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Connecting the Countryside via E-Commerce: Evidence from China†

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This paper estimates the impact of the first nationwide e-commerce expansion program on rural households. To do so, we combine a randomized control trial with new survey and administrative microdata. In contrast to existing case studies, we find little evidence for income gains to rural producers and workers. Instead, the gains are driven by a reduction in cost of living for a minority of rural households that tend to be younger, richer, and in more remote markets. These effects are mainly due to overcoming logistical barriers to e-commerce rather than additional investments to adapt e-commerce to the rural population. (*JEL* I31, L81, O12, O18, P25, P36)

The number of people buying and selling products online in China has grown from practically zero in the year 2000 to more than 400 million by 2015, surpassing the United States as the largest e-commerce market.[1] Most of this growth has taken place in cities, but the Chinese government recently announced the expansion of e-commerce to the countryside as a national policy priority. The objective is to foster rural economic development and reduce the rural-urban economic divide.[2] Other developing countries with large rural populations, such as Egypt, India, and Vietnam, have recently announced similar e-commerce expansion plans.[3]

These policies have been motivated by a growing number of case studies on highly successful "e-commerce villages" that have experienced rapid output growth

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[1] This is in both number of users and total sales. See, e.g., PFSweb (2016) and Statista (2016).

[2] Alleviating poverty through rural e-commerce has featured in the government's *No.1 Central Document* each year since 2014.

[3] See, e.g., Egypt Ministry of Communications and Information Technology 2016, India Ministry of Electronics and Information Technology 2016, Prime Minister of Vietnam 2016, and UNCTAD's new technical assistance platform, "eTrade for All: Unlocking the Potential of E-Commerce in Developing Countries" (UNCTAD 2016).

by selling both agricultural and nonagricultural products to urban markets via e-commerce. One of the most prominent examples is China: by 2018, the largest e-commerce platform, Taobao, had branded more than 3,000 rural marketplaces as "Taobao villages" based on their high concentration of online sales (AliResearch 2018).[4] Inspired by these success stories, much of the current policy focus has been on rural producers. By lowering trade and information costs to urban markets, e-commerce is meant to raise rural incomes through higher demand for local production, better access to inputs, and stronger incentives for rural entrepreneurship. There has been less emphasis on the potential benefits to rural consumers. However, recent descriptive evidence from urban China suggests that e-commerce demand is strongest in smaller and more remote cities, pointing to potentially large consumer gains in rural areas.[5]

The recent growth of e-commerce in a number of rural markets has captured the imagination of policymakers, but important questions remain about whether market integration through online trading platforms can have a broad and significant impact on rural development. There is also little evidence on the characteristics of households and markets that may benefit more or less from e-commerce and on the effectiveness of investments targeted at lifting different types of barriers to rural e-commerce access.[6] To answer these questions, this paper studies the first nationwide e-commerce expansion program. From 2014 to 2018, this program connected more than 40,000 Chinese villages to e-commerce. Our analysis combines a randomized control trial (RCT), which we implemented across villages in collaboration with a large Chinese e-commerce firm, with a new collection of household and store price survey microdata and the universe of transaction records from the firm's internal database.

E-commerce is the ability to buy and sell products through online transactions coupled with transport logistics for local parcel delivery and pickup from producers. Bringing e-commerce to the countryside in developing countries requires more than internet access. The internet is already available in most of the Chinese countryside due to both smartphones and expanding broadband access. Instead, there are two current barriers to rural e-commerce trading, which we refer to as the logistical and the transactional barriers. The logistical barrier relates to the lack of modern commercial parcel delivery services. These providers already operate distribution networks across Chinese cities but have not entered large parts of the countryside. One well-known challenge to rural transport logistics is the so-called "last mile" between urban logistical hubs and small pockets of rural population. The transactional barrier refers to the potential lack of familiarity with navigating online platforms or access to online payment methods that rural households may face. Villagers

[4] See, e.g., World Bank publications by Luo and Niu (2019) and Luo, Wang, and Zhang (2019). E-commerce villages have also received widespread media attention (e.g., "How Is Internet Shopping Changing Rural Villages in China?: Online Shopping in Rural China," *BBC*, 2015, https://www.bbc.co.uk/programmes/p033z2yw/p033yssy; Connor 2016; Freedman 2017; and Weller 2017).

[5] In the United States, the share of e-commerce in 2015 retail sales was about 10–15 percent (Federal Reserve Bank of St. Louis 2016). In China, Dobbs et al. (2013) report this share to be as high as 20–30 percent in smaller cities, and Fan et al. (2018) find this share increases by 1.2 percentage points as city population decreases by 10 percent.

[6] These questions complement the recent literature on the consumer gains from e-commerce in the United States (e.g., Brynjolfsson, Hu, and Smith 2003; Goldmanis et al. 2010; Dolfen et al. 2017).

may also not trust transactions that occur before inspecting the product or without interacting with buyers in person.

To overcome these barriers, the Chinese government recently partnered with a large firm that operates a popular Chinese e-commerce platform. The program aims to invest in the necessary transport logistics to offer e-commerce in rural villages at the same price, convenience, and service quality that buyers and producers face in their county's main city center. To this end, the e-commerce firm builds warehouses as logistical nodes for rural parcel delivery/pickup near the urban center and fully subsidizes transport between the county's city center to and from the participating villages. To address additional transactional barriers specific to the rural population, the program installs an e-commerce terminal in a central village location. A terminal manager employed by the firm is available to assist villagers in buying and selling products through the firm's e-commerce platform. Villagers can pay upon receipt of their products or get paid upon pickup of their shipments in cash at the terminal location. The terminal is available in addition to the platform's online app-based interface for buying and selling.

An advantage of this setting is that we can study the reduction in trading frictions through e-commerce without confounding the counterfactual with the effects of first-time internet access or reductions in transport costs more broadly. The participating villages were already connected to the internet, and the program makes no changes on this front. Furthermore, the program only directly affects trading partners through e-commerce, while other trade costs, for example, to control villages, remain unchanged.[7] The RCT and data analysis that we describe below exploit this empirical setting to provide evidence on the local economic effects of e-commerce trading access on rural households.[8] In addition to evaluating the program's overall impact, we use the features of this setting to provide evidence on the relative importance of trade cost reductions (logistical barrier) and additional investments targeted at adapting e-commerce to the rural population (transactional barrier).

The analysis proceeds in two steps. In the first step, we randomize the arrival of e-commerce across 100 villages in 3 provinces and 8 counties and use our survey microdata to estimate the impact on local economic outcomes. We then bring to bear the firm's internal database covering the universe of transactions for about 12,000 villages in 5 provinces where the program had entered by April 2017. These data allow us to provide additional evidence on a number of questions outside the scope of the fieldwork. In particular, we investigate whether consumption or production-side effects take longer to materialize than the 12-month window we are able to study in the experiment and whether our household survey data may have missed rare but highly successful tail events on the producer side.

We interpret these results through the lens of a simple theoretical framework to quantify their implications for household welfare. We find no evidence of significant

[7] In this way we relate to but also differ from existing literature on the effects of transport cost reductions on rural markets (e.g., van de Walle 2009; Casaburi, Glennerster, and Suri 2013; Asher and Novosad 2020) and on the effects of the internet on rural markets (e.g., Chapman and Slaymaker 2002; Goyal 2010; Forman, Goldfarb, and Greenstein 2012; World Bank 2016). The empirical context and RCT allow us to study a different counterfactual of recent policy interest.

[8] We do not also attempt a social cost-benefit analysis of this program, which would require additional detailed and confidential information on the cost side from both the e-commerce firm and local and national governments, to which we do not have access.

gains or losses on the production and income sides of the local economy. This finding remains when using the firm's database to quantify village out-shipments up to 2.5 years after program arrival and using the universe of transaction records instead of survey samples. Instead, we find that the gains from e-commerce are driven by a reduction in cost of living for retail consumption. This effect is sizable (5 percent) among the group of rural households that are induced to use the new e-commerce option. These users, however, only represent about 15 percent of rural households, which are on average richer, younger, and living in more remote markets. In terms of channels, we find that the gains are concentrated among villages that were not previously serviced by commercial parcel delivery, suggesting the program's effects are mainly due to overcoming the logistical barrier rather than additional investments to lift transactional barriers specific to rural households. Consumer gains are strongest for durable product groups, such as electronics and appliances. We also find suggestive evidence of additional product variety in local stores, from sourcing new products through e-commerce. However, we find no evidence of procompetitive effects on local store prices for preexisting merchandise.

Overall, our findings put into context the transformative effects of e-commerce on rural markets that have been documented in numerous case studies on e-commerce villages in China and elsewhere. Our results suggest that such success stories are not representative of the countryside as a whole and should not be used as a guide to set policy expectations. Adding to this insight the significant heterogeneity that we document on the consumption side, access to e-commerce appears to offer economic gains to certain groups of the rural population and in certain places rather than being broad-based. As this evidence is based on one of the first and so far largest e-commerce expansion policies in the developing world, these findings are particularly relevant for the growing number of governments that have recently announced similar plans using China as a blueprint.[9] In this light, we hope that our work inspires future research aimed at investigating the local factors and potential complementary interventions, such as, for example, business training for e-commerce or access to credit, that enable certain groups and places to reap the gains from trade through e-commerce.

I. Experimental Design and Data

The experiment takes place in eight counties located in Anhui, Henan, and Guizhou provinces. The unit of randomization is the village. For each county, we obtain a list of villages where the firm plans to introduce the e-commerce program. We ask the firm to extend this list by five suitable village candidates in the county that would not have been part of the list in the absence of our research. We then randomly select five control villages and seven to eight treatment villages per county from this extended list. The remaining villages receive the e-commerce program as planned. The full sample in which we collect survey data thus includes 40 control villages and 60 treatment villages, randomly selected from 432 village candidates. Compliance with our assignments is not complete: the program was rolled out in 38

[9] In addition to the country plans discussed above, Thailand's recent "smart village" program has been designed based on field visits to Taobao villages in China ("eCommerce Ministry Touts Taobao Model," *Bangkok Post*, December 24, 2018, https://www.bangkokpost.com/business/1599750/commerce-ministry-touts-taobao-model).

of the 60 treatment villages and in 5 of the 40 control villages. We therefore report both intent-to-treat and treatment-on-treated effects. The main reason for imperfect compliance is that we are able to randomize treatments before the terminal manager applicants receive job offers, and some candidates end up rejecting.[10] Finally, in one of the counties, the local government suspended our team's data collection midway, leaving 4 of the 100 villages without endline data. The online Appendix provides additional details, maps, and descriptive statistics discussed below.

Household Survey Data.—For the baseline survey at the end of 2015 and beginning of 2016, we collect data from 28 households per village. Fourteen of those households are randomly sampled within a 300 meter radius of the planned terminal location ("inner zone"), and 14 households are randomly sampled from other parts of the village ("outer zone"). The second round of data collection occurs one year after the baseline.[11] We collect data from the same households as in the first round and were also able to extend the original sample by ten randomly sampled households within the inner zone. We collect detailed information about household retail consumption expenditures split across nine categories and for production and business inputs. We also collect information on household incomes, hours worked, occupations and sectors of different members, asset ownership, financial accounts, internet use, and migration.

The median age of all household members in the baseline survey is 44, and the median household size is 3. The primary earner is a farmer in 60 percent of households, and 82 percent of them completed at least primary school. Rural households are significantly poorer than in urban China: mean monthly income and retail expenditure per capita are about ¥876 and ¥732 respectively. Eighty percent of primary earners work inside the village. However, on average half of household retail expenditures occur outside the village, requiring a round trip to the nearest township center that takes on average one hour. Close to 40 percent of households report having used the internet, more than 50 percent own smartphones, and close to 30 percent report owning a laptop or personal computer. Almost all households own a television. At the same time, e-commerce penetration is very limited compared to urban regions: both the average share of household retail expenditure on e-commerce deliveries and the share of revenues from online selling in monthly income are less than 1 percent. Neither of these change over time in the endline survey among control villages.

Local Retail Price Survey Data.—We aim to collect 115 price quotes in each village. We sample products across nine retail consumption categories based on expenditure shares of rural households in Anhui and Henan from the 2012 China Family Panel Study (CFPS). We also include production and business inputs. We sample stores to be representative of local retail outlets (stores and market stalls). In villages with few stores, we sample all of them. We sample products within stores to capture a representative selection of locally purchased items within that store and

[10] Incomplete acceptance rates are standard in this setting and unrelated to the experiment (as applicants were unaware).

[11] The fast pace of the program's expansion places bounds on the timing of the endline. Our control villages ranked highly when the firm decided to launch additional waves of program expansion that were rolled out shortly after the endline.

product group. Each price quote is at the barcode-equivalent level when possible (recording brand, product name, packaging type, size, flavor if applicable). In the endline survey, we collect price quotes of the same products and retail outlets. In cases of either store closure or product disappearance, we include a new price quote within the same product category. The median number of sampled stores is three per village. The median floor space is 50 square meters, and the median store has not added new products within the last month.

Firm's Administrative Database.—We complement the survey data with administrative records from two different divisions of the firm covering five provinces (the three RCT provinces plus Guangxi and Yunnan, where the firm was also active). The first database covers the universe of e-commerce purchases made through the program in every participating village from November 2015 to April 2017. The data cover approximately 27.3 million transaction records across 12,000 villages over this period. For each transaction, the database contains information about the product category, number of units, amount paid, and a unique buyer identifier. Given that many villages had already been in operation for several months prior to November 2015, these data cover adjustment periods beyond the 12-months window that our RCT captures: transactions are observed up to 2 years and 4 months post-installation. The second database covers the universe of sales transactions—that is, out-shipments from the villages—through the firm's distribution network for the same universe of roughly 12,000 villages in the 5 provinces from January 2016 to April 2017. For each transaction, the database records the village of origin and the weight of the out-shipment in kilograms. The total number of e-commerce out-shipments over this period is roughly 500,000.

II. Analysis

A. *Evidence from Survey Data*

We run regressions of the following form:

$$y_{hv}^{Post} = \alpha + \beta_1 Treat_v + \gamma y_{hv}^{Pre} + \epsilon_{hv}, \tag{1}$$

where y_{hv} is an outcome of interest for household h living in village v.[12] For outcomes from the retail price data, h indexes individual price quotes or store-level outcomes instead. The variable $Treat_v$ is either an indicator of randomly assigned treatment status when estimating the intent-to-treat effect (ITT) or actual treatment status when estimating the treatment-on-the-treated effect (TOT) and instrumenting with intended treatment. We cluster standard errors at the level of the treatment (village level) and report point estimates both individually and after combining outcomes into category indices following Kling, Liebman, and Katz (2007) (KLK).

Table 1 presents estimation results for the average effects on household consumption (panel A), incomes (panel B) and local retail prices (panel C). Our

[12]While improving precision, none of the significant findings below rely on the inclusion of baseline outcomes y_{hv}^{Pre}.

Table 1—Average Effects

	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
Panel A. Consumption								
	Monthly retail expenditure per capita in renminbi		Has bought something through e-commerce option (yes = 1)		Share of e-commerce option in monthly total retail expenditure		Share of e-commerce option in monthly durables expenditure	
Treat	−22.09	−41.20	0.0484	0.0894	0.00668	0.0124	0.0408	0.0686
	(31.99)	(60.22)	(0.0167)	(0.0268)	(0.00239)	(0.00435)	(0.0160)	(0.0263)
R^2	0.038		0.008		0.006		0.012	
Control mean	592.21		0.0501		0.00277		0.0152	
First-stage F-statistic		44.01		45.31		44.03		52.43
Observations	3,436	3,436	3,518	3,518	3,434	3,434	768	768
Panel A. Consumption (continued)					*Panel B. Nominal incomes*			
	Share of e-commerce option in monthly nondurables expenditure		Consumption effects (KLK index)		Monthly income per capita in renminbi		Income effects (KLK index)	
Treat	0.00538	0.01	0.478	0.885	−7.864	−14.53	−0.0309	−0.0572
	(0.00196)	(0.00356)	(0.0336)	(0.126)	(70.78)	(129.9)	(0.0349)	(0.0646)
R^2	0.003		0.118		0.038		0.002	
Control mean	0.0027		0.00		915.51		0.00	
First-stage F-statistic		44.11		44.94		45.33		45.01
Observations	3,433	3,433	3,539	3,539	3,437	3,437	3,538	3,538
Panel C. Local retail prices								
	log prices		Product replacement dummy		Product addition dummy		Price effects (KLK index)	
Treat	0.0189	0.0352	−0.00392	−0.00747	2.194	4.020	−0.217	−0.389
	(0.0142)	(0.0263)	(0.0300)	(0.0569)	(1.073)	(2.278)	(0.134)	(0.260)
R^2	0.893		0.00		0.277		0.010	
Control mean	1.9813		0.0828		0.626		0.00	
First-stage F-statistic		41.66		39.82		19.69		24.05
Observations	6,877	6,877	8,956	8,956	312	312	343	343

Notes: Table reports point estimates from specification (1). Outcomes in panels A and B are at the household level. KLK consumption index based on 11 variables related to substitution into e-commerce, all entering positively (reducing price index). KLK income index based on 14 variables related to income generation, 13 entering positively and one negatively. In panel C, the first four columns are at the individual product item level. The final four columns are at the store level. KLK retail index based on four store-level variables, with two entering positively (reducing price index) and two negatively. See Section IIA for discussion. Standard errors are clustered at the level of villages.

discussion here focuses on the TOT results. On the consumption side, we find that the program leads to an uptake of on average 9 percent of households using the new e-commerce option in treatment villages compared to control villages. As documented by the nonzero mean among control villages, this effect masks additional uptake due to users in nearby control villages, increasing the effect on uptake to about 14 percent of village households. We further investigate such spillovers at the end of this section. The treatment effect on the new option's share in total household retail expenditure is 1.24 percent for the average village household. Thus, households that report having used the e-commerce option spent on average $0.0124/0.089 = 14.1$ percent of their retail consumption during the past month. We find stronger effects on durables compared to nondurables. For durables, the share of household expenditure is 6.9 percent for the average household, indicating a 45 percent shift in durable consumption to the new e-commerce option among uptaking households.[13] For nondurables, the treatment effect on the share of household retail expenditure is 1 percent for the average household, indicating that ever-users spend on average about 11 percent of total nondurables expenditure on the new e-commerce option. While households do shift part of their expenditures to e-commerce, there are no significant treatment effects on total monthly retail expenditures. The last column of Table 1, panel A, combines 11 outcomes related to substitution into e-commerce into a single index, defined as the equally weighted average of z-scores that are calculated by subtracting the mean and dividing by the standard deviation of the control group. The treatment effect on this index is 0.89 and significant at the 1 percent level.[14]

Table 1, panel B, reports point estimates on incomes per capita that are close to zero and not statistically significant. As above, we also report a single income-related index combining 14 outcomes related to income generation. We find no effects on either annual or monthly incomes, from agricultural or nonagricultural sources, on labor supply as measured by hours worked by the primary (or secondary) earner or on online selling activity, online revenues, sourcing of business inputs, or business creation offline or online. In terms of precision, the ITT point estimate on the income index indicates detectable positive effects down to about 2.6 percent of a standard deviation (one-sided 95 percent CI).

In Table 1, panel C, we find no significant reduction in local store prices for continuing products that we observe in the same local retailer in both baseline and endline data. The point estimate is close to zero and positive and not statistically significant. Given our sampling framework, the unweighted average effect on local retail prices is akin a Laspeyres price index for local retail consumption. We also find no effect when combining four outcomes related to local retail prices and product exit/additions into a single index. We find one piece of evidence suggestive of knock-on effects on preexisting local stores. The effect on the number of new products per store over the past month is four goods and is significant at the 10 percent level.

Heterogeneity.—In Table 2 we explore the heterogeneity of these effects. We begin by investigating the effect of the program as a function of preexisting availability of commercial parcel delivery at the village level. Villages serviced by commercial parcel delivery operators during our baseline survey already had access to local e-commerce deliveries. Interacting the treatment with preexisting parcel delivery status therefore allows us to shed light on the combined effect of removing both logistical and transactional barriers (among villages without preexisting parcel delivery) from the effect of removing only the transactional barrier (adding a terminal interface in villages with preexisting parcel delivery).[15] Next, we investigate heterogeneity across a basic set of household demographics that have been documented in

[13] For households that purchased durables over the past three months, the treatment effect on uptake is 15.3 percent instead of 9 percent. This yields an effect on the average durables consumption share among uptakers of $0.069/0.153 = 45$ percent.

[14] See online Appendix B for details on the KLK indices in Table 1.

[15] The transport subsidy does not affect villages previously serviced by parcel delivery, as logistics operators offered service in a few rural locations at the same rate as elsewhere in the county prior to program entry.

Table 2—Heterogeneity across Households and Villages

	Household has bought something through e-commerce option (yes = 1)		Monthly income per capita in renminbi		log local retail prices	
Type of heterogeneity	ITT	TOT	ITT	TOT	ITT	TOT
Panel A. Village was previously connected to parcel delivery (yes = 1)						
Treat	0.0578	0.106	−15.00	−27.15	0.0114	0.0215
	(0.0188)	(0.0283)	(77.55)	(140.1)	(0.0144)	(0.0273)
Treat × delivery	−0.0606	−0.111	50.17	96.91	0.0417	0.0739
	(0.0253)	(0.0443)	(171.1)	(339.0)	(0.0377)	(0.0572)
First-stage F-statistic		2.682		2.694		17.26
Panel B. Village distance to township center						
Treat	−0.0144	−0.00652	−23.61	−43.80	−0.0219	−0.0322
	(0.0281)	(0.0411)	(181.7)	(289.1)	(0.0375)	(0.0632)
Treat × log dist. township	0.0384	0.0606	0.422	0.422	0.0216	0.0358
	(0.0161)	(0.0223)	(97.49)	(152.0)	(0.0198)	(0.0336)
First-stage F-statistic		15.55		15.66		16.96
Panel C. Primary earner's age						
Treat	0.141	0.223	−136.5	−238.0		
	(0.0505)	(0.0777)	(172.5)	(286.5)		
Treat × age	−0.00172	−0.00251	2.563	4.554		
	(0.000773)	(0.00129)	(2.734)	(4.825)		
First-stage F-statistic		16.04		16.34		
Panel D. Primary earner's education						
Treat	0.0408	0.0979	52.81	119.7		
	(0.0206)	(0.0412)	(83.52)	(195.0)		
Treat × years of education	0.00164	−0.000432	−8.672	−17.80		
	(0.00266)	(0.00504)	(12.14)	(24.03)		
First-stage F-statistic		8.456		8.662		
Panel E. Household income per capita						
Treat	0.00863	0.0220	35.83	59.45		
	(0.0214)	(0.0375)	(96.84)	(165.5)		
Treat × log income	0.00708	0.0120	−9.201	−15.78		
	(0.00327)	(0.00544)	(21.22)	(36.32)		
First-stage F-statistic		22.67		22.57		
Panel F. Household distance to planned terminal						
Treat	0.142	0.227	185.8	400.0		
	(0.0600)	(0.110)	(350.6)	(697.5)		
Treat × log dist. terminal	−0.0177	−0.0264	−36.53	−79.65		
	(0.0100)	(0.0196)	(61.53)	(128.5)		
First-stage F-statistic		9.899		9.325		
Panel G. Combined						
Treat	0.153	0.287	174.5	330.1	−0.0398	−0.0435
	(0.0811)	(0.141)	(329.9)	(612.1)	(0.0362)	(0.0531)
Treat × delivery	−0.0401	−0.106	102.1	253.3	0.0413	0.0517
	(0.0286)	(0.0690)	(121.1)	(308.1)	(0.0361)	(0.0622)
Treat × log dist. township	0.0457	0.0809	−42.86	−93.17	0.0284	0.0380
	(0.0173)	(0.0296)	(58.39)	(128.5)	(0.0188)	(0.0312)
Treat × age	−0.00181	−0.00314	0.587	1.266		
	(0.000775)	(0.00130)	(2.555)	(4.602)		
Treat × years of education	0.000384	−0.00377	−2.230	−1.954		
	(0.00267)	(0.00497)	(10.01)	(21.43)		
Treat × log income	0.00907	0.0162	−8.451	−14.28		
	(0.00339)	(0.00556)	(22.00)	(37.97)		
Treat × log dist. terminal	−0.0248	−0.0411	−16.48	−34.37		
	(0.0109)	(0.0222)	(45.01)	(94.93)		
First-stage F-statistic		0.479		0.419		1.579

Notes: Based on the same samples as Table 1. See Section IIA for discussion. Standard errors are clustered at the village level.

recent studies of internet and e-commerce use in China (respondent age, education, and income per capita) (China Internet Network Information Center 2015a, b). We also consider residential distance to the planned terminal location and a measure of village remoteness (motivated by Fan et al. 2018) based on road travel distance to the nearest township center. One should note that these interaction terms are not causally identified by experimental variation and provide additional suggestive evidence.

We first run regressions in which one characteristic at a time is interacted with the treatment, then a combined regression with all interactions included jointly. On the consumption side, we find that the effect on program uptake is driven by villages that were not initially connected to commercial parcel delivery services. The treatment effect is 10.6 percent among the roughly 85 percent of villages not previously connected to commercial parcel delivery but a relatively precise zero for villages with preexisting parcel delivery. On the production and local retail sides, we find no significant effects in either group of villages, confirming the earlier pooled results.[16] Turning to other potential sources of heterogeneity, we find that younger, richer households that are in closer proximity to the planned terminal and in more remote villages experience stronger uptake on the consumption side. For example, consumption uptake would close to double if average incomes were to double and primary earners were on average ten years younger. Somewhat surprisingly, we find no significant heterogeneity with respect to the years of education.

Spillovers.—We investigate the role of spillovers that could bias findings from the survey data. For example, if trade linkages with surrounding villages are an important driver of the local economy, then the comparison between treated and control villages could miss income or retail price effects. More simply, residents in control villages could use e-commerce terminals in a nearby treated village. To investigate these forces, we follow Miguel and Kremer (2004) and use variation in a village's exposure to other nearby treated villages after controlling for proximity to all villages (see online Appendix C). On the consumption side, we find evidence of positive spillovers from nearby terminals in other villages, as previewed above. In contrast, we find no evidence of cross-village spillovers on retail stores or on the production side. Consistent with the absence of income or price spillovers, we also confirm in microdata from the 2010 census that the fraction of village market access driven by trade with other nearby rural markets is minor (less than 3 percent).[17]

B. *Evidence from Firm Database*

We use the firm's internal transaction database to provide evidence on two questions that are outside the scope of the fieldwork.[18] First, to what extent are consumption and production responses to e-commerce access increasing beyond

[16] In line with the pooled results, online Appendix A reports some evidence that effects on product additions and stores sourcing online are stronger in villages without preexisting parcel delivery.

[17] Given how small villages are compared to cities, and that a small fraction of all villages participate in the program, GE effects on urban centers are unlikely in our setting.

[18] Online Appendix D also uses these data to investigate the representativeness of our RCT village sample and the timing/seasonality of the survey data collection.

our survey's 12-month time window? Second, are our survey data missing rare but highly successful tail events on the production side that could shift the average effect on local household incomes?

To answer these questions, we use the universe of transaction records from 5 provinces and about 12,000 villages that had been treated by April 2017 to estimate the following event study specification:

$$y_{vm} = \theta_v + \delta_m + \sum_{j=-3}^{24} \beta_j MonthsSinceEntry_{jvm} + \epsilon_{vm}, \quad (2)$$

where v indexes villages, δ_m is a set of month fixed effects between November 2015 and April 2017, and θ_v is a village fixed effect. Each observation in equation (2) is a village in a given month. The variable y_{vm} is one of four village-level monthly outcomes: number of buyers, number of purchase transactions, number of out-shipments, and total weight of out-shipments in kilograms. We create a balanced panel in the sense that each of the villages appears once per month in the panel for each of the 18 months for which we have data (16 months in the shipment data). This spans terminal observations of up to 17 months pre-installation for villages connected in April 2017 and up to 28 months post-installation for the earliest villages connected by the program. A negative index j denotes the number of months prior to program entry. A positive j indexes the number of months since the program started operation, so β_0 is a measure of average outcomes for villages during the month of their installation, β_1 captures averages one month after installation, and so on. We assign an index of $j = 24$ to all observations equal to or beyond 24 months after program entry, so β_{24} captures average outcomes among villages that have been in operation for more than two years. Each of the β_0–β_{24} is estimated relative to the omitted category that is the period preceding program entry (zeros by construction since the program did not exist).

Figure 1 presents the event study plots for village-level outcomes on the consumption and production sides. On the consumption side, we find little evidence of increasing uptake past our survey's one-year timeline. Program usage increases rapidly for about two to four months after opening, and then plateaus at around 85 buyers and 280 transactions per month per village. On the production side, we find evidence that the number and total weight of out-shipments increase smoothly over time after program entry and beyond the 12-month window covered in our survey data. The effect increases by roughly 50 percent when comparing the point estimate on the total weight of out-shipments 12 months post-entry to that more than 2 years post-entry. These results suggest that production-side adjustments take longer to fully materialize than our survey's one-year horizon. Despite this positive trend, the average monthly estimated effects at the village level remain small more than two years post implementation at around ten out-shipments with a combined weight of 30 kilograms.

Turning to the second question, our sampling of 38 households per village in the survey data collection may be insufficient to capture rare but very successful events on the production side. To investigate this issue, we use the universe of out-shipments depicted in Figure 1 and make the following assumptions to get an upper-bound estimate for these shipments' potential income creation in the local village economy: we assume (i) that the entire value of these shipments is local

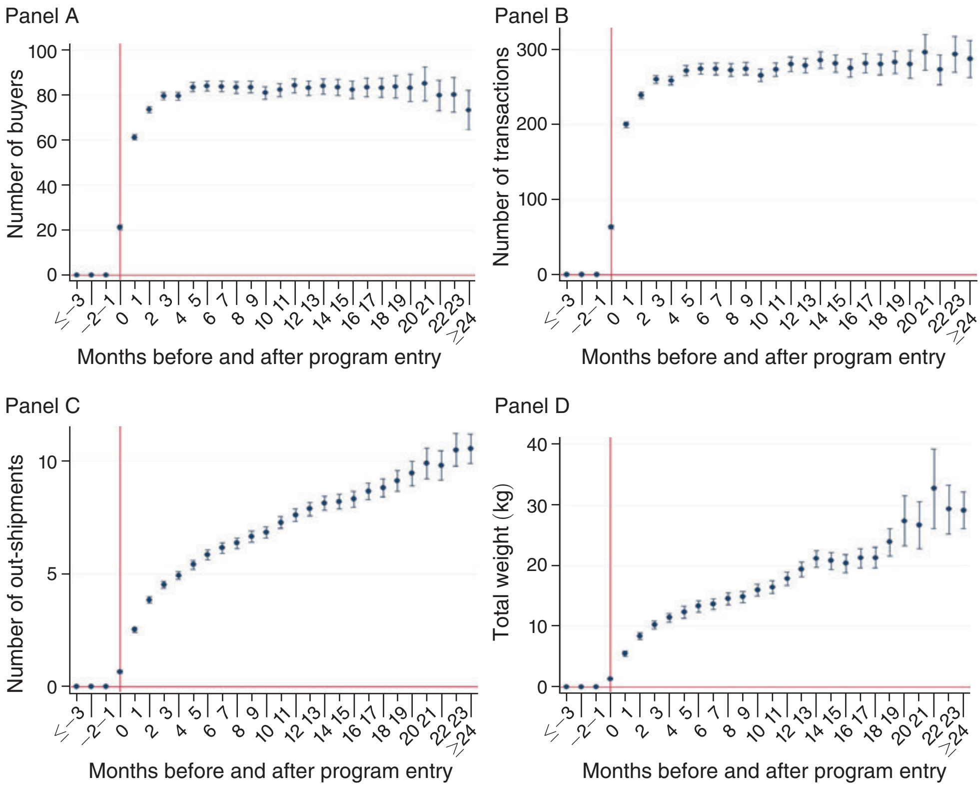

FIGURE 1. TIMELINE OF ADJUSTMENT: VILLAGE E-COMMERCE CONSUMPTION AND OUT-SHIPMENTS

Notes: Figure shows point estimates from a regression of depicted outcomes on months since program entry and village and month fixed effects. Outcomes are the number of buyers (panel A), number of transactions (panel B), number of out-shipments (panel C), and total weight of out-shipments (panel D) per village. The data are from the e-commerce firm's internal database and contain the universe of village purchase transactions from November 2015 to April 2017 and the universe of sales transactions from January 2016 to April 2017 in the five provinces of Anhui, Guangxi, Guizhou, Henan, and Yunnan (roughly 12,000 villages in total). The last point estimate of each plot pools months 24 to 28. The figure shows 95 percent confidence intervals based on standard errors that are clustered at the village level. See Section IIB for discussion.

value added and (ii) that the average value per kilogram of these shipments is as high as that of Chinese exports to the rest of the world.[19] Even under these assumptions, we find that e-commerce out-shipments account for on average at most a 0.17 percent increase in local income per capita more than 2 years after the program's arrival. To conclude, this average longer-term effect—that we can estimate precisely in Figure 1 using the firm's transaction data—would still be consistent with the statistical zero results on incomes and the production side that we find using the RCT survey data after one year.[20]

[19] On average ¥66.50 per kilogram in 2015 and 2016 (World Integrated Trade Solution database).

[20] Related to this, much of the existing literature on information and communication technology in developing countries have estimated effects after relatively short periods. For example, Jensen (2007) documents significant effects of Indian cell phone towers on market prices and other outcomes within weeks post-installation. More recently, Hjort and Poulsen (2019) document effects of fast-speed internet on local employment and incomes in Africa that arise within 3–12 months post-installation.

III. Evaluation

In the final part, we interpret the program's observed effects through the lens of a simple theoretical framework. The most robust effect that we find is on the substitution of local households' retail expenditures to the new e-commerce shopping option. To quantify the cost of living implications consistent with these estimates, we follow a revealed-preference approach as in recent work by Atkin, Faber, and Gonzalez-Navarro (2018) and structure household preferences into three tiers: the upper tier is Cobb-Douglas over broad product groups $g \in G$ (durables and nondurables) in total consumption, the middle tier is CES across retailers $s \in S$ selling that product group (for example, local stores, market stalls, or the e-commerce option), and the final tier is across individual products within groups $b \in B_g$, which can be left unspecified (see online Appendix E for more details). The direct consumer gains from the arrival of the e-commerce option, measured as a percentage of initial household expenditure, can then be expressed as follows:

$$(3) \qquad \frac{Gains_h}{Initial\ Expenditure_h} = \prod_{g \in G} \left(\left(\sum_{s \in S_g^C} \phi_{gsh}^1 \right)^{\frac{1}{\sigma_g - 1}} \right)^{\alpha_{gh}} - 1,$$

where σ_g is the elasticity of substitution across retail options to source consumption in product group g, α_{gh} is the initial expenditure share on that product group for household group h, and $\sum_{s \in S_g^C} \phi_{gsh}^1$ is the share of retail expenditures that is not spent on the new e-commerce option post-intervention (where $s \in S_g^C$ indexes continuing local retailers and ϕ_{gsh}^1 is the endline expenditure share on retailer s in product group g of household group h).

To estimate this expression, we require information about the program's effect on $\sum_{s \in S_g^C} \phi_{gsh}^1$ and the parameters α_{gh} and σ_g. For the α_{gh}, we use our baseline data on household expenditure shares across product groups. For ex post expenditure shares on the new e-commerce option, we use the treatment effects among the 85 percent of villages without preexisting parcel delivery connections reported in Table 2. These villages experienced the removal of both logistical and transactional barriers to e-commerce trading. We include mean program usage among control villages in these treatment effects to account for program spillovers as discussed above.

We perform this welfare computation for two different groups of local households: first for the average sample household, for whom the average effect on the terminal share of total retail consumption is 1.6 percent, and second for households that report ever having used the terminal for consumption, for whom this effect is 14 percent. We also estimate price index effects separately for durable and nondurable consumption. And we report estimates both with and without reweighting households according to sampling weights. Finally, we calibrate σ_g using estimates from Atkin, Faber, and Gonzalez-Navarro (2018) for households in Mexico with incomes comparable to those of rural Chinese households in our survey ($\sigma_N = 3.87$ for nondurables and $\sigma_D = 3.85$ for durables).

Table 3 reports the estimation results. The average reduction in retail cost of living among households that experienced the lifting of both logistical and transactional barriers is 0.82 percent. This effect increases to 5.6 percent among the roughly 15 percent of households that ever used the new e-commerce option. These effects

Table 3—Average Effects on Household Welfare

	Unweighted (effects in sample)			Weighted (effects in village population)		
	Durables consumption	Nondurables consumption	Total retail consumption	Durables consumption	Nondurables consumption	Total retail consumption
Reduction in retail cost of living for all households	3.379%	0.481%	0.824%	2.962%	0.429%	0.73%
	(0.03)	(0.003)	(0.005)	(0.03)	(0.003)	(0.005)
Reduction in retail cost of living among users	19.884%	3.806%	5.597%	16.637%	3.217%	4.722%
	(0.221)	(0.028)	(0.034)	(0.224)	(0.025)	(0.032)

Notes: Table reports average household gains in terms of percentage point reductions in retail cost of living for different consumption categories and groups of households. Estimates are based on equation (3) using treatment effects on household substitution into the new e-commerce option. The left panel reports unweighted results, and the right panel adjusts the weight of each household using sampling weights. Standard errors are bootstrapped across 1,000 iterations, taking into account that the treatment effects are point estimates. See Section III for discussion.

are slightly lower at 0.73 and 4.7 percent respectively when weighting our sample households to represent the average population living in these villages. Underlying these effects are strong consumer gains in durable consumption: 3 percent for the average village household and 16.6 percent among users. For reference, retail consumption across all product groups accounts for on average 55 percent of total household expenditure among the rural households in the sample.[21]

Finally, to investigate the distribution of these gains, we use treatment effects from the joint heterogeneity specification in Table 2, panel G. We estimate this specification with the dependent variable being the household expenditure share on the new e-commerce option for either durables or nondurables. For each sample household in treatment villages without preexisting parcel delivery, we then compute a fitted value of the effect on $\sum_{s \in S_g^C} \phi_{gsh}^{t1}$, based on the primary earner's age, income per capita, residential distance to the planned terminal, and distance to the nearest township center (remoteness), included jointly. Figure 2 shows these graphs. Ranking households along each of these dimensions, we find more than fourfold differences in the price index effect within the sample. For example, the average rural household with a 25-year-old primary earner experiences a reduction in retail cost of living of about 1.5 percent (without conditioning on uptake), which drops below 1 percent past the age of 40 and close to 0 past the age of 60.

Overall, our findings suggest that the welfare gains from e-commerce trading access are limited to certain groups of rural households and particular markets rather than being broad-based. First, we show that the income and production-side effects that have been the focus of the existing literature on e-commerce villages are not representative of the countryside, even when focusing on a sample of rural markets in the RCT that were chosen by the firm for successful e-commerce expansion. Second, we find strong heterogeneity in the consumer gains from e-commerce across villages and households within them. In this light, we hope this work can inspire additional research to investigate what types of local factors or complementary

[21] We also evaluate robustness to alternative σ_g. Assuming $\sigma_N = 2.87$ and $\sigma_D = 2.85$ yields larger gains (a 1.27 percent reduction in retail cost of living on average and 8.74 percent among users). Assuming $\sigma_N = 4.87$ and $\sigma_D = 4.85$ yields slightly smaller effects (0.61 and 4.12 percent, respectively).

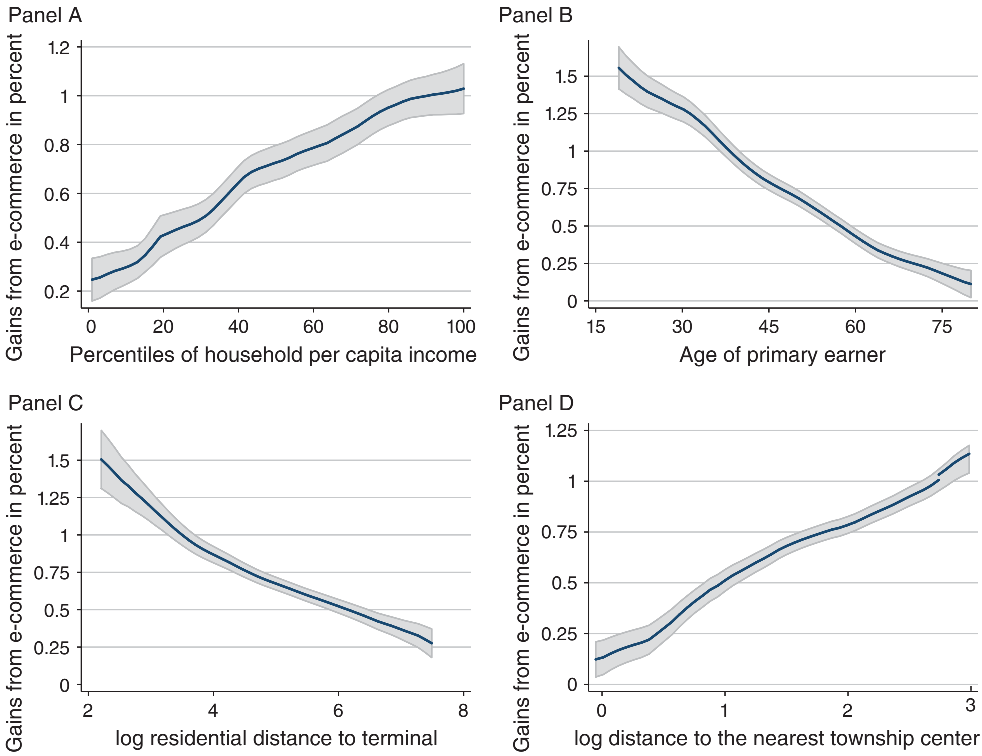

Figure 2. Heterogeneity of Gains from E-Commerce

Notes: Figure shows predicted average gains (users and nonusers) in terms of percentage point reductions in household retail cost of living as a function of household per capita income (panel A), age of primary earner (panel B), residential distance to terminal (panel C), and distance to the nearest township center (panel D). Predictions are based on treatment effects from Table 2, panel G. The figure depicts 95 percent confidence intervals that are based on clustering standard errors at the village level. See Section III for discussion.

interventions allow rural markets to reap the gains from trade through e-commerce for both producers and consumers.

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Online Appendix: Connecting the Countryside via E-Commerce: Evidence from China

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Appenix A presents additional figures and tables. Appendix B describes the construction of the K-L-K outcome indices. Appendix C presents additional analysis on the role of GE spillovers. Appendix D provides additional estimation results using the firm's admin database. Appendix E provides details on the welfare analysis. Appendix F presents details on the program, experimental design, field staff training, quality management and data.

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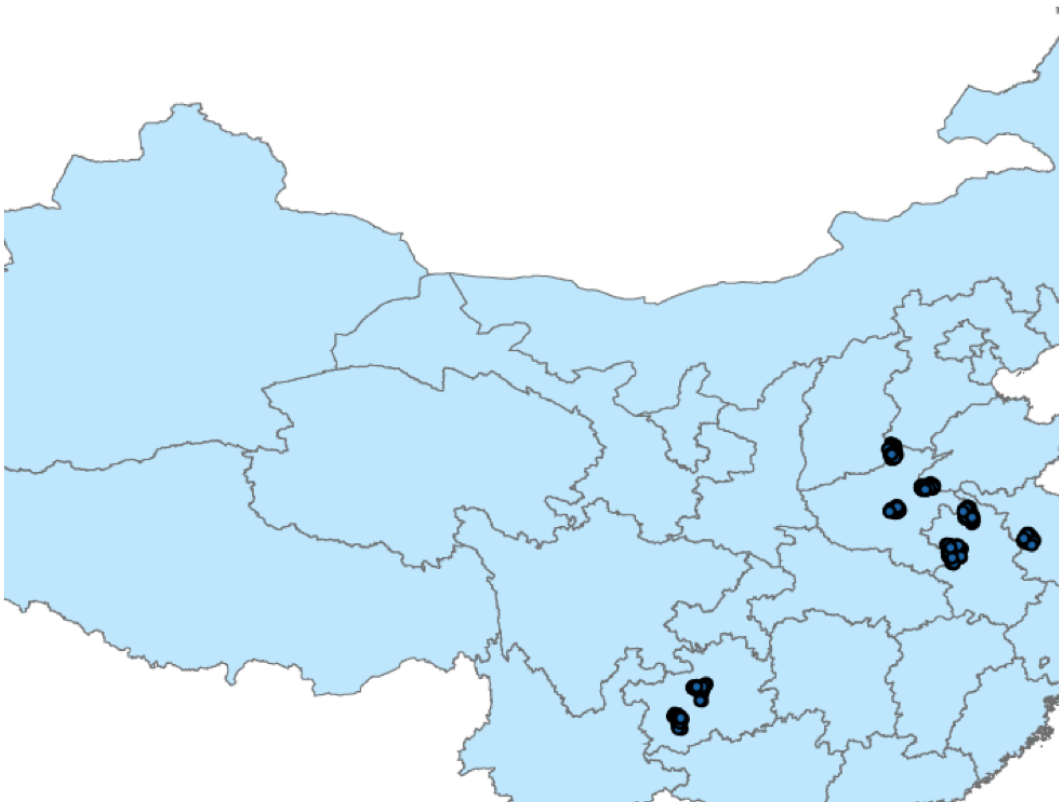
Appendix A: Additional Figures and Tables

Figure A.1: Provinces and Counties Where RCT Was Implemented



Notes: Map shows the location of our eight RCT counties in the three provinces of Anhui, Guizhou and Henan. The dots indicate participating villages and the boundaries indicate Mainland Chinese provinces. Section Design/Data Section and Appendix F for discussion.

Table A.1: Descriptive Statistics: Individual Level

		Full Sample at Baseline	Treatment Villages at Baseline	Control Villages at Baseline	P-Value (Treat-Control=0)	Control Villages at Endline
Age	Median	44.000	44.000	43.000		46.000
	Mean	38.950	39.329	38.407	0.208	39.943
	Standard Deviation	23.580	23.658	23.460		23.759
	Number of Obs	8491	5001	3490		4194
Gender (Female=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.534	0.526	0.546	0.025	0.537
	Standard Deviation	0.499	0.499	0.498		0.499
	Number of Obs	8484	5001	3483		4188
Employed (for age>15) (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.767	0.766	0.769	0.882	0.762
	Standard Deviation	0.423	0.424	0.422		0.426
	Number of Obs	6070	3590	2480		3015
Farmer (for age>15) (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.527	0.527	0.526	0.971	0.513
	Standard Deviation	0.499	0.499	0.499		0.500
	Number of Obs	6369	3760	2609		3144
No Schooling (for age>15) (No School=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.270	0.273	0.266	0.745	0.319
	Standard Deviation	0.444	0.446	0.442		0.466
	Number of Obs	6368	3758	2610		3132
Completed Junior High School (for age>15) (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.437	0.429	0.449	0.419	0.422
	Standard Deviation	0.496	0.495	0.498		0.494
	Number of Obs	6368	3758	2610		3132
Completed Senior High School (for age>18) (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.104	0.104	0.104	0.969	0.097
	Standard Deviation	0.305	0.305	0.305		0.296
	Number of Obs	6286	3719	2567		3096

Notes: See Design/Data Section and Appendix F for discussion.

Table A.2: Descriptive Statistics: Household Level

		Full Sample at Baseline	Treatment Villages at Baseline	Control Villages at Baseline	P-Value (Treat-Control=0)	Control Villages at Endline
Age of Primary Earner	Median	50.000	50.000	50.000		52.000
	Mean	49.824	49.953	49.631	0.634	51.395
	Standard Deviation	12.673	12.710	12.621		13.547
	Number of Obs	2548	1530	1018		1348
Gender of Primary Earner (Female=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.288	0.295	0.276	0.457	0.295
	Standard Deviation	0.453	0.456	0.447		0.456
	Number of Obs	2547	1530	1017		1348
Primary Earner Went to School (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.815	0.814	0.817	0.874	0.750
	Standard Deviation	0.388	0.389	0.386		0.433
	Number of Obs	2550	1531	1019		1342
Primary Earner Is Farmer (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.590	0.600	0.577	0.620	0.587
	Standard Deviation	0.492	0.490	0.494		0.493
	Number of Obs	2549	1531	1018		1348
Primary Earner Self-Employed (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.073	0.087	0.053	0.036	0.072
	Standard Deviation	0.261	0.282	0.224		0.259
	Number of Obs	2549	1531	1018		1348
Household Size	Median	3.000	3.000	3.000		3.000
	Mean	3.114	3.053	3.205	0.075	2.987
	Standard Deviation	1.422	1.420	1.421		1.397
	Number of Obs	2740	1647	1093		1405
Household Monthly Income Per Capita in RMB	Median	350.000	339.000	375.000		466.667
	Mean	876.412	841.198	929.473	0.365	1028.960
	Standard Deviation	1717.456	1687.169	1761.560		2005.311
	Number of Obs	2740	1647	1093		1405
Household Monthly Retail Expenditure Per Capita in RMB	Median	381.000	372.833	400.500		364.000
	Mean	732.017	663.034	835.966	0.135	686.616
	Standard Deviation	2304.540	1139.788	3368.220		1512.058
	Number of Obs	2735	1644	1091		1405
Household Monthly Expenditure on Business Inputs Per Capita in RMB	Median	0.000	0.000	0.000		0.000
	Mean	123.417	123.007	124.033	0.981	128.464
	Standard Deviation	1033.757	1076.656	966.070		1069.516
	Number of Obs	2736	1644	1092		1405
Any Member of the Household Has Ever Used the Internet (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.368	0.354	0.390	0.249	0.427
	Standard Deviation	0.482	0.478	0.488		0.495
	Number of Obs	2739	1646	1093		1402
Household Owns a Smartphone (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.526	0.509	0.552	0.153	0.551
	Standard Deviation	0.499	0.500	0.498		0.498
	Number of Obs	2731	1642	1089		1400

Notes: See Design/Data Section and Appendix F for discussion.

Table A.3: Descriptive Statistics: Household Level – Continued

		Full Sample at Baseline	Treatment Villages at Baseline	Control Villages at Baseline	P-Value (Treat-Control=0)	Control Villages at Endline
Share of Household Monthly Expenditure on E-Commerce Deliveries	Median	0.000	0.000	0.000		0.000
	Mean	0.007	0.006	0.007	0.693	0.008
	Standard Deviation	0.050	0.046	0.057		0.049
	Number of Obs	2720	1637	1083		1397
Share of E-Commerce Sales in Household Monthly Income	Median	0.000	0.000	0.000		0.000
	Mean	0.003	0.001	0.006	0.103	0.003
	Standard Deviation	0.052	0.030	0.074		0.051
	Number of Obs	2055	1244	811		1161
Distance in Meters to Planned Terminal Location	Median	231.556	232.891	231.454		203.629
	Mean	290.346	293.364	285.797	0.789	286.631
	Standard Deviation	243.450	247.778	236.820		267.061
	Number of Obs	2740	1647	1093		1405
Share of Retail Expenditure Outside of Village	Median	0.553	0.489	0.623		0.598
	Mean	0.500	0.470	0.545	0.193	0.531
	Standard Deviation	0.395	0.402	0.379		0.385
	Number of Obs	2720	1637	1083		1397
Share of Business Input Expenditure Outside of Village	Median	1.000	1.000	1.000		1.000
	Mean	0.613	0.610	0.618	0.916	0.633
	Standard Deviation	0.465	0.470	0.457		0.463
	Number of Obs	926	558	368		544
Travel Time One-Way to Main Shopping Destination Outside Village (minutes)	Median	20.000	20.000	20.000		20.000
	Mean	29.892	29.941	29.826	0.962	28.862
	Standard Deviation	27.825	27.380	28.429		26.187
	Number of Obs	2234	1284	950		1188
Travel Cost One-Way to Main Shopping Destination Outside Village (RMB)	Median	2.000	2.000	1.500		1.000
	Mean	3.739	3.847	3.591	0.715	4.236
	Standard Deviation	10.092	11.774	7.196		16.780
	Number of Obs	2216	1278	938		1185
Household Owns a PC or Laptop (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.283	0.276	0.295	0.631	0.284
	Standard Deviation	0.451	0.447	0.456		0.451
	Number of Obs	2731	1642	1089		1400
Household Owns a Car (Yes=1)	Median	0.000	0.000	0.000		0.000
	Mean	0.108	0.107	0.110	0.851	0.131
	Standard Deviation	0.311	0.309	0.313		0.337
	Number of Obs	2731	1642	1089		1400
Household Owns a Motorcycle (Yes=1)	Median	0.000	0.000	1.000		0.000
	Mean	0.486	0.456	0.532	0.031	0.467
	Standard Deviation	0.500	0.498	0.499		0.499
	Number of Obs	2731	1642	1089		1400
Household Owns a TV (Yes=1)	Median	1.000	1.000	1.000		1.000
	Mean	0.977	0.977	0.977	0.953	0.977
	Standard Deviation	0.149	0.148	0.150		0.150
	Number of Obs	2731	1642	1089		1400

Notes: See Design/Data Section and Appendix F for discussion.

Table A.4: Descriptive Statistics: Local Retail Prices

		Full Sample at Baseline	Treatment Villages at Baseline	Control Villages at Baseline	P-Value (Treat-Control=0)	Control Villages at Endline
Number of Stores at Village Level	Median	3.00	3.00	2.00		2.00
	Mean	4.15	4.38	3.79	0.33	3.61
	Standard Deviation	2.94	2.91	2.98		2.99
	Number of Obs	99	60	39		38
Establishment Space in Square Meters	Median	50.00	50.00	40.00		50.00
	Mean	99.07	74.42	146.76	0.35	121.33
	Standard Deviation	320.38	89.60	532.73		375.35
	Number of Obs	361	238	123		126
Number of Establishment's New Products Added Over Last Month	Median	0.00	0.00	0.00		0.00
	Mean	1.43	1.56	1.17	0.57	0.63
	Standard Deviation	7.44	8.88	3.42		2.26
	Number of Obs	330	215	115		126
Prices of All Retail Consumption (9 Product Groups) in RMB	Median	7.00	7.00	6.00		6.00
	Mean	71.03	76.74	61.43	0.47	71.23
	Standard Deviation	411.24	433.67	370.33		390.31
	Number of Obs	9382	5884	3498		3259
Price Was Not Displayed on Label (Needed to Ask=1)	Median	1.00	1.00	1.00		1.00
	Mean	0.67	0.66	0.67	0.97	0.73
	Standard Deviation	0.47	0.47	0.47		0.44
	Number of Obs	8977	5597	3380		3370
Prices of Business or Production Input in RMB	Median	10.00	10.00	8.80		9.00
	Mean	45.63	42.88	49.78	0.76	43.84
	Standard Deviation	195.09	206.23	177.46		97.92
	Number of Obs	444	267	177		111
(1) Prices of Food and Beverages in RMB	Median	4.38	4.60	4.00		4.00
	Mean	11.58	11.81	11.21	0.73	10.05
	Standard Deviation	24.35	23.31	25.99		17.75
	Number of Obs	4853	3021	1832		1834
(2) Prices of Tobacco and Alcohol in RMB	Median	12.00	13.00	12.00		13.00
	Mean	28.81	30.35	26.36	0.46	29.32
	Standard Deviation	53.97	59.45	43.77		55.16
	Number of Obs	1331	818	513		531
(3) Prices of Medicine and Health Products in RMB	Median	10.00	10.00	9.98		8.40
	Mean	26.13	24.40	29.31	0.66	18.50
	Standard Deviation	43.35	38.46	51.11		33.77
	Number of Obs	399	258	141		90
(4) Prices of Clothing and Accessories in RMB	Median	15.00	12.00	20.00		22.00
	Mean	46.31	45.69	47.79	0.90	57.00
	Standard Deviation	74.71	71.49	82.13		85.66
	Number of Obs	401	282	119		65
(5) Prices of Other Everyday Products in RMB	Median	10.00	10.00	9.00		9.00
	Mean	14.68	14.53	14.93	0.93	13.10
	Standard Deviation	31.03	32.69	28.06		18.17
	Number of Obs	1462	916	546		626
(6) Prices of Fuel and Gas in RMB	Median	5.00	5.00	5.00		5.83
	Mean	11.65	15.36	8.08	0.26	5.82
	Standard Deviation	21.46	28.88	9.59		0.23
	Number of Obs	53	26	27		4
(7) Prices of Furniture and Appliances in RMB	Median	110.00	85.00	187.00		398.00
	Mean	1009.49	1001.66	1026.34	0.95	1167.30
	Standard Deviation	1504.81	1583.03	1333.52		1350.70
	Number of Obs	183	125	58		43
(8) Prices of Electronics in RMB	Median	449.00	609.50	17.50		1799.00
	Mean	917.05	976.41	782.14	0.59	1782.71
	Standard Deviation	1224.37	1242.82	1184.20		871.58
	Number of Obs	144	100	44		45
(9) Prices of Transport Equipment in RMB	Median	1440.00	1980.00	30.00		2800.00
	Mean	1700.66	1794.74	1534.21	0.71	2578.24
	Standard Deviation	1822.07	1770.33	1922.34		1697.82
	Number of Obs	108	69	39		21

Notes: See Design/Data Section and Appendix F for discussion.

Table A.5: Descriptive Statistics: Firm's Transaction Data

	Number of Purchase Transactions	Number of Buyers	Number of Out-Shipments	Number of Terminals	Number of Counties	Number of Provinces	Number of Days	Number of Months	Sum of Payments (RMB)	Sum of Out-Shipments (Weight in kg)
Full Sample	27,270,532	3,785,019	500,743	11,941	175	5	547	18	4,480,424,896	1,169,673
3 Provinces	20,647,373	2,832,872	442,319	8,561	116	3	547	18	3,409,227,245	1,019,373
8 Counties	1,835,897	216,529	44,148	706	8	3	503	17	330,930,097	95,908
RCT Villages	130,769	15,099	3,158	40	8	3	482	16	17,618,900	7,817

Notes: The table provides information from the purchase and the sales transaction databases. The purchase database covers all village transactions in 5 provinces over the period November 2015 until April 2017. The sales transaction database covers all out-shipments from the same locations over the period January 2016 to April 2017. See Section Design/Data for discussion.

Table A.6: Average Effects: Consumption

Dependent Variables		Intent to Treat	Treatment on Treated	Log Distance (IV using Treat)
Monthly Total Retail Expenditure Per Capita	Treat or Log Dist	-22.09	-41.20	10.79
		(31.99)	(60.22)	(15.67)
	R-Squared	0.038		
	First Stage F-Stat		44.01	48.31
	Number of Obs	3,436	3,436	3,436
Household Has Ever Bought Something through E-Comm Option (Yes=1)	Treat or Log Dist	0.0484***	0.0894***	-0.0234***
		(0.0167)	(0.0268)	(0.00697)
	R-Squared	0.008		
	First Stage F-Stat		45.31	49.83
	Number of Obs	3,518	3,518	3,518
Household Has Bought Something in Past Month (Yes=1)	Treat or Log Dist	0.0263***	0.0490***	-0.0128***
		(0.00981)	(0.0171)	(0.00445)
	R-Squared	0.009		
	First Stage F-Stat		43.93	47.95
	Number of Obs	3,482	3,482	3,482
Share of E-Comm Option in Total Monthly Retail Expenditure	Treat or Log Dist	0.00668***	0.0124***	-0.00326***
		(0.00239)	(0.00435)	(0.00114)
	R-Squared	0.006		
	First Stage F-Stat		44.03	47.98
	Number of Obs	3,434	3,434	3,434
Share of E-Comm Option in Monthly Business Inputs	Treat or Log Dist	-0.00707	-0.0155	0.00403
		(0.00779)	(0.0195)	(0.00507)
	R-Squared	0.003		
	First Stage F-Stat		15.59	17.85
	Number of Obs	1,191	1,191	1,191
Share of E-Comm Option in Monthly Non-Durables	Treat or Log Dist	0.00538***	0.0100***	-0.00262***
		(0.00196)	(0.00356)	(0.000933)
	R-Squared	0.003		
	First Stage F-Stat		44.11	48
	Number of Obs	3,433	3,433	3,433
Share of E-Comm Option in Monthly Durables	Treat or Log Dist	0.0408**	0.0686***	-0.0191***
		(0.0160)	(0.0263)	(0.00727)
	R-Squared	0.012		
	First Stage F-Stat		52.43	44.14
	Number of Obs	768	768	768
Share of E-Comm Option in Monthly Food and Beverages (1)	Treat or Log Dist	0.00121	0.00223	-0.000582
		(0.000823)	(0.00152)	(0.000398)
	R-Squared	0.001		
	First Stage F-Stat		45.63	49.84
	Number of Obs	3,359	3,359	3,359

Dependent Variables		Intent to Treat	Treatment on Treated	Log Distance (IV using Treat)
Share of E-Comm Option in Monthly Tobacco and Alcohol (2)	Treat or Log Dist	0.000608	0.00123	-0.000330
		(0.000515)	(0.00109)	(0.000287)
	R-Squared	0.001		
	First Stage F-Stat		33.02	32.67
	Number of Obs	1,653	1,653	1,653
Share of E-Comm Option in Monthly Medicine and Health Products (3)	Treat or Log Dist	0.000693	0.00126	-0.000329
		(0.000689)	(0.00124)	(0.000324)
	R-Squared	0.000		
	First Stage F-Stat		51.06	54.55
	Number of Obs	2,416	2,416	2,416
Share of E-Comm Option in Monthly Clothing and Accessories (4)	Treat or Log Dist	0.0466***	0.0736***	-0.0201***
		(0.0140)	(0.0217)	(0.00594)
	R-Squared	0.019		
	First Stage F-Stat		70.53	65.25
	Number of Obs	1,268	1,268	1,268
Share of E-Comm Option in Monthly Other Household Products (5)	Treat or Log Dist	0.00437	0.00816	-0.00217
		(0.00396)	(0.00715)	(0.00190)
	R-Squared	0.001		
	First Stage F-Stat		43.87	47.76
	Number of Obs	2,336	2,336	2,336
Share of E-Comm Option in Monthly Heating, Fuel and Gas (6)	Treat or Log Dist	0	0	0
		(0)	(0)	(0)
	R-Squared	.		
	First Stage F-Stat		.	.
	Number of Obs	1,463	1,463	1,463
Share of E-Comm Option in Monthly Furniture and Appliances (7)	Treat or Log Dist	0.0546**	0.0908**	-0.0253**
		(0.0217)	(0.0368)	(0.0101)
	R-Squared	0.019		
	First Stage F-Stat		47.51	42.04
	Number of Obs	380	380	380
Share of E-Comm Option in Monthly Electronics (8)	Treat or Log Dist	0.0698**	0.111**	-0.0339**
		(0.0347)	(0.0527)	(0.0159)
	R-Squared	0.023		
	First Stage F-Stat		42.35	26.54
	Number of Obs	231	231	231
Share of E-Comm Option in Monthly Transport Equipment (9)	Treat or Log Dist	0.0357*	0.0565*	-0.0152*
		(0.0203)	(0.0319)	(0.00878)
	R-Squared	0.014		
	First Stage F-Stat		41.19	42.37
	Number of Obs	139	139	139

Notes: Table reports point estimates from specification (1). The first column reports ITT and the second column TOT. The third column replaces the binary TOT with log residential distances to the nearest e-commerce terminal (using village-level ITT as instrument as for second column). Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Table A.7: Average Effects: Incomes

Dependent Variables		Intent to Treat	Treatment on Treated	Log Distance (IV using Treat)
Monthly Income Per Capita in RMB	Treat or Log Dist	-7.864	-14.53	3.974
		(70.78)	(129.9)	(35.61)
	R-Squared	0.038		
	First Stage F-Stat		45.33	42.83
	Number of Obs	3,437	3,437	3,437
Monthly Income Per Capita Net of Costs in RMB	Treat or Log Dist	-20.09	-37.20	10.19
		(70.80)	(129.9)	(35.51)
	R-Squared	0.037		
	First Stage F-Stat		44.78	42.54
	Number of Obs	3,390	3,390	3,390
Monthly Income Per Capita Net of Transfers in RMB	Treat or Log Dist	-12.55	-23.21	6.360
		(72.18)	(132.4)	(36.25)
	R-Squared	0.051		
	First Stage F-Stat		45.16	42.67
	Number of Obs	3,445	3,445	3,445
Annual Income Per Capita in RMB	Treat or Log Dist	-45.95	-85.08	23.33
		(586.9)	(1,080)	(296.3)
	R-Squared	0.046		
	First Stage F-Stat		44.77	42.23
	Number of Obs	3,388	3,388	3,388
Monthly Agricultural Income Per Capita	Treat or Log Dist	-70.23	-130.3	35.61
		(140.3)	(257.7)	(70.34)
	R-Squared	0.033		
	First Stage F-Stat		44.23	42.33
	Number of Obs	3,448	3,448	3,448
Monthly Non-Agricultural Income Per Capita	Treat or Log Dist	-46.65	-86.06	23.55
		(137.3)	(249.6)	(68.28)
	R-Squared	0.157		
	First Stage F-Stat		45.74	43.51
	Number of Obs	3,441	3,441	3,441
Weekly Hours Worked by Primary Earner	Treat or Log Dist	1.008	1.879	-0.516
		(3.383)	(6.285)	(1.723)
	R-Squared	0.000		
	First Stage F-Stat		43.80	41.21
	Number of Obs	3,310	3,310	3,310
Weekly Hours Worked by Secondary Earner	Treat or Log Dist	-0.0606	-0.110	0.0317
		(3.886)	(7.002)	(2.020)
	R-Squared	0.000		
	First Stage F-Stat		45.39	40.21
	Number of Obs	1,866	1,866	1,866

Dependent Variables		Intent to Treat	Treatment on Treated	Log Distance (IV using Treat)
Member of Household Has Ever Sold through E-Commerce (Yes=1)	Treat or Log Dist	-0.00700	-0.0129	0.00353
		(0.00562)	(0.0104)	(0.00282)
	R-Squared	0.347		
	First Stage F-Stat		45.30	42.71
	Number of Obs	3,504	3,504	3,504
Member of Household Has Sold through E-Commerce In Past Month (Yes=1)	Treat or Log Dist	-0.00132	-0.00244	0.000667
		(0.00237)	(0.00438)	(0.00119)
	R-Squared	0.038		
	First Stage F-Stat		44.30	42.34
	Number of Obs	3,498	3,498	3,498
E-Commerce Sales in Past Month in RMB	Treat or Log Dist	-10.09	-18.75	5.109
		(12.89)	(23.94)	(6.504)
	R-Squared	0.012		
	First Stage F-Stat		44.26	42.39
	Number of Obs	3,498	3,498	3,498
Share of E-Commerce Sales in Household Monthly Income	Treat or Log Dist	-0.00120	-0.00224	0.000614
		(0.00176)	(0.00330)	(0.000901)
	R-Squared	0.032		
	First Stage F-Stat		41.62	38.41
	Number of Obs	2,830	2,830	2,830
Primary Earner Working As Farmer (Yes=1)	Treat or Log Dist	-0.0229	-0.0425	0.0116
		(0.0319)	(0.0597)	(0.0164)
	R-Squared	0.140		
	First Stage F-Stat		44.42	41.58
	Number of Obs	3,327	3,327	3,327
Member of Household Started a Business Over Last 6 Months (Yes=1)	Treat or Log Dist	-0.00802	-0.0149	0.00407
		(0.00631)	(0.0120)	(0.00327)
	R-Squared	0.001		
	First Stage F-Stat		44.37	42.34
	Number of Obs	3,468	3,468	3,468
New Business Selling in Part Online (Yes=1)	Treat or Log Dist	0.000212	0.000394	-0.000108
		(0.00159)	(0.00294)	(0.000803)
	R-Squared	0.000		
	First Stage F-Stat		44.33	42.37
	Number of Obs	3,468	3,468	3,468

Notes: Table reports point estimates from specification (1). The first column reports ITT and the second column TOT. The third column replaces the binary TOT with log residential distances to the nearest e-commerce terminal (using village-level ITT as instrument as for second column). Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Table A.8: Average Effects: Local Retail Prices

Dependent Variables		Intent to Treat	Treatment on Treated	Dependent Variables		Intent to Treat	Treatment on Treated
Log Prices (All)	Treat	0.0189	0.0352	Log Prices of Food and Beverages (1)	Treat	0.0368**	0.0706*
		(0.0142)	(0.0263)			(0.0185)	(0.0375)
	R-Squared	0.893	0.893		R-Squared	0.870	0.870
	First Stage F-Stat		41.66		First Stage F-Stat		39.37
	Number of Obs	6,877	6,877		Number of Obs	3,686	3,686
Product Replacement Dummy (Not Counting Store Closures) (Yes=1)	Treat	-0.00516	-0.00983	Log Prices of Tobacco and Alcohol (2)	Treat	0.0212	0.0421
		(0.00947)	(0.0181)			(0.0340)	(0.0662)
	R-Squared	0.000	-0.002		R-Squared	0.809	0.810
	First Stage F-Stat		39.82		First Stage F-Stat		32.39
	Number of Obs	8,956	8,956		Number of Obs	1,071	1,071
Store Closure (at Product Level) (Yes=1)	Treat	0.00124	0.00236	Log Prices of Medicine and Health Products (3)	Treat	-0.0474	-0.0756
		(0.0294)	(0.0556)			(0.0741)	(0.122)
	R-Squared	0.000	0.000		R-Squared	0.794	0.795
	First Stage F-Stat		39.82		First Stage F-Stat		19.18
	Number of Obs	8,956	8,956		Number of Obs	266	266
Number of New Products Per Store	Treat	2.194**	4.020*	Log Prices of Clothing and Accessories (4)	Treat	0.0809	0.115
		(1.073)	(2.278)			(0.111)	(0.158)
	R-Squared	0.277	0.212		R-Squared	0.845	0.842
	First Stage F-Stat		19.69		First Stage F-Stat		42.80
	Number of Obs	312	312		Number of Obs	152	152
Store Owner Sources Products Online (Yes=1)	Treat	-0.00145	-0.00261	Log Prices of Other Household Products (5)	Treat	-0.0328	-0.0619
		(0.0258)	(0.0461)			(0.0382)	(0.0744)
	R-Squared	0.000	-0.001		R-Squared	0.756	0.755
	First Stage F-Stat		23.76		First Stage F-Stat		28.85
	Number of Obs	341	341		Number of Obs	1,268	1,268
Log Prices of Business Inputs	Treat	0.00229	0.00337	Log Prices of Heating, Fuel and Gas (6)	Treat	-0.0115	-0.0440
		(0.129)	(0.186)			(0.0955)	(0.332)
	R-Squared	0.811	0.811		R-Squared	0.007	-0.095
	First Stage F-Stat		24.86		First Stage F-Stat		0.795
	Number of Obs	237	237		Number of Obs	12	12
Log Prices of Non-Durables	Treat	0.0211	0.0398	Log Prices of Furniture and Appliances (7)	Treat	-0.0347	-0.0617
		(0.0146)	(0.0276)			(0.0881)	(0.156)
	R-Squared	0.860	0.860		R-Squared	0.952	0.953
	First Stage F-Stat		40.36		First Stage F-Stat		6.757
	Number of Obs	6,455	6,455		Number of Obs	109	109
Log Prices of Durables	Treat	-0.0320	-0.0522	Log Prices of Electronics (8)	Treat	-0.0892	-0.163
		(0.0711)	(0.115)			(0.305)	(0.570)
	R-Squared	0.951	0.952		R-Squared	0.884	0.890
	First Stage F-Stat		9.753		First Stage F-Stat		3.180
	Number of Obs	185	185		Number of Obs	23	23
				Log Prices of Transport Equipment (9)	Treat	0.0297	0.0398
						(0.0840)	(0.110)
					R-Squared	0.946	0.946
					First Stage F-Stat		22.67
					Number of Obs	53	53

Notes: Table reports point estimates from specification (1). The first column reports ITT and the second column TOT (using village-level ITT as instrument). Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Table A.9: Role of Logistical and Transactional Barriers

Effects on Consumption

Dept Variables		Intent to Treat	Treatment on the Treated	Log Distance (IV Using Treat)
Monthly Total Retail Expenditure Per Capita	Treat or Log Dist	-26.91	-49.34	13.67
		(36.29)	(68.00)	(18.71)
	Treat or Log Dist * Delivery	31.64	58.94	-15.50
		(69.36)	(140.5)	(30.43)
	First Stage F-Stat		2.388	19.39
	Number of Obs	3,436	3,436	3,436
Household Has Ever Bought Something through E-Comm Option (Yes=1)	Treat or Log Dist	0.0578***	0.106***	-0.0293***
		(0.0188)	(0.0283)	(0.00775)
	Treat or Log Dist * Delivery	-0.0606**	-0.111**	0.0304***
		(0.0253)	(0.0443)	(0.0102)
	First Stage F-Stat		2.682	19.63
	Number of Obs	3,518	3,518	3,518
Household Has Bought Something in Last Month (Yes=1)	Treat or Log Dist	0.0329***	0.0604***	-0.0168***
		(0.0111)	(0.0189)	(0.00522)
	Treat or Log Dist * Delivery	-0.0422***	-0.0790**	0.0204***
		(0.0155)	(0.0329)	(0.00729)
	First Stage F-Stat		2.513	19.10
	Number of Obs	3,482	3,482	3,482
Share of E-Comm Option in Total Monthly Retail Expenditure	Treat or Log Dist	0.00799***	0.0147***	-0.00407***
		(0.00275)	(0.00489)	(0.00136)
	Treat or Log Dist * Delivery	-0.00835***	-0.0154***	0.00422***
		(0.00295)	(0.00543)	(0.00144)
	First Stage F-Stat		2.413	19.25
	Number of Obs	3,434	3,434	3,434
Share of E-Comm Option in Total Monthly Business Inputs	Treat or Log Dist	-0.00830	-0.0190	0.00501
		(0.00827)	(0.0222)	(0.00589)
	Treat or Log Dist * Delivery	0.0179	0.0334	-0.00818
		(0.0113)	(0.0250)	(0.00633)
	First Stage F-Stat		6.346	7.094
	Number of Obs	1,191	1,191	1,191
Share of E-Comm Option in Total Monthly Non-Durables	Treat or Log Dist	0.00639***	0.0117***	-0.00325***
		(0.00225)	(0.00401)	(0.00112)
	Treat or Log Dist * Delivery	-0.00648**	-0.0119***	0.00329***
		(0.00247)	(0.00453)	(0.00119)
	First Stage F-Stat		2.413	19.26
	Number of Obs	3,433	3,433	3,433
Share of E-Comm Option in Total Monthly Durables	Treat or Log Dist	0.0497***	0.0825***	-0.0240***
		(0.0177)	(0.0286)	(0.00823)
	Treat or Log Dist * Delivery	-0.0705***	-0.120***	0.0322***
		(0.0258)	(0.0443)	(0.0113)
	First Stage F-Stat		3.150	18.33
	Number of Obs	768	768	768

Effects on Incomes

Dept Variables		Intent to Treat	Treatment on the Treated	Log Distance (IV Using Treat)
Monthly Income Per Capita in RMB	Treat or Log Dist	-15.00	-27.15	7.579
		(77.55)	(140.1)	(39.08)
	Treat or Log Dist * Delivery	50.17	96.91	-25.08
		(171.1)	(339.0)	(86.90)
	First Stage F-Stat		2.694	2.737
	Number of Obs	3,437	3,437	3,437
Monthly Income Per Capita Net of Costs in RMB	Treat or Log Dist	-20.24	-37.09	10.33
		(77.47)	(140.5)	(39.07)
	Treat or Log Dist * Delivery	6.011	9.303	-3.362
		(167.6)	(317.4)	(81.28)
	First Stage F-Stat		2.810	2.852
	Number of Obs	3,390	3,390	3,390
Monthly Income Per Capita Net of Transfers in RMB	Treat or Log Dist	-13.87	-25.27	7.041
		(77.86)	(140.7)	(39.18)
	Treat or Log Dist * Delivery	12.70	23.04	-6.473
		(188.3)	(367.2)	(93.22)
	First Stage F-Stat		2.635	2.696
	Number of Obs	3,445	3,445	3,445
Annual Income Per Capita in RMB	Treat or Log Dist	70.33	124.2	-34.68
		(645.0)	(1,168)	(325.6)
	Treat or Log Dist * Delivery	-734.1	-1,462	368.3
		(1,484)	(2,755)	(692.5)
	First Stage F-Stat		2.501	2.603
	Number of Obs	3,388	3,388	3,388
Member of Household Has Ever Sold through E-Commerce (Yes=1)	Treat or Log Dist	-0.00857	-0.0156	0.00433
		(0.00608)	(0.0111)	(0.00309)
	Treat or Log Dist *	0.0102	0.0188	-0.00513
		(0.0141)	(0.0280)	(0.00715)
	First Stage F-Stat		2.561	2.598
	Number of Obs	3,504	3,504	3,504
Share of E-Commerce Sales in Household Monthly Income	Treat or Log Dist	-0.00172	-0.00316	0.000882
		(0.00210)	(0.00387)	(0.00108)
	Treat or Log Dist *	0.00282	0.00540	-0.00145
		(0.00233)	(0.00441)	(0.00121)
	First Stage F-Stat		2.402	2.342
	Number of Obs	2,830	2,830	2,830
Primary Earner Working as Peasant (Yes=1)	Treat or Log Dist	-0.0192	-0.0352	0.00979
		(0.0341)	(0.0624)	(0.0174)
	Treat or Log Dist *	-0.0284	-0.0609	0.0143
		(0.0813)	(0.185)	(0.0464)
	First Stage F-Stat		2.503	2.533
	Number of Obs	3,327	3,327	3,327
Member of Household Has Started a Business Over Last 6 Months (Yes=1)	Treat or Log Dist	-0.00328	-0.00601	0.00167
		(0.00635)	(0.0116)	(0.00322)
	Treat or Log Dist * Delivery	-0.0297	-0.0604	0.0149
		(0.0183)	(0.0536)	(0.0130)
	First Stage F-Stat		2.517	2.566
	Number of Obs	3,468	3,468	3,468

Effects on Retail Prices

Dept Variables		Intent to Treat	Treatment on the Treated
Log Prices (All)	Treat	0.0114	0.0215
		(0.0144)	(0.0273)
	Treat * Delivery	0.0417	0.0739
		(0.0377)	(0.0572)
	First Stage F-Stat		17.26
	Number of Obs	6,877	6,877
Product Replacement Dummy (Not Counting Store Closures) (Yes=1)	Treat	-0.00680	-0.0129
		(0.0108)	(0.0206)
	Treat * Delivery	0.00907	0.0173
		(0.0213)	(0.0415)
	First Stage F-Stat		2.648
	Number of Obs	8,956	8,956
Store Closure (at Product Level) (Yes=1)	Treat	0.00111	0.00209
		(0.0355)	(0.0668)
	Treat * Delivery	0.000779	0.00162
		(0.0423)	(0.0805)
	First Stage F-Stat		2.648
	Number of Obs	8,956	8,956
Number of New Products Per Store	Treat	1.403*	2.352*
		(0.828)	(1.354)
	Treat * Delivery	3.403	7.993
		(3.876)	(12.77)
	First Stage F-Stat		1.247
	Number of Obs	312	312
Store Owner Sources Products Online (Yes=1)	Treat	0.0250**	0.0416**
		(0.0122)	(0.0201)
	Treat * Delivery	-0.0911	-0.185
		(0.0814)	(0.166)
	First Stage F-Stat		1.320
	Number of Obs	341	341
Log Price of Business Inputs	Treat	-0.0858	-0.108
		(0.134)	(0.182)
	Treat * Delivery	0.289	0.473
		(0.273)	(0.447)
	First Stage F-Stat		1.972
	Number of Obs	237	237
Log Price of Non-Durables	Treat	0.0192	0.0366
		(0.0157)	(0.0308)
	Treat * Delivery	0.0137	0.0214
		(0.0362)	(0.0585)
	First Stage F-Stat		16.09
	Number of Obs	6,455	6,455
Log Prices of Durables	Treat	-0.118	-0.144
		(0.0880)	(0.104)
	Treat * Delivery	0.164	0.288
		(0.134)	(0.366)
	First Stage F-Stat		0.488
	Number of Obs	185	185

Notes: Left panel shows outcomes related to household consumption, middle panel shows outcomes related to household incomes and right panel shows outcomes related to local retail prices. The first column reports ITT and the second column TOT. The third column replaces the binary TOT with log residential distances to the nearest e-commerce terminal (using village-level ITT as instrument as for second column). Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Table A.10: Role of GE Spillovers

Dependent Variables		Treatment on Treated without Spillovers	ToT with Spillovers: Number of Terminals within 3 km Outside of Village	ToT with Spillovers: Number of Terminals within 10 km Outside of Village
Any Member of Household Has Ever Sold through E-Comm (Yes=1)	Treat Dummy	-0.0128	-0.0134	-0.0147
		(0.0103)	(0.0101)	(0.0101)
	Exposure to Terminals Outside the Village		-0.00131	-0.00234
			(0.0101)	(0.00202)
	Exposure to Other Villages		-0.00334***	-0.000277
			(0.00102)	(0.000363)
	First Stage F-Stat	45.30	47.63	44.61
	Number of Obs	3,504	3,504	3,504
Household Has Ever Bought Something through E-Comm Option (Yes=1)	Treat Dummy	0.0894***	0.0793***	0.0873***
		(0.0268)	(0.0263)	(0.0264)
	Exposure to Terminals Outside the Village		0.0658**	-0.00606
			(0.0312)	(0.00567)
	Exposure to Other Villages		-0.00246	0.00258**
			(0.00539)	(0.00112)
	First Stage F-Stat	45.31	47.83	44.59
	Number of Obs	3,518	3,518	3,518
Share of E-Comm Option in Total Retail Expenditure	Treat Dummy	0.0124***	0.0101**	0.0119***
		(0.00435)	(0.00399)	(0.00422)
	Exposure to Terminals Outside the Village		0.0159*	-0.00129
			(0.00833)	(0.000929)
	Exposure to Other Villages		-0.000595	0.000507**
			(0.000524)	(0.000228)
	First Stage F-Stat	44.03	46.57	43.50
	Number of Obs	3,434	3,434	3,434
Log Local Retail Prices (All Prices)	Treat Dummy	0.0352	0.0338	0.0386
		(0.0263)	(0.0258)	(0.0252)
	Exposure to Terminals Outside the Village		0.00353	0.00382
			(0.0314)	(0.00562)
	Exposure to Other Villages		-0.00318	-0.00135
			(0.00314)	(0.000950)
	First Stage F-Stat	41.66	43.89	43.95
	Number of Obs	6,877	6,877	6,877

Notes: The first column reports the baseline TOT. The second column adds exposure to other intent-to-treat villages within a 3 km radius, controlling for the total number of eligible villages within this radius. The third column adds exposure to other intent-to-treat villages within a 10 km radius, controlling for the total number of eligible villages within this radius. See Appendix C for discussion. Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Table A.11: Fraction of Market Access to Other Rural Markets in County

	Fraction of Market Access from Rural Markets in Same County						Fraction of Market Access from Participating Rural Markets in Same County					
Measure of Market Size:	Access to Population			Access to GDP			Access to Population			Access to GDP		
	Median	Mean	Std Dev	Median	Mean	Std Dev	Median	Mean	Std Dev	Median	Mean	Std Dev
Panel A: Distance Elasticity of -1												
All Rural Townships in East, Middle and Southwest China (10,214 Townships)	0.0082	0.011	0.01	0.0031	0.0044	0.005	0.0014	0.0018	0.0017	0.0005	0.0007	0.0008
Rural Townships in 3 RCT Provinces (2,291 Townships)	0.012	0.016	0.014	0.0037	0.0059	0.0062	0.0020	0.0027	0.0023	0.0006	0.0010	0.0010
Rural Townships in 8 RCT Counties (58 Townships)	0.011	0.012	0.006	0.0031	0.0041	0.0029	0.0018	0.0020	0.0010	0.0005	0.0007	0.0005
Panel B: Distance Elasticity of -1.5												
All Rural Townships in East, Middle and Southwest China (10,214 Townships)	0.027	0.037	0.042	0.01	0.016	0.024	0.0045	0.0062	0.0070	0.0017	0.0027	0.0040
Rural Townships in 3 RCT Provinces (2,291 Townships)	0.036	0.049	0.055	0.012	0.02	0.028	0.0060	0.0082	0.0092	0.0020	0.0033	0.0047
Rural Townships in 8 RCT Counties (58 Townships)	0.034	0.038	0.033	0.011	0.014	0.013	0.0057	0.0063	0.0055	0.0018	0.0023	0.0022

Notes: Table reports the mean, median and standard deviation of the fraction of trade market access coming from other rural markets in the same county. See Appendix C for discussion.

Table A.12: Are Sample Villages Representative?

	(1)	(2)	(3)	(4)	(5)	(6)
		Full Sample			3 Provinces	
Dependent Variables:	Number of Users	Number of Transactions	Sales (RMB)	Number of Users	Number of Transactions	Sales (RMB)
		Panel A: Purchase Database				
RCT_Sample Dummy	-4.110	0.0605	-6,034	0.149	12.65	-3,747
	(7.751)	(25.33)	(4,061)	(7.734)	(25.32)	(4,066)
Months Fixed Effects	✓	✓	✓	✓	✓	✓
Control for Months Since Program Entry	✓	✓	✓	✓	✓	✓
Observations	125,204	125,204	125,204	100,098	100,098	100,098
R-squared	0.037	0.047	0.029	0.031	0.046	0.03
Number of Village Clusters	11,731	11,731	11,731	8,471	8,471	8,471

	(7)	(8)	(9)	(10)
	Full Sample		3 Provinces	
Dependent Variables:	Number of Transactions	Weight (kg)	Number of Transactions	Weight (kg)
		Panel B: Out-Shipment Database		
RCT_Sample Dummy	1.712**	5.154	1.364*	4.68
	(0.753)	(4.332)	(0.752)	(4.333)
Months Fixed Effects	✓	✓	✓	✓
Control for Months Since Program Entry	✓	✓	✓	✓
Observations	120,483	120,483	95,744	95,744
R-squared	0.06	0.023	0.067	0.026
Number of Village Clusters	11,904	11,904	8,591	8,591

Notes: Table reports point estimates from a regression of the reported outcomes on a dummy equal to one if a village is one of our 100 RCT villages in addition to month fixed effects and the number of months since program entry. Columns 1 to 3 and 7 to 8 report results for all participating villages in the five provinces of Anhui, Guangxi , Guizhou, Henan, and Yunnan over the period November 2015 to April 2017. The sample in columns 4 to 6 and 9 to 10 are all villages in our three survey provinces Anhui, Guizhou, and Henan. The upper panel presents point estimates from regressions based on the purchase transaction database over the period November 2015 to April 2017. The lower panel presents point estimates from regressions based on the sales transaction database over the period January 2016 to April 2017. See Appendix D for discussion. Standard errors are clustered at the level of village terminals. * 10%, ** 5%, *** 1% significance levels.

Table A.13: Role of Seasonality

	(1)	(2)	(3)	(4)	(5)	(6)
		Full Sample			3 Provinces	
Dependent Variables:	Number of Users	Number of Transactions	Sales (RMB)	Number of Users	Number of Transactions	Sales (RMB)
			Panel A: Purchase Database			
RCT Sample Month Dummy	0.893***	-4.671***	-1,565***	0.568**	-5.290***	-585.9
	(0.255)	(0.818)	(451.5)	(0.274)	(0.863)	(458.0)
Village Fixed Effects	✓	✓	✓	✓	✓	✓
Control for Months Since Program Entry	✓	✓	✓	✓	✓	✓
Observations	125,204	125,204	125,204	100,098	100,098	100,098
R-squared	0.694	0.68	0.219	0.679	0.667	0.227
Number of Village Clusters	11,731	11,731	11,731	8,471	8,471	8,471

	(7)	(8)	(9)	(10)
	Full Sample		3 Provinces	
Dependent Variables:	Number of Transactions	Weight (kg)	Number of Transactions	Weight (kg)
		Panel B: Out-Shipment Database		
RCT Sample Month Dummy	-0.387***	-1.256***	-0.498***	-1.407***
	(0.0225)	(0.125)	(0.0261)	(0.138)
Village Fixed Effects	✓	✓	✓	✓
Control for Months Since Program Entry	✓	✓	✓	✓
Observations	120,483	120,483	95,744	95,744
R-squared	0.592	0.432	0.57	0.422
Number of Village Clusters	11,904	11,904	8,591	8,591

Notes: Table reports point estimates from a regression of the reported outcomes on a dummy equal to one if a village is one of our 100 RCT villages in addition to village fixed effects and the number of months since program entry. Columns 1 to 3 and 7 to 8 report results for all participating villages in the five provinces of Anhui, Guangxi , Guizhou, Henan, and Yunnan over the period November 2015 to April 2017. The sample in columns 4 to 6 and 9 to 10 are all villages in our three survey provinces Anhui, Guizhou, and Henan. The upper panel presents point estimates from regressions based on the purchase transaction database over the period November 2015 to April 2017. The lower panel presents point estimates from regressions based on the sales transaction database over the period January 2016 to April 2017. See Appendix D for discussion. Standard errors are clustered at the level of village terminals. * 10%, ** 5%, *** 1% significance levels.

Table A.14: Quantification Using Alternative Demand Parameters

	$\sigma_D = 2.87$, $\sigma_N = 2.85$			$\sigma_D = 3.87$, $\sigma_N = 3.85$			$\sigma_D = 4.87$, $\sigma_N = 4.85$		
	Durables Consumption	Non-Durables Consumption	Total Retail Consumption	Durables Consumption	Non-Durables Consumption	Total Retail Consumption	Durables Consumption	Non-Durables Consumption	Total Retail Consumption
Reduction in Retail Cost of Living for All Households	5.256% (0.048)	0.739% (0.005)	1.27% (0.007)	3.379% (0.03)	0.481% (0.003)	0.824% (0.005)	2.489% (0.022)	0.357% (0.003)	0.61% (0.003)
Reduction in Retail Cost of Living Among Users	32.416% (0.378)	5.904% (0.044)	8.735% (0.054)	19.884% (0.221)	3.806% (0.028)	5.597% (0.034)	14.331% (0.155)	2.808% (0.021)	4.117% (0.025)

Notes: Table reports average household gains in terms of percentage point reductions in household retail cost of living across alternative parameterizations of household demand. Estimates are based on equation (3) using treatment effects on household substitution into new e-commerce option. See Evaluation Section for discussion. Standard errors are bootstrapped across 1000 iterations.

Table A.15: Test for Effects on Attrition and Migration

Dependent Variables		Intent to Treat	Treatment on Treated	Log Distance (IV using Treat)
Attrition (Yes=1)	Treat or Log Dist	0.0138	0.0258	-0.00740
		(0.0239)	(0.0445)	(0.0127)
	R-Squared	0.000		
	Number of Obs	2,629	2,629	2,629
	First Stage F-Stat		44.24	35.90
Number of Household Members Who Moved Back to the Village	Treat or Log Dist	0.0255	0.0472	-0.0129
		(0.0400)	(0.0734)	(0.0199)
	R-Squared	0.001		
	Number of Obs	3,526	3,526	3,526
	First Stage F-Stat		45.27	42.71
Number of Household Members Who Moved Away from the Village	Treat or Log Dist	-0.00345	-0.00637	0.00174
		(0.0184)	(0.0338)	(0.00922)
	R-Squared	0.012		
	Number of Obs	3,523	3,523	3,523
	First Stage F-Stat		45.44	43.84
Would You Be Willing to Migrate to a City If a Good Job Opportunity Presented Itself? (Yes=1)	Treat or Log Dist	-0.0249	-0.0458	0.0125
		(0.0191)	(0.0348)	(0.00953)
	R-Squared	0.025		
	Number of Obs	3,527	3,527	3,527
	First Stage F-Stat		45.76	44.15

Notes: Table reports point estimates from specification (1). The first column reports ITT and the second column TOT. The third column replaces the binary TOT with log residential distances to the nearest e-commerce terminal (using village-level ITT as instrument as for second column). See Appendix F for discussion. Standard errors are clustered at the level of villages. * 10%, ** 5%, *** 1% significance levels.

Appendix B: K-L-K Indices

Table 1 reports treatment effects after combining several outcomes related to consumption, incomes and local retail prices into three indices. We follow Kling et al. (2007) ("K-L-K") and construct equally weighted averages of z-scores that we compute by subtracting outcomes by the mean of the variable in the control group and dividing by the standard deviation of the variable in the control group. The z-scores are signed such that effects on all index components point in the same direction (i.e. price index reductions or income growth). If a household (or store) has a valid response to at least one component measure of an index, then any missing values for other component measures are imputed at the random assignment group mean. This results in differences between treatment and control means of an index being the same as the average of treatment and control means of the components of that index, so that the index can be interpreted as the average of results for separate measures scaled to standard deviation units.[1]

[1] For two outcomes of the consumption index discussed below, the control mean and standard deviation were zero. In those cases, we instead use the standard deviation of the variable observed in the full sample.

The consumption index is based on 11 variables related to household substitution of expenditures into the new e-commerce shopping option, all entering the index positively. Those outcomes are whether a household reports ever having used the new option, reported usage over the past month, and the shares of household total retail expenditure spent on 9 consumption categories (food and beverages, tobacco and alcohol, medicine and health, clothing and accessories, other every-day products, fuel and gas, furniture and appliances, electronics, transport equipment). The treatment effects on each of these outcomes are reported as part of appendix Table A.6.

The income index is based on 14 variables related to income generation, labor supply, online selling activity and online sourcing of inputs. Those outcomes are monthly income per capita, annual income per capita, monthly income from agriculture, monthly income from non-agriculture, monthly hours of work by primary earner, monthly hours of work by secondary earner, whether anyone in household has ever sold online, sold over the last month, revenues from online sales over past month, share of online revenues in total monthly income, whether primary earner is a farmer (entering negatively), whether any household member has started a new business over past 6 months, whether the new business sells in part online, and the share of monthly online purchases in total expenditures on inputs and materials. The treatment effects on each of these outcomes are reported as part of appendix Table A.7.

The local retail index is based on 4 store-level measures related to effects on the local retail cost of living. Those outcomes are the average of log price changes of continuing product items within the store (entering negatively), the number of new product additions over the past month (positively), the number of product replacements (measured as the fraction of products reported in the baseline survey that were no longer available at endline) (negatively), and whether or not the store owner reports sourcing products online (positively). The treatment effects on each of these outcomes are reported as part of appendix Table A.8.

Appendix C: Role of Spillovers

To investigate the role of spillovers, we pursue two different approaches. First, we follow an approach similar to Miguel & Kremer (2004):

$$y_{hv}^{Post} = \alpha + \beta_1 Treat_v + \beta_2 Exposure_v^{treat} + \beta_3 Exposure_v^{all} + \gamma y_{hv}^{Pre} + \epsilon_{hv}, \tag{A.1}$$

where $Exposure_{vk}^{treat}$ measures the proximity of village v to other program villages, and $Exposure_{vk}^{all}$ measures proximity to all villages on the candidate list from which we randomly selected our control villages. Even though exposure to other program villages is not randomly assigned, our randomization means that conditional on exposure to all candidate villages, exposure to other treatment villages is plausibly exogenous. Using this design, β_2 is an estimate of the the strength of cross-village spillovers. We measure exposure as the number of intent-to-treat villages within 3 or 10 km distance bins of a given village. Table A.10 reports the estimation results. We find some evidence of positive spillover effects of nearby terminals within 3 km of the village. These effects imply a larger total average effect on e-commerce uptake. Consumption uptake increases from 9 percent in Table A.6 to 14 percent once we take into account positive spillovers from nearby villages, which is 13 percent of the village population after adjusting for sampling weights. In contrast, we find no evidence of cross-village spillovers on local retail stores, or on the production side of the economy.

Second, to further investigate these channels in the absence of experimental variation in program saturation rates,[2] we also pursue an approach grounded in trade theory. In particular, we can quantify the fraction of a rural location's total trade market access that is due to trading exposure to other rural markets in the same county. This fraction provides additional information on the extent of rural-to-rural spillovers from other sample villages in our setting. If a sizable share of local market access is due to trading relations with other local rural markets, then indirect effects on local product prices and incomes from treatments in other villages could become an important force. If, on the other hand, local product and factor prices are predominantly determined by access to larger urban markets, then rural-to-rural spillovers could have negligible effects on local prices and incomes across our sample villages.

Following e.g. Head & Mayer (2014), the market access of location v to all other rural and urban markets $j \neq v$ is:

$$MA_v = \sum_{j \neq v} \tau_{jv}^{-\theta} \, Y_j \tag{A.2}$$

where τ_{jv} is the bilateral trade cost, θ is the elasticity of trade flows with respect to trade costs, and Y_j is a measure of j's market size.[3] MA_v is thus a weighted sum of economic activity outside of market v, with weights that are inversely related to bilateral trade costs. To compute the fraction of total market access that is due to bilateral linkages with other rural markets in the same county (i.e. MA_v^R / MA_v), we compute (6) both across bilateral connections to all other markets (denominator), and only summing across bilateral connections with other rural markets in the same county (numerator). Alternatively, we restrict the numerator to bilateral connections with respect to the fraction of rural markets in the county that are participating in the program to compute the share of market access due to rural locations with program terminals. That fraction was about 1/6th of all rural markets in participating counties over our sample period.

To compute these measures, we use the township-level data from the Chinese Population Census in 2010 described in Appendix F below (National Bureau of Statistics of China, 2011). These data provide us with the populations residing in each of roughly 45,000 township-level administrative units. In addition, we use the coordinates of township centroids to construct the full matrix of bilateral distances in km. Following the trade literature, we use these bilateral distances to parameterize $\tau_{jv}^{-\theta}$: using the finding that the elasticity of trade flows with respect to distance is approximately -1,[4] we measure $\tau_{jv}^{-\theta}$ as the inverse bilateral distance in km when summing across the j market sizes. Alternatively, we also use a larger distance elasticity of -1.5 that gives more weight to markets in closer proximity. For market size Y_j, we use either population or population multiplied by the value added per worker for rural and non-rural workers measured at the

[2]As part of our negotiations and collaboration with the firm's local implementation teams, it was not feasible to also attempt a two-stage cluster randomization design that would have allowed us to randomly vary saturation rates.

[3]To be consistent with structural gravity in trade models, the measure Y_j of j's market size should include a multilateral resistance term capturing j's own degree of access to all other markets (see e.g. Head & Mayer (2014)). In (A.2), we abstract from this and compute a first-order approximation of the structural gravity expression for MA_v. In practice, both measures have been found to yield very similar results in recent empirical work, as they are highly correlated (e.g. Donaldson & Hornbeck (2016)).

[4]See e.g. Disdier & Head (2008) for a meta-analysis of this point estimate.

province level for 2010. The first metric provides an inverse distance-weighted measure of market access to populations outside the township, while the second provides an approximate measure of access to GDP. Finally, we define rural and urban markets following the administrative classification across township-level units we obtain in the census data. For computational feasibility, when constructing the full matrix of bilateral connections, we compute the total market access of rural townships with respect to all other township units (both rural and urban) within each of the 3 broad administrative regions of China in which our sample counties are located: East China (7 provinces), Middle China (3 provinces) and Southwest China (5 provinces).[5]

The above provides us with four measures of the ratio of total market access that is due to access to other rural populations or rural GDP within the the same county: measured either in terms of access to population or to GDP, and measured either in terms of access to all rural markets in the county or only the fraction of rural markets that on average participate in the e-commerce program. We compute the median, mean and standard deviations of these 4 ratios for all rural townships located in the three regions of China, as well as only for townships in our 3 sample provinces, or only for townships in the 8 sample counties. Furthermore, we compute each of these measures both for the baseline distance elasticity of -1, and when using -1.5 instead.

Appendix Table A.11 presents the estimation results. Overall, we find that other rural markets in the same county account for a tiny fraction of total trade market access for the median or the average rural market place. This result is driven by the fact that nearby rural markets within the same county account for a small fraction of the market size that is concentrated in vastly larger urban centers. This is particularly the case when using economic output as the measure of market size, but also holds for raw populations. For example, the median fraction of market access from nearby rural markets in terms of GDP is 0.37 percent in our sample provinces, and 1.2 percent in terms of population access. These fractions slightly increase when giving more weight to nearby markets using a higher distance elasticity, but remain close to zero in both cases when computing rural-to-rural market access only with respect to the average fraction of rural markets that are participating in the program in any given county over our sample period. These findings are in line with the absence of significant GE spillover effects on market prices or nominal incomes shown in our first approach above, and serve to provide some further corroborating evidence in this context.

Appendix D: Additional Results from the Firm's Database

Are the RCT Sample Villages Representative?

Results are based on the firm database we describe in Section 1 of the paper (Anonymous Firm, 2017). One concern is that the 8 counties that our RCT takes place in may not be representative of program villages in the Chinese countryside more broadly. To assess whether the RCT villages are representative of the population of program villages in China, we use the 5-province transaction database on both purchases and sales transactions to estimate regressions of the following form:

[5] The 8 counties of our RCT fall into one these three zones. Omitting regions outside each zone is somewhat conservative, as their inclusion would increase the denominator of the rural-to-total market access ratios.

$$y_{vm} = \theta_m + \beta \text{RCTSample}_v + \gamma \text{MonthsSinceEntry}_{vm} + \epsilon_{vm},$$

where v indexes villages and θ_m is a set of monthly dummies indexed by m for the 18 months of operation from November 2015 to January 2017. y_{vm} is one of five village-level monthly outcomes (number of buyers, number of purchase transactions, total terminal sales, number of out-shipments and total weight of out-shipments in kg), *RCTSample* is a dummy for whether the village is in our RCT sample, and *MonthsSinceEntry* controls for the number of months that the program has been in operation in v as of month m. The standard errors ϵ_{vm} are clustered at the village level.

The results in appendix Table A.12 show no remarkable differences between our RCT villages and the population of program villages in these 5 provinces. The same is true if we compare our RCT villages to all villages in our 3 survey provinces. The RCT sample seems marginally more successful on the out-shipment side, but the magnitudes are tiny. These results provide some reassurance against the potential concern that the e-commerce firm directed our team towards 8 counties that systematically differ from the program's target locations in the Chinese countryside.

Did We Collect Endline Data During Particular Months?

The timeline of pre-treatment data collection was determined by the roll-out schedule of the e-commerce firm, and we could not finance more than a single post-treatment round. As a result of these constraints, our survey cannot measure the impact of seasonality on treatment effects. We therefore use the transaction database to study seasonality effects by estimating:

$$y_{vm} = \theta_v + \beta RCTMonth_m + \gamma MonthSinceEntry_{vm} + \epsilon_{vm},$$

where *RCTMonth* is a dummy for our survey months i.e., a dummy equal to 1 if month m is either in December, January, April or May, which are the four calendar months during which we conducted our survey. We again cluster standard errors ϵ_{vm} at the village level. The results are in appendix Table A.13. We find slightly higher numbers of buyers during survey months relative to the rest of the calendar year, and slightly lower numbers of purchase transactions and out-shipments. In both cases, the point estimates are very small: about one additional buyer per month, a reduction of between 4 to 5 in the number of monthly purchase transactions, and a reduction of less than one out-shipment per month on the selling side. We conclude that seasonality is unlikely to be a significant driver underlying the findings of the RCT.

Appendix E: Welfare Evaluation

Following recent work by Atkin et al. (2018), we propose a three-tier demand system to describe household retail consumption across product groups, retail shopping options and products. In the upper tier, shown in equation A.3, there are Cobb-Douglas preferences over broad product groups $g \in G$ (durables and non-durables) in total consumption. In the middle tier, shown in equation A.4, there are asymmetric CES preferences over local retailers selling that product group $s \in S$ (e.g. local stores, market stalls or the e-commerce option). In the final tier, there are preferences over the individual products within the product groups $b \in B_g$ that we can leave unspecified for now.

$$U_h = \prod_{g\in G} [Q_{gh}]^{\alpha_{gh}} \tag{A.3}$$

$$Q_{gh} = (\sum_{s\in S_g} \beta_{gsh} q_{gsh}^{\frac{\sigma_g-1}{\sigma_g}})^{\frac{\sigma_g}{\sigma_g-1}}, \tag{A.4}$$

where α_{gh} and β_{gsh} are (potentially household group-specific) preference parameters that are fixed across periods. Q_{gh} and q_{gsh} are product-group and store-product-group consumption aggregates with associated price indices P_{gh} and r_{gsh} respectively, and σ_g is the elasticity of substitution across local retail outlets. For each broad product group, consumers choose how much they are going to spend at different retail outlets based on the store-level price index r_{gsh} (which itself depends on the product mix and product-level prices on offer across outlets).

While the demand system is homothetic, we capture potential heterogeneity across the income distribution by allowing households of different incomes to differ in their expenditure shares across product groups (α_{gh}) and their preferences for consumption bundles at different stores within those product groups (β_{gsh} and the preference parameters that generate q_{gsh}). As shown by Anderson et al. (1992), these preferences can generate the same demands as would be obtained from aggregating many consumers who make discrete choices over which store to shop in. Building on Feenstra (1994), the following expression provides the exact proportional cost of living effect (CLE) under this demand system as a fraction of initial household expenditures:

$$\frac{CLE}{e(\mathbf{P}^0, u_h^0)} = \prod_{g\in G} \left((\frac{\sum_{s\in S_g^C} \phi_{gsh}^1}{\sum_{s\in S_g^C} \phi_{gsh}^0})^{\frac{1}{\sigma_g-1}} \prod_{s\in S_g^C} (\frac{r_{gsh}^1}{r_{gsh}^0})^{\omega_{gsh}} \right)^{\alpha_{gh}} - 1 \tag{A.5}$$

where S_g^C denotes the set of continuing local retailers within product group g, $\phi_{gsh}^t = r_{gsh}^t q_{gsh}^t / \sum_{s\in S_g} r_{gsh}^t q_{gsh}^t$ is the expenditure share for a particular retailer of product group g, and the ω_{gsh}s are ideal log-change weights.[6]

For each product group g, the expression has two components. The $\prod_{s\in S_g^C} (\frac{r_{gsh}^1}{r_{gsh}^0})^{\omega_{gsh}}$ term is a Sato-Vartia (i.e. CES) price-index for price changes in continuing local stores that forms the pro-competitive price effect.[7] The price terms r_{gsh}^t are themselves price indices of product-specific prices p_{gsb}^t within local continuing stores which, in principle, could also account for new product varieties or exiting product varieties using the same methodology. While we name these price changes pro-competitive, they may derive from either reductions in markups or increases in productivity at local stores (distinctions that do not matter on the cost-of-living side, but would generate different magnitudes of profit and income effects that we capture on the nominal income side).

The $(\frac{\sum_{s\in S_g^C} \phi_{gsh}^1}{\sum_{s\in S_g^C} \phi_{gsh}^0})^{\frac{1}{\sigma_{gh}-1}}$ term captures the gains to customers of the e-commerce option in the nu-

[6]In particular, $\omega_{gsh} = \left(\frac{\tilde{\phi}_{gsh}^1 - \tilde{\phi}_{gsh}^0}{\ln \tilde{\phi}_{gsh}^1 - \ln \tilde{\phi}_{gsh}^0}\right) / \sum_{s\in S_g^C} \left(\frac{\tilde{\phi}_{gsh}^1 - \tilde{\phi}_{gsh}^0}{\ln \tilde{\phi}_{gsh}^1 - \ln \tilde{\phi}_{gsh}^0}\right)$, which in turn contain expenditure shares of different retailers within product groups, where the shares consider only expenditure at continuing retailers $\tilde{\phi}_{gsh}^t = r_{gsh}^t q_{gsh}^t / \sum_{s\in S_g^C} r_{gsh}^t q_{gsh}^t$.

[7]Notice that the assumption of CES preferences does not imply the absence of pro-competitive effects as we do not impose additional assumptions about market structure (e.g. monopolistic competition).

merator, from both a direct price index effect due to the new shopping option and potential other local store entry induced by this change, and local store exit in the denominator, i.e. the exit effect.

Now consider the case—as in the final section of the paper—where the program's effect on cost of living is driven by the direct price index effect. In that case, the expenditure share spent on continuing local retailers ($\sum_{s \in S_g^C} \phi^1_{gsh}$) is lower than unity only due to substitution into the new e-commerce option. The consumer gains from the program as a proportion of initial household spending are then:

$$\frac{CLE}{e(\mathbf{P}^0, u_h^0)} = \prod_{g \in G} \left(\left(\sum_{s \in S_g^C} \phi^1_{gsh} \right)^{\frac{1}{\sigma_g - 1}} \right)^{\alpha_{gh}} - 1. \tag{A.6}$$

The welfare gain from a new shopping option is a function of the market share of that outlet post-entry and the elasticity of substitution across stores. The revealed preference nature of this approach is clear. If consumers greatly value the arrival of the new option—be it because it offers low prices p^1_{gsb}, more product variety that reduces r^1_{gsh} or better amenities β_{gsh} –the market share is higher and the welfare gain greater. Hence, these market share changes capture all the potential consumer benefits of shopping through the e-commerce option. The magnitude of the welfare gain depends on the elasticity of substitution. Observed e-commerce market shares will imply smaller welfare changes if consumers substitute between local shopping options very elastically, and larger welfare changes if they are inelastic. A similar logic would apply to effects on the entry of local retailers, or on the exit of local stores (where a large period 0 market share means large welfare losses, again tempered by the elasticity of substitution).

Appendix F: RCT and Data Appendix

Data and code of the published paper are provided in Couture et al. (2020).

F.1 Program Description and Background

Following the announcement of the policy objective to expand e-commerce to the Chinese countryside as part of the so-called Number One Central Document in January 2014, the Chinese government entered a partnership with a large firm that operates a popular Chinese e-commerce platform. The program's objective is to provide e-commerce access in rural markets at the same price, convenience and service quality that buyers and producers face in their county's main city center. The firm's objective as part of the program is to penetrate the vast and largely untapped e-commerce market outside of Chinese cities. Rural expansion is one of the firm's strategic priorities over the coming years.

The program makes two main types of investments to enable villagers to buy and sell online through the firm's platform. First, the program invests in the local distribution network, which the firms views as a necessary condition to provide e-commerce access in rural areas. Before the arrival of the program, most villages were not serviced by commercial parcel delivery operators, who had not solved the problem of the "last mile" transportation between dispersed rural households and urban county centers.[8]

The program sets out to change this lack of service with logistics investments targeted at

[8]To receive packages via mail in absence of commercial parcel delivery services, rural households have to travel to the county or township center to pick up the package after receiving notification by mail that it has arrived.

e-commerce. In particular, the firm oversees the construction of warehouses that serve as logistical nodes to pool all e-commerce-related transportation requests to and from the participating villages. These warehouses are located close to the main urban center of the counties with good cross-county transport access. The program also fully subsidizes the transportation cost between these warehouses and participating villages, so that rural households face the same delivery costs and prices as households in the urban parts of the county. The rationale for this subsidy is that village deliveries and pickups start from a low basis, which due to economies of scale in rural transportation makes the starting phase of e-commerce prohibitively costly for village customers despite the investments in warehouses. The calculation of the government and the firm is that as the scale of rural e-commerce grows, per unit transport costs will decline enough to remove the need for a subsidy. Neither the warehouses nor the last-mile subsidy can be used for shipments outside of the firm's e-commerce platform.

The second investment is the installation of a program terminal in a central village location. The e-commerce terminal is a PC, keyboard and mouse connected to a flat-screen monitor mounted on the wall of a dedicated shop space and displaying the firm's website. On the screen, consumers and producers can choose their purchases or see their sales requests on the platform. The firm employs a terminal manager to assist local households in buying and selling products through the firm's e-commerce platform. The terminal manager receives a reward of about 3-5 percent for each transaction completed through the terminal. Before deciding on terminal installations, the firm solicits applications from potential local store operators and schedules an exam for the applicants. The score of this exam is one of the criteria that the firm uses to determine whether a village is a candidate. Villagers can pay in cash when the products arrive at the store for pickup, or they get paid upon delivery of their products for pickup at the store location if selling online. Instead of using the terminal interface, households can also use the firm's e-commerce platform remotely on smartphones or PCs to order product deliveries or pickups at the terminal location. When referring to the new e-commerce option in the text, we include all types of use of the e-commerce platform. The firm views the option to use the village terminals as overcoming three challenges that are specific to the rural population. First, local households may not be used to or comfortable with navigating online platforms. Second, they often do not have access to online payment methods. And third, they may not trust online purchases or sales before inspecting the goods in person or having interacted with buyers directly.

F.2 Surveyor Training and Quality Management

Piloting and Surveyor Training Our survey supervisors are professionals from the Research Center for Contemporary China (RCCC) at Peking University. All RCCC supervisors have previous experience conducting large scale surveys in rural China. Before each of the two survey rounds, we traveled to Beijing to lead a one-day training workshop targeted at the supervisors and a group of graduate students from Renmin University and Jinan University, who were working with us as research assistants on this project. This training walked the RCCC supervisors and our graduate students through each step of the survey design, data collection protocols and quality control protocols that we had shared with them to study carefully in advance. Given budget and time constraints, the survey was paper based. Prior to our baseline survey, RCCC supervisors and our team of graduate students tested our survey design in a pilot survey of 45 households in two villages located in the rural parts of Hebei Province.

In the field, each supervisor was in charge of a team of six surveyors. In addition to the supervisors, two of our trained graduate students accompanied each team in the field. The role of the graduate students was to both support and monitor the recruitment and training of the local surveyors and the data collection, and to report back to us with detailed daily progress reports. Given differences in local dialects and rural conditions, the RCCC recruited surveyors among local university students from the provinces in which the data collection took place. All surveyors were familiar with the local dialect and customs of the rural areas in their home province. Each surveyor completed at least two full days of training and supervised practice questionnaire interviews before joining our field survey team. As part of the training, we provided surveyors with a number of supporting documents. In particular, they received an example of a completed representative survey questionnaire, detailed instructions on how to assist households in answering the questionnaire, a set of cards containing descriptions and examples of consumption products within categories or income-generating activities within sectors, and a set of solutions and best practices for common survey challenges. As described in Appendix F.5 below, we also trained surveyors to use separate pre-prepared spreadsheets to list individual household purchase transactions within product categories or income flows by type of activity. These spreadsheets were used for households to list individual transactions over a given period of time and within categories, before aggregating this information up to complete the final survey questionnaire cells. As part of their training, surveyors were trained to double-check with respondents any answer to the questionnaire that appears inconsistent with a previous answer.

Data Quality Management and Cleaning Surveyors conducted the household survey in teams of two. During the interview, surveyors completed the questionnaire, along with supporting documents used to help households recall, categorize and sum up their consumption expenditures or earnings (we further describe data collection and variable construction for expenditure and earning variables below). As part of quality control, supervisors reviewed one randomly chosen completed questionnaire, supporting documents, and interview audio tape from each surveyor at the end of every day.[9] In addition, our graduate students monitored the survey teams by accompanying them for part of their interviews, and reported back to the supervisors and our team in case of concerns. During recruiting and surveyor training, the surveyors had been informed that lack of accuracy, diligence or patience in the interviews would lead to the termination of employment, while a good record guaranteed a letter of recommendation confirming participation in our research project.

We also asked our surveyors to rate each household respondent along a number of dimensions such as cooperativeness, reliability, level of understanding, and level of interest in our survey. Surveyors also recorded the presence of any other household or non-household member whose presence could affect answers to our questionnaire. In our analysis of the data, we paid special attention to the reliability rating: 1. completely reliable, 2. mostly reliable, and 3. sometimes not reliable. Whenever surveyors rated a respondent as "sometimes not reliable", they also wrote down an explanation for this rating. On the basis of these written explanations, we created a clean household survey dataset. This dataset excludes 0.25 percent

[9] Some households opted out of audio-recording.

of unreliable/uncooperative households entirely from the sample. In other cases, surveyors' explanation suggested that only answers to a particular section of our questionnaire were unreliable. Using this information, we set all income variables to missing for 1.06 percent of all household respondents, all consumption variables to missing for 0.4 percent of households, and all income and consumption variables to missing for 1.31 percent of households. The descriptive statistics in Tables A.1 to A.4 are based on this cleaned household survey dataset. When using total nominal retail expenditure or incomes in RMB as part of the dependent variables on the left-hand side of the regressions, we censor these reported values at the one-percent level from the left and right tails within the survey round.[10] Similarly, price changes between rounds are censored at the 1 percent level. The point estimates remain statistical zeros in all these cases, as is the case post-censoring in the draft, but the standard errors slightly increase. Appendix F.5 below provides additional information about variable construction.

F.3 Experimental Design

Appendix Figure A.1 presents a map of the locations where the RCT takes place. Tables A.1 to A.4 present descriptive statistics.

Selection of Provinces and Counties

There are two main factors determining our survey location in Anhui, Henan and Guizhou, and the 8 counties within these provinces. First, our survey location depended on the timing of the program's roll-out across different provinces and counties, which had been decided before our collaboration with the firm. Second, we were guided by the internal evaluation of the program's senior managers as to whether the provincial and county managers in question would be willing to cooperate with our research protocol. These counties are: Huoqiu (Anhui), Linying (Henan), Linzhou (Henan), Minquan (Henan), Suixi (Anhui), Tianchang (Anhui), Xifeng (Guizhou) and Zhenning (Guizhou). In Appendix D, we are also able to investigate the representativeness of our sample villages relative to all participating villages using the firm's internal transaction data in 5 provinces over this period.

Selection of Villages and Randomization

The unit of randomization is the village. For each county, we obtain a list of candidates that had been extended by 5 promising village candidates that would have not been part of the list in absence of our research. The three main factors determining the village selection within a county from the firm's operational perspective are i) a sufficient level of local population, ii) accessibility by roads, and iii) the presence of a capable store applicant (as measured by the applicant's test score). Overall, we are able to implement randomization on a broad pool of villages selected for participation in the program. This pool, however, is not a random sample of China' rural areas, but instead is likely a group of villages positively selected within each county, with better expected conditions for e-commerce usage on both consumption and production sides.

Upon receipt of this extended list of village candidates for each county, we randomly select 5 control villages and 7-8 treatment villages. The remaining villages on the extended list receive program terminals as planned. The full sample thus includes 40 control villages and 60 treatment

[10] Given more than one percent of observations report zero incomes, nominal incomes are only censored at the one-percent level from the right tail.

villages across the 8 counties, which we selected from a total number of candidates of 432 villages that we received in the extended listings from the 8 county operations teams (on average 54 villages per county). We restrict the list of villages entering the stratification and randomization to villages with at least 2.5 km distance to the nearest village on the county list, where possible.[11] We then stratify treatment and control villages along four dimensions. First, we balance the selection of treatment and control to both have a ratio of 85:15 with respect to pre-existing availability of commercial package delivery (85% not available, 15% available), which is close to the observed ratio among all candidate villages. We obtain information on the availability of commercial package delivery for each village on the candidate list from the program's local county managers (who are not aware what we require that piece of information for). As we discuss below, having villages in our sample with pre-existing commercial delivery services allows us to further investigate the effect of the program that is driven by the terminal access point (i.e. the effect of lifting only the transactional barrier), relative to the effect of providing both the terminal access point and the necessary logistics for local e-commerce deliveries and pick-ups (i.e. the effect of lifting both the transactional and logistical barrier to e-commerce). We further stratify the selection of treatment and control villages on the basis of the equally-weighted average of the z-scores for three village variables: the local store applicants' test score, the village population, and the ratio of non-agricultural employment over the local population. We obtain the last variable from the establishment-level data of the Chinese Economic Census of 2008 which surveys every non-agricultural establishment in the counties (National Bureau of Statistics of China, 2009).

Sampling of Households, Response Rates and Attrition

Our team was granted a two-week window for data collection, after receiving the extended candidate list of candidate villages from the local operation team in each county. Given this tight timeline, we were unable to conduct a village census for sampling purposes. Instead, our survey teams created detailed maps of all residences in the village to implement a random walk procedure.[12]

From each village's map, we defined an "inner zone" of residences within a 300 meter radius of the planned terminal location and an "outer zone" outside that radius. In the baseline data collection (December 2015 and January 2016 in Anhui and Henan, and April and May 2016 in Guizhou), the objective was to sample 14 households from the inner zone and 14 households from the outer zone. To randomly sample households within these zones, we selected 24 residences in both inner and outer zones. The household sampling proceeds as follows: we first randomly assign numbers to all residences within the zone on the map from 1 to n, and then define a rounded integer number $n/24$. Starting from household number 1, we then collect survey data from every household number in steps of the integer $n/24$ until we have completed 14 surveys within the zone. For the endline data collection (12 months after baseline in each

[11] In counties with relatively short candidate lists we had to marginally extent this threshold, leading to a small number of villages with less than 2.5 km distances to the nearest other villages on the candidate list. The mean and median distances for villages without terminals to the nearest terminal location were 10.6 and 9.1 km respectively. Also see related spillover analysis in Appendix C.

[12] We use the boundary of the "natural village" as opposed to the "administrative village". Both of these are known delineations in China. The natural village captures a geographically contiguous rural population. Administrative villages are units with a village committee. In some cases, the administrative village includes more than one natural village.

village), we implement the same procedure for all households that were not part of the baseline survey to select 10 additional households within the inner zone.[13] In the few cases in which there were fewer than 24 residences within the inner zone, we extended the radius until we obtain at least 24 residences on the map. If either the survey respondent or the primary earner of the initially surveyed household no longer resides at the same address, we record this in our data and replace the household with another randomly sampled household within the same sampling zone (inner or outer). In our welfare analysis, we report results both before and after weighting each sampled household in proportion to the share of the village population in its sampling zone.

After introducing our survey to households, our surveyors asked for the household member with the best knowledge of household consumption expenditures and household incomes to respond to the questionnaire. In case nobody answered the door, or in case this most suited household member was not at home during our surveyors' first visit, the surveyors returned at least twice to complete the interview, often outside of working hours. Surveyors were also instructed to skip households with a most knowledgeable respondent older than 75. Overall, our surveyors found willing and able respondents in two thirds of visited residences (66.1 percent).[14] In the endline, we sampled 10 additional households from the inner zone. We used the same sampling methodology as in the baseline. Given expected sample attrition and the objective of 10 randomly selected additional households, the survey teams created a list of 22 new residential addresses in the inner zone and 6 new addresses in the outer zone. In the endline, we replaced a household respondent from the baseline whenever either the household had moved, the primary earner was no longer living there or the original baseline respondent was unavailable after three interview attempts. Using this rule, 71 percent of baseline respondents completed our questionnaire in the endline. As documented in appendix Table A.15, this percentage does not differ in treatment and control villages.

F.4 Retail Price Survey

Store Sampling Prior to the field survey, RCCC supervisors performed a census of all retail stores and market stalls ("stores" for short) located in the village and within a 15-minute walking distance of the boundaries of the natural village. Most villages have fewer than five stores, so in most villages we sampled products from all stores and market stalls in the vicinity of the village. If there were more than 15 stores in a village, we instructed supervisors to collect a representative sample of local retail information, giving more weight (i.e. more price quotes) to more popular establishments within product groups.

Product Sampling and Data Collection The data collection for the local retail price survey was conducted by the trained RCCC supervisors. We aim to collect data on 115 price quotes for each village. 100 of these prices are from the same 9 household consumption categories for retail products as in our household survey (food and beverages, tobacco and alcohol, medicine and health, clothing and accessories, other every-day products, fuel and gas, furniture and appliances, electronics, transport equipment), and 15 price quotes are for local production and

[13] This extended sample was possible due to a small remaining positive balance on the project account that we decided to invest in expanding the household survey sample.

[14] Of the one third of addresses at which our surveyors did not encounter willing and able respondents, 56.6 percent had nobody at home during any of our three visits, 30.5 percent refused to participate in the survey, 7.5 percent had no qualified respondent (due to old age), and 5.4 percent had no one living there.

business inputs. Our protocol for the price data collection closely follows the IMF/ILO standards for store price surveys that central banks collect to compute the CPI statistics. The sampling of products across consumption categories is based on budget shares of rural households in Anhui and Henan that we observe in the microdata of the China Family Panel Study (CFPS) for 2012. Reflecting these consumption weights, supervisors in the baseline survey data aim to collect 47/100 price quotes in food and beverages, 15/100 in tobacco and alcohol, 9/100 in medicine and health, 9/100 in clothing and accessories, 4/100 in other every-day products, 4/100 in fuel and gas, 4/100 in furniture and appliances, 4/100 in electronics and 4/100 in transport equipment. In addition, we collect 15 price quotes for purchases of inputs to production or businesses.[15]

We provided supervisors with pre-prepared price surveys reflecting the number of observations to be collected for each product group. As for the collection of data on household expenses that we discuss above and in Appendix F.5 below, the supervisors were provided with detailed product cards that list product groups within each of the 10 broad categories above, as well as examples of product types within those subgroups of products. They also received instructions on product sampling, for instance about how to evaluate the popularity of an individual product by measuring shelf space and recurrence across different stores. To ensure that we can match identical products in both survey rounds, supervisors saved a picture of each product and recorded product characteristics at the barcode-equivalent level, including packaging type, size, and a detailed product description (name, brand, flavor, etc) wherever possible.[16] For 78 percent of products collected in the baseline, we were able to find the exact same product in the same store one year later in the endline. As documented in appendix Table A.8, this percentage is somewhat smaller in intent to treat villages than in control villages, but this difference is not statistically significant. One challenge of surveying prices in rural China is a frequent lack of price tags displayed in store. As shown in Table A.4, about two thirds of the surveyed products lacked a price tag. In these cases, supervisors asked the store owner for the price that villagers would pay for the product. As part of quality control, we asked supervisors to rate the reliability of store owners' price quotes as good, average or poor. None of the reported findings change in sign, size or statistical significance when limiting the sample to price quotes from labeled products only or excluding reportedly unreliable price quotes.

F.5 Variable Construction

To collect data on household consumption expenditures and incomes from different activities, we trained the surveyors in using separate pre-prepared spreadsheets before filling out the final survey questionnaires. For expenditures, there is one spreadsheet for each of the nine categories that we include in retail consumption, and a separate sheet for business inputs. This allowed households to recall and list all relevant expenses or income flows within a given product group or type of activity over a given period of time. This transaction-level information was then aggregated in the presence of the household to complete the final survey questionnaire sections on expenditures or income flows.

[15]Supervisors sometimes failed to find enough products in a given category within the village. This was often the case for the durable goods categories. In such cases, supervisors replaced products in these missing categories with additional price quotes for products in "other every-day products".

[16]Some store owners refused to let supervisors take pictures. In such cases, we identify identical products in the endline data based on the same store and the detailed recorded product description.

To help respondent recall and categorize their expenditures, surveyors also received cards with examples of products in each category. The product cards break down the retail consumption space into 169 product types within the 10 broad categories we list above. After recording each item in a given category, surveyors go through the list of items and ask respondents how much they paid for each listed purchase. In addition to allocating transactions to different consumption product groups, the surveyors also recorded the modality of each listed purchase transaction (e.g. online vs offline, in the village vs outside the village). This procedure was implemented covering a two-week time window for non-durable household consumption, and a three-month time window for durable goods categories. To obtain total monthly retail expenditure, we multiply the bi-weekly expenditure on non-durables by a factor of 2 and divide durable good expenditure by a factor of 3, and sum up across the 9 consumption categories. For expenditures on the new e-commerce option, we include both direct use of the terminal interface as well as remote usage by ordering deliveries to the terminal through the firm's app. The majority of terminal usage are done in person at the terminal rather than remotely. In most village cases, deliveries and pickups can be made at the terminal location (90 percent). In about 10 percent of cases, the logistics operators offered delivery to the home address too.

To construct total household income, our surveyors again used a pre-prepared spreadsheet to assist households in recording each of their individual income sources over the last month. We defined four income categories: farm earnings, non-farm earnings, remittances (money or in-kind) from family not living in the home, and all other income (e.g. pension, returns from savings, gifts). In addition, we recorded sector of activity and occupation categories for each economically active member of the household. To help household respondents recall and categorize earnings, surveyors used cards with detailed examples of income sources in each category and proceeded to collect each flow on the spreadsheet before filling out the final survey questionnaire in the presence of the household. Our measure of income per capita is the sum of all income sources in these four categories, divided by the number of household members. Our measure of income net of transfers subtracts gifts and remittances from family not living in the home.[17] Our measure of income per capita net of costs subtracts the recorded household expenses used to generate the reported flows of income. The income variables exclude the market value of home production for own consumption.[18] Including this as part of household income has no effect on the statistical zeros that we report in the analysis.

Finally, for households who were either replaced or added as part of our extended sample in the second round (from 28 to 38 households), we define y_{hv}^{Pre} in specification (1) as the mean pre-treatment outcome of households living in the same zone (inner or outer) in the same village. The implicit assumption is that households were not induced to move within or across villages as a result of the program. As reported in appendix Table A.15, we find no evidence that households in treated villages are more or less likely to reside at the same address at endline. We also find no treatment effect on migration decisions of members within households.

[17] Remittances represent on average 13 percent of total household income in our sample.

[18] The market value of all food and beverages that the household produces for its own consumption amounts to on average less than 10 percent of household incomes.

F.6 Township-Level Data on Trade Market Access

As part of our analysis of potential spillover effects on the control group in Appendix C, we estimate the fraction of a rural location's total trade market access that stems from trading relationships with other rural locations in the same county, as opposed to access to larger urban markets within and outside the county. To do this, we use geocoded township-level data from the Chinese Population Census in 2010 (National Bureau of Statistics of China, 2011), which contains information on the recorded population for each of roughly 45,000 township-level administrative units in China,[19] the coordinates of the centroid of each of those units, the type of township-level unit (e.g. urban zones, rural townships) and data on the value added per rural and urban worker at the province level for 2010. See Appendix C for further discussion and details about the estimation.

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[19] This includes both the registered and non-registered population currently residing in the unit at the time of the census. Townships are the most disaggregated unit of observation that we can obtain the full census database for. In China's administrative hierarchy, townships are one layer above villages. In the countryside, townships include on average about 14 villages. In urban regions, township-level units are one level below urban districts.