observations with at least 10 reported wages in each parish-year-season cell.

28 In the 2013 UNPS data, agricultural workers employed at other than their own farm work around 150 days per year.

29 For estimating local trade costs or land endowment we do not include potential yields.
5 Counterfactual Analysis

Using the model, solution method and calibration described in the previous sections, this section presents the counterfactual analysis. We proceed with four main sets of results. We first present the analysis of how the welfare impacts of a modern input subsidy differ between a local intervention and one at scale – among the same sample of farmers – and quantify the underlying mechanisms. Second, we use our framework to document new insights on how the sign and extent of GE forces can differ as a function of saturation rates at different geographical scales. Third, we investigate the role of capturing a realistic, granular economic geography for counterfactual analysis. Fourth, we explore the sensitivity of our findings across alternative parameter values and other features of the model’s parameterization outlined at the end of Section 2.

5.1 Local Effects vs Scaling Up

To fix ideas, we focus on the effects of a subsidy for modern inputs (chemical fertilizers and hybrid seed varieties). We investigate the effects of an intervention that gives a 75 percent cost subsidy for these inputs across all crops. Using the production-side parameterization of the model, this intervention is akin to a positive productivity shock to producing crop $k$ under modern production technology $\omega = 1.30$

We run two types of counterfactuals in the calibrated model. As depicted in Figure 1, households are located in roughly 4,500 rural parish markets and 70 urban centers. In the local intervention, we randomly select a 2.5 percent sample in each of the rural parishes (roughly 100,000 households nationwide). For each of these markets, we then shock this random sample of households with the subsidy for modern inputs and solve for the counterfactual equilibrium as stated in Section 2. This is akin to running 4500 separate small-scale RCTs. For the intervention at scale, we offer the subsidy to all farming households in the economy (including the original 2.5 percent sample). In both types of counterfactuals, we solve for changes in household-level outcomes across all 4.5 million Ugandan households. We then compare the changes in economic outcomes for the sample of households treated in the original, local-only intervention to their economic outcomes when the intervention is also scaled to the rest of the Ugandan countryside.

Figures 2-4 and Tables 4 and 5 present the main counterfactual results. In Figure 2 we start by documenting the difference in welfare effects between the at-scale and local interventions across all $\sim 100,000$ sample households. The left panel show the at-scale impact minus the local intervention impact, in percentage points, for these households. The right panel aggregates to average effects at the level of parish markets, to facilitate comparison between the average treatment effect that a given parish would experience at scale to the average treatment effect that

\[ \hat{B}_{i,k1} = 0.25 + \frac{\sum_{n \in N_k^i} \alpha_{i,n,k1}}{\sum_{N_k^i \in N_k^i} \alpha_{i,n,k1}}. \]

To simplify the exercise, we leave aside for the moment the public finance dimension of the subsidy (e.g. financed by a lump-sum tax). Given that modern inputs are imported in our setting, this simplification should not omit potentially important GE effects.
would be typically be measured in a local experiment. The black lines plot the distribution of these differences, with the vertical bar showing the average difference. To shed light on distributional impacts, the blue and red lines show the same effects for the top and bottom quintiles (roughly 20,000 households each) of land shares in initial household income. Those in the bottom quintile – whom we refer to as “land-poor” – are smallholder farmers whose land profits are relatively small and who therefore get a larger fraction of their income from labor (including the implicit value used on their own farm, as well as any explicit value they receive from selling their daily labor to other, larger farms). Those in the top quintile – whom we refer to as “land-rich” – are larger landowners who have greater crop income and who tend to be net buyers of labor.

Two main insights emerge. First, the distribution is wide, with households experiencing more than +/- 5 percentage point changes in their welfare impact when the intervention is scaled-up (with the average household experience a decrease of about 1 percentage point, or about 20% of the average local welfare effect in Table 4). Second, scaling up the intervention has very different effects on land-rich vs. land-poor households. We see the mass of land-rich households lies to the left of zero, suggesting that they tend to lose at scale relative to how they fare under the local intervention, while the mass of land-poor households lies to the right, on average gaining at scale. Table 4 shows the point estimates of both local and at-scale effects across these different groups.

To further investigate the distributional implications of scaling in this context, Figure 3 presents non-parametric estimates of the local and at-scale welfare effect as a function of initial land income shares. We see in the left panel that while the local intervention strongly benefits land-rich households more than the land-poor (by up to 5.5 percentage points on average), the at-scale intervention significantly flattens this gradient (reducing this gap to 2 percentage points). Driving this compression is the fact that, consistent with Figure 2, land-poor households experience gains that are on average larger at-scale than they are under the local intervention, with the poorest households experiencing welfare gains that are 1.5 percentage points larger at-scale; in contrast, land-rich households fare worse at scale, with the richest households experiencing a 2 percentage point drop in their welfare gains relative to what they enjoy under the local intervention. Qualitatively very similar differences are present in the right panel when comparing land-rich and -poor households within markets, after conditioning on parish market fixed effects.

Changes in welfare are changes in real incomes, with the price index defined as the ideal price index over manufacturing and agricultural consumption given by the nested Stone-Geary preferences stated in the model’s parameterization at the end of Section 2.

Appendix figure A.4 also presents positive relationships between our measure of land shares and households’ land ownership in acres or households’ calibrated total incomes. Fink et al. (2020) document similar patterns in another African context (Zambia).

The numbers presented in Table 4 are simple averages across the roughly 100,000 farmers that received the subsidy in the local interventions. This average welfare effect differs from the aggregate welfare effect of the policy for two reasons. First, the simple average will be lower than the aggregate effect in our setting, as we document a positive correlation between effect size and initial incomes. Second, even after income-weighting the effect, Table 4 does not include the welfare gains among Uganda’s urban areas.
Figures 4 and 5 and appendix Figures A.5-A.8 present insights about the underlying mechanisms driving these differences at scale. The left panel in Figure 4 presents the difference between the at-scale effect and the local effect on components of nominal incomes across the initial land share distribution, while the right panel presents the same for components of the price index. Table 5 presents point estimates of the average effects on incomes, wages, and manufacturing and crop prices in the local intervention (Panel A) and at scale (Panel B).

In the left panel of Figure 4, we see that total nominal incomes appreciate by up to 1.5 percentage points for the land-poor and depreciate by up to 2 percentage points among the land-rich when comparing the at-scale intervention to the effects of the local intervention. To understand what drives these differences, we further decompose the differential effect on total household incomes into a component from growing crops (land income) and a component from labor income – multiplying the percentage difference in land or labor income changes by the initial income shares. GE forces on average decrease the positive effect on land income at scale compared to the local intervention for both land-rich and land-poor households, as the price of the local non-traded factor of production (labor) appreciates and (most) crop output prices fall (see Table 5). Wages and labor income increase on average as a result. Both effects favor the initially land-poor, who experience larger increases in their labor earnings and lesser reductions in their land earnings.  

The right panel of Figure 4 presents the impact of scaling up on the price index overall, as well as the decomposed effects on the manufacturing and agricultural price indices. Manufacturing prices appreciate by about 1 percentage point on average, dispersing the gains from the input subsidy from the rural population to households living in cities. The agricultural price index falls by about 0.75% on average, with some crops experiencing significant reductions and others increases (see Table 5). As we can see from the figure, land-poor households on average spend a higher fraction on food (their agricultural price index is closer to the overall price index). This should lead land-poor households to benefit more from crop price reductions (and suffer less from manufacturing price increases). This channel is muted, however, because within agriculture prices fall more among crops more heavily consumed by the land-rich. As we will discuss below, these heterogeneous crop price effects are mainly due to differences in pre-existing technology usage across crops. Overall, the change in the total price index between the at-scale and the local intervention is small, and doesn't vary substantially across the land share distribution. Price changes therefore matter less than nominal income changes for both the average and distributional effects of scaling up.

Figure 5 provides additional evidence on the role of remoteness. Theory would suggest that the gap between the effects of a local vs. at-scale intervention increase in market remoteness,

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34 This is driven both by higher pre-existing labor income shares among the land-poor as well as slight differences in average wage and crop price effects due to differences in crop and technology usage across households and markets. Appendix Figure A.5 shows the same graph without the initial income share weighting (no longer summing up to the total income effect), documenting about 1 percentage point more positive wage effects at scale (compared to the local effect) among the land-poor, but also about 1 percentage point more negative land earning effects at scale.
as GE effects on crop output prices are strongest in more remote markets, where local prices are less pinned down by world prices at border crossings or proximity to cities. The left panel of ?? confirms this hypothesis for access to other markets within Uganda (measured by the inverse distance-weighted sum of population in all other markets and cities $d$ in Uganda for each origin $o$ on the x-axis: \[ \sum_{d \neq o} \frac{\text{pop}_d}{\text{distance}_{od}} \]). The right panel plots the same relationship with respect to distance to the nearest border crossing on the x-axis. Both panels have the difference in households’ welfare impact (at scale - local) in percentage points on the y-axis. Deviations from the findings of local interventions due to GE forces that arise at larger scales tend to be most pronounced in relatively remote rural market places. Finally, appendix Figures A.6-A.8 provide additional evidence on the roles played by initial technology usage and crop planting decisions in shaping effects at scale vs. the local intervention. We document that land-rich households benefit more from the local intervention in part because of significantly higher pre-existing usage of modern technology (higher cost shares for fertilizer and hybrid seeds). Average crop prices fall most at-scale among crops with higher pre-existing usage of modern technology, and farmers planting these crops gain more in the local intervention (and relatively lose more at scale).

5.2 GE Forces as a Function of the Intervention’s Scale

Experimental approaches to capturing GE effects often employ “randomized saturation” designs, in which the fraction of individuals treated is randomized across geographic areas or “clusters” in order to study the market-level outcomes that emerge (see e.g. Baird et al. (2011); Burke et al. (2019); Egger et al. (2022)). Here we present results on how the GE effects in our context evolve as the intervention is scaled up to an increasingly large fraction of households and as the geographic scale of the cluster is varied. Both have implications for the optimal design and lessons that can be learned from randomized saturation approaches.

Panel A of Figure 6 presents the welfare impact of the subsidy on the original local farmer sample as a function of the nationwide fraction of the rural population that is also treated. The left-most point on the x-axis corresponds to the local intervention, where only parish-level samples of 2.5% of the local population are treated. The right-most point on the x-axis corresponds to the at-scale intervention where 100% of rural Ugandan households receive the subsidy treatment. The point estimates going from left to right plot the average treatment effect on the same initial 2.5% household sample across increases in the national saturation rate in steps of 10 percentage points of the rural population.\(^{35}\)

The left figure in Panel A traces the average welfare impact, while the right figure displays the average effect separately for the bottom and top quintiles of the initial land income shares. The main insight that emerges is that the extent of GE forces appears to be a monotonic and roughly linear function of the national saturation rate, both for the average effect in the left figure and the

\(^{35}\)We solve for counterfactual outcomes after randomly selecting additional fractions of households within all parishes in increments of 10% until reaching full saturation. The first 10% national saturation treats an additional 7.5% of the local population in all parishes.
distributional implications of the policy on the right in Panel A. These findings are reassuring, as they would in principle support comparisons between just two discrete levels of saturation, as has become common practice in randomized saturation designs.

That said, Panel A varies the saturation at the national level. In practice, randomized saturation designs typically randomize the saturation within some smaller geographic unit ("cluster"). Panel B of Figure 6 explores the role played by the size of these clusters. To illustrate, we consider the case of a study design that uses subcounties (of which there are 811 in Uganda during our study period) as the unit at which saturation is randomized. These are relatively large geographical units compared to the typical “clusters” in the literature as we discuss below. For example, Egger et al. (2022) randomize treatment saturation at the level of sublocations in Kenya (groups of 10-15 villages), which are smaller than Uganda’s subcounties.

Consider, specifically, a design that randomly selects 51 subcounties in which to implement this design (each randomly picked within one of the 51 districts of Uganda). First, just to demonstrate that these 51 subcounties are not distinct in some important way, we replicate the exercise from Panel A (increasing saturation rates nationwide in increments of 10 percentage points) for this random subset of subcounties; the blue line in Panel B shows results that closely mirror those in Panel A. Next, we consider the more feasible randomized saturation design in which – rather than varying the saturation rate at the national level – the saturation rate is varied at the subcounty level, with the rate of saturation goes from 0% to 100% just within the 51 study subcounties as we move from left to right along the x-axis. Results are presented in orange.

Two main insights emerge from this exercise. First, in contrast to changes in national saturation rates, for which we see the impact of the program decreasing monotonically with scale, we find almost no changes in the average impact of the program as a function of sub-county-level saturation rates, even at 100% saturation within these areas (see left side of Panel B). This means that a design that randomizes the saturation at the subcounty level, even with extreme differences in saturation rates, would not be able to measure GE-driven changes in the average impact across these rates. One might then incorrectly conclude there is no change to the program’s average impact from scaling up. Second, one would also draw the wrong distributional implications from a randomized saturation design at the subcounty level. While at the national level, declines in the average welfare impact are predominantly driven by a reduction in welfare gains among the top quintile of land-rich households, we find weaker reductions among the land-rich and stronger increases in gains among the land-poor as a function of local saturation rates – offsetting one another so that the average effect across farmers is close to constant. The forces behind these trends are that farmers’ crop prices react differently to saturation rates at more or less local geographical scales: increasing nationwide saturation rates has significant implications on output prices (see Table 5), whereas changes in the saturation within sub-county populations has much more muted implications on output prices. As a result, local increases in saturation mainly imply that parts of the land revenue gains are capitalized into the local non-traded factor of production (labor), explaining why averages are close to unaffected,
whereas land-poor farmers gain more (and land-rich farmers lose less) as a function of program saturation compared to nationwide saturation.

These results suggest some caution in extrapolating from the reduced form results observed in a randomized saturation design what welfare impacts would look like under a nationwide program. Even when randomizing saturation at the subcounty level – which in Uganda encompasses on average 32 villages and 30,000 individuals, and therefore is larger than most units used in the existing randomized saturation literature\(^{36}\) – this may still be too “local” in scale, and therefore unable to generate the type of GE forces that would emerge under a nationwide (or e.g. state-level) roll-out. That does not mean these designs are not still very useful for making predictions of impacts at scale, but rather that the variation they generate may need to be combined with approaches such as the one described here in order to make predictions for impacts at national scale (more on this in Section 6).

5.3 The Role of a Granular Economic Geography

We next explore the role of a realistic, granular economic geography that our model is able to capture for the counterfactual analysis. To this end, we compare the effect of the at-scale intervention in our national 2.5% sample of rural households across models with alternative geographies. In the first alternative model, we follow the tradition in CGE analysis and most of macroeconomics, and estimate GE counterfactuals in a single integrated national market – assuming no trade costs for output or inputs within Uganda. In the second alternative model, we instead follow the literature in international trade and assume the Ugandan economy is subject to iceberg (ad valorem) trade costs and structural gravity in a standard Armington model at the level of parish markets trading crops. Except for changing assumptions on the nature of trade frictions and product differentiation in agriculture, we keep the rest of the model and its calibration as in our baseline.

Figure 7 shows the comparison to a single integrated market in the left panel, and the comparison to the Armington model in the right panel. In both graphs, the y-axis displays percentage point differences in the welfare impact of the at-scale intervention between the baseline model minus the effect in the alternative model across the \(\sim 100,000\) households as a function of initial land income shares on the x-axis as before. The dotted red lines indicate the sample average of these differences. On average, the single integrated market would imply a roughly 15% increase of the welfare gains at scale compared to our baseline counterfactual. In terms of distributional implications, the single-market economy would not give rise to the reversal of the policy’s regressivity at scale that we find in our baseline: land-poor households on the left of the x-axis experience lower gains at scale without trading frictions compared to a granular economic geography, whereas land-rich households on the right experience significantly larger gains compared to our preferred approach. Comparing this to the left panel in Figure 3, the single market would capture less than half the GE adjustment on the distributional implications.

\(^{36}\)See e.g. Baird et al. (2011), Burke et al. (2019), Egger et al. (2022).
at scale compared to the local effect. The mechanisms behind these differences are that crop price adjustments are more muted in a single national market place, as world market prices at the border are more binding across markets in the national economy. This decreases the asymmetry between the local intervention (at unchanged initial output prices) and the intervention at scale – benefiting land-rich households at scale whose output prices decrease less compared to a world with a granular economic geography.

The comparison to the Armington model in the right panel of Figure 7 documents that both the average welfare gains as well as the distributional implications meaningfully differ when assuming ad-valorem iceberg trade costs and structural gravity to hold given product differentiation within crops across parish markets. With ad valorem trade costs and assuming product differentiation in agriculture as we typically do for manufacturing varities, the average effect on rural household welfare is roughly one third of the effect in our baseline model, and the distributional effects at scale are significantly shifted against land-rich households in the at-scale intervention.

5.4 Alternative Parameter Values

Appendix figure A.9 presents the counterfactual results for the intervention at scale under alternative parameter assumptions on the supply side ($\kappa$ and $\mu$) and the demand side ($\sigma$). We see that the magnitude of the lower-tier supply elasticity, $\kappa$, is quite important for our estimates. Higher values of $\kappa$ increase the estimated welfare effects at-scale, as farmers are more responsive to price changes in how they allocate their land (upper left panel) across technology choices within a given crop. This may help explain why some RCTs have found larger effects over the long-run, as greater time for adjustment may result in larger elasticities (Bouguen et al., 2019).

Higher values of $\kappa$ also lead to larger differences between the local and at-scale intervention in GE, as others’ being more responsive leads to larger output and factor price changes at scale compared to local intervention (at original prices). This highlights the importance of careful identification of this parameter. Using exogenous variation in prices coming from experiments, as we do here, can increase our confidence in our estimates of this key parameter for a given policy context. This therefore represents an important role that can be played by experiments, a point we return to in Section 6.

Conversely, our estimates are less sensitive to the upper-tier supply elasticity (across crops) or the value of the demand elasticity $\sigma$ (upper right and lower panels). In our setting, cost shares do not differ substantially across crops, and while we find above that crops in GE are affected differently by the subsidy policy, cost share differences remain relatively minor (compared to shifting across production regimes within crops). Similarly, how households trade off these crops in consumption is therefore also less critical for the changes in the policy’s impact locally vs at scale. In other contexts, however, with e.g. different cost shares in production, or with an intervention targeted at one particular crop, both $\mu$ and $\sigma$ could play more important roles in shaping the effects at scale (and their difference relative to the local effects).
6 Discussion

This paper develops a toolkit that can be combined with field and quasi-experiments to investigate GE treatment effects at scale.\textsuperscript{37} We see these two approaches as complementary and hope that, in combination, one can expand what can be learned from (quasi-)experiments or quantitative GE models alone. In the following discussion, we explore some concrete ways in which we view these toolkits as complementary, and then discuss some practical considerations for combining the two approaches.

6.1 Complementary toolkits

What do approaches such as ours bring to experiments? Muralidharan and Niehaus (2017) discuss three ways in which the impact of policies implemented at scale can differ from those measured in small-scale RCTs: (1) GE and spillover effects: factor and output prices or other market-level features may shift in ways that alter treatment effects and their distribution; (2) external validity: treatment heterogeneity may mean that results measured among the study sample differ from those that would be experienced by the broader population; and (3) implementation differences: program logistics may be different at scale, as implementation moves from a researcher-run or pilot program to a large-scale operation run by governments or other big organizations.

Our approach provides a new toolkit to investigate and quantify the first two issues. On GE effects, the quantitative model developed here is explicitly targeted at analyzing how input and output prices adjust – and the resulting ripple effects on factor usage, production, consumption, and ultimately household welfare – when policies are implemented at scale. By simulating effects in the whole population or among areas not in the study sample, this toolkit also speaks to external validity, to the extent that treatment effects and GE forces vary based on dimensions that are modeled in our framework (such as heterogeneity in revenue or consumption impacts driven by variation in initial crop allocations, technology and factor usage in production, expenditure shares in consumption, or trade costs and linkages to other markets). Our approach does not have much to say about the third issue of implementation differences, other than to say that estimates will be more accurate the closer the experiment is to the final at-scale policy.\textsuperscript{38} Our toolkit can also provide guidance for experiments “ex ante” to inform the experimental design and data collection (including questions of stratification and power calculations), as we discuss below.

Conversely, what do smaller-scale experiments bring to quantitative GE models like the one developed in this paper? We see three important roles. The first, which we demonstrate here,

\textsuperscript{37}To this end, we are working on publishing step-by-step user-friendly calibration and solution packages across different commonly used computation programs.

\textsuperscript{38}In principle, one could investigate counterfactuals with alternative assumptions on how implementation at scale may change the direct incidence or take-up of the subsidy, and quantify implications at scale based on those assessments. In practice more research may be needed in this space to learn about such differences (Duflo (2017)).
is to use exogenous variation from RCTs or quasi-experiments to more credibly identify some of the key parameters both on the supply and demand sides of the model. As documented in the previous section, these elasticities matter for the extent and incidence of GE forces at scale. Long-run RCTs are particularly useful here, as they give time for adjustment and are therefore more likely to capture long-run elasticities.

A second benefit from RCTs is that the fieldwork and data collection can provide key moments for the model calibration that are frequently outside the scope of available administrative or other microdata. For example, in our analysis above we brought to bear knowledge of bilateral market-to-market trade flows for trade cost estimation.

A third role for RCTs is model validation. Randomized saturation designs, like the ones explored in the previous section, can be particularly useful here as they can provide empirical counterparts to model-predicted GE forces. Although we show that randomized saturation designs do not necessarily, in reduced form, yield GE impacts at a broader scale of program roll-out (i.e. beyond the level of clusters as defined in the RCT), they can still be useful for estimating “sublocation GE effects” – changes in crop and factor prices and other market-level features driven by local differences in saturation – that can be compared to model-based counterfactuals based on the same geographical clusters to validate features of the model’s parameterization and functional forms in Section 2. Such validation can then lend credibility to predicted effects at a larger geographical scale, at which saturation randomization may not be feasible.

6.2 Combining the toolkits in practice

Assuming one wants to combine an experiment with an approach like ours, how does one go about it in practice? In this section, we detail some practical considerations for both data collection and research design, following the complementarities outlined above.

In terms of data collection, researchers will want to collect data on production and consumption of all major crops, not just those directly targeted by the intervention, as in GE multiple output and factor markets can be affected. These data are crucial for estimating both supply- and demand-side elasticities, as well as for calibrating, for example, cost shares or technology use in production functions across crops. Given that wage effects can play an important role, capturing input expenditures on labor (including own labor) is particularly crucial, albeit often difficult to measure. For the model calibration at scale (capturing the entire country), collecting similar covariates to those included in nationwide administrative datasets (and asking similar questions) can support the extrapolation part of the model calibration (where not all household outcomes in the initial equilibrium may be observed in national census data). Finally, collecting data on market prices and trade flows is useful for estimating trade costs between markets as well as between households and markets. A large literature in international trade and economic geography has documented that (easier-to-observe) freight rates only account for a fraction of overall trading frictions across space (e.g. Allen (2014)). As we lay out in Section 4, knowledge of where trade flows occur, their direction and the market prices at both origin and destination
can be used to estimate trade costs in a theory-consistent way.

Our toolkit also offers guidance in terms of the research design. When randomized saturation designs are planned, researchers can use estimates of parameter values (drawn from our study or others in the literature), to calibrate the model ahead of time in an exercise mimicking a power calculation. Such model-based simulations could inform decisions about, for example, the level at which to randomize saturation, the degree of cross-cluster spillovers or the degree of saturation needed to detect treatment effects on GE outcomes. A calibrated version of our model can also be used for stratification to make the estimated treatment effects representative of the overall population. In particular, our model embraces a number of sources for heterogeneous treatment effects that are not generally included among the standard demographic characteristics used for stratification – such as measures of a market’s trading costs for farmers within the market region or to other destinations (market access/remoteness), differences in regional production functions or household expenditure shares for the same crops. In particular, rather than merely stratifying on a number of factors, our model would allow researchers to stratify on predicted treatment effects (both locally and at scale). Finally, identifying the exact parameters to be used in model estimation ex-ante may point researchers to additional experimental variation that is needed.39

7 Conclusion

Policy interventions aimed at increasing agricultural productivity in developing countries have been a centerpiece in the global fight against poverty. Much of the recent evidence in this space has been based on field experiments and natural experiments, with some well-known limitations that variation from local shocks may not speak to the GE implications once the policy is scaled up to the regional or national level.

In this paper, we develop a rich but tractable quantitative GE model of farmer-level production, consumption and trading. To capture a number of salient features that we document in this context, the model departs from the workhorse “gravity” structure in international trade and economic geography in several dimensions. We then propose a new solution method that allows us to study GE counterfactuals in this rich environment, without imposing infeasible additional data requirements. To showcase our approach, we then bring to bear administrative microdata on household locations, production, consumption and the transportation network within and across local markets to calibrate the model to the roughly 4.5 million households populating Uganda in 2002. We use a combination of existing RCTs and variation from natural experiments to estimate the model’s key parameters.

39For example, even with randomized saturation designs that generate variation in agricultural prices, one may not be able to use this variation to estimate demand for these goods, as many consumers of these products are also producers and therefore price changes can generate changes in income. Separate experiments to identify demand-side elasticities may be needed, such as e.g. the randomized price experiment used in Bergquist and Dinerstein (2020).
We find that the average effect of a subsidy for chemical fertilizers and hybrid seed varieties on rural household real incomes can differ substantially when implemented at scale compared to results from a local intervention that leaves output and factor prices largely unaffected. We show that this difference extends to the policy’s distributional implications, which are regressive according to results from the local intervention, but much less so when implemented at scale. We also use our framework to document a number of new findings about the sign and extent of GE impacts as a function of saturation rates at different geographical scales. We find that while GE forces appear to be a monotonic and roughly linear function of saturation rates within a given geographical area, both their average size and distributional impact depend on the geographical scale at which saturation is being implemented.

The framework we lay out in this paper is aimed at providing a useful toolkit that can be used to complement the empirical findings from experiments and quasi-experiments related to developing country agriculture. While we hope to break new ground in this context, this paper by no means exhausts the interesting dialogue between reduced-form evidence and model-based counterfactuals. For example, from theory to field work that dialogue could be used to inform the design of future RCTs to include data collection targeted at estimating key supply and demand elasticities in a given context. From fieldwork to theory, on the other hand, that dialogue could yield additional results on model validation, with a focus not just on the local effects in a given market place, but also using potential experimental estimates of the GE forces from two-stage cluster randomization designs. These and related questions provide an exciting agenda for future research in this area.

References


8 Figures and Tables

Figures

Figure 1: Ugandan Markets and Transportation Network

The figure displays the location of local parish markets, urban markets, border crossings and the road network in Uganda. See Section 3 for discussion of the data and Section 5 for the counterfactual analysis based this geography.
Figure 2: Difference in the Effect at Scale vs. of Local Intervention

The figure plots distributions of the difference in welfare changes from at-scale versus local interventions in percentage points for the identical representative sample of 100k randomly selected rural households (left panel), and averages across parishes (right panel). Vertical bars indicate mean differences. See Section 5 for discussion.
Figure 3: Distributional Implications

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals.

See Section 5 for discussion.
Figure 5: Effects as a Function of Market Integration

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
Figure 6: GE Forces as a Function of Saturation

Panel A: National Saturation

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
Figure 7: Role of a Granular Geography

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals.

See Section 5 for discussion.
### Tables

**Table 1: Estimation of $\sigma$**

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>OLS (1)</th>
<th>IV (2)</th>
<th>OLS (3)</th>
<th>IV (4)</th>
</tr>
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<td>Log P</td>
<td>-4.8067***</td>
<td>-0.9446</td>
<td>-5.0225***</td>
<td>-1.0020*</td>
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<tr>
<td></td>
<td>(0.2686)</td>
<td>(0.6230)</td>
<td>(0.3362)</td>
<td>(0.5473)</td>
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</table>

Observations: 1,247

| 1st Stage F-Stat | 321 | 659 |

See Section 4 for discussion. Standard errors clustered at level of communities. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

**Table 2: Estimation of $\kappa$**

<table>
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<td>Cross-Section</td>
<td>Panel</td>
<td>Cross-Section</td>
</tr>
<tr>
<td>Treat</td>
<td>-0.75***</td>
<td>0.62*</td>
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<tr>
<td></td>
<td>(0.05)</td>
<td>(0.36)</td>
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<td>Log Input Price</td>
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</tr>
<tr>
<td></td>
<td></td>
<td>(0.49)</td>
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Observations: 63

| 1st Stage F-Stat | 204.57 | 204.51 |

See Section 4 for discussion. Standard errors clustered at level of communities. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
Table 3: Estimation of $\mu$

Panel A: First Stage Regressions. Dependent Variable is $\log(\pi_{ikt})$

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<tr>
<td></td>
<td>log($\pi_{ikt}$)</td>
<td>log($\pi_{ikt}$)</td>
<td>log($\pi_{ikt}$)</td>
<td>log($\pi_{ikt}$)</td>
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<tr>
<td>All Years</td>
<td>All Years</td>
<td>2005-13</td>
<td>2005-13</td>
<td></td>
</tr>
</tbody>
</table>

| IV0                | -0.4644*** | -0.3663** | -0.9344   | -1.8073*  |
|                    | (0.1216)   | (0.1719)  | (0.6402)  | (1.0427)  |

Observations: 27,650 27,647 4,580 4,580
HH-Crop FX: yes yes yes yes
Crop-Year FX: yes . yes .
Region-Crop-Year FX: no yes no yes
Number of clusters: 135 135 92 92

Panel B: Dependent Variable is Log Harvest ($\log(y_{ikt})$)

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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>IV</td>
<td>OLS</td>
<td>IV</td>
<td>OLS</td>
<td>IV</td>
<td>OLS</td>
<td>IV</td>
</tr>
<tr>
<td>All Years</td>
<td>All Years</td>
<td>All Years</td>
<td>All Years</td>
<td>All Years</td>
<td>2005-13</td>
<td>2005-13</td>
<td>2005-13</td>
<td>2005-13</td>
</tr>
</tbody>
</table>

| log$\pi_{ikt}$     | 0.3574*** | 0.7969*   | 0.3569*** | 0.6325    | 0.4146*** | 0.9248*** | 0.4253*** | 0.9038*** |
|                    | (0.0164)  | (0.4221)  | (0.0164)  | (0.4952)  | (0.0341)  | (0.3097)  | (0.0325)  | (0.2163)  |

Observations: 27,966 27,650 27,963 27,647 4,486 4,282 4,480 4,276
HH-Crop FX: yes yes yes yes yes yes yes yes
Crop-Year FX: yes yes . . yes yes yes .
Region-Crop-Year FX: no no yes yes no no yes yes
Number of clusters: 135 135 135 135 95 95 95 95
1st Stage F-Stat: 14.60 4.543 32.81 17.93
### Panel C: Dependent Variable is Log Adjusted Output

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<td>All Years</td>
<td>All Years</td>
<td>All Years</td>
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<td>2005-13</td>
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<td>logπ_{ikt}</td>
<td>0.4108***</td>
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<td>0.4061***</td>
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<td></td>
<td>(0.0358)</td>
<td>(0.5423)</td>
<td>(0.0362)</td>
<td>(0.7251)</td>
<td>(0.0601)</td>
<td>(0.4214)</td>
<td>(0.0620)</td>
<td>(0.3154)</td>
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<td>27,966</td>
<td>27,650</td>
<td>27,963</td>
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<td>4,486</td>
<td>4,282</td>
<td>4,480</td>
<td>4,276</td>
</tr>
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<td>HH-Crop FX</td>
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<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
<td>yes</td>
</tr>
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<td>Crop-Year FX</td>
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<td>yes</td>
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<td>yes</td>
<td>yes</td>
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<td>.</td>
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<tr>
<td>Region-Crop-Year FX</td>
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<td>yes</td>
<td>no</td>
<td>no</td>
<td>yes</td>
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<td>Number of clusters</td>
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<td>135</td>
<td>135</td>
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<td>95</td>
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<td>95</td>
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<tr>
<td>1st Stage F-Stat</td>
<td>14.60</td>
<td>4.543</td>
<td>32.81</td>
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See Section 4 for discussion. Standard errors clustered at level of counties. *** p < 0.01, ** p < 0.05, * p < 0.1
Table 4: Effect on Household Welfare

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>All Households</th>
<th>All Households</th>
<th>Bottom 20%</th>
<th>Bottom 20%</th>
<th>Middle 20%</th>
<th>Middle 20%</th>
<th>Top 20%</th>
<th>Top 20%</th>
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</thead>
<tbody>
<tr>
<td>Welfare At Scale</td>
<td>(0.0653)</td>
<td>(0.0658)</td>
<td>(0.0590)</td>
<td>(0.0807)</td>
<td>(0.0692)</td>
<td>(0.0760)</td>
<td>(0.1141)</td>
<td>(0.0967)</td>
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<tr>
<td>Observations</td>
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<td>104,361</td>
<td>19,829</td>
<td>19,829</td>
<td>19,829</td>
<td>19,829</td>
<td>20,872</td>
<td>20,872</td>
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<tr>
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<td>4502</td>
<td>3577</td>
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<td>4130</td>
<td>4087</td>
<td>4087</td>
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</table>

Standard errors clustered at market-level.

*** p<0.01, ** p<0.05, * p<0.1

The table presents effects from the local and from the intervention at scale for the identical representative sample of 100k randomly selected rural households.

See Section 5 for discussion.
Table 5: Channels

Panel A: Local Effects

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1) Income Local</th>
<th>(2) Wage Local</th>
<th>(3) P_manu Local</th>
<th>(4) P_banana Local</th>
<th>(5) P_bean Local</th>
<th>(6) P_cassava Local</th>
<th>(7) P_coffee Local</th>
<th>(8) P_groundnut Local</th>
<th>(9) P_maize Local</th>
<th>(10) P_millet Local</th>
<th>(11) P_sorghum Local</th>
<th>(12) P_sweetpot Local</th>
</tr>
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<tbody>
<tr>
<td>Effect</td>
<td>4.3325***</td>
<td>0.6550***</td>
<td>0.0000</td>
<td>-0.0481***</td>
<td>-0.0150**</td>
<td>-0.0012***</td>
<td>-0.0260***</td>
<td>-0.5200***</td>
<td>-0.0259***</td>
<td>0.0101***</td>
<td>0.1064***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0648)</td>
<td>(0.0159)</td>
<td>(0.0000)</td>
<td>(0.0038)</td>
<td>(0.0072)</td>
<td>(0.0003)</td>
<td>(0.0085)</td>
<td>(0.0194)</td>
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<td>(0.0008)</td>
<td>(0.0038)</td>
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<tr>
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<td>104,361</td>
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<td>104,361</td>
<td>104,361</td>
<td>104,361</td>
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<td>104,361</td>
<td>104,361</td>
<td>104,361</td>
<td>104,361</td>
</tr>
<tr>
<td>No Clusters</td>
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<td>4502</td>
<td>4502</td>
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<td>4502</td>
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</table>

Panel B: At-Scale Effects

<table>
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<tr>
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<th>(1) Income At Scale</th>
<th>(2) Wage At Scale</th>
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<th>(4) P_banana At Scale</th>
<th>(5) P_bean At Scale</th>
<th>(6) P_cassava At Scale</th>
<th>(7) P_coffee At Scale</th>
<th>(8) P_groundnut At Scale</th>
<th>(9) P_maize At Scale</th>
<th>(10) P_millet At Scale</th>
<th>(11) P_sorghum At Scale</th>
<th>(12) P_sweetpot At Scale</th>
</tr>
</thead>
<tbody>
<tr>
<td>Effect</td>
<td>3.4595***</td>
<td>2.8838***</td>
<td>0.9739***</td>
<td>-0.1075***</td>
<td>-0.3004***</td>
<td>-0.1369***</td>
<td>-0.0069***</td>
<td>0.1102***</td>
<td>-4.4588***</td>
<td>0.2085***</td>
<td>0.0146***</td>
<td>0.8932***</td>
</tr>
<tr>
<td></td>
<td>(0.0698)</td>
<td>(0.0569)</td>
<td>(0.0035)</td>
<td>(0.0053)</td>
<td>(0.0102)</td>
<td>(0.0327)</td>
<td>(0.0006)</td>
<td>(0.0115)</td>
<td>(0.0455)</td>
<td>(0.0072)</td>
<td>(0.0030)</td>
<td>(0.0125)</td>
</tr>
<tr>
<td>Observations</td>
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<td>104,361</td>
<td>104,361</td>
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<td>104,361</td>
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<td>4502</td>
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<td>4502</td>
</tr>
</tbody>
</table>

Standard errors clustered at market-level.

*** p<0.01, ** p<0.05, * p<0.1

The table presents effects from the local and from the intervention at scale for the identical representative sample of 10k randomly selected rural households. See Section 5 for discussion.
9 Appendix

Appendix 1: Stylized Facts

In this section we use the data described in Section 3 to document the empirical context and a number of stylized facts.

Major Crops, Regional Specialization and Price Gaps, Subsistence, Trading and Land Allocations

Appendix Figures A.1, A.2 and Tables A.1-A.5 present a number of basic stylized facts about the empirical context. Unless otherwise stated, these are drawn from the UNPS panel data of farmers. First, Table A.1 documents that the 9 most commonly grown crops (matooke (banana), beans, cassava, coffee, groundnuts, maize, millet, sorghum and sweet potatoes) account for 99 percent of the land allocation for the median farmer in Uganda (and for 86 percent of the aggregate land allocation).

Second, Figure A.1 and Table A.2 document a significant degree of regional specialization in Ugandan agricultural production across regions. Table A.2 provides information that these regional differences translate into meaningful variation in regional market prices across crops: the across-district variation in average crop prices accounts for 20-60 percent of the total variation in observed farm-gate prices.

Third, Table A.3 documents that the majority of all farmers are either net sellers or net buyers, rather than in subsistence, and this holds across each of the 9 major crops. The table also presents evidence that there are significant movements in and out of subsistence, conditional on having observed subsistence at the farmer level in a given season. Fourth, Table A.4 documents that farmers buy and sell their crops mostly in local markets, which in turn are connected to other markets through wholesale traders. Finally, Table A.5 documents that farmers frequently reallocate their land allocations across crops over time.

Product Differentiation Across Farmers

Appendix Table A.6 looks at evidence on product differentiation across farmers. The canonical approach in models of international trade sets focus on trade in manufacturing goods across countries, where CES demand coupled with product differentiation across manufacturing varieties imply that all bilateral trading pairs have non-zero trade flows. In an agricultural setting, however, and focusing on households instead of entire economies, this assumption would likely be stark. Consistent with this, the survey data collected by Bergquist et al. (2022) suggest that less than 5 percent of possible bilateral trading connections report trade flows in either of the crops covered by their dataset (maize and beans). This finding reported in Table A.6 provides corroborating evidence that agricultural crops in the Ugandan empirical setting are unlikely well-
captured by the assumption of product differentiation across farmers who produce the crops. Our solution method will explicitly account for these zero trade flows and allow for endogenous switching on and off of trade flows as a result of treatment at-scale.

**Household Preferences**

Appendix Figure A.2 reports a non-parametric estimate of the household Engel curve for food consumption. We estimate flexible functional forms of the following specification:

\[
\text{FoodShare}_{it} = f(\text{Income}_{it}) + \theta_{mt} + \epsilon_{it}
\]

where \(\theta_{mt}\) is a parish-by-period fixed effect and \(f(\text{Income}_{it})\) is a potentially non-linear function of household \(i\)'s total income in period \(t\). The inclusion of market (parish)-by-period fixed effects implies that we are comparing how the expenditure shares of rich and poor households differ while facing the same set of prices and shopping options. As reported in the figure, the average food consumption share ranges from 60 percent among the poorest households to about 20 percent among the richest households within a given market-by-period cell. In our model, these nonhomothetic preferences will allow for distributional effects due to changing food prices that result from the scaled intervention.

**Nature of Trade Costs**

The magnitude and nature of trade costs between farmers and local markets and across local markets play an important role for the propagation of output and factor price changes between markets along the transportation network. The canonical assumption in models of international trade is that trade costs are charged ad valorem (as a percentage of the transaction price). Ad valorem trade costs have the convenient feature that they enter multiplicatively on a given bilateral route, so that the pass-through of cost shocks at the origin to prices at the destination is complete (the same percentage change in both locations). In contrast, unit trade costs –charged per unit of the good, e.g. per sack or kg of maize– enter additively and have the implication that price pass-through is a decreasing function of the unit trade costs paid on bilateral routes. Market places farther away from the origin of the cost shock experience a lower percentage change in destination prices, as the unit cost makes up a larger fraction of the destination's market price.

To explore the nature of trade costs across Ugandan markets, we replicate results reported in \textit{Bergquist et al.} (2022). Specifically, we estimate:

\[
t_{odkt} = (p_{dkt} - p_{okt}) = \alpha + \beta p_{okt} + \theta_{od} + \phi_t + \epsilon_{odkt}
\]

where \(t_{odkt}\) are per-unit trade costs between origin \(o\) and destination \(d\) for crop \(k\) (maize or beans) observed in month \(t\), \(p_{okt}\) are origin unit prices, \(\theta_{od}\) are origin-by-destination fixed effects, and \(\phi_t\) are month fixed effects. Alternatively, origin-by-destination-by-month fixed effects \((\theta_{odt})\) can be
Following Bergquist et al. (2022), we estimate these specifications conditioning on market pairs for which we observe positive trade flows in a given month. If trade costs include an ad valorem component, we would expect the coefficient $\beta$ to be positive and statistically significant. On the other hand, if trade costs are charged per unit of the shipment (e.g. per sack), we would expect the point estimate of $\beta$ to be close to zero.

One concern when estimating these specifications is that the origin crop price $p_{okt}$ appears both on the left and the right-hand sides of the regression, giving rise to potential correlated measurement errors. This would lead to a mechanical negative bias in the estimate of $\beta$. To address this concern, we also report IV estimation results in which we instrument for the origin price in a given month with the price of the same crop in the same market observed in the previous month.

As reported in Table A.7, we find that $\beta$ is slightly negative and statistically significant in the OLS regressions, but very close to zero and statistically insignificant after addressing the concern of correlated measurement errors in the IV specification. Taken together with existing evidence from field work (e.g. Bergquist and Dinerstein (2020)), these results suggest that trade costs in this empirical setting are best-captured by per-unit additive transportation costs.

**Modern Technology Adoption**

Many policy interventions that are run through agricultural extension programs are aimed at providing access, information, training and/or subsidies for modern technology adoption among farmers. One important question in this context is whether adopting modern production techniques could be captured by a Hicks-neutral productivity shock to the farmers' production functions for a given crop. Alternatively, adopting modern techniques could involve more complicated changes in the production function, affecting the relative cost shares of factors of production, such as land and labor.

To provide some descriptive evidence on this question, we run specifications of the following form:

$$\text{LaborShare}_{ikt} = \alpha + \beta \text{ModernUse}_{ikt} + \theta_m + \phi_k + \gamma_t + \epsilon_{ikt}$$

where $\text{LaborShare}_{ikt}$ is farmer $i$'s the cost share of labor relative to land (including both rents paid and imputed rents) for crop $k$ in season $t$ (there are two main seasons per year), $\text{ModernUse}_{ikt}$ is an indicator whether the farmer uses modern inputs for crop $k$ in season $t$ (defined as chemical fertilizer or hybrid seeds), and $\theta_{mkt}$, $\phi_k$ and $\gamma_t$ are district, crop and season fixed effects. Alternatively, we also include individual farmer fixed effects ($\theta_i$).

As reported in appendix Table A.8, we find that the share of labor costs relative to land costs increases significantly as a function of whether or not the farmer uses modern production techniques. This holds both before and after the inclusion of farmer fixed effects (using variation only within-farmer across crops or over time). These results suggest that modern technology
adoption is unlikely to be well-captured by a simple Hicks-neutral productivity shift in the production function. As a result, interventions at scale that affect the use of modern technologies may also have knock-on effects on local labor demand and wages. Our model will allow for such effects.

Appendix 2: Additional Figures and Tables

Table A.1: Main Crops

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<th>(1) Aggregate</th>
<th>(2) Median</th>
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<td>(0.0078)</td>
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<tr>
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<td>(0.0063)</td>
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<td>(0.0000)</td>
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<td>(0.0000)</td>
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<td>cropID==Millet</td>
<td>0.0315</td>
<td>0.0000</td>
</tr>
<tr>
<td></td>
<td>(0.0021)</td>
<td>(0.0000)</td>
</tr>
<tr>
<td>cropID==Sorghum</td>
<td>0.0524</td>
<td>0.0000</td>
</tr>
<tr>
<td></td>
<td>(0.0037)</td>
<td>(0.0000)</td>
</tr>
<tr>
<td>cropID==Sweet Potatoes</td>
<td>0.0886</td>
<td>0.0259</td>
</tr>
<tr>
<td></td>
<td>(0.0061)</td>
<td>(0.0070)</td>
</tr>
</tbody>
</table>

Observations 45 45
Total Share 0.859 0.986

*** p<0.01, ** p<0.05, * p<0.1

Aggregate and median shares for each of the 9 crops are computed for each of four years of data from the UNPS. The table reports the means and standard deviations across the 4 rounds of data. See Section 3 for discussion of the data.
The figure displays the crop with the highest land allocation in each Ugandan district. We use the UNPS data to compute the mean of each crop's land shares across 4 rounds of data. See Section 3 for discussion of the data.
Table A.2: Regional Price Gaps

<table>
<thead>
<tr>
<th>Crop</th>
<th>District Dummies</th>
<th>Urban dummy</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>F-statistic</td>
<td>Adjusted R-sq</td>
</tr>
<tr>
<td>Maize</td>
<td>6.83***</td>
<td>0.29</td>
</tr>
<tr>
<td>Millet</td>
<td>2.59***</td>
<td>0.36</td>
</tr>
<tr>
<td>Sorghum</td>
<td>2.71***</td>
<td>0.30</td>
</tr>
<tr>
<td>Cassava</td>
<td>5.68***</td>
<td>0.22</td>
</tr>
<tr>
<td>Beans</td>
<td>4.75***</td>
<td>0.29</td>
</tr>
<tr>
<td>Groundnuts</td>
<td>2.22***</td>
<td>0.26</td>
</tr>
<tr>
<td>Simsim</td>
<td>3.69***</td>
<td>0.19</td>
</tr>
<tr>
<td>Sweet Potatoes</td>
<td>7.95***</td>
<td>0.33</td>
</tr>
<tr>
<td>Banana</td>
<td>4.10***</td>
<td>0.13</td>
</tr>
<tr>
<td>Coffee</td>
<td>5.65***</td>
<td>0.62</td>
</tr>
<tr>
<td>District FE</td>
<td>yes</td>
<td>yes</td>
</tr>
</tbody>
</table>

See Section 3 for discussion of the data.
Table A.3: Farmer Trading vs Subsistence

Panel A

<table>
<thead>
<tr>
<th>Crop</th>
<th>Subsistence</th>
<th>Net buyer</th>
<th>Net seller</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Maize</td>
<td>33.65</td>
<td>22.50</td>
<td>43.85</td>
<td>1,049</td>
</tr>
<tr>
<td>Millet</td>
<td>31.12</td>
<td>38.07</td>
<td>30.82</td>
<td>331</td>
</tr>
<tr>
<td>Sorghum</td>
<td>31.02</td>
<td>34.98</td>
<td>33.99</td>
<td>303</td>
</tr>
<tr>
<td>Beans</td>
<td>44.87</td>
<td>10.73</td>
<td>44.40</td>
<td>1,081</td>
</tr>
<tr>
<td>Groundnuts</td>
<td>32.38</td>
<td>22.61</td>
<td>45.01</td>
<td>491</td>
</tr>
<tr>
<td>Simsim</td>
<td>25.47</td>
<td>26.71</td>
<td>47.83</td>
<td>161</td>
</tr>
<tr>
<td>Sweet Potatoes</td>
<td>21.60</td>
<td>63.03</td>
<td>15.37</td>
<td>898</td>
</tr>
<tr>
<td>Cassava</td>
<td>43.91</td>
<td>33.54</td>
<td>22.56</td>
<td>1,157</td>
</tr>
<tr>
<td>Banana</td>
<td>44.11</td>
<td>15.71</td>
<td>40.18</td>
<td>764</td>
</tr>
<tr>
<td>Coffee</td>
<td>0.97</td>
<td>9.95</td>
<td>89.08</td>
<td>412</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>34.27</strong></td>
<td><strong>27.88</strong></td>
<td><strong>37.85</strong></td>
<td><strong>6,647</strong></td>
</tr>
</tbody>
</table>

Panel B

<table>
<thead>
<tr>
<th>Year</th>
<th>Subsistence to Trade</th>
<th>Trade to Subsistence</th>
</tr>
</thead>
<tbody>
<tr>
<td>2009</td>
<td>24.90</td>
<td>38.83</td>
</tr>
<tr>
<td>2010</td>
<td>22.38</td>
<td>30.65</td>
</tr>
<tr>
<td>2011</td>
<td>24.61</td>
<td>31.32</td>
</tr>
<tr>
<td>2013</td>
<td>21.28</td>
<td>39.53</td>
</tr>
<tr>
<td><strong>Average</strong></td>
<td><strong>23.35</strong></td>
<td><strong>35.00</strong></td>
</tr>
</tbody>
</table>

See Section 3 for discussion of the data.
Table A.4: Farmers Sell Their Crops to Local Markets

<table>
<thead>
<tr>
<th>Selling Mode</th>
<th>Count_in_1000</th>
<th>Share</th>
</tr>
</thead>
<tbody>
<tr>
<td>Government/LC</td>
<td>285.8</td>
<td>0.00400</td>
</tr>
<tr>
<td>Private trader in local village/market</td>
<td>44269</td>
<td>0.672</td>
</tr>
<tr>
<td>Private trader in district market</td>
<td>7081</td>
<td>0.107</td>
</tr>
<tr>
<td>Consumer at market</td>
<td>9744</td>
<td>0.148</td>
</tr>
<tr>
<td>Neighbor/ Relative</td>
<td>3907</td>
<td>0.0590</td>
</tr>
<tr>
<td>Other (specify)</td>
<td>610.6</td>
<td>0.00900</td>
</tr>
<tr>
<td>Total</td>
<td>65898</td>
<td>1</td>
</tr>
</tbody>
</table>

See Section 3 for discussion of the data.
Table A.5: Farmers Re-Allocate Their Land Across Crops Over Time

Panel A

<table>
<thead>
<tr>
<th>Crop</th>
<th>Entry rate</th>
<th>Exit rate</th>
</tr>
</thead>
<tbody>
<tr>
<td>Maize</td>
<td>46.79</td>
<td>16.98</td>
</tr>
<tr>
<td>Millet</td>
<td>13.03</td>
<td>42.21</td>
</tr>
<tr>
<td>Sorghum</td>
<td>7.87</td>
<td>45.30</td>
</tr>
<tr>
<td>Beans</td>
<td>34.10</td>
<td>9.78</td>
</tr>
<tr>
<td>Groundnuts</td>
<td>19.01</td>
<td>42.59</td>
</tr>
<tr>
<td>Simsim</td>
<td>5.07</td>
<td>45.19</td>
</tr>
<tr>
<td>Sweet Potatoes</td>
<td>37.39</td>
<td>31.07</td>
</tr>
<tr>
<td>Cassava</td>
<td>44.85</td>
<td>17.10</td>
</tr>
<tr>
<td>Banana Food</td>
<td>17.69</td>
<td>11.18</td>
</tr>
<tr>
<td>Coffee</td>
<td>9.66</td>
<td>18.84</td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td><strong>17.53</strong></td>
<td><strong>22.47</strong></td>
</tr>
</tbody>
</table>

Panel B

See Section 3 for discussion of the data.
Table A.6: Product Differentiation (Missing Trade Flows)

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Proportion_Trading</td>
<td>0.0429***</td>
<td>0.0432***</td>
</tr>
<tr>
<td></td>
<td>(0.0021)</td>
<td>(0.0021)</td>
</tr>
<tr>
<td>Observations</td>
<td>9,146</td>
<td>9,146</td>
</tr>
</tbody>
</table>

*** p<0.01, ** p<0.05, * p<0.1

See Section 3 for discussion of the data.

Figure A.2: Household Preferences (Non-Homotheticity)

See Section 3 for discussion of the data.
### Table A.7: Nature of Trade Costs

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS</td>
<td>OLS</td>
<td>IV (Lagged Price)</td>
<td>IV (Lagged Price)</td>
</tr>
<tr>
<td>Origin Price</td>
<td>-0.0605***</td>
<td>-0.0419**</td>
<td>-0.0081</td>
<td>-0.0002</td>
</tr>
<tr>
<td></td>
<td>(0.0188)</td>
<td>(0.0206)</td>
<td>(0.0256)</td>
<td>(0.0274)</td>
</tr>
<tr>
<td>Observations</td>
<td>8,524</td>
<td>8,430</td>
<td>7,153</td>
<td>7,079</td>
</tr>
<tr>
<td>Pair FX</td>
<td>yes</td>
<td>.</td>
<td>yes</td>
<td>.</td>
</tr>
<tr>
<td>Month FX</td>
<td>yes</td>
<td>.</td>
<td>yes</td>
<td>.</td>
</tr>
<tr>
<td>Pair-by-Month FX</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>yes</td>
</tr>
</tbody>
</table>

Standard errors clustered at level of bilateral pairs.

*** p<0.01, ** p<0.05, * p<0.1

See Section 3 for discussion of the data.
### Table A.8: Technology Adoption and Production Cost Shares

<table>
<thead>
<tr>
<th>VARIABLES</th>
<th>(1)</th>
<th>(2)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Use Modern</td>
<td>0.1056***</td>
<td>0.0423***</td>
</tr>
<tr>
<td></td>
<td>(0.0126)</td>
<td>(0.0112)</td>
</tr>
<tr>
<td>Observations</td>
<td>26,037</td>
<td>25,889</td>
</tr>
<tr>
<td>District FX</td>
<td>yes</td>
<td>.</td>
</tr>
<tr>
<td>Crop FX</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Season FX</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>Farmer FX</td>
<td>no</td>
<td>yes</td>
</tr>
</tbody>
</table>

Standard errors clustered at level of farmers.

*** p<0.01, ** p<0.05, * p<0.1

See Section 3 for discussion of the data.
Table A.9: Calibrated Cost Shares in Production

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Traditional</td>
<td>Traditional</td>
<td>Traditional</td>
<td>Modern</td>
<td>Modern</td>
<td>Modern</td>
</tr>
<tr>
<td>cropID1==Beans</td>
<td>0.5107</td>
<td>0.4893</td>
<td>0.0000</td>
<td>0.4607</td>
<td>0.3852</td>
<td>0.1541</td>
</tr>
<tr>
<td></td>
<td>(0.0259)</td>
<td>(0.0259)</td>
<td>(0.0000)</td>
<td>(0.0041)</td>
<td>(0.0139)</td>
<td>(0.0154)</td>
</tr>
<tr>
<td>cropID1==Cassava</td>
<td>0.5566</td>
<td>0.4434</td>
<td>0.0000</td>
<td>0.4429</td>
<td>0.3785</td>
<td>0.1786</td>
</tr>
<tr>
<td></td>
<td>(0.0503)</td>
<td>(0.0503)</td>
<td>(0.0000)</td>
<td>(0.0180)</td>
<td>(0.0187)</td>
<td>(0.0176)</td>
</tr>
<tr>
<td>cropID1==Coffee</td>
<td>0.6777</td>
<td>0.3223</td>
<td>0.0000</td>
<td>0.5428</td>
<td>0.2683</td>
<td>0.1889</td>
</tr>
<tr>
<td></td>
<td>(0.0571)</td>
<td>(0.0571)</td>
<td>(0.0000)</td>
<td>(0.0164)</td>
<td>(0.0202)</td>
<td>(0.0122)</td>
</tr>
<tr>
<td>cropID1==Groundnuts</td>
<td>0.5134</td>
<td>0.4866</td>
<td>0.0000</td>
<td>0.4204</td>
<td>0.4253</td>
<td>0.1543</td>
</tr>
<tr>
<td></td>
<td>(0.0231)</td>
<td>(0.0231)</td>
<td>(0.0000)</td>
<td>(0.0190)</td>
<td>(0.0450)</td>
<td>(0.0271)</td>
</tr>
<tr>
<td>cropID1==Maize</td>
<td>0.5000</td>
<td>0.5000</td>
<td>0.0000</td>
<td>0.4153</td>
<td>0.4335</td>
<td>0.1512</td>
</tr>
<tr>
<td></td>
<td>(0.0272)</td>
<td>(0.0272)</td>
<td>(0.0000)</td>
<td>(0.0520)</td>
<td>(0.0559)</td>
<td>(0.0159)</td>
</tr>
<tr>
<td>cropID1==Matooke</td>
<td>0.6343</td>
<td>0.3657</td>
<td>0.0000</td>
<td>0.6180</td>
<td>0.2564</td>
<td>0.1256</td>
</tr>
<tr>
<td></td>
<td>(0.0455)</td>
<td>(0.0455)</td>
<td>(0.0000)</td>
<td>(0.0394)</td>
<td>(0.0275)</td>
<td>(0.0119)</td>
</tr>
<tr>
<td>cropID1==Millet</td>
<td>0.5285</td>
<td>0.4715</td>
<td>0.0000</td>
<td>0.5485</td>
<td>0.3381</td>
<td>0.1134</td>
</tr>
<tr>
<td></td>
<td>(0.0174)</td>
<td>(0.0174)</td>
<td>(0.0000)</td>
<td>(0.0074)</td>
<td>(0.0039)</td>
<td>(0.0035)</td>
</tr>
<tr>
<td>cropID1==Sorghum</td>
<td>0.5563</td>
<td>0.4437</td>
<td>0.0000</td>
<td>0.5774</td>
<td>0.3321</td>
<td>0.0905</td>
</tr>
<tr>
<td></td>
<td>(0.0216)</td>
<td>(0.0216)</td>
<td>(0.0000)</td>
<td>(0.0062)</td>
<td>(0.0060)</td>
<td>(0.0051)</td>
</tr>
<tr>
<td>cropID1==Sweet Potatoes</td>
<td>0.5088</td>
<td>0.4912</td>
<td>0.0000</td>
<td>0.4721</td>
<td>0.3642</td>
<td>0.1637</td>
</tr>
<tr>
<td></td>
<td>(0.0258)</td>
<td>(0.0258)</td>
<td>(0.0000)</td>
<td>(0.0735)</td>
<td>(0.0800)</td>
<td>(0.0107)</td>
</tr>
</tbody>
</table>

See Section 4 for discussion and Section 3 for description of the data.
Figure A.3: Relative World Price Changes Over the Sample Period

See Section 4 for discussion of the data.
Table A.10: Predicted Local Trade Costs and Measures of Remoteness

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Transport Cost</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>per unit</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distance to Community Road</td>
<td>0.358**</td>
<td>0.503***</td>
<td>2.001***</td>
<td>3.859***</td>
<td>6.120***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.181)</td>
<td>(0.135)</td>
<td>(0.635)</td>
<td>(0.975)</td>
<td>(2.344)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>544</td>
<td>6,331</td>
<td>5,460</td>
<td>2,282</td>
<td>805</td>
<td></td>
</tr>
</tbody>
</table>

**Crop Trade Costs**

|                      |              |            |            |            |            |            |
|                      |              |            |            |            |            |            |
| Predicted $t_{100}$  |              |            |            |            |            |            |
|                      | 0.358**      | 0.503***   | 2.001***   | 3.859***   | 6.120***   |            |
|                      | (0.181)      | (0.135)    | (0.635)    | (0.975)    | (2.344)    |            |
| Observations         | 544          | 6,331      | 5,460      | 2,282      | 805        |            |

**Labor Trade Costs**

|                      |              |            |            |            |            |            |
|                      |              |            |            |            |            |            |
| Predicted $t_{100}$  |              |            |            |            |            |            |
|                      | 0.024        | 0.093      | 0.092      | -0.015     | -0.061***  |            |
|                      | (0.022)      | (0.068)    | (0.168)    | (0.369)    | (0.005)    |            |
| Observations         | 6,317        | 5,448      | 2,275      | 803        | 7,853      |            |

See Section 4 for discussion. All distances are measured in km. Mean share of HHs hiring-in labor is 42% outside estimation sample.

Standard errors clustered at level of households. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
Figure A.5: Wage and Land Income Effects (Without Income-Share Weights)

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
Figure A.8: Effects as a Function of Initial Crop Shares

The figure plots estimates from local polynomial regressions. Shaded areas indicate 95 percent confidence intervals. See Section 5 for discussion.
Appendix 3: Model and Solution Method

We first present the excess demand functions \( \chi_{j,g} \) used in the text to define the equilibrium, and then we present the excess demand functions for the price discovery step. In the final part, we develop the proof for uniqueness in price discovery for the special case with only iceberg trade costs and no trade with Foreign.
Excess Demand Functions

The excess demand function for farmers $\chi_{i,g} (\bullet)$ are given by

$$
\chi_{i,k} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, \{v_{i,k',\omega'}\}_{k',\omega'}, I_i \right) = \varphi_{i,k} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, I_i \right) I_i - \tilde{\psi}_{i,k} \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) Y_i \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) \quad \forall k \in K_A,
$$

$$
\chi_{i,k} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, \{v_{i,k',\omega'}\}_{k',\omega'}, I_i \right) = \varphi_{i,k} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, I_i \right) I_i \quad k = M,
$$

$$
\chi_{i,n} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, \{v_{i,k',\omega'}\}_{k',\omega'}, I_i \right) = \phi_{i,n} \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) Y_i \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) \quad \forall n \in N_I,
$$

$$
\chi_{i,n} \left( \{a_{i,k'}p_{i,k'}\}_{k'}, \{v_{i,k',\omega'}\}_{k',\omega'}, I_i \right) = \left[ 1 + \phi_{i,n} \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) \right] Y_i \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) - I_i \quad n = L,
$$

where $\tilde{\psi}_{i,k} \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right) \equiv \sum_{\omega} \frac{\psi_{i,k,\omega} \left( \{v_{i,k',\omega'}\}_{k',\omega'} \right)}{1 - \sum_{\omega} \alpha_{i,n,k,\omega}}$ is output (in value) of crop $k$ per unit of land returns by farmer $i$.

Similarly, the excess demand functions for urban households $\chi_{h,g} (\bullet)$ are given by

$$
\chi_{h,k} \left( \{a_{h,k'}p_{h,k'}\}_{k'}, \{v_{h,k',\omega'}\}_{k',\omega'}, I_h \right) = \varphi_{h,k} \left( \{a_{h,k'}p_{h,k'}\}_{k'}, I_h \right) I_h \quad \forall k \in K_A,
$$

$$
\chi_{h,k} \left( \{a_{h,k'}p_{h,k'}\}_{k'}, \{v_{h,k',\omega'}\}_{k',\omega'}, I_h \right) = \left[ \varphi_{h,k} \left( \{a_{h,k'}p_{h,k'}\}_{k'}, I_h \right) - 1 \right] I_h \quad k = M,
$$

$$
\chi_{h,k} \left( \{a_{h,k'}p_{h,k'}\}_{k'}, \{v_{h,k',\omega'}\}_{k',\omega'}, I_h \right) = 0 \quad \forall n \in N_I.
$$

We include $\{v_{h,k',\omega'}\}_{k',\omega'}$ as an argument in $\chi_{h,k} (\bullet)$ so that $\chi_{j,g} \left( \{a_{j,k}p_{j,k}\}_{k}, \{v_{j,k,\omega}\}_{k,\omega}, I_j \right)$ capture urban households – since the function does not depend on these arguments, there is no need to define them.\(^1\) Finally, for Foreign we have

$$
\chi_{F,g} \left( \{p_{j,g}\}_g \right) = \begin{cases} 
-\infty & \text{if} \quad p_{F,g} < p^*_{F,g} \\
\left( -\infty, \infty \right] & \text{if} \quad p_{F,g} = p^*_{F,g} \\
\infty & \text{if} \quad p_{F,g} > p^*_{F,g}
\end{cases}
$$

Excess demand as functions of data $\mathbb{D}$ and prices $\{p_{j,g}\}$ for farmers and urban households (used for the price discovery step) are given by

$$
\chi_{i,k} \left( \{p_{i,g}\}_g ; \mathbb{D} \right) = \xi_{i,k} I_i - \sum_{\omega} p_{i,k,y_{i,k,\omega}} \quad \forall k \in K_A,
$$

$$
\chi_{i,k} \left( \{p_{i,g}\}_g ; \mathbb{D} \right) = \xi_{i,k} I_i \quad k = M,
$$

$$
\chi_{i,n} \left( \{p_{i,g}\}_g ; \mathbb{D} \right) = \sum_{k,\omega} \alpha_{i,n,k,\omega} p_{i,k,y_{i,k,\omega}} \quad \forall n \in N_I,
$$

\(1\) In parallel to our treatment of land for farmers, we assume that there is no market for household labor in urban areas, and hence the equilibrium system does not have to determine the price of this good.
\[ \chi_{i,n} \left( \{ p_{i,g} \} \; ; \; D \right) = \sum_{k,\omega} \alpha_{i,n,k,\omega} p_{i,k} y_{i,k,\omega} - p_{i,n} L_i \quad n = L, \]

\[ \chi_{h,k} \left( \{ p_{h,g} \} \; ; \; D \right) = \xi_{h,k} I_h \quad \forall k \in K_A, \]

\[ \chi_{h,k} \left( \{ p_{h,g} \} \; ; \; D \right) = (\xi_{h,k} - 1) I_h \quad k = M, \]

\[ \chi_{h,n} \left( \{ p_{h,g} \} \; ; \; D \right) = 0 \quad \forall n \in N_1, \]

\[ \chi_{F,g} \left( \{ p_{j,g} \} \; ; \; D \right) = \begin{cases} -\infty & \text{if } p_{F,g} < p_{F,g}^* \\ -\infty, \infty & \text{if } p_{F,g} = p_{F,g}^* \\ \infty & \text{if } p_{F,g} > p_{F,g}^* \end{cases} \quad \forall g. \]

**Price Discovery**

In this subsection we show that, in the case with only iceberg trade costs (i.e., \( \ell_{o,d,g} = 0 \) for all \( o, d, g \)), the price discovery step described in the previous section is well defined in the sense that there is a unique set of prices \( \{ p_{j,g}, p_{m,g} \} \) that solves the system of equations 17-19 (for a given set of Foreign prices). To do so, we think of that system of equations as characterizing the equilibrium of a competitive exchange economy, and so the goal is to prove that this economy has a unique equilibrium.

We consider an equivalent economy where there is a single market with an expanded set of goods (which we now call varieties) given by

\[ \mathcal{V} \equiv \{ (o, g) \in J \times K \mid y_{o,g} > 0 \}, \]

where \( J \) is the set of all agents excluding Foreign. A variety of good \( g \) produced by agent \( o \) is indexed by \( (o, g) \in J \times K \). Agent \( o \)'s endowment of \( (o, g) \) is \( y_{o,g} \). Naturally, no other agent \( o' \neq o \) has a positive endowment of \( (o, g) \) and so \( y_{o,g} \) is also the total endowment of variety \( (o, g) \) in the economy.

To proceed, we let \( \bar{\tau}_{o,d,g} \) be the minimum cost at which variety \( (o, g) \) can be transported from its origin to destination \( d \).\(^2\) Letting \( p_{o,g} \) denote the price of variety \( (o, g) \in \mathcal{V} \), the price at which agent \( d \) has access to variety \( (o, g) \) is then \( \bar{\tau}_{o,d,g} p_{o,g} \). Letting \( p \equiv \{ p_{o,g} \}_{(o,g) \in \mathcal{V}} \), the excess demand function (in value) for a variety \( (o, g) \in \mathcal{V} \) is given by

\[ X_{o,g}(p) = \sum_{d \in J \setminus \{F\}} X_{d,o,g}(p) - p_{o,g} y_{o,g}, \]

\(^2\)Formally,

\[ \bar{\tau}_{o,d,g} \equiv \min_{\phi \in \Phi_{o,d,g}} \prod_{(o',d') \in \phi} \tau_{o',d',g}, \]

where \( \Phi_{o,d,g} \) is the set of directed paths from \( o \) to \( d \) for good \( g \) and each element \( \phi \in \Phi_{o,d,g} \) is a sequence of edges \( \{(o_n, d_n)\}_{n=1}^N \) such that \( o_1 = o, d_N = d, \) and \( o_{n+1} = d_n \).
where $X_{d,o,g}(\bullet)$ is the expenditure of agent $d$ on variety $(o,g)$. For $d \in \mathcal{J}$, and letting $\xi_{d,g} \in [0,1]$ denote the expenditure share of gross income of agent $d \in \mathcal{J}$ (i.e., $\sum_{g \in \mathcal{K}} p_{d,g} y_{d,g}$) on good $g$, we have

$$X_{d,o,g}(p) \in \begin{cases} [0, \xi_{d,g} I_d] & \text{if } o \in \arg \min_{o' \in \mathcal{J} \cup \mathcal{F}} p_{o',g} \hat{\tau}_{o',d,g} \\ 0 & \text{if } o \notin \arg \min_{o' \in \mathcal{J} \cup \mathcal{F}} p_{o',g} \hat{\tau}_{o',d,g} \end{cases},$$

$$I_d = \sum_{g} p_{d,g} y_{d,g}.$$

In turn, for $d = F$ we have

$$X_{F,o,g}(p) \in \begin{cases} 0 & \text{if } p_{o,g} > p^*_{F,g} \\ [0, \infty[ & \text{if } p_{o,g} = p^*_{F,g} \\ \infty & \text{if } p_{o,g} < p^*_{F,g} \end{cases}.$$

We henceforth follow the convention that $y_{o,g} = 0 \implies p_{o,g} = \infty$ and $X_{d,o,g}(p) = 0$, and also let

$$X_F(p) = \sum_{d \in \mathcal{J}, g} X_{d,F,g}(p)$$

denote the aggregate expenditure on goods from Foreign (imports).

The equilibrium is a set of prices $p$ such that the excess demand (in value) for all varieties in $\mathcal{V}$ is zero,

$$X_{o,g}(p) = 0, \forall (o,g) \in \mathcal{V}. \quad (A.1)$$

We further assume that each agent $j \in \mathcal{J}$ produces at least one good (to ensure positive income) and has a positive expenditure share on each good that it produces:

**Assumption A1**: Endowments and demand.

1. $\sum_{g \in \mathcal{K}} y_{o,g} > 0, \forall o \in \mathcal{J}$.
2. $y_{o,g} > 0 \implies \xi_{o,g} > 0, \forall o \in \mathcal{J}, g \in \mathcal{K}$.

For future purposes, note that the second part of this assumption implies that an increase in any price $p_{o,g'}, (o,g') \in \mathcal{V}$ leads to a strict increase in the value of demand $X_{o,g}(p)$ for any variety $(o,g)$ with $\xi_{o,g} > 0$.

We say that a set of prices $p$ is connected if there is only one trading block, i.e. there is no partition $\{\mathcal{J}_1, \mathcal{J}_2\}$ of $\mathcal{J}$ such that for all $g \in \mathcal{K}$ we have (i) $X_{d,o,g}(p) = X_{o,d,g}(p) = 0, \forall o \in \mathcal{J}_1, d \in \mathcal{J}_2$ (i.e., no trade between the two blocks) and (ii) $X_{F,o,g}(p) = 0, \forall o \in \mathcal{J}_1$ or $X_{F,o,g}(p) = 0, \forall o \in \mathcal{J}_2$.

---

3Recall that the set of goods includes labor, intermediates and crops. Gross income for a household is composed of the value of endowment of crops plus labor income. Subtracting the cost of intermediate goods and labor (as an input) yields disposable income, which is spent on consumption goods.
(i.e., it is not the case that both trade blocks trade with Foreign). Given Assumption A1, we now show that there can be at most one connected $p$ that solves the system of equations A.1. We do so by appealing to the result in Corollary 1 of Berry, Gandhi, and Haile (2013) – henceforth BGH – which states sufficient conditions under which a function is injective on a set. Applying this result to our excess demand function $\{X_{o,g}(p)\}_{o,g}$ over the set of connected $p$, we then get our desired result.

To apply the results of BGH we need to define “good 0,” which is critical for the concept of “connected substitutes.” We do this by considering each variety $(o, g) \in V$ as a regular good and by thinking of the value of imports, $X_F(p)$, as the “demand for good 0.” Trade balance then implies that

$$X_F(p) = -\sum_{o,g} X_{o,g}(p),$$

as in equation (2) of BGH.\(^4\) We next show that Assumptions 1-3 in Corollary 1 of BGH are satisfied in our setting.

Translated to our context and notation, Assumption 1 in BGH states that the set of possible prices $P$ is a Cartesian product.\(^5\) This is immediately satisfied since $X_{o,g}(p)$ is satisfied for all prices $p$ with $p_{o,g} \in [0, \infty]$.

Given that demand is Cobb-Douglas and that higher prices lead to higher income (weakly), it is then easy to verify that import demand, $X_F(p)$, increases weakly with the price of any domestic variety in $V$ while demand for variety $(o, g)$, $X_{o,g}(p)$, increases weakly with the price of any other variety $(o', g') \in V$ with $(o', g') \neq (o, g)$. This shows that varieties in our context are weak substitutes, and hence Assumption 2 in BGH is satisfied.

To verify that Assumption 3 in BGH is satisfied, we use the equivalent condition stated in BGH’s Lemma 1. Translated to our context, this condition states that for any nonempty subset $V_0$ of $V$ either (i) there is a variety $(o, g) \in V_0$ such that $X_F(p)$ increases strictly in $p_{o,g}$ or (ii) there is a variety $(o', g') \in V \setminus V_0$ such that $X_{o',g'}(p)$ increases strictly in $p_{o,g}$. We now show that this condition is satisfied by considering the three possible cases.

First, if there is an agent $o$ and two goods $g$ and $g'$ such that $(o, g) \in V_0$ and $(o, g') \in V \setminus V_0$ then an increase in $p_{o,g}$ leads to an increase in revenues for agent $o$ and an increase in demand for variety $(o, g')$ through an income effect given our Assumption A1.

Second, suppose that for any agent $o$ either all or none of the varieties are in $V_0$ (otherwise we are back to case one just above). Suppose also that there is a variety $(o, g) \in V_0$ and a variety $(o', g') \in V \setminus V_0$ such that agent $o$ purchases good $g'$ from $o'$, i.e. such that $X_{o,o',g}(p) > 0$. In that case, an increase in the price $p_{o,g}$ leads to an increase in revenues for agent $o$ and an increase in demand for variety $(o', g')$ again through an income effect.

Finally, the third case is one where, for any agent $o$, either all or none of the varieties are in $V_0$,

\(^4\)BGH add $+1$ to demand for good “0,” but this does not affect any results nor assumptions on monotonicity.

\(^5\)Here we look at prices, thus reversing all signs of the slopes in BGH, who focus instead on demand shifters (denoted with $x$). Our set $P$ corresponds to the set $X$ in BGH, while the set of all connected prices $P^* \in P$ corresponds to $X^* \subset X$ in BGH.
and where no agent $o$ purchases goods from agents that have varieties outside $\mathcal{V}_0$. As we focus on connected price vectors, this implies that there is non-zero demand for some Foreign good by some agent $o$ that has some varieties $(o, g)$ in $\mathcal{V}_0$. As such, an increase in the price $p_{o,g}$ leads to an increase in the demand for Foreign goods $X_F(p)$. 