Telephone Directories for Mobile Phone Networks*

Preliminary and incomplete. Please do not circulate. Comments welcome.

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March 8, 2018

Abstract

Mobile phones have spread throughout developing countries, largely without complementary information services that allow users to search the mobile phone network. We develop a model that predicts both productive and distributive benefits from information services that lower the cost of communication between households and enterprises. To test hypotheses implied by the model we conduct a set of paired RCTs in central Tanzania, centered on the production and distribution of a telephone directory relevant to agricultural households. We randomized enterprises into the directory during a trial period, and randomized household access to the directory at the village level. The directory had substantial impacts on both sides. Enterprises saw large increases in the volume of calls and the use of mobile money. Directory recipients increased search activities and the use of mobile phones for business purposes. There is suggestive evidence of improved farming outcomes for recipients. Survey-based and incentivized measures of willingness-to-pay to be listed (for enterprises) or to receive a directory (for potential recipients,) allow us to estimate the optimal level of directory subsidization.

Keywords: mobile phones; search costs; telephone directories; small and medium enterprises; agriculture.
JEL codes: O13, D83, Q13, M37.

*We thank USAID, BASIS AMA at UC Davis, and the Hitachi Center for financial support. For helpful comments and discussions we thank seminar participants at BASIS AMA technical meetings, Washington, Cornell, Gottingen, the AAEA annual meeting, and NEUDC. We are deeply grateful to our partners at the Institute of Rural Development Planning in Tanzania, especially Adalbertus Kamanzi and Straton Matei. Asia Amri, Grant Bridgman, Alex Katura, Beda Kakuru Henry, Editha Kokushubira, Godfrey Kusekwa, Nimwindie Mchano, Joyce Mdeka, Adili Michael, Neema Mkuna, Geofrey Mwemezi, Audrey Royston, and Jessica Rudder provided excellent research assistance. Any errors are our responsibility.

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1 Introduction

The rapid spread of mobile phones across the globe represents one of the fastest and most comprehensive instances of technological adaptation in human history. By foregoing the construction of nationwide landline telephone systems in favor of mobile phones, developing countries have avoided billions of dollars worth of infrastructure investment, while still enjoying sharp reductions in the cost of communicating across distance.

There is a key difference between the way that mobile phones have rolled out and the way that landlines spread in the late 1800s and early 1900s. There are generally no operator or directory services for mobile phone networks in developing countries. That is the entry point for this study. In this paper we describe the findings of a two-sided RCT that involved the production, distribution, and analysis of a mobile telephone directory. The intervention is motivated by a simple theory that distinguishes between the cost of communication and the other cost components of a search process. The key role of the theory is to demonstrate that the *first* piece of information exchanged between two parties on a phone network – the contact details that allow them to communicate in the future at low cost – is qualitatively different from other information, and far more important than is generally recognized.

The printed, paper telephone directory for this study was designed to be relevant for farming households in a geographically contiguous area of central Tanzania. The directory acted as a treatment both for the enterprises listed in it, and the households that received a copy of it. We find substantial impacts on both sides. Listed enterprises saw large increases in the volume of incoming calls and the use of mobile money. Directory recipients increased their search activities and business contacts outside the village. They also made greater use of mobile money and created non-farm enterprises in response to the treatment. There is suggestive evidence of improved farming outcomes for recipients, through greater use of certain inputs, lower risk of crop failure, and weakly higher output prices.

The paper proceeds as follows. In the following section we provide additional background and context for the study. Section 3 provides the theoretical framework. In Section 4 we describe the directory and the study area. Sections 5 and 6 describe the design and

findings from the enterprise and recipient RCTs, respectively. We conclude in Section 7 with an analysis of recipient willingness-to-pay for a copy of the directory, and enterprise willingness-to-pay to be listed.

2 Background and Context

The mobile telephone transformation has been rapid and all-encompassing. Figure 1 shows the time path of mobile phone adoption over the period 2006-2015, in various regions. In 2006, there were 18 mobile phone subscriptions per 100 people in sub-Saharan Africa. By 2015, that figure had risen to 76, and it is surely higher now.[1] South Asia followed a trajectory similar to that of sub-Saharan Africa. Other regions of the world had higher adoption rates early on, but grew more slowly. During the decade shown, all of the developing and middle income regions substantially narrowed the gap with the OECD countries.

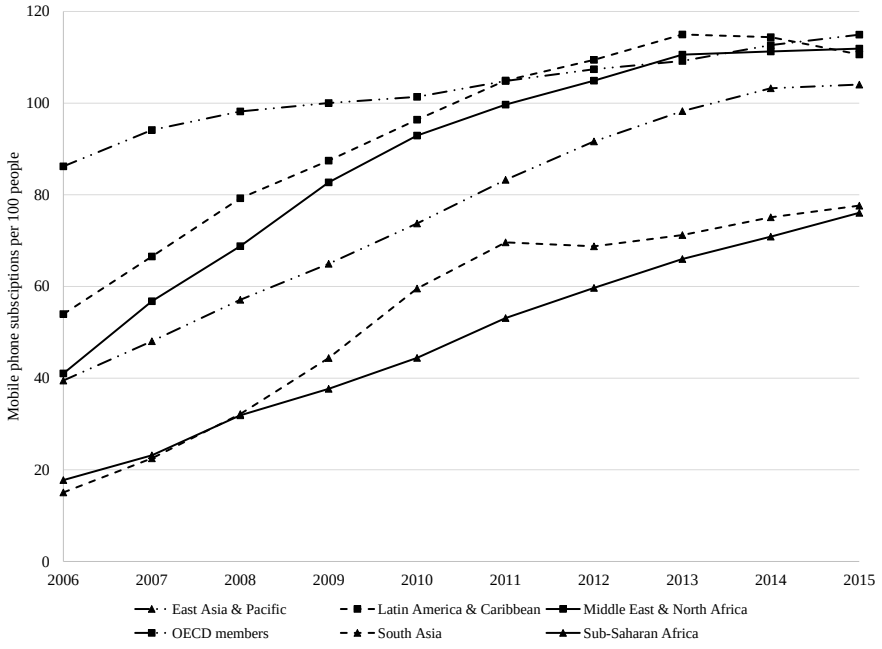


Figure 1: Mobile phone subscriptions per 100 people, by region, 2006-2015

Source: World Development Indicators.

When landlines spread through currently wealthy countries, more than a century ago,

[1] In the 2014-2015 LSMS-ISA data from Tanzania, 79% of households have at lease on mobile phone (authors' calculation).

they did so in conjunction with a key complementary service that has not been replicated in the mobile phone revolution. Landlines were accompanied by information services that allowed users to search the phone network. In the early decades this service took the form of the human operator, who provided a range of services in addition to connecting parties via a switchboard (Brooks, 1976). Operators could steer traffic to particular businesses, or assist callers in finding a relevant business based on its characteristics rather than its name (Barrett, 1935). In the first half of the 20th century operators were replaced by automated exchanges. Printed telephone directories, which had existed since the late 1880s and represented an important marketing platform since their creation, rose to prominence as the primary mechanism by which phone users would search the network.[2] In the last two decades, Internet-based directory services have largely replaced printed directories as the primary search device for phone users in wealthy countries.

Mobile phone users in developing countries do not have access to such services.[3] The implication of this is that most individuals' phone-based networks are functions of their face-to-face networks. People learn of new numbers by interacting directly with the party whose number they wish to acquire, or by following a thread through their face-to-face network to acquire a number. If you ask a resident of a rural village in Tanzania to describe the process by which he or she would locate the phone number of a business in a nearby town, the response will invariably involve personal travel, communication with prior contacts, or both (we have done this dozens of times). A mobile phone might be involved in the search, but at its root the process relies on pre-existing networks. This is very different from how phone users in wealthy countries find the contact information of new potential trading partners. The lack of systematic directory services not only reduces the productivity of phones, it also skews the private returns toward those with strong pre-existing social networks and the capacity to travel and gather numbers.

The directory intervention in this paper provides recipients with contact details for

[2]For a humorous and insightful history of the telephone book, see Shea (2010).

[3]In many countries a Yellow Pages is printed and distributed in major cities. These booklets include only the small fraction of businesses that are formal, urban, and have landlines.

agriculture-related enterprises located nearby. No attempt is made to push other specific types of information to farmers, or to influence the type of information exchanged between parties once one chooses to communicate with the other. This distinguishes the current study from most of those in the literature. Early papers examining the effect on mobile telephony on agricultural outcomes used the roll-out of mobile phone towers to show that lower communication costs reduced price spreads between markets (Jensen, 2007; Aker, 2010).[4] A large and growing set of impact evaluation studies estimate the effects of phone-mediated information interventions on the prices received by and production choices of farmers.[5] Many studies in this domain are based on the provision of specific, narrowly targeted information, such as crop prices at nearby markets, that researchers or their partner organizations push to farmers via mobile phones. The modal impact of these studies has been zero, though some have had the intended effects on a subset of outcomes. One possible interpretation of the large set of null results in this area is that information markets work better than they appear, making it difficult for service providers to identify a specific type of information product that has broad benefits for farmers in treated communities.

Contact information is qualitatively different from other types of information provided to farmers. The directory is not intended to directly affect households' agricultural decisions by changing information sets. Rather, the directory substantially lowers the cost of acquiring any other type of information that a recipient might want to acquire from a listed enterprise. The telephone directory effectively breaks the link between individuals' mobile phone networks and their face-to-face networks.

The directory also has important implications for the listed enterprises. The large majority of enterprises in Tanzania engage in no formal advertising. It is plausible that the high cost of advertising the enterprise's existence to potential customers is a key constraint on growth. In that respect this paper contributes to the important literature on SME growth (or lack thereof) in developing countries (Beck and Demirguc-Kunt, 2006; Ayyagari, Beck

[4]Aker and Mbiti (2010), Aker (2011) and Nakasone, Torero and Minten (2014) provide reviews of the literature.

[5]See, among others, Muto and Yamano (2009); Fafchamps and Minten (2012); Nakasone et al. (2013); Courtois and Subervie (2014); Aker and Fafchamps (2014); Hildebrandt et al. (2015); Asad (2016).

and Demirguc-Kunt, 2007). The directory represents a first step toward the development of a widely available marketing platform for SMEs in sub-Saharan Africa, with potentially important implications for long-run productivity growth in the sector.

3 Theory

Consider a household i that has fixed wealth w_i at the start of period t. The household can consume its wealth as a money-metric consumption good, c. It can also spend money to engage in trade with one or more of J enterprises, indexed $j = 1 \dots J$. Trading provides the household with a match-specific net gain, ϕ_{ij}, in units of a composite good t. This composite good takes account of the quality of the goods or services exchanged, the price, and all search and transaction costs other than the cost of communication. We adopt this general approach because it describes parsimoniously the wide range of possible household-enterprise interactions (households may buy goods from some enterprises, sell to others, or engage only in information exchange). Assume initially that the ϕ_{ij} are known and fixed. The binary variable t_{ij} takes a value of 1 if a trade takes place between household i and enterprise j, and 0 otherwise.

Household utility is represented with the money-metric function $u(c_i, t_i) = c_i + v(t_i)$, where $t_i = \sum_{j=1}^{J} t_{ij}\phi_{ij}$ and the function v satisfies $v' > 0$ and $v'' < 0$. Trading gains enter the v function, rather than entering u directly, to capture diminishing returns to trades. This reflects the reality that many of the economic activities of rural households – sourcing inputs, gathering price information, selling output – exhibit diminishing marginal returns.

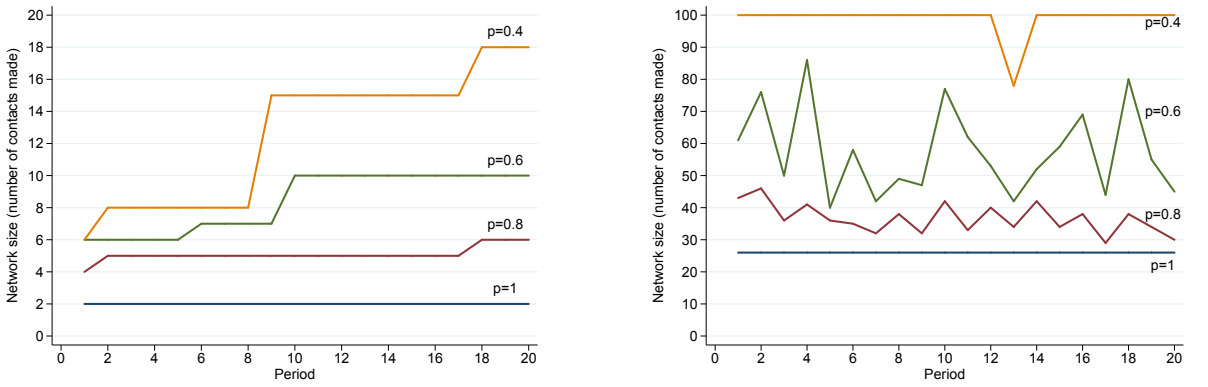
The only transaction cost not represented in ϕ_{ij} is the cost of communication. We separate the cost of communication from other search costs in order to highlight the importance of information clearinghouses like telephone directories. For a trade to take place, the household and the enterprise must communicate. Most such communications are initiated by the household, and we will restrict attention to that case. The cost of communication is s_{ij}^0 if the two parties have never previously communicated, and $s_{ij} < s_{ij}^0$ if they have. For

simplicity we set $s^0_{ij} = s^0$ and $s_{ij} = s$ for all i, j. Think of s as the cost of a phone call, while s^0 is the cost of traveling in person to establish a new contact. Acquiring information from someone about *how to communicate with them in the future* – i.e., getting their phone number – is qualitatively different from acquiring other types of information. Our interest is in the possibility that high costs of acquiring contact information can act as a bottleneck on search.

The household's utility maximizing choice can be characterized by the set of contacts that it makes. In period 0 the cost of communicating with all enterprises is s^0. Let the $\phi_{i1} \ldots \phi_{iJ}$ be ordered from largest to smallest, and define $t^k_i = \sum_{j=1}^{k} \phi_{ij}$. The household engages in k trades, where k is the positive integer such that $v'(t^k_i) > s^0$ and $v'(t^{k+1}_i) < s^0$. The k enterprises with whom the household trades represent its initial network. In future periods the household continues to interact only with those initial k enterprises, because the marginal benefit of communicating with the $k+1$th enterprise does not exceed s^0.

Simple modifications to the model allow the household's network to stochastically evolve. One possibility is to allow for idiosyncratic shocks to trading, due to stock-outs or other interruptions. Suppose that after the household communicates with enterprise j, the trade is only completed with probability p. The expected number of period 0 trades is then $m \geq k$, where m is the positive integer such that $v'(pt^m_i) > s^0$ and $v'(pt^{m+1}_i) < s^0$. Households will develop networks of different sizes based on their match specific valuations, ϕ_{ij}, and the realized distribution of non-completed trades.

Panel A of Figure 2 shows the simulated time path of contacts made by a household with CRRA utility over the composite good, $v(t) = \frac{A}{1-\lambda} t^{1-\lambda}$. The vertical axis shows the number of enterprises with which the household communicates, regardless of whether a trade takes place. In the deterministic case, $p = 1$, the household's network is time invariant. As the probability of completing a trade decreases, the household searches widely with increasing probability, and its network grows. Once a contact is established, the household tends to contact all previously contacted enterprises, because doing so costs only $s < s^0$ (though it is not necessary that the number of contacts per period grows monotonically).



A. Baseline case, $s^0 = 1$, $s = 0.25$, $\lambda = 0.65$, $A = 4$ B. With directory, $s^0 = s = 0.25$, $\lambda = 0.65$, $A = 4$

Figure 2: Simulated time path of enterprise contacts for a single household

Source: Authors.

The communication cost function provides an entry point for modeling the impact of a telephone directory. Suppose that the household receives a directory that lists contact information for the J enterprises. This effectively sets $s^0 = s$ for all enterprises. The cost of communicating with someone previously unknown becomes the same as the cost of communicating with a known contact. Panel B of Figure 2 shows the time path of enterprise contacts under this scenario, with the same parameterization as Panel A. Note the difference in vertical axis scales. The key takeaway is that the number of contacts made by the household increases substantially in all cases. The number contacts no longer increases monotonically when $p < 1$. In general, the probability that the number of contacts would be non-decreasing in time is increasing in the difference $s^0 - s$.

The household's willingness-to-pay (WTP) for the telephone directory can be easily derived. For the deterministic case with $p = 1$, let l represent the utility-maximizing number of contacts after the household receives the directory ($l = 26$ in Panel B of Figure 2). Utility without the directory is $u_0 = u(c_i, t_i^k) = w_i - ks^0 + v(t_i^k)$. Utility with the directory is $u_1 = u(c_i, t_i^l) = w_i - ls + v(t_i^l)$, which is not less than u_0. The household's maximum WTP is the difference $u_1 - u_0$. In the limit as $s^0 \to s$, u_1 converges to u_0.

We have so far considered the case of a household in isolation. Yet, the information in a directory is not fully rival. Sharing may take place between networked households. Suppose

that the household knows N other households that have a copy of the directory. Let $\gamma(N)$ be the probability that the household *cannot* use someone else's directory (at no cost). Clearly $\gamma(0) = 1$, and we assume $\gamma(N) \to 0$ as N grows. The expected cost of communication with an unknown enterprise is now a function of the size of the household's network, which can be written as $s(N) = s(1 - \gamma(N)) + s^0\gamma(N)$. Expected utility for a household without its own copy of the directory is $u_2 = w_i + \gamma(N)(v(t_i^k) - ks^0) + (1 - \gamma(N))(v(t_i^l) - ls)$. It is clear that $u_1 > u_2 > u_0$, and the household's WTP for a directory is decreasing in N.

4 The *Kichabi* Telephone Directory

In this section we describe the telephone directory that represents the primary intervention for both sides of the experiment.

Many of the businesses that are relevant for farmers are small and informal, such as households with milling machines, individual transporters with bicycles, or kiosk owners who buy and sell food crops. There are no existing registries or enterprise surveys in Tanzania that cover this population. We conducted our own surveys to collect the information for the directory.

Figure 3 shows the project timeline. The first survey activity, in July-August 2014, was a census of the agriculture-related enterprises in a geographically contiguous area of central Tanzania. We chose four districts for the study, three in the Dodoma region and one in the Manyara region. Within each district we conducted the census in villages that satisfied at least one of two criteria: (*i*) population of at least 4,000 inhabitants in the most recent census, or (*ii*) ward capital. (The administrative structure in Tanzania is region-district-ward-village-subvillage, and a typical ward contains 3-5 villages.) By these criteria the census covered 49 out of 108 villages in the study area. We also conducted the census in two nearby cities, Dodoma and Babati, which represent important commercial centers for some villages in the study.

Figure 4 shows the study area, with the census towns and villages marked. Dodoma,

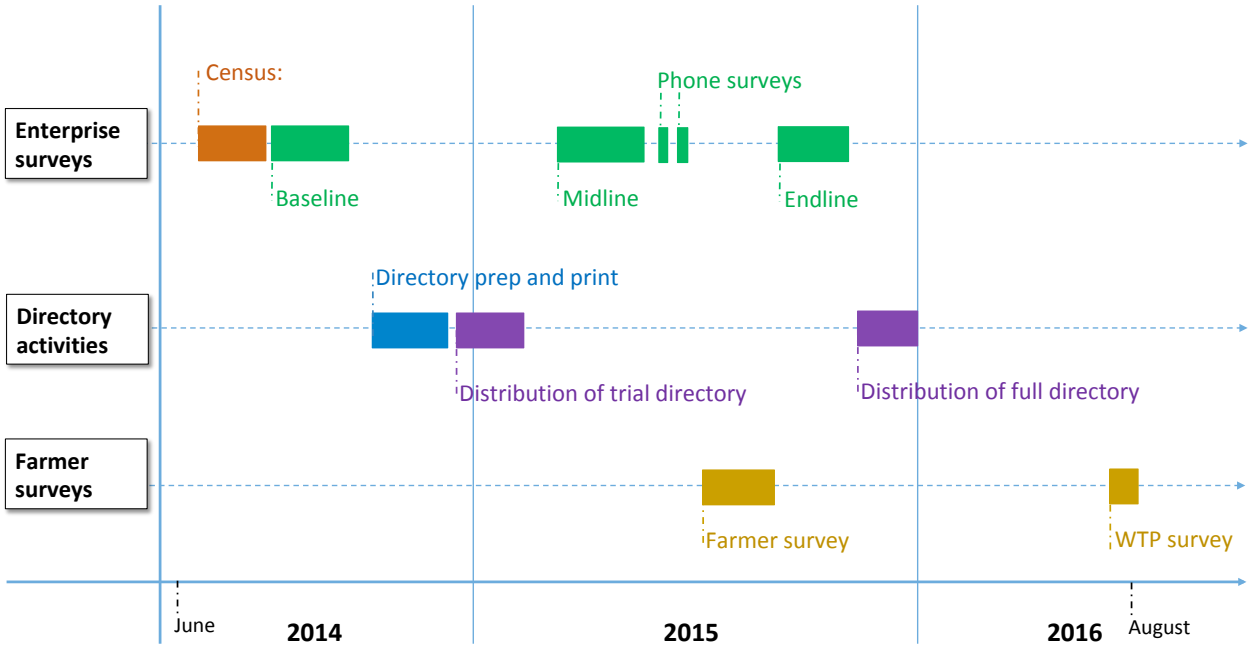


Figure 3: Timeline of fieldwork activities

in the southwest corner, is the capital of Tanzania and the largest city in the study. Other large towns include Kondoa, in the northwest; Kibaya, in the northeast; and Babati, the only census town not shown on the map, which lies north of the northwest corner of the map. The pictured region is roughly 8,000 square miles, with most villages in a 5,000 square mile area. This is a region of semi-arid plains, with some lightly forested areas. There is one rainy season, from January to May. Planting takes place from December to early February, and harvest is from May to July. Maize and sunflower are the primary crops, and most households plant additional crops such as beans, cassava, or potatoes.

In each census village we followed a standard protocol, systematically walking each subvillage to search for enterprises. We covered eight sectors: wholesale trade, retail trade, transport, hiring/renting, agricultural processing, skilled tradespeople, non-agricultural services, and financial services. The team approached approximately 2100 enterprises, and enrolled 1,506 (72%). The final study population consists of 1,495 enterprises, after data cleaning. During the census we collected basic details only: enterprise name, location, sector, respondent name, phone number(s),[6] number of employees, and a short description of

[6]We encouraged respondents to designate the number of the person responsible for day-to-day operations

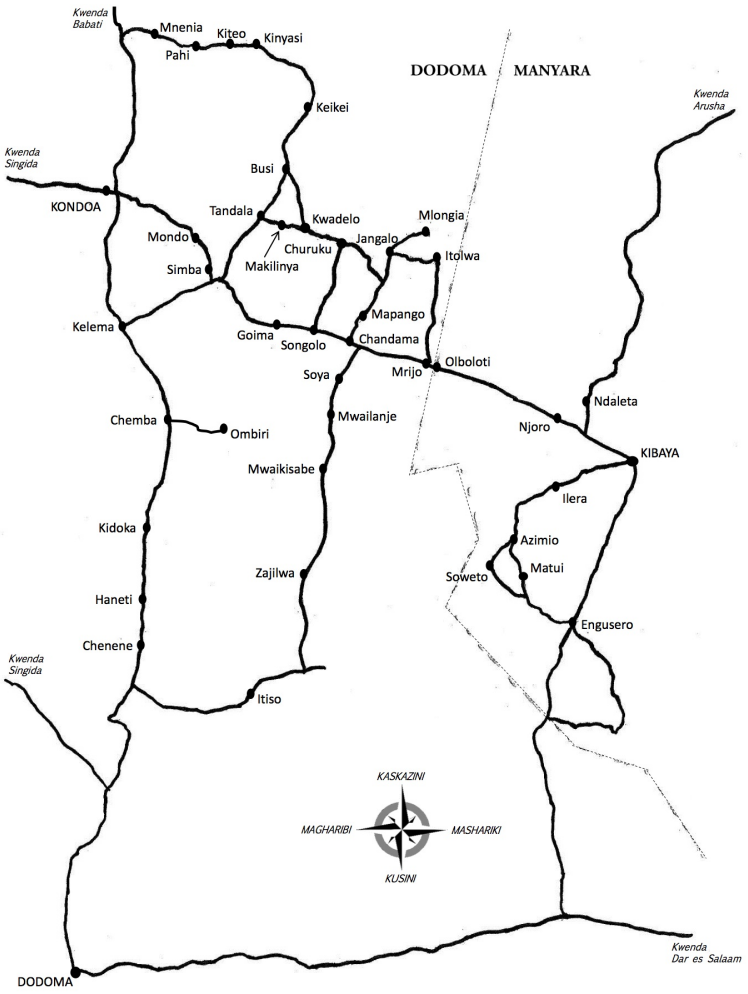


Figure 4: Map of study area

the enterprise. The descriptions allowed for some differentiation between otherwise indistinguishable enterprises in the same sector and location. Additional details about the census are in Appendix A.

The census data populate the telephone directory. We printed the directory as a folded A4 booklet called *Kichabi*, short for *kitabu cha biashara* ("business book" in Swahili). The directory lists enterprises alphabetically by village, sector, subvillage, and enterprise name. Figure 5 shows a snapshot of the booklet, with entries from the villages "MNENIA" and "MONDO." The primary phone number for the business is at right. The letter codes "A", "T", and "V" indicate mobile networks. If the enterprise has a second phone number

as the primary number, rather than that of the owner (if different).

it is listed in the description column. The larger group of Mnenia entries are retailers, differentiated by the description field: *Sokoni* is "at the market," *matunda* indicates a specialty in selling fruit, *Biashara ndogodogo* is a "small business," likely a kiosk. The Mondo entries shown are all *Fundi*, skilled tradespeople, in subvillage Araa Kati. All three are tailors (*Fundi cherehani*).

Kijiji-sekta au jina la biashara	***Kitongoji/mtaa***	***Maelezo ya shughuli, sekta nyingine, au namba nyingine***	***Namba ya simu***
Kavindi Supplier	Msikitini	Jumla; mazao ya kilimu	A 789032035
Mnunuzi na Muuzaji wa mihogo - Hija	Msikitini	Jumla; mazao ya biashara; mahindi	V 757517853
Subira Group - Wauzaji wa miche ya miti na asali	Msikitini	A 787158359	A 787456754
MNENIA - Wafanyabiashara wa Rejareja			
A Shop	Msikitini	Duka	T 652625962
Genge la Mariam	Msikitini	Biashara ndogodogo	T 714319223
Genge la Shangazi	Msikitini	Biashara ndogodogo	A 684319959
Kidisa Bustani	Msikitini	Sokoni	A 682264585
Maguo Shop	Msikitini	Duka; nafaka; A 783288699	T 717205419
Muuzaji wa Mbogamboga - Vudu	Msikitini	Biashara ndogodogo; viungo; matunda	A 782776215
Salum Shop	Msikitini	Duka	A 787011534
Yusuf Spare Shop	Msikitini	Duka; T 719996930	T 715634797
MONDO - Fundi			
Fundi Cherehani - Jera	Araa Kati	Fundi cherehani	A 788610072
Fundi Cherehani - Mama Mchungaji	Araa Kati	Fundi cherehani; A 681323267	A 685698421
Fundi Cherehani - Mama Zahara	Araa Kati	Fundi cherehani; T 659921925	A 785521659

Figure 5: A snapshot of directory entries

We printed and distributed two versions of the directory. The first, a trial directory, served as the intervention for both listed enterprises and recipient farmers (details below). The trial directory included only a subset of the enterprises. The second printing, the full directory, included all 1,495 enterprises. We distributed the full directory to avoid potentially disadvantaging control group enterprises for the long run. The trial directory was distributed in December 2014 and January 2015, as households were finishing land preparation and beginning to plant crops. The full directory was distributed at the end of 2015. At that point we distributed almost 7,000 copies: one for every census enterprise, and one for every attendee at the distribution meetings, including the treated farmers who had received the trial directory.

To distribute the trial directory we randomly selected in each study ward *(a)* one village where we had not conducted the census, and *(b)* one village where we had. The latter group consists of the larger villages and the ward capitals.[7] This led to a distribution

[7]We did not distribute booklets in the cities, Dodoma or Babati. We conducted the census in these villages in order to add value to the directory, but focused the directory distribution on farmers in village.

area of 47 villages. In each village we held a distribution meeting, coordinated in advance with village leaders. At the meeting we introduced the directory, provided examples of how to use it, and answered questions. We then distributed 70 directories. Most meetings had more than 70 participants, in which case we chose recipients by drawing numbers. The team recorded the names and contact information of everyone in attendance, and noted who received the directories.[8] In total we distributed 3290 trial directories.

The two-sided nature of this experiment introduced a unique challenge for statistical power. If we were only running an RCT on the enterprises, we would have distributed directories in every village, in order to maximize the effectiveness of the treatment. We rationed the directory distribution in order to generate experimental variation for recipient farmers. Likewise, if we were only running a farmer experiment, we would have included all enterprises in every printing of the booklet, to make the intervention as beneficial as possible. Final design choices, balancing the needs of the two RCTs, were based on our pilot work and on discussions with local partners.

5 Enterprise Experiment: Design and Findings

We turn now to the enterprise RCT. The intervention for this part of the experiment is the listing of the enterprise in the trial directory that was distributed in December 2014 and January 2015. For treated firms, this represents an exogenous increase in exposure, a form of advertisement, to a large set of potential customers or trading partners in the surrounding area. The evaluation period ran from January to September-October 2015, when we conducted the endline enterprise survey.

[8] A few meetings had less than 70 participants, usually because of heavy rain. In those cases, everyone in attendance received a directory, and the remaining were distributed evenly among residents of different subvillages, with a request that they give them to their neighbors. Team members returned to the village a few days later to record the names and contact information of the last group of directory recipients.

5.1 Enterprise randomization

Prior to printing the trial directory we randomized enterprises into three categories, in two steps. First, we assigned 7 of the 49 villages to a Pure Control (PC) group, stratifying on district.[9] Enterprises in the PC group were not printed in the trial version of the directory.[10] Second, in the remaining 42 villages, we assigned firms at the subvillage-sector level to either Treatment (T) or Control (C), stratifying at the village-sector level. Treated enterprises were listed in the trial directory; control firms were not. Figure 6 shows a schematic of the firm experimental design.

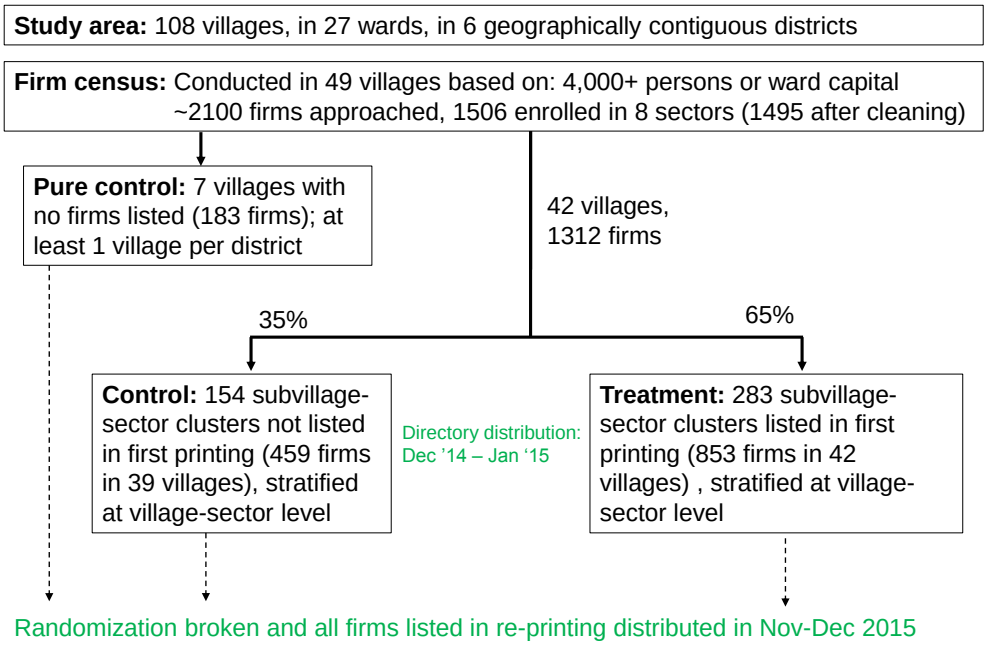


Figure 6: Enterprise experiment design

Randomization at the subvillage-sector level shut down the possibility of highly localized spillovers, which could be positive or negative. While such effects would be interesting to measure, we were concerned about a lack of statistical power. Assigning treatment at the

[9]The cities of Dodoma and Babati are large enough that each *mtaa*, or neighborhood, was treated as a separate "village" for purposes of treatment assignment.

[10]The Pure Control group was established to allow us to detect within-village, between-subvillage spillovers. Villages in Tanzania tend to be larger in both geographical area and population than those in many other countries of sub-Saharan Africa. Subvillages in Tanzania are more akin to villages elsewhere. These spillovers could be positive, if the directory increases foot traffic to the village and improves business for everyone, or negative, if the listings draw customers to treated subvillages at the expense of control subvillages.

subvillage-sector level also reduced the likelihood that we would list one enterprise, but not its next door rival, which may have upset owners of control firms who learned their treatment status.[11] For the same reason, and to increase the usefulness of the directory for recipient farmers, we assigned clusters to the T group with 0.65 probability and the C group with 0.35 probability.

5.2 Enterprise surveys and descriptive statistics

After the census, we conducted five rounds of surveys with study enterprises: baseline, midline, phone survey 1, phone survey 2, and endline (Figure 3). All surveys other than the baseline occurred post-treatment. We conducted four follow-up surveys in a relatively short time span because we expected impacts on noisy outcomes, such as incoming phone calls, to be easier to detect with repeated post-treatment measures (McKenzie, 2012).

We randomly selected 440 enterprises for the baseline survey, over-sampling from two sectors that we hypothesized could be important to farmers but that represented small shares of enterprises in the directory: transporters, and hiring and renting. The latter group includes tractors for rent. Figure 7 shows the distribution of enterprises across sectors, with an indication of the share surveyed at baseline. Roughly half of the listed enterprises are merchants or retailers, some of whom buy crops from local farmers, and many of whom can provide information about the going rates for crops.

The baseline survey was conducted in September-October 2014. The survey covered a range of topics, including: tenure and operating months; use of mobile phones; employment; sales, revenues, and costs; and marketing and advertising activities. The primary outcome variable in the study is the number of incoming business-related phone calls received during recent days. During the baseline the recall period was seven days. Enumerators asked respondents if they could look through the phone history together, to improve the accuracy of responses for this variable.

[11] We made repeated efforts during the census to tell enterprise owners about the trial periods during which some enterprises would not be listed. Nevertheless, we wanted to minimize potential discontent.

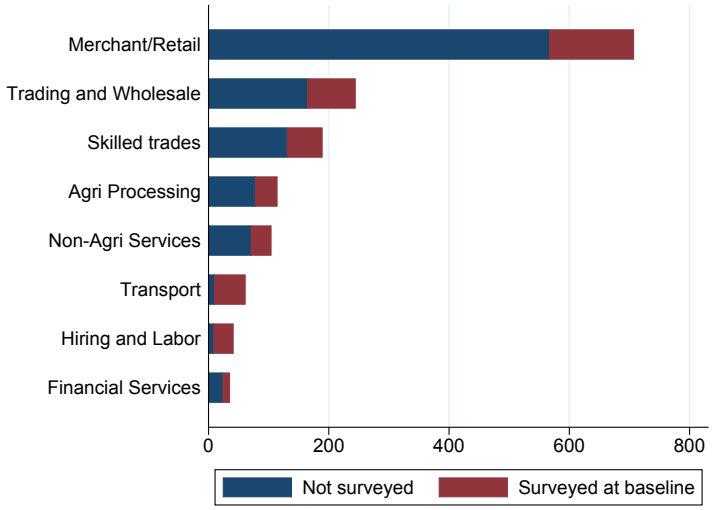


Figure 7: Distribution of enterprises in the census and baseline survey

In March-May 2015 we conducted a midline survey. This was six months after the baseline, and 3-4 months after directory distribution. This survey covered many of the same outcomes as the baseline, with the addition of a module for businesses that had temporarily or permanently closed in the interim. At midline the team successfully re-surveyed 421 of the original 440 sample enterprises (96%). We also interviewed 18 replacement firms, bringing the midline sample size to 439 enterprises. In this and all subsequent survey waves we gave respondents the opportunity to change the phone number listed for the business, if necessary (though few ever did).

In response to ongoing qualitative work and informal discussions with sample enterprises, we added two key outcome variables at the midline: missed calls, and incoming calls from new customers (each over the last two days). We also changed the recall period for the primary outcome variable, number of incoming business-related calls, from seven days to two. We made this adjustment because both respondents and enumerators felt that seven-day recall was onerous and subject to measurement error.[12]

From late May to early July 2015 we conducted two short phone surveys with the enterprises from the midline sample. The phone surveys covered only three primary outcome

[12] ANCOVA estimation is robust to this type of recall adjustment (McKenzie, 2012).

variables: number of incoming calls, number of incoming calls from new customers, and number of missed calls. Enumerators reached 392 midline firms in the first phone survey round, and 375 in the second round.[13]

The endline survey with the enterprises took place in September-October 2015. The endline survey followed the same broad outlines as the baseline and midline surveys.

Table 1 presents summary statistics for surveyed enterprises at baseline.[14] Most businesses have a single male owner, who works alone or with a single employee. Roughly a third of enterprises employed a family member of the owner during the previous week. The majority of enterprises work out of a storefront, market stall, household dwelling, or fixed open-air location; 15% describe themselves as "mobile businesses." The average enterprise owner (or manager, in less than 10% of cases) has 170-190 contacts stored in his/her phone, and receives roughly one business call per day.

Column 4 of Table 1 shows the p-value on the treatment dummy variable from separate regressions of each variable on the treatment dummy and the strata fixed effects. Standard errors in these regressions are clustered at the strata level. There is only one variable with a statistically significant difference by treatment at baseline, and it happens to be the most important first-level outcome variable: number of incoming business calls. Control firms received 0.31 more calls per week (p-value = 0.07). Otherwise the samples are balanced on the wide range of characteristics considered in Table 1.

The baseline survey took place between harvest and planting, a slow time of year for many agriculture-related enterprises. Later in the study period we observe substantial increases in calling volumes for both treated and control firms. Figure 8 shows the scatter and local polynomial regression of the number of incoming calls per day against the interview date. The first cluster of observations in Figure 8 is the baseline survey. The central group

[13]There are two reasons that these re-survey rates were lower than those at midline. The first is that the phone surveys moved quickly, allowing little time to re-contact enterprises that were not found in the first attempts. The second is that phone numbers for some enterprises that changed their phone number between the baseline and midline surveys were not updated for the phone surveys (which was an oversight).

[14]For the continuous variables used in this paper, we designated as an outlier any observation more than five standard deviations away from the mean, calculated separately for each survey wave. These we replaced with the median. This led to 0-2 replacements in the vast majority of cases.

Table 1: Balance table: enterprise characteristics at baseline

	Treatment	All control	N	p-value
	(1)	(2)	(3)	(4)
Interviewee is male (=1)	0.83	0.84	440	0.66
Interviewee age	38.16	38.51	440	1.17
Interviewee is owner (=1)	0.91	0.87	440	1.02
Single owner, is male (=1)	0.80	0.79	440	0.37
Mobile business (=1)	0.15	0.15	440	0.82
Business based at home (=1)	0.22	0.20	440	1.60
Electricity access (=1)	0.74	0.77	440	1.40
Days open per week	6.28	6.16	438	0.82
Family workers in last week (=1)	0.34	0.29	440	0.52
Num. family workers	0.53	0.49	440	0.51
Permanent workers in last week (=1)	0.18	0.23	440	0.54
Num. permanent workers	0.35	0.43	440	0.78
Temporary workers in last week (=1)	0.18	0.24	440	1.26
Num. temporary workers	0.58	0.76	439	0.62
Number of sales, last week	18.02	14.49	401	0.60
Made sales on credit, last week (=1)	0.41	0.41	440	0.99
Number of business purchases, last week	1.13	3.20	426	1.58
Sales revenue, last two days	1.5e+05	2.5e+05	422	0.50
Number of contacts in phone	172.21	189.47	410	0.94
Business calls received, last week	6.55	6.86	384	0.07*
Business calls made, last week	5.44	6.44	380	0.10
Business texts received, last week	1.75	1.91	338	0.39
Business texts sent, last week	1.41	1.66	326	0.29
Phone accesses internet (=1)	0.18	0.19	440	1.54
Use internet for business (=1)	0.09	0.10	440	0.53
Mobile money incoming, last week (=1)	0.34	0.42	440	0.55
Mobile money outgoing, last week (=1)	0.29	0.37	440	0.95

Notes: Authors' calculations from baseline survey with firm sample. Columns 1 and 2 are sample means. Column 4 are the p-values on the treatment dummy variable in regressions of each variable on a binary variable for the treatment and dummy variables for the randomization strata.

includes the midline and two phone surveys. The final cluster is the endline. The periods of greatest calling activity occur in May and June, during the phone surveys. Yet even at endline, which took place one year after the baseline, the average number of incoming calls per day was substantially higher in 2015 than in 2014. This reflects both the treatment effect from the directory (as we will see below), and secular trends in Tanzania toward increased

use of mobile phones for business purposes.

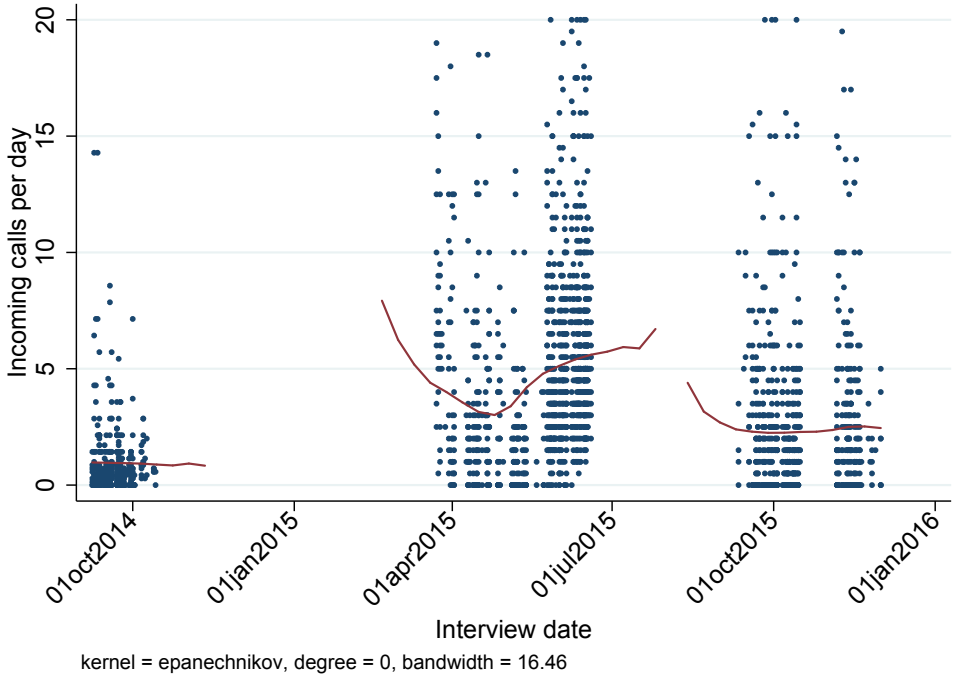


Figure 8: Enterprises: incoming business-related calls per day

5.3 Empirical specification for impacts on enterprises

With baseline values and repeated follow-ups for most enterprise outcomes, we use an ANCOVA specification to estimate the impacts of the directory listing on enterprises. The general form of the estimating equation is:

$$Outcome_{ir} = \alpha + \beta T_i + Outcome_{i0} + X_{i0} + round + strata + \epsilon_{ir} \quad (1)$$

where $Outcome_{ir}$ is the value of the outcome for enterprise i in follow-up round r, with $r = 1 \ldots 4$ for the midline, phone survey 1, phone survey 2, and endline, respectively; T_i is a binary variable equal to 1 if the enterprise is in the treatment group; $Outcome_{i0}$ is the baseline value of the outcome variable (set to zero if missing); X_{i0} is an indicator for whether the baseline value is missing; *round* is a set of round dummy variables; *strata* is a set of strata dummy variables; and ϵ_{ir} is a statistical error term. The survey data do not cover the

universe of treatment clusters in the study population,[15] so we cluster standard errors at the level of treatment assignment (Abadie et al., 2017).[16]

In practice we use the full version of equation (1) only for the primary outcome of interest, the number of incoming business-related phone calls. This was the only outcome variable collected at baseline and in every follow-up round. Two other primary outcomes, number of missed calls and number of calls from new customers, were not collected at baseline. For these we estimate the OLS equivalent of (1), without the baseline control.

We also examine impacts on a number of secondary outcomes. These are not secondary in importance. Rather, they are important economic variables that might be affected by the treatment, but via the primary effects. These include a range of communication, mobile money, employment, and sales outcomes. Data on these secondary outcomes were collected at baseline, midline, and endline, but not in the two phone surveys. For these we use the ANCOVA specification of (1) on the two rounds of follow-up data.

5.4 Results: impacts on enterprises

Table 2 shows the estimates of $\hat{\beta}$ from equation (1). In the first "Primary outcomes" panel we see that listed enterprises experienced a substantial increase in the number of incoming business-related calls. The treatment effect of 2.46 additional calls during the prior two days represents a 27% increase over the control group mean of 9.11. Estimated effects on the number of calls from new customers and the number of missed calls are smaller in magnitude and not statistically significant.

Social desirability bias is a natural concern in this setting, as treated respondents may have intuited the study goals and exaggerated the number of incoming calls.[17] As a verification step, enumerators asked respondents if they could look through the phone

[15] There are 437 clusters in the study population, 272 of which are represented in the survey data.

[16] Pure control enterprises are not included in this part of the analysis, because there is no variation in treatment assignment within PC villages. We use the PC villages for the willingness-to-pay study, below.

[17] For what it's worth, our qualitative work and ongoing conversations with the study participants gave us little reason to worry about this. Enterprise owners treated this as a business opportunity and never seemed inclined to spare the feelings of the study team members.

Table 2: Impact results for enterprises, ANCOVA

Dependent variable	Coeff. (1)	s.e. (2)	p-val (3)	N (4)	Control mean (5)	% change (6)
Primary outcomes (full sample)						
Number of business calls received	2.46***	0.86	0.004	1398	9.11	27.0
Number of calls from new customers	0.28	0.30	0.351	1276	2.56	11.0
Number of missed calls	0.10	0.16	0.518	1398	1.28	8.1
Primary outcomes (if enumerator able to check phone history)						
Number of business calls received	2.67***	1.03	0.010	1063	8.83	30.3
Number of calls from new customers	0.25	0.36	0.482	977	2.49	10.1
Number of missed calls	0.38**	0.18	0.035	1064	1.12	34.4

Dependent variable	Coeff.	s.e.	p-val	N	Control mean	% change
Secondary outcomes						
Communication						
Number of outgoing business calls	0.33	0.40	0.407	725	2.72	12.2
Number of incoming SMS messages	0.05	0.18	0.784	726	1.43	3.4
Number of outgoing SMS messages	0.06	0.21	0.787	726	1.20	4.8
Mobile money						
Use mobile money (=1)	0.08*	0.05	0.087	573	0.67	11.6
.... to receive payments (=1)	0.09*	0.05	0.060	573	0.57	15.9
.... to send payments (=1)	0.11**	0.06	0.045	573	0.54	20.5
Employment						
Any workers besides owner (=1)	0.06	0.05	0.272	741	0.48	11.7
Number of workers	0.25	0.23	0.282	741	1.08	22.9
Any paid, non-family workers (=1)	0.10	0.07	0.135	741	0.24	40.7
Number of paid workers	0.12	0.18	0.514	741	0.69	16.8
Sales and revenue						
Number of business purchases	1.14	1.27	0.369	328	1.33	86.1
Number of sales transactions	1.09	2.72	0.690	473	18.46	5.9
Sales revenues (TSH)	60221	137161	0.661	470	335840	17.9

Notes: Authors' estimates from survey data. All regressions other than those for "new customers" and "missed calls" in the top panel include a control for the baseline value of the dependent variable, as well as survey round and strata fixed effects. Standard errors clustered at the level of treatment assignment. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

history together. When the analysis is limited to the approximately 75% of respondents who agreed to this at least once during the surveys, results are stronger. The coefficient on number of incoming calls increases to 2.67 and is still highly significant. The effect on the number of missed calls increases almost fourfold, and is statistically different from zero.

These findings should not be interpreted as a form of heterogeneity analysis, because we do not know the process that led respondents to agree to having the phone history checked. Rather, the results on this subgroup provide assurance that measurement error in the key outcome variables is not systematically correlated with treatment assignment in a way that biases treatment effects upwards, at least on three quarters of the sample.

The lower panel of Table 2 reports impacts on secondary outcomes. There are no negative coefficient estimates, and many are of substantial magnitude. For the most part these estimates are imprecise. The exception is the group of estimates related to mobile money. Effects on these outcomes are significant with 91-96% confidence, and of substantial economic magnitude. Treated enterprises are 12% more likely to use mobile money at all, and 21% more likely to send outgoing mobile money transfers.

In appendix section B we provide an analysis of heterogeneous results by sector for two sets of enterprise outcomes: the primary outcomes on incoming calls, and the mobile money outcomes. These findings are suggestive at best, as we are not well-powered to measure separate effects by sector. The estimated effect on incoming phone calls is positive for 7 of the 8 sectors, but only individually statistically significant for the Trading/Wholesale and Skilled Trades sectors, with the former effect being the larger and more significant. One interpretation of this result is that traders and wholesalers generally conduct business across space, so the directory is a more substantial treatment for this group, since the entire study area represents a pool of potential customers. Heterogeneous effects on mobile money outcomes are not as easy to discern. Hiring and labor – which includes tractors for rent – is the only sector with a positive and statistically significant individual treatment effect.

6 Farmer Experiment: Design and Findings

In this section we describe the farmer RCT. The intervention for this part of the experiment is the receipt of the trial directory in December 2014 or January 2015. For directory recipients and anyone with whom they share the directory, this represents a substantial increase in

potential suppliers, customers, information brokers or trading partners who can be contacted at low cost. The evaluation period begins in January 2015 and ends with the household survey in August of that year.

6.1 Farmer randomization

In Section 4 we explained that the distribution of the trial directories was randomized at the village level, stratifying on ward and and on whether the village was included in the census (which is related to village size). This randomization provides the treatment variation for the recipients. In total, 52 villages were assigned to Control (no directory distribution), and 47 villages were assigned to Treatment (distribution of 70 directories each). Figure 9 shows a schematic of the recipient RCT.

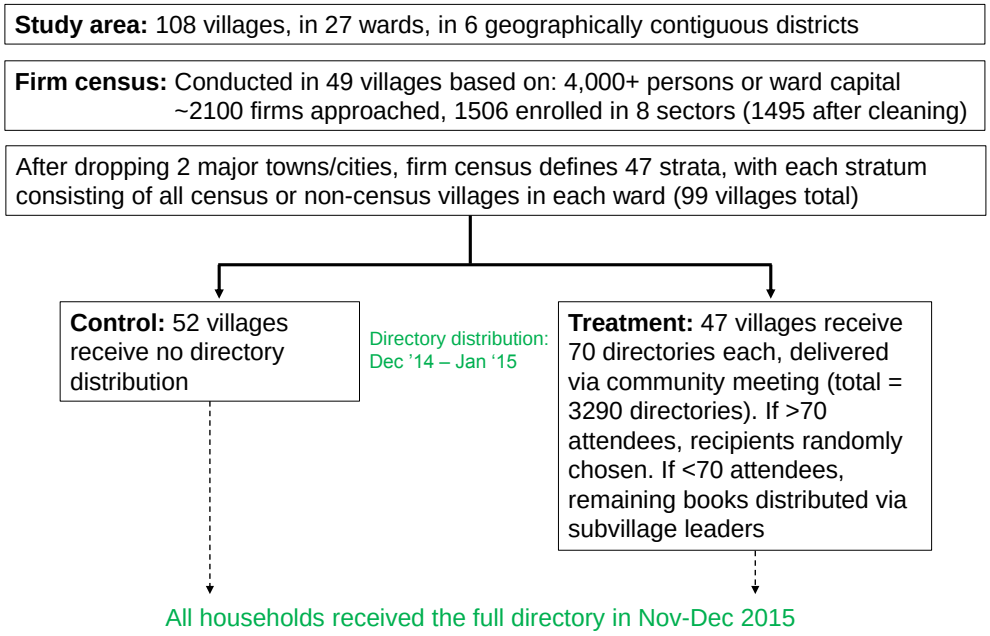


Figure 9: Household experiment design

In each treated village, individual directory recipients were chosen at random from those who attended a distribution meeting.[18] Participation was limited to individuals 18

[18]In a small number of villages there were fewer than 70 people at the meeting. In those cases, the remaining directories were distributed to non-attendees in each subvillage, with the assistance of meeting attendees.

years or older who were somehow engaged in agricultural production. The distribution meeting was announced in advance by village leaders. We asked village leaders to promote the distribution meeting in all subvillages and to encourage attendance by a wide range of persons (wealthy and poor, women and men, living near to and far from the village center). Nevertheless, we do not know the exact process by which villagers learned of the meeting. To generate a comparable sample of respondents in control villages, we held identical meetings in summer 2015, prior to the distribution of complete versions of the directory. The advertising and execution of the distribution meetings was the same for treatment and control villages. The only difference was that in the control villages, we did not distribute the directories on the day of the meeting. Instead, we returned to distribute directory booklets a couple of months later, after the household survey and the enterprise endline.

6.2 Survey and descriptive statistics for farmers

We conducted only one survey of participating farmers, in July-August 2015. Surveys in control villages took place in the days after the distribution meeting. Surveys in treatment villages were timed to coincide with the control surveys in the same stratum. We conducted surveys in 70 of the 99 villages, randomly selecting survey villages after stratifying on ward and village size. In each survey village, 12 respondents were randomly selected from the lists of directory recipients. After accounting for a missed survey day in one village and dropping a small number of observations with incomplete data, the final sample includes 831 farmers: 423 treatment, and 408 control.

The farmer survey covered a range of topics relevant to agriculture, communication, and trade. In addition to key covariates, we focused on collecting two categories of outcome measures: primary outcomes related to using a mobile phone for search and contacting businesses, and secondary outcomes related to input use, production, crop sales, and household enterprises. Table 3 provides sample means and p-values from balance test regressions for variables that are time invariant or that evolve slowly enough that effects are unlikely over the study period. The two groups are balanced on most variables. The difference that is

Table 3: Balance table for farmer characteristics

	Treatment	Control	N	p-value
	(1)	(2)	(3)	(4)
Age (years)	43.26	44.53	831	0.50
Male (=1)	0.85	0.74	831	0.02**
Years in village	32.32	30.58	831	0.09*
Household size (number of people)	6.02	6.17	831	0.83
Number of women age 15+	1.62	1.67	831	0.64
Number of men age 15+	1.67	1.62	827	0.53
Can read Swahili (=1)	0.92	0.90	831	0.44
Num. of other HH members who can read	3.10	3.20	830	0.47
Household connected to grid (=1)	0.10	0.08	831	0.54
Asset index	0.08	-0.08	798	0.56

Notes: Authors' calculations from baseline survey with firm sample. Columns 1 and 2 are sample means. Column 4 are the p-values on the treatment dummy variable in regressions of each variable on a binary variable for the treatment and dummy variables for the randomization strata.

both statistically and economically meaningful is in gender composition: treated farmers are 11 percentage points more likely to be male. If this is not spurious, it may reflect differences in the opportunity cost of men's or women's time between December-January and July-August. There is also a small but statistically significant difference in the number of years the recipient has spent in the village. We include these two variables as controls in all regressions (dropping them has no substantive affect on findings).

Over the 8-month study period, 27% of treated farmers report contacting one or more enterprises in the directory. We take this as suggestive evidence of usage, but do not treat it as a formal outcome, because this variable seems particularly susceptible to social desirability bias. The average number of calls to directory enterprises, among the callers, was 1.65. Almost three quarters of recipients report sharing the directory with members of their household, and 43% report sharing it with at least one person outside the household. In discussions with recipients we heard numerous accounts of someone lending the directory to a friend who needed it for a business or trading activity. The calling rates by the recipients are likely a lower bound on usage. We discuss this further below.

6.3 Empirical specification for impacts on directory recipients

Because directory recipients were only surveyed once, after treatment, we cannot use an ANCOVA specification to estimate impacts. Instead we use the following specification:

$$Outcome_{sj} = \alpha + \beta Treatment_v + X_{vj} + strata + \epsilon_{vj} \quad (2)$$

where j indexes recipients and v indexes the villages (treatment cluster). The matrix X includes the two time invariant recipient characteristics that exhibited imbalance, gender and number of years living in the village. We estimate equation (2) with standard errors clustered at the village (treatment cluster) level.

We estimate impacts on a range of relevant outcomes, in three categories. The first includes general outcomes related to communication or phone-mediated linkages outside the village. The second category is for agricultural choices and outcomes. The third is for outcomes related to non-farm enterprises.

6.4 Results: impacts on recipients

In Table 4 we show the estimated treatment coefficient from specification (2) for a wide range of outcomes. There are numerous economically and statistically significant results. The breadth and variety of significant effects related to communication suggests substantial pent-up demand for information and links beyond current networks.

In the top panel of Table 4 we see substantial increases in treated respondents' use of phones and links beyond the village. There is no extensive margin effect on making phone calls, because there is almost no baseline variation (96% of control respondents made a phone call during the last two weeks). Treatment increased the probability of sending SMS messages by 19%, increased the number of contacts in the phone by 35% (this result holds up if we limit the analysis to recipients who allowed the enumerator to count the contacts), and increased spending on phone credit by 14%, though that final result is not quite statistically significant at conventional levels (p-value = 0.107). There are no impacts on incoming calls

Table 4: Impacts of directory on farmers, OLS

Dependent variable	Coeff. (1)	s.e. (2)	p-value (3)	N (4)	Control mean (5)	% change (6)
Communication and general extra-village linkages (last two weeks, unless noted)						
Outgoing communication						
Made calls (=1)	-0.02	0.02	0.217	738	0.96	-2.2
Sent SMS (=1)	0.11***	0.03	0.002	738	0.60	18.7
Spending on phone credit (TSH)	559.93	342.46	0.107	786	4060.74	13.8
Number of contacts in phone, as of interview	47.95***	11.35	0.000	683	137.51	34.9
Incoming communication						
Received calls (=1)	-0.01	0.02	0.652	738	0.97	-0.7
Received SMS (=1)	0.05	0.04	0.216	738	0.76	6.9
Mobile money						
Sent mobile money (=1)	0.12***	0.03	0.001	738	0.32	36.3
Received mobile money (=1)	0.07*	0.04	0.051	738	0.36	19.8
Ordering deliveries over recent agricultural season						
Ordered goods from outside village (=1)	0.06*	0.03	0.052	831	0.26	22.4
Used phone to order goods (=1)	0.08**	0.03	0.022	831	0.18	42.7
Agricultural outcomes (most recent agricultural season)						
Crop failures						
Maize crop failure (=1)	-0.07**	0.03	0.038	743	0.27	-26.7
Sunflower crop failure (=1)	-0.03	0.03	0.276	684	0.12	-23.3
Input search						
Used phone for input acquisition (=1)	0.09***	0.03	0.004	776	0.18	52.6
Actively searched for inputs (=1)	0.03	0.02	0.213	776	0.84	3.7
Active search: phone calls (=1)	0.10***	0.03	0.001	776	0.13	74.5
Active search: travel outside village (=1)	-0.01	0.03	0.807	776	0.22	-3.5
Active search: discuss within village (=1)	0.04	0.03	0.120	776	0.79	5.7
Sourced inputs from outside village (=1)	0.02	0.05	0.645	776	0.45	5.2
Input usage						
Fertilizer (=1)	-0.01	0.02	0.615	776	0.04	-20.7
Borrowed or rented land (=1)	0.05**	0.02	0.022	776	0.16	33.4
Pesticides (=1)	0.01	0.01	0.402	776	0.01	74.2
Purchased seeds (=1)	-0.01	0.02	0.799	776	0.92	-0.6
Tractors or plow animals (=1)	0.03	0.03	0.304	776	0.91	3.2
Hired labor (=1)	0.12***	0.03	0.001	776	0.50	23.1
Total spending on inputs (TSH)	-31107	38782	0.425	776	436167	-7.1
Output price search						
Any output price search (=1)	0.04	0.04	0.350	776	0.74	5.2
Searched outside own village, if any search (=1)	0.13**	0.05	0.010	616	0.50	25.1
Used phone to price search, if any search (=1)	0.10**	0.04	0.017	616	0.17	60.9
Output price						
Log of crop sales price (TSH)	0.07	0.04	0.131	271	10.22	0.7
Sales coordination						
Used phone to coordinate with buyer (=1)	0.05**	0.02	0.010	677	0.05	99.0
Used phone to coordinate transport (=1)	0.01	0.01	0.139	677	0.00	415.4
Livestock						
Sold cattle or goats (=1)	-0.04	0.03	0.175	831	0.20	-22.0
Searched for prices, cond. on selling (=1)	0.09	0.08	0.270	178	0.43	20.5
Searched sales price by phone, if any search (=1)	0.43***	0.10	0.000	79	0.22	197.4
Log of livestock sales price (TSH)	-0.09	0.07	0.211	87	10.68	-0.9
Bought cattle or goats (=1)	0.00	0.02	0.836	831	0.10	4.6
Searched for prices, cond. on buying (=1)	0.22	0.15	0.152	95	0.35	63.3
Searched purchase price by phone, if any search (=1)	–	–	–	37	0.00	
Log of livestock purchase price (TSH)	0.20	0.16	0.206	95	11.42	1.8
Non-farm enterprise outcomes (most recent agricultural season, unless noted)						
Has non-farm enterprise as of interview (=1)	0.16***	0.04	0.000	829	0.28	58.8
Conditional on having business:						
Purchased business inputs (=1)	0.00	0.04	0.998	297	0.87	0.0
Used phone to acquire inputs (=1)	0.09	0.06	0.117	297	0.63	14.9
Business made sales (=1)	-0.03	0.04	0.535	297	0.91	-2.9

Notes: Authors' estimates from survey data. All regressions include strata fixed effects. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

or SMS messages. Treated individuals were 36% more likely to send mobile money, and 20% more likely to receive it. Villagers in Tanzania sometimes order delivery of goods from nearby towns by passing a request through the transporter or a contact in town. Treated farmers are 22% more likely to order such a delivery, and 43% more likely to use their phone to coordinate the order.

The middle panel of Table 4 shows the estimated effects on farming outcomes. For most of these regressions we limit the analysis to farmers who planted maize, sunflower, or both.[19] We focus first on outcomes involving search or communication. Directory recipients are 53% more likely to use their phone in some aspect of input acquisition, and 75% more likely to use a phone to actively source inputs. There is no extensive margin impact on actively searching for inputs, possibly because the baseline value is high (84%), or because searching by phone substitutes for other forms of search. In a similar vein, treated farmers are not significantly more likely to search for crop prices. However, treated farmers are 25% more likely to search crop prices outside of their villages,[20] and 61% more likely to use their phone to search for prices.

Treatment had a positive but imprecise effect on the prices received for maize or sunflower. The point estimate indicates 7% higher prices, with a p-value of 0.13. We pooled crops and estimated this regression in logs because there were so few sales: only 271, out of 1,427 maize or sunflower plantings. The study area experienced a short but severe drought during a critical period of crop growth in 2015. Output volumes were low, and few farmers had any marketable surplus.[21] Given the substantial impacts on searching prices by phone and searching outside the village, it is plausible that in a year with more sales we would see a statistically significant impact on prices.

Among respondents who talked to potential buyers (regardless of whether they made

[19]This is not very restrictive, as these are the dominant crops in the area. We do this because we collected more details for these crops than for all others.

[20]The dependent variable for that regression is not from a binary survey question; rather, it is constructed from a more detailed set of questions in which respondents identify the specific locations where they checked maize or sunflower prices.

[21]Many recipients who did not use the directory reported that they would have, if they had any crops to sell.

a sale), treated farmers were 99% more likely to coordinate with a potential crop buyer by phone. Almost no farmers, treatment or control, report using a phone to discuss possible sales transactions with a transporter. Similarly, treated farmers who sold cattle or goats were almost 200% more likely to use their phone to search sales prices, but not a single farmer who bought cattle or goats reported using the phone to search purchase prices. We would not have predicted this pattern *ex ante*. However, it is reassuring that there are communication-related outcomes that exhibit no effects, as this is a strong indication that those outcomes with positive effects are less likely to reflect social desirability bias.

Finally, in the bottom panel of Table 4 we see that treatment had a substantial impact on the probability of running a non-farm enterprise (NFE). Treated households are 16 percentage points more likely to run an NFE, a 59% increase over the control mean. This echoes a consistent piece of anecdotal evidence from the field: small business owners and potential entrepreneurs were extremely enthusiastic about the directory. Even though we designed the directory to serve farmers, there is substantial overlap in the network contacts relevant for farmers and for other small businesses. Non-farm enterprise owners face relatively high travel costs, and recognize in the directory a low-cost avenue for expanding their business networks.[22]

7 Estimating willingness-to-pay

The impact estimates for enterprises and farmers indicate substantial pent-up demand for communication, information, and expansion of business networks. During fieldwork we encountered broad enthusiasm for the directory: unlisted enterprises clamored to be listed, and leaders of un-treated areas asked us to deliver directories to their villages. In such an atmosphere it is reasonable to ask, why hadn't this been done already?

[22]Control farmers were recruited about half a year after treated farmers, so one might be concerned that this result reflects differential selection. However, if anything, this should work against us finding anything. Business owners were clamoring to be listed in the directory, and to receive a directory themselves. If this excitement affected enrollment in the study, it would have made NFE owners more likely to attend the control village distribution meetings than the treatment village meetings.

There are many reasons that the market may be slow to provide a profitable good or service, especially in rural areas of low-income countries. Yet, at the heart of this question is a more fundamental one about the nature of the directory: how similar are the socially optimal and privately optimal levels of directory provision? If there is sufficient willingness-to-pay among potential beneficiaries on both sides of the directory that creating it would lead to net gains in social welfare, but the private sector is not delivering it, then the directory may exhibit at least some of the characteristics of a public good. This is especially plausible on the recipient side, where positive spillovers are likely through directory sharing. In fact, because the booklets are easy to share, we expect farmer demand for a copy of the directory to be decreasing in the number of directories available in the community. Conversely, because enterprise owners are competing with each other for customers, we would expect enterprise owners to be more interested in listing their businesses if their rivals are listed in the directory.

To address this set of questions, we measured willingness-to-pay (WTP) on both sides of the directory. The findings provide insight into both the public good nature of the directory, and the relationship between prior exposure and demand.

7.1 Farmer WTP to receive a directory

In July-August 2016 we returned to the study area to experimentally measure the distribution of farmer willingness-to-pay for the directory among a new group of farmers. For this exercise we selected 12 villages in the original study area, stratifying on district. We selected 6 villages that we had not previously visited (new villages), 3 where we had conducted the census and distributed directories (large, return villages), and 3 where we had not conducted the census but had distributed directories as part of the farmer RCT (small, return villages). In each village we held 2 meetings with 30 respondents each, except in one village where scheduling problems limited us to a single meeting. Team members and village leaders worked together to recruit a broad cross-section of village members, including women, older people, poor households, and households from every subvillage. We restricted participation to those who had not previously received a copy of the directory. The total sample for this experiment

consists of 330 respondents in return villages, and 360 in new villages.

During the meetings we administered a variant of the Becker, DeGroot, Marschak mechanism (Becker, DeGroot and Marschak, 1964). After an introduction to the directory and a practice round, we revealed 10 possible directory prices, ranging from free to a price above the maximum WTP observed during piloting. We then asked each participant to write down on a slip of paper the maximum price that they would be willing to pay for the directory – their bid – on that very day.[23] Respondents were told that they would be given a few hours to gather the cash, if necessary. After responses were collected, one person from the group was invited to blindly draw one of the ten prices. Respondents who had bid an amount greater than or equal to the drawn price were allowed to buy the directory at the drawn price. The distribution of bids represents an incentivize-compatible estimate of the distribution of maximum WTP for the directory.

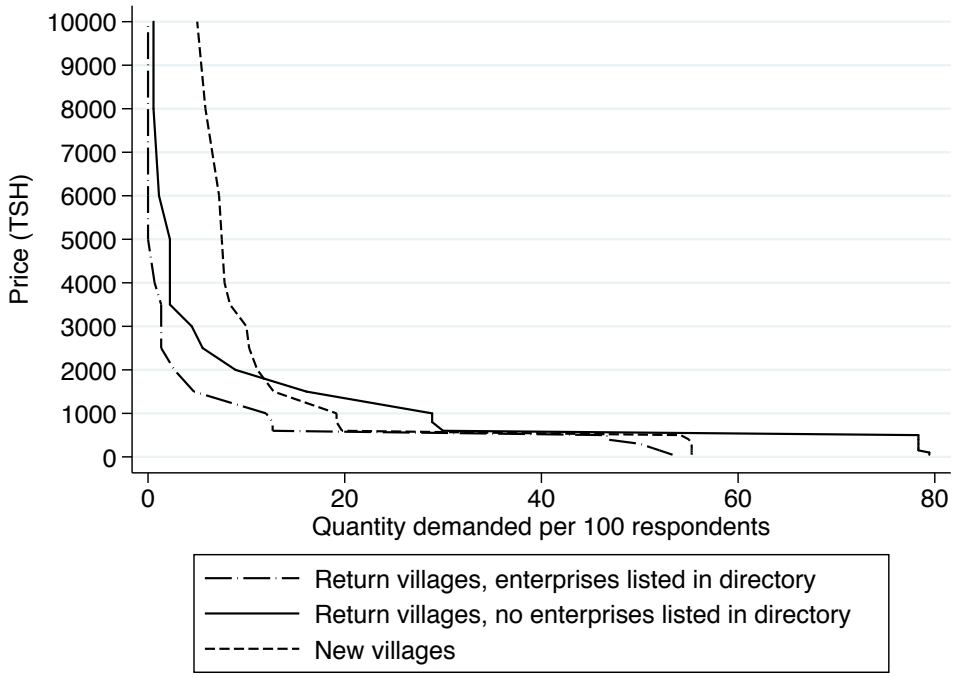


Figure 10: Farmer demand for a copy of the directory

[23]For some participants, seeing the list of possible prices in advance may have acted as a form of information or anchoring treatment. After piloting we decided that the risk associated with this possibility was merited by the strong preference of participants to interact with a fixed set of possible prices.

Overall, mean farmer WTP is 835 Tanzania shillings (about 0.40 USD). Nearly two thirds of respondents (62%) bid a positive amount. This indicates a higher rate of positive interest than might be inferred from the 27% of recipients in the farmer RCT who reported using the directory in the first 7-8 months after receiving it. The difference could be due to the depressive effect of the poor 2015 harvest on usage in the RCT. Many farmers in the RCT told us that they had planned to use the directory but ultimately did not because they had no crops to sell. It is also possible that farmers in the WTP experiment bid above their individual valuation, in anticipation of sharing the directory with non-participants who would value it more.

Figure 10 shows non-parametric demand curves based on the farmer bids, separately by village type. In new villages, a small group of respondents exhibits extremely high willingness-to-pay: 5% of the sample are willing to pay 10,000 TSH, or almost $5, for a copy. At all prices, quantity demanded per 100 respondents is lowest in the large return villages. These are places that have some enterprises listed in the directory, and that received the most copies of the full directory in Nov-Dec 2015 (listed enterprises and farmers received full directories). The apparent lower WTP in these villages could reflect the broader availability of the *Kichabi* booklet, or the presence of a larger local business network.

To further examine heterogeneity in farmer WTP, we estimate descriptive regressions with the bid as the dependent variable. These regressions make use of dummy variables for village type – i.e., treatment status in the RCTs – as well as covariates collected in a brief survey conducted before the WTP meetings.[24] Table 5 shows the results. All estimated coefficients have the expected signs. Coefficients on the dummy variables for village type reproduce the ordering in Figure 10, though they are not statistically significant. The only economically and statistically significant coefficient is on the asset index, a latent variable estimated from 6 other wealth-related variables (acres cultivated, livestock ownership, and dwelling characteristics). A standard deviation increase in this index is associated with a 20-23% increase in WTP, relative to the excluded group mean.

[24]Summary statistics for the survey data are provided in Appendix table A2.

Table 5: Heterogeneity in farmer WTP for a copy of the directory, OLS

Dependent variable: maximum WTP from elicitation experiment				
	(1)	(2)	(3)	(4)
Small return village (no census)	-251.3	-302.8	-340.6	-303.6
	(433.1)	(409.2)	(404.9)	(378.1)
Large return village (census)	-662.3	-716.5	-690.7	-766.3
	(433.6)	(492.5)	(492.3)	(487.6)
Previously seen directory booklet (=1)		-230.6	-291.8	-253.1
		(223.0)	(224.7)	(233.2)
Respondent is female (=1)		-393.3	-298.5	-288.2
		(409.8)	(411.2)	(381.9)
Age in years		-8.6*	-8.8*	-6.8**
		(4.9)	(4.7)	(3.2)
Asset index			243.9**	206.6*
			(102.9)	(119.2)
Mean of dep. var., excluded group	1042	1057	1057	1057
Observations	684	674	674	674
R-squared	0.02	0.03	0.05	0.08
Additional controls	No	No	No	Yes

Notes: Authors' estimates from incentivized willingness-to-pay and survey data. The excluded village group is for the new villages. "Additional controls" include dummy variables for primary occupation (farming, government, private sector, self-employed, other) and education of household head. Standard errors clustered at the village-meeting level. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

7.2 Enterprise WTP to be listed in the directory

For enterprise respondents we elicited a measure of willingness-to-pay to be listed in the directory. Enterprise WTP was measured during the endline survey, in September-October 2015, using a contingent valuation approach. It was not practical to elicit enterprise WTP using a real-stakes experimental design like that used for farmers, because doing so would have required that we follow through and re-print directories listing only those enterprises that successfully bid to be listed, which would have conflicted with various other study aims.

Toward the end of the endline survey, enumerators read a standardized script describing a hypothetical scenario in which the directory would be re-printed and distributed in new locations. The scenario involved the printing and distribution of 5,000 copies of the directory – 100 apiece in 50 towns. Enumerators made it clear that this was a hypothetical exercise. They then asked respondents whether they would be willing to pay the following (in TSH) for their enterprise to be listed in this printing: 0, 2000, 5000, 10000, 15000, 20000 (the exchange rate was roughly 2100 TSH per 1 USD). For this analysis we use both the panel enterprises studied in Section 5 and an additional 484 enterprises surveyed only at endline, for a total of 881 observations. Additional details about the elication and analysis of enterprise WTP are in Appendix Section C.

Figure 11 shows demand curves for three types of enterprises: Treatment, Control, and Pure Control.[25] Because respondents selected their maximum WTP from a list, rather than providing an open-ended response, these curves represent lower bounds on demand. The ordering of demand is consistent across the range of quantities shown: Pure Control enterprises are willing to pay the least, followed by Control enterprises, followed by Treated enterprises. Mean willingness-to-pays across all enterprises is 3621 TSH, or roughly 1.72 USD. Among all groups there is positive demand even at the very high price of 20,000 TSH per listing.

To further examine heterogeneity in enterprise WTP, we regress the lower bound on

[25]Recall that Pure Control enterprises are in villages where no enterprises were listed in the trial directory, whereas Control enterprises were not listed themselves, but are in villages where some enterprises were listed.

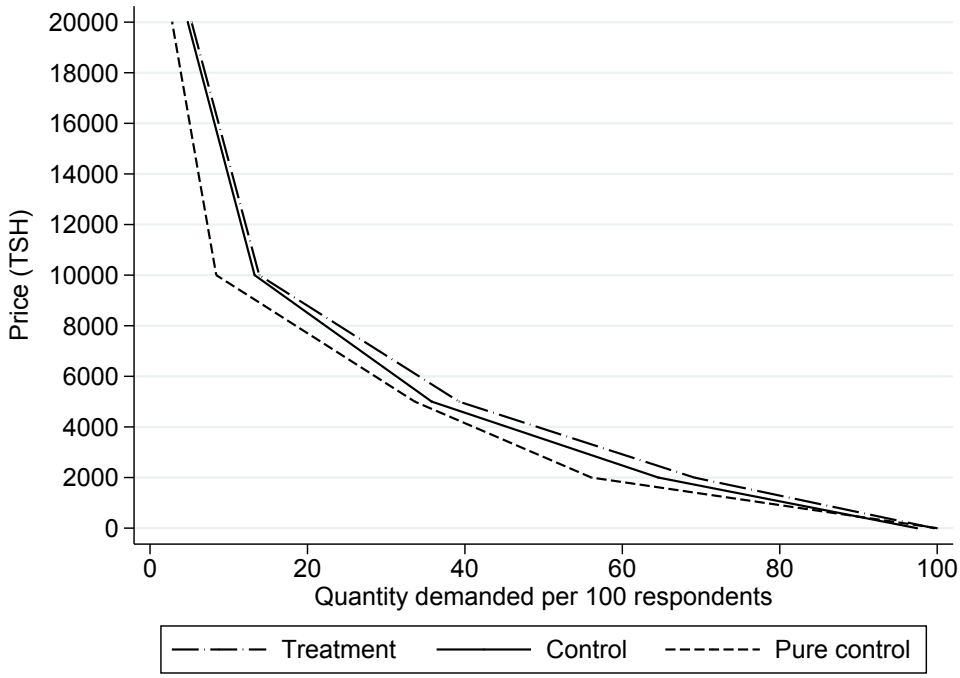


Figure 11: Enterprise demand to be listed

enterprise WTP implied by the survey results on treatment variables from the enterprise RCT. Table 6 shows the results. Standard errors are clustered at the village level, because assignment into "Pure Control" or "Treat/Control" was implemented at the village level, stratifying on district. In column 1 we see that treatment – a listing in the 2015 trial directory – increases enterprise WTP by 789 TSH, or 23%. Column 2 confirms the relationships implied by Figure 11: both Treatment and Control firms are willing to pay more than Pure Control firms, though only the former difference is statistically significant. Finally, in column 3 we pool the Treatment and Control groups against the Pure Control group, to estimate the impact of being in a village where any enterprises were listed. The result is similar to that in column 1: any exposure to the directory has an economically and statistically significant impact on enterprise WTP.

In Table 6 the statistically significant variation lies primarily in the comparison of Treated and Control firms to Pure Control firms, rather than in a comparison of Treat to Control (in column 2 we cannot reject that the Treat and Control coefficients are equal).

Table 6: Impact of treatment on enterprise WTP for a future listing, OLS

Dependent variable: willingness-to-pay to be listed, lower bound			
	(1)	(2)	(3)
Treat	789.4*	1023.7*	
	(448.2)	(522.1)	
Control		336.5	
		(539.5)	
Treat or Control			814.3*
			(482.3)
Mean of dependent var., excluded group	3380	2832	2832
Observations	881	881	881
R^2	0.08	0.08	0.07
Fixed effects	Dist-sector	Dist-sector	Dist-sector

Notes: Authors' calculations from original survey data. Treated enterprises were listed in the trial directory; Control enterprises were in villages where some enterprises were listed, but were not themselves listed in the trial directory; Pure Control enterprises are in villages where no enterprises were listed in trial directory. Standard errors in parentheses, clustered at village level. ***: significant at 0.01, **: significant at 0.05; *: significant at 0.1.

The natural interpretation is that Treated firms perceive a positive benefit to the listing, and that Control firms learn about this benefit from the Treated firms in their villages. The smaller magnitude of the Control effect suggests that learning is incomplete. A related interpretation is that respondents who observe the directory's impact perceive a competitive disadvantage to not being listed. The differences between treatment groups are not driven by differences in the degree of trust that the survey team would follow through on the hypothetical commitment to the printing, because all respondents received their copy of the final directory, with all firms listed, just before the endline interview.

Appendix

A Enterprise census and directory: additional details

In each census village, team members followed a standardized protocol to locate enterprises. Enumerators first divided up the primary markets and commercial areas into sections, and systematically walked each block or market row looking for storefront enterprises or individuals selling a good or service in public. Once this was completed, the enumerators repeated the procedure in the neighborhoods immediately surrounding the primary commercial area of the village. If there were multiple markets or commercial areas, as is common with larger villages, the team repeated the census procedure in as many locations as needed. Once this was complete, team members asked the assistance of local leaders in identifying businesses that might be harder to locate, such as tractors for rent that are parked in someone's compound, or milling machines in unmarked buildings.

At each business, an enumerator approached the workers and asked to speak to a manager or owner. If no one present was able to speak for the business, the team returned later or attempted to contact the owner by phone. After describing the project and acquiring consent, the enumerator collected basic details about the business: name, sector, number of employees, primary location (village and subvillage), and phone number(s). Many enterprises did not have a formal name before enrolling in the directory. For example, an individual running a grain mill used by households in the neighborhood would have had little prior reason to name his milling enterprise. In such cases, enumerators assisted respondents in choosing a name. We did not collect physical addresses, because they do not exist in the large majority of towns and villages. Enumerators also gave respondents the option of specifying a number of keywords from drop-down menus that provided additional details about their businesses, such as an indication of specialization in trading certain kinds of crops. Enumerators emphasized that this was a trial study run by researchers, and that we would distribute more than one version of the directory, sometimes not listing all firms. They

assured enterprise owners that after the trial period ended, all enterprises would be listed in a final directory printing, and we would re-issue directories both to the original recipients and to the enterprises themselves.

We invited firms in 8 agriculture-related sectors to enroll in the directory: wholesale trade, retail trade, transport, hiring/renting, agricultural processing, skilled tradespeople, non-agricultural services, and financial services. One key design challenge involved setting boundaries on the types of enterprises to include from these sectors. From the outset the study was focused on farmers and on generating a directory relevant to the productive activities of farming households. However, because farming takes a wide array of inputs, including the time and physical labor of household members, one could describe almost any enterprise as somehow "related to agriculture." After extensive piloting and discussion the team settled on boundaries for each sector by discussing cases that had emerged in the pilot.[26]

Figure 12 shows the cover of the directory at left, and an example page at right.

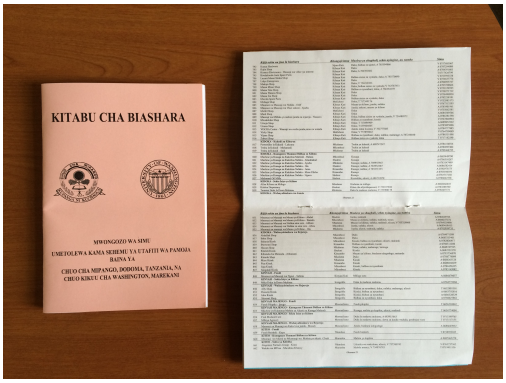


Figure 12: The KICHABI directory

In addition to the enterprise content, the first two pages of the directory were used to provide additional relevant details. One page showed the map from Figure 4. The next page listed some basic information about the project, and provided an explanation of how

[26]For example, we included veterinarians and pharmacists in the "non-agricultural services" sector, but not clothing or shoe vendors, even though the latter sell goods that complement human capital inputs to production. There are dozens of clothing vendors at many markets, so including them would have diluted the directory and added substantial time to each field visit.

to purchase calling bundles for each of the major networks, which are cheaper than paying separately for each call. We provided these details to help minimize the costs of making cross-network calls for phone users who may have had little reason to do so previously.

B Impacts on enterprises: heterogeneity analysis

Table A1 shows estimated coefficients from ANCOVA regressions based on specification (1) in the main text, with the addition of interaction terms between enterprise sector and the treatment variable.

Table A1: Heterogeneous impacts by enterprise sector, marginal effects, ANCOVA

	Full sample			Phone history checked			Full sample		
Dep. variable:	Incoming calls	New customers	Missed calls	Incoming calls	New customers	Missed calls	Use mobile money	Receive mobile money	Send mobile money
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Trading/Wholesale	3.94***	0.11	0.18	4.03***	0.20	0.54**	0.08	0.05	0.04
	(0.00)	(0.80)	(0.37)	(0.01)	(0.70)	(0.02)	(0.28)	(0.55)	(0.68)
Merchant/Retail	0.76	0.26	0.21	0.72	0.21	0.08	0.03	0.06	0.03
	(0.31)	(0.40)	(0.18)	(0.39)	(0.53)	(0.64)	(0.62)	(0.31)	(0.63)
Transport	0.63	-0.08	0.06	1.02	0.08	0.13	0.03	0.08	0.04
	(0.73)	(0.90)	(0.86)	(0.64)	(0.91)	(0.71)	(0.79)	(0.51)	(0.69)
Hiring and Labor	-0.77	0.46	-0.01	-0.81	0.47	-0.31	0.39***	0.49***	0.30
	(0.64)	(0.34)	(0.95)	(0.71)	(0.35)	(0.41)	(0.01)	(0.01)	(0.12)
Agri Processing	-0.04	0.13	-0.71*	1.93	0.66	0.06	0.16	0.08	0.18
	(0.98)	(0.75)	(0.05)	(0.37)	(0.26)	(0.79)	(0.26)	(0.60)	(0.21)
Skilled trades	0.41	-0.15	0.33	2.28*	0.22	0.21	-0.10	-0.14	-0.08
	(0.69)	(0.72)	(0.11)	(0.06)	(0.63)	(0.39)	(0.21)	(0.10)	(0.41)
Non-Agri Services	0.79	-0.66	0.00	1.69	-0.56	0.30	-0.01	0.03	0.04
	(0.63)	(0.38)	(0.99)	(0.27)	(0.60)	(0.47)	(0.93)	(0.81)	(0.71)
Financial Services	5.82	0.77	-0.18	6.90	2.23	0.51	-0.26	-0.19	-0.07
	(0.25)	(0.63)	(0.83)	(0.37)	(0.31)	(0.59)	(0.20)	(0.47)	(0.82)
Observations	1576	1444	1578	1224	1130	1226	648	648	648
R-squared	0.23	0.15	0.09	0.22	0.17	0.13	0.21	0.24	0.18

Notes: Authors' estimates from survey data. All regressions other than those for "new customers" and "missed calls" include controls for the baseline value of the dependent variable, as well as survey round and village fixed effects. Standard errors clustered at the treatment cluster level. Coefficients are marginal effects. P-values from F-tests in parentheses. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

C Eliciting willingness-to-pay: additional details

C.1 Measurement and analysis of farmer WTP

To the extent that recipients perceive private returns from access to the directory, they will be willing to pay for a copy. A number of factors affect farmer WTP, including: location; wealth and/or cost of capital; strength of existing network connections; link between household economic activities and the demand for new trading partners; trust in the validity of the numbers in the directory; access to a copy of the directory through friends or neighbors; and various others.

The directory is a quasi-public good. The information contained within it is non-rivalrous, although the physical directory itself is a rival good in any fixed moment of time. One likely implication of this is that willingness-to-pay by directory recipients will be decreasing in the number of directories available in the village. Communal sharing norms in Tanzania are such that most villagers would have no problem borrowing a neighbors directory for a few moments if they need to look up a number, particularly if there are many neighbors who have a copy. This suggests that WTP will be lower in previously treated villages, even among those who do not have a copy, than in villages where no one has a copy.

We elicited household willingness-to-pay with an incentivized version of a Becker, DeGroot, Marschak experiment, administered through community meetings in 12 villages in the study area. These meetings took place in July-August 2016, more than 6 months after completing the distribution of the complete directories. The sample of villages consisted of 6 that we had not previously visited (new villages), and 6 where we had either conducted census activities, distributed the directory, or both (old villages). In each village we held 2 meetings with 30 respondents each (except in 1 village, where a late start forced us to hold only 1 meeting). We only invited individuals to participate if they had not previously received a copy of the directory. Prior to the meeting, enumerators conducted a brief survey with participants to record key covariates. During the meeting, participants were first given a presentation on the directory and allowed to inspect a copy of it. Then the research team

ran a practice, no-stakes version of the WTP experiment using a different set of prices from that used during the actual game, and answered questions. The team then administered the game, which consisted of showing participants laminated cards with 10 possible prices and asking each person to write down on a slip of paper the maximum price that they would be willing to pay (the bid), that very day, for a copy of the directory. Respondents were told that they would be able to go home for cash if necessary. After responses were collected, one person from the group was invited to blindly draw a price card from a bucket, with the understanding that the price drawn would be the effective price that day. The team then completed directory sales with respondents who had bid a price equal to or greater than the drawn price.

If participants fully understand the game, this approach generates an incentivized estimate of the distribution of maximum WTP for a copy of the directory. The distribution is biased downward (left-shifted) by the coarseness of the price set used, but we found during piloting that this loss of information was more than justified by the improved understanding of participants from interacting with a fixed, known set of possible prices.

Table A2 shows summary statistics from the surveys conducted prior to the farmer WTP elicitation meetings.

Table A2: Farmer WTP sample: summary statistics, N=680

	Mean	s.d.
	(1)	(2)
Maximum WTP (TSH)	835	1836
Respondent is female (=1)	0.31	0.46
Age in years	43.41	16.38
Primary occupation:		
Farmer (=1)	0.93	0.26
Public sector employee (=1)	0.01	0.11
Private sector employee (=1)	0.01	0.08
Self-employed outside agriculture (=1)	0.03	0.17
Other (=1)	0.02	0.15
Walk time to village office (minutes)	22.57	28.09
Walk time to bus (minutes)	42.55	55.17
Number of trips outside village, last month	1.48	2.36
Number of phone calls outside village, last month	3.72	10.61
Number of people in household	5.59	2.59
Previously seen directory booklet (=1)	0.16	0.36
Household head is male (=1)	0.88	0.32
Education of household head:		
None	0.17	0.38
Some primary (=1)	0.11	0.31
Completed primary (=1)	0.60	0.49
Some secondary (=1)	0.03	0.16
Completed secondary or higher (=1)	0.05	0.21
Other (=1)	0.05	0.21

Notes: Authors' calculations from survey data.

C.2 Measurement and analysis of enterprise WTP

The full script of the WTP elicitation question for enterprise respondents is as follows:

ENUMERATOR: Read the following statement to the respondent.

"After completing our current activities we do not plan to distribute more telephone directories for this study. However, it is possible that in the future a different group of researchers, an NGO, or the government may wish to print telephone directories like Kichabi, as a service for business people and households. Because research funds might not be available to pay for the printing, we are interested in learning whether businesses would be willing to pay to be listed. This is just a hypothetical question at present we have no plans to print or distribute directories after this year.

Suppose that in September 2016 you were offered the chance to pay to list your business in a directory. 100 copies of the directory would be distributed in 50 towns and villages in this area that did not receive any of the Kichabi directories from this study, including some large towns such as Dodoma and Babati.

I am going to read some prices to you. Please say yes if you would be willing to pay the price that I state so that your firm would be listed in this future directory. Say no if you would not be willing to pay the price."

ENUMERATOR: Read the below prices in order, beginning with 0, and note the response.

a.	0	☐	0=No 1=Yes
b.	2,000	☐	0=No 1=Yes
c.	5,000	☐	0=No 1=Yes
d.	10,000	☐	0=No 1=Yes
e.	15,000	☐	0=No 1=Yes
f.	20,000	☐	0=No 1=Yes

For any respondent that answered "No" to paying 20000 TZS, we can use the sequence of responses to interval bound the respondent's WTP. For those answering "Yes" to all prices, we either impose an assumption about the upper bound on WTP, or leave the upper bound unspecified. In practical terms this makes little difference to the results.

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