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Centre for Economic Policy Research
33 Great Sutton Street, London EC1V 0DX, UK
Tel: +44 (0)20 7183 8801
www.cepr.org

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Abstract

Mobile phones reduce the cost of communicating with existing social contacts, but do not eliminate frictions in forming new relationships. We report the findings of a two-sided randomized control trial in central Tanzania, centered on the production and distribution of a "Yellow Pages" phone directory with contact information for local enterprises. Enterprises randomly assigned to be listed in the directory receive more business calls, make greater use of mobile money, and are more likely to employ workers. There is evidence of positive spillovers, as both listed and unlisted enterprises in treatment villages experience significant increases in sales relative to a pure control group. Households randomly assigned to receive copies of the directory make greater use their phones for farming, are more likely to rent land and hire labor, have lower rates of crop failure, and sell crops for weakly higher prices. Willingness-to-pay to be listed in future directories is significantly higher for treated enterprises.

JEL Classification: O13, D83, Q13, M37

Keywords: mobile phones, Search costs, telephone directories, Small and medium enterprises, agriculture, Tanzania

Brian Dillon - bmd28@cornell.edu
Cornell University

Jenny Aker - jenny.aker@tufts.edu
Tufts University

Joshua Blumenstock - jblumenstock@gmail.com
University of California, Berkeley and CEPR

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How Important is the Yellow Pages? Experimental Evidence from Tanzania*

Brian Dillon† Jenny C. Aker‡ Joshua E. Blumenstock§
Cornell University Tufts University U.C. Berkeley

February 18, 2020

Abstract

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†Cornell University, Dyson School of Applied Economics and Management, bmd28@cornell.edu.
‡Tufts University. jennaker@hotmail.com
§University of California at Berkeley, School of Information, jblumenstock@berkeley.edu.

1 Introduction

The rapid diffusion of mobile phones represents one of the fastest and most comprehensive technological transformations in human history (Comin and Mestieri, 2014). In the last decade, economists have begun to document the implications of this transition. Early studies show that mobile phones reduce search costs and facilitate arbitrage in agricultural markets (Jensen, 2007; Aker, 2010); more recent analysis evaluates the effectiveness of phone-based information services that push curated information to rural households (Aker, Ghosh and Burrell, 2016). The existing literature does not emphasize the difference between the *cost of communicating* with established contacts and the *cost of searching* for new contacts. This is an important distinction.

For two people to communicate on a phone network, two criteria must be satisfied: both parties must have access to a phone, and one must have the phone number of the other. Under those conditions, communication is possible at a fraction of the pre-phone cost. Access to a mobile phone does not reduce the cost of searching for the contact information of unknown parties. When landline telephones first proliferated, they were accompanied by complementary information services to facilitate search (Brooks, 1976). In the early 20th century this service was provided by switchboard operators. Operators were eventually replaced by printed directories, information lines, and the web. At no point have large populations used landline telephones without the benefit of a complementary service that supplies contact information. Yet, that is how much of the world uses mobile phones.

This study asks two related questions. First, do households benefit from having access to a "Yellow Pages" directory that contains contact information for nearby enterprises? Second, do enterprises benefit from being listed in such a directory? If information frictions prevent households from discovering and contacting enterprises, a directory should facilitate more efficient search, and improve outcomes for both households and enterprises.

To address these questions, we conducted a two-sided randomized controlled trial (RCT) in a rural area of central Tanzania. We began in 2014 by conducting a census of all enterprises in eight sectors relevant to farming households. The census covered 1,494 enterprises operating in 49 villages and cities in a roughly 5,000 square mile area. We randomly selected roughly half of these enterprises to have their contact information included in a printed phone book, which was organized by location and sector. At the beginning of the next planting season, in December 2014 and January 2015, we distributed 3,290 printed phone directories via community meetings in randomly selected villages. Because of the high likelihood of spillovers through directory sharing, we randomized distribution at the village level. The experiment ran for nearly one year, covering a full cycle of agricultural activities.[1]

We find that receiving a directory had an economically and statistically significant impact on 17 of 45 outcomes examined for farming households. Compared to control households, recipients were 36% more likely to send mobile money and 20% more likely to receive it, 22% more likely to order goods for delivery from outside their village, 75% more likely to use their phone to source inputs, and 25% more likely to search for output prices outside their village. They were 23% more likely to hire labor, 33% more likely to borrow or rent farmland, and 27% less likely to experience a maize crop failure. Treated households were 59% more likely to run a non-farm enterprise from the household, suggesting that the directory influenced economic activity beyond the intended effects on farming. All results remain significant after corrections for multiple testing within outcome categories.

We find a meaningful but imprecise effect on the prices received for selling crops. Pooling sales of maize and sunflower, the two main cash crops, we find that treated households received 6.8% higher prices (p-value = 0.13). This result may be under-powered, as the study year had little rainfall and only a small share of households sold any crops. Our analysis uncovered no statistically significant impacts on crop inputs beyond land and labor (e.g. fertilizer, pesticides, traction animals), no impacts on most dimensions of livestock activity (sales, purchases, prices), and no impact on the extensive margin propensity to make phone calls. Many of these null effects are on outcomes that exhibit little overall variation.

The benefits of the directory do not appear to be limited to certain types of households. Ex ante, we hypothesized that impacts would be larger in magnitude for women and

[1] Out of concern for the control enterprises, we broke the experiment after one year and re-distributed a large number of directories with all enterprises listed. This was planned from the outset.

for poorer recipients, because in this setting men and wealthy individuals are better able to travel and gather phone numbers. We find no support for these hypotheses. Splitting households into above/below median wealth groups, we do find that some impacts are only statistically different from zero for one group. Yet, for all outcomes we cannot reject the null hypothesis that the treatment effects are the same. We find a similar pattern when we split on gender. With 90% confidence, we can reject the null of equal average treatment effects for men and women for only 3 out of 45 outcomes.

We also find economically and statistically significant impacts on 8 of 19 outcomes examined for enterprises. Over four post-treatment surveys, listed enterprises received on average 2.46 more business-related phone calls over the two most recent operating days, a 27% increase over the control mean of 9.11. Treated enterprises were 12% more likely to use mobile money and 20% more likely to hire workers. For other enterprise outcomes—outgoing communication, sales, and revenue—we find uniformly positive point estimates, but no statistically significant differences between treatment and control enterprises. The single-season treatment may have been too short to detect impacts in these domains. However, further analysis suggests that the estimated treatment effect is attenuated by positive spillovers to the control group (the unlisted enterprises in villages where some other enterprises were listed). Compared to a pure control group of firms from villages where no enterprises were listed, both treatment and control enterprises enjoyed significant increases in the total number and total value of sales over the previous two operating days. The directory increased the overall level of business activity in treated villages—likely mediated by initial phone calls to treated enterprises—which improved outcomes even for the unlisted group.

After the RCT, we used an incentive compatible auction based on Becker, DeGroot and Marschak (1964) to elicit local residents' willingness-to-pay (WTP) for a copy of the directory. Although we excluded previous directory recipients from the WTP experiment, those living in places where we had distributed booklets in the RCT were willing to pay substantially less. This is consistent with the widespread sharing of booklets documented in our surveys, and the partially non-rival nature of the directory contents. We also measured the WTP of study enterprises to be listed in a hypothetical future directory printing, using a stated preference approach. Owners of treated enterprises were willing to pay significantly more than owners of control enteprises, who were themselves willing to pay weakly more than owners of pure control enterprises. The experience of being listed clearly led to upward revisions of enterprise owners' prior valuation of the directory. Back-of-the-envelope calculations suggest that printing directories and charging enterprises for a listing would be a profitable exercise in central Tanzania.

Our findings advance multiple literatures. In classic search models (Stigler, 1961), buyers canvas known sellers, incurring some marginal cost for acquiring information. Phones lower this marginal cost, which in turn can facilitate arbitrage in agricultural markets, reducing price spreads and increasing total surplus (Jensen, 2007; Aker, 2010). Phone networks, and the mobile money ecosystem they enable, have also reduced the cost of transferring money, which in turn has increased resilience to shocks and reduced poverty (Jack and Suri, 2014; Suri and Jack, 2016; Blumenstock, Eagle and Fafchamps, 2016). But if the fixed cost of acquiring the phone number of an unknown contact is sufficiently large, marginal costs may be irrelevant. In this sense, contact information is qualitatively different from other types of information.

A large body of work examines the way that contacts, and the networks that they collectively form, influence agricultural outcomes in low-income countries. The literature has generally found that networks facilitate learning about the availability and best use of new agricultural technologies, although the effects are mixed in some settings (Foster and Rosenzweig, 1995; Munshi, 2004; Bandiera and Rasul, 2006; Conley and Udry, 2010; Maertens, 2017; Cai, De Janvry and Sadoulet, 2015; Kondylis, Mueller and Zhu, 2014; Magruder, 2018). Of course, contact information is only valuable if recipients make use of it. Relational contracts are important when contract enforcement is incomplete (Banerjee and Duflo, 2000; Baker, Gibbons and Murphy, 2002; Brown, Falk and Fehr, 2004). Repeated interaction is common in the rural markets of sub-Saharan Africa, where trading traditionally involves cash-and-carry exchange with known agents (Fafchamps, 2004). At the outset of our study it was an open question whether recipients would trust the directory and venture beyond their known and trusted partners. Clearly, many did.

Finally, this study advances the literature on small firms in developing countries. The poor average performance and slow growth of small and medium enterprises (SMEs) is a persistent puzzle. Prior work emphasizes the importance of entry costs (Ayyagari, Beck and Demirguc-Kunt, 2007) and access to finance (Beck and Demirguc-Kunt, 2006; De Mel, McKenzie and Woodruff, 2008, 2012) as factors influencing the firm size distribution. Other evidence suggests that the average SME stands to gain substantially from changing how it operates, either by switching technologies (Atkin et al., 2017), improving management practices (Bloom et al., 2013), or even keeping more change on hand (Beaman, Magruder and Robinson, 2014). The findings of this paper suggest that the lack of marketing opportunities is another important barrier. The directory listing acts like a form of advertising, in a setting where marketing is almost exclusively word-of-mouth. In this respect our findings complement those of Jensen and Miller (2018). In India, Jensen and Miller find that the market shares of high quality boat-builders increase at the expense of low quality builders, after reductions in market segmentation driven by improved information access. If the directory kickstarts a similar process among agricultural service providers in Tanzania, we may see long-run changes in market structure, as consumers sort to higher quality enterprises.

The rest of the article proceeds as follows. Section 2 provides background and context for the study, details on the experimental design, and a description of the data. Section 3 provides the empriical framework. In Section 4 we present results, first for the listed enterprises and then for the recipient households. Section 5 concludes.

2 Experimental Design

2.1 Background

Landline telephone systems spread through currently wealthy countries over a century ago. Human switchboard operators were central to the early operation of landlines, and provided services beyond manually connecting two lines. Operators could steer traffic to particular parties, or assist callers in finding a relevant business based on partial information (Barrett, 1935; Brooks, 1976). Printed telephone directories first appeared in the US in the late 1880s, and operated alongside operators, although coverage varied across space (Shea, 2010). In the first half of the 20th century, human operators were gradually replaced by automated exchanges. Printed directories rose to prominence as the primary mechanism for searching the network. In the last two decades, Internet-based directory services have largely replaced printed directories in wealthy countries.

Mobile phones have proliferated more rapidly and comprehensively than did landlines. Figure 1 shows the time path of mobile phone adoption from 2006–2015. Over that period, the number of mobile phone subscriptions per 100 people in sub-Saharan Africa rose from 18 to 76. In Tanzania, the number of mobile phone subscriptions per 100 people rose from 0.3 to 77.2 over the period 2000-2018 (World Bank, 2020). The large majority of mobile phones in rural areas are simple phones, not smartphones, although that is slowly changing (GSMA, 2019). Landlines are available in major cities of Tanzania, but are used only by government, wealthy households, and established formal sector businesses.

There is a printed directory available for landlines in Tanzania. It is not clear how comprehensive it is, or how frequently it is updated. There is no directory of mobile phone numbers. Mobile phone users change their numbers more often than landline users, and both government employees and private sector workers tend to use their personal mobile lines for work. It would be difficult to to generate a directory that associates phone numbers with specific entities using only administrative data from phone companies. One implication of the lack of directories is that most individuals' phone-based networks are functions of their face-to-face networks. People acquire contact information by first interacting in person, or by following a thread through their face-to-face network.[2]

A diverse Information and Communications Technology (ICT) service sector has ma-

[2]If you ask a resident of a rural village in Tanzania to describe the process by which they would locate the phone number of a business in a nearby town, as we have done many times, the response will invariably involve personal travel, communication with prior contacts, or both.

terialized alongside the mobile network. In the agriculture sector alone there were over 140 ICT-based services operating in low-income countries as of 2015 (Aker, Ghosh and Burrell, 2016), and more are added every year (GSMA, 2019). These services take a variety of forms, including one-way provision of price or weather information via text message, interactive voice response (IVR) systems for agricultural extension, smartphone apps to assist with farm management, and others. The directory service in our experiment is different. Rather than provide a specific type of farming-related information, the directory thickens the local network and allows recipients to pursue whatever information they like. Our intervention covers the fixed cost of acquiring contact information, lowering the marginal cost of future communication with a wide range of agents.

2.2 The *Kichabi* Telephone Directory Experiment

The *Kichabi* Telephone Directory Experiment was a two-sided large-scale RCT designed to measure the causal effects of a printed, enterprise-focused, mobile telephone directory on both the enterprises listed in the directory and the households that received a copy of it. *Kichabi* is short for *kitabu cha biashara*, or "business book" in Swahili. To develop and distribute the directory, we partnered with the Institute of Rural Development Planning (IRDP) in Dodoma, Tanzania. IRDP is well known in the area and lent credibility to our interactions with residents and officials.

Our study took place in a geographically contiguous area in the Dodoma and Manyara regions of Tanzania, covering approximately 5,000 square miles in six districts. The area is predominantly agricultural, with one cultivation season from roughly January to May. The major crops are maize and sunflower. A range of enterprises provide services to farmers, including formal enterprises like input supply shops, pharmacies, and large-scale traders, and small informal enterprises such as households with milling machines, roadside mechanics, bicycle transporters, and local retailers that buy and sell food crops. There is a well-established system of ordering goods for delivery from outside the village, using local buses for transport. Farmers can sell crops to traders who visit the village, at weekly markets, or in nearby towns and cities. Physical inputs such as seeds and fertilizer are typically acquired at weekly markets or at agricultural supply shops in large villages and cities.

Figure 2 provides the timeline for the experiment and surveys. In July–August 2014 we conducted an enterprise census in all large villages, defined as those with 4,000 or more inhabitants in the 2012 national census, or that serve as the sub-district (ward) capital. We also included the trading cities of Dodoma and Babati.[3] The census covered 49 locations in total. Enumerators systematically walked each location, inviting participation by the owners of enterprises in eight sectors: wholesale trade, retail trade, transport, hiring/renting, agricultural processing, skilled tradespeople, non-agricultural services, and financial services. During the census we collected basic details only: enterprise name, location (subvillage or neighborhood), sector, respondent name, phone number(s), number of employees, and specialization details (e.g., traders could specify the crops they buy and sell). Enterprises were offered no incentive to participate beyond the potential benefits from the directory listing. A total of 1,506 owners enrolled their enterprise in the directory, out of approximately 2,100 that were invited.[4] We did not formally track reasons for refusal, but a commonly stated concern was potential exposure to tax authorities. After removing enterprises with information missing, we retained a sample of 1,494 enterprises in 47 villages and 2 cities.

Enterprise treatment assignment took place in two stages. First, we randomly assigned 7 locations (5 villages and one neighborhood of each of the two cities) to a Pure Control (PC) group. The 181 enterprises in the PC group were not listed in the experiment directory. Next, we randomly assigned each subvillage-sector group of the remaining enterprises to treatment (65%) or control (35%), stratifying on village-sector.[5] The 853 treated enterprises were listed in the experiment directory; the 459 control enterprises were not. Randomization at the subvillage-sector level ensured that neighboring competitors shared a

[3] Additional details about the study area are provided in Appendix A.

[4] A mix up in the tracking system prevents us from knowing the exact number of enterprises that refused participation, as some enterprises in a few locations may have been approached and recorded separately by multiple enumerators.

[5] A typical village in this part of Tanzania is divided into 2-4 subvillages, each with an administrative office and sometimes a small cluster of commercial buildings such as kiosks and grain stores. Subvillages may be separated from each other by a 10-30 minute walk. The two cities of Dodoma and Babati are also divided into smaller administrative units that we refer to as neighborhoods.

treatment status. We assigned clusters to the treatment with 0.65 probability to make the directory as useful as possible for the recipients.[6]

We printed the *Kichabi* as a folded A4 size booklet (Figure 3). Enterprises are listed alphabetically by village, sector, subvillage, and enterprise name. Figure 4 shows a snapshot of entries from the villages Mnenia and Mondo. The primary phone number for each enterprise is at right. The letter codes "A", "T", and "V" indicate mobile network operators. If the enterprise has a second phone number it is listed in the description column.

In December 2014 and January 2015 we distributed 3,290 directories in 47 randomly selected villages. We stratified by ward and by whether we had / had not conducted the census in each village (which is almost equivalent to stratifying by ward and village size), and selected one distribution village from each stratum.[7] Randomization at the village level was necessary because of the high likelihood of directory sharing. In each selected village we held a community meeting, advertised in advance. We did not limit attendance to farmers, although almost all residents of the study area are engaged in agriculture, and we advertised the meetings as relevant to farming. During the meeting we introduced the directory, demonstrated how to use it, and answered questions. We then distributed 70 copies of the directory to randomly selected attendants. We recorded the names and contact information of those who attended the meeting, and noted who received the directories.[8]

In July–August 2015 we held a second set of distribution meetings in 34 new villages, randomly selected using the strata that we had used for the previous December distribution. We held these meetings in order to identify a control group sample for the recipient experiment. The content of these meetings and the steps taken to recruit attendants were identical

[6]Throughout the study we were worried about the possibility of negative spillovers, if benefits to treated enterprises came at the expense of control. We took numerous steps to monitor for and mitigate any negative spillovers, including: creating a pure control group, treating more than half the sample, collecting multiple follow-up surveys in a single season, and breaking the experiment after one year by distributing thousands of complete directories that listed every enterprise.

[7]We excluded the cities of Dodoma and Babati for distribution of the experimental directories, because the target popultion consisted of residents of rural towns and villages.

[8]In a few cases the meeting was attended by fewer than 70 people. In those cases, we asked representatives of each subvillage to deliver the few remaining booklets to community members they believe would have been most inclined to attend the meeting if held at a different time. We followed up a few days later to learn the names and contact information of the individuals who received the directories in this way.

to those used for experiment directory distribution. The only difference is that we did not immediately distribute directories after the second set of meetings, because we wanted to survey control recipients prior to distribution. Instead, we returned in December 2015 and distributed the directories. The RCT was concluding at this point, so we distributed complete directories that included all of the census enterprises. At that time we attempted to distribute roughly 7,000 complete directories: one to every census enterprise, one to every attendee at both sets of distribution meetings, and some extra copies to village leaders.[9] While it would have been instructive to continute the RCT for a second agricultural cycle, we had committed to re-distributing the full directory at the outset, to minimize any long-term disadvantage to the control group in the event that the directory was effective.

In July–August 2016 we returned to the study area to experimentally measure the distribution of willingness-to-pay (WTP) for the directory among a new group of potential recipients. The WTP experiment served two purposes. The first was to estimate the revenue that could be raised by selling directories, to inform public or private actors interested in providing this service in the future. The second was to test whether WTP was lower for those living in treated villages, where directories were already widely available (although past recipients of directories were excluded from participating in these experiments). Any such effect would be consistent with widespread sharing of directories and confirm the partially non-rival nature of the information.

Stratifying on district, we selected 12 villages for the WTP study: six that we had not previously visited (new villages), three where we had conducted the census and distributed directories (large return villages), and three where we had only distributed directories (small return villages).[10] In each village we held two meetings with 30 respondents each, except in one village where scheduling problems limited us to a single meeting. Prior directory recipients were not eligibile to participate. We worked with village leaders to recruit a broad swath of participants, including female household heads, older people, heads of poor households,

[9] We sent text messages to everyone involved in the study, inviting them to come to the village office to pick up their booklet on a specific day. If they did not show up, we left a copy for them with village leaders.

[10] There do not exist villages where we conducted the census but did not distribute directories. After the 2015 endline survey we gave a copy of the complete directory to every listed enterprises.

and individuals from every subvillage. The total sample consisted of 690 participants, 330 in return villages and 360 in new villages.

During the WTP meetings we administered a variant of the Becker, DeGroot, Marschak (BDM) mechanism (Becker, DeGroot and Marschak, 1964). After an introduction to the directory and a practice round, we revealed 10 possible directory prices, ranging from zero to a price above the maximum 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 on that same day.[11] Participants were told that they would be given a few hours to gather the cash. After responses were collected, one participant drew one of the ten prices at random. Participants 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.

2.3 Data and Summary Statistics

Data for our empirical analysis come from three survey efforts administered by our team. Surveys were implented using the SurveyBe program on tablets running the Android operating system. For continuous variables, we designated a response as an outlier if it was more than five standard deviations from the mean, and replaced those values with the median.

1. Enterprise Surveys. Our team conducted five rounds of surveys with the owner or manager of 440 randomly selected study enterprises: baseline, midline, phone survey 1, phone survey 2, and endline (Figure 2). The baseline occurred prior to the onset of treatment (the distribution of the experiment directories); all other surveys took place after. We chose to measure some outcomes four times post-treatment in order to improve precision on noisy variables (McKenzie, 2012).

The baseline, midline, and endline surveys covered broadly similar topics, including communication, mobile money, sales, revenue, and employment. These surveys were con-

[11]Seeing the list of possible prices in advance may have anchored some respondents on those options. We piloted various approaches, and chose this one because the risk associated with some minor anchoring was outweighed by the strong preference of participants to interact with a known set of possible prices.

ducted in person by survey team members, at a time convenient for the respondent. The baseline began in the second week of September 2014 and lasted for a little over a month. The midline began in the third week of March 2015 and was 90% complete by the second week of May. Nineteen respondents either could not be located or refused to participate in the midline survey; we interviewed 18 replacements. The endline survey took place from September–November 2015, with a three-week break in the middle to accommodate the presidential election. We successfully reinterviewed 381 baseline respondents at endline. We attributed the lower re-survey rate to the election. Tanzanians return to their home towns to vote, and many enterprises were closed for an extended period to accommodate travel.

The two phone surveys were conducted from our team office at IRDP, in Dodoma. Phone survey 1 ran from the last week of May through the second week of June, and phone survey 2 began immediately after and ran for ten days. These surveys were timed to match periods of peak activity in the agriculture sector. The survey was very short and focused on one main outcome: the number of business-related phone calls received in the prior 2 business days. The goal was to improve precision of impact estimates on this key outcome without the imposition of another lengthy survey. We reached 392 respondents during phone survey 1 (89.1%), and 375 respondents during phone survey 2 (85.2%).[12]

Panel A of Table 1 contains baseline summary statistics for the surveyed enterprises. Column 1 provides means for treated enterprises. Column 2 provides means for enterprises assigned to control or pure control. Variables are grouped as follows (from top to bottom): respondent characteristics, general enterprise characteristics, employment, sales/revenue, and communication. To justify the interpretation of our estimation results as causal effects, we verify random assignment by separately regressing each variable in Panel A of Table 1 on a treatment indicator and stratification dummy variables. Column 4 reports the p-values on the treatment dummy from those regressions. Only one estimate is statistically significant at baseline, in the equation for the number of business calls received. This single depar-

[12]Response rates would likely have been higher if we had been able to extend the survey duration. A prior mobile phone survey in Tanzania had average response rates over 96%, but required consistent follow-up to accommodate respondents' changing availability (Dillon, 2012).

ture from balance is well within the expected range from random chance. Unfortunately, it happens to be present for our primary outcome variable. We use an analysis of covariance (ANCOVA) estimation strategy that controls for this baseline imbalance (Section 3.1).

The baseline survey took place during the slow time of year for agriculture-related enterprises. At baseline the mean treatment (control) enterprise received 6.55 (6.86) business-related phone calls over the previous week. In the four post-treatment surveys we narrowed the recall period to the previous two business days, in anticipation of a seasonal uptick in calling activity and potentially large treatment effects. Figure 5 shows the scatter plot and a non-parametric regression of incoming call volume over time. The increase in phone activity during March–June 2015 is related to the ongoing cultivation, harvest, and marketing periods, the busiest times of year for many study enterprises.

The four communication outcomes in the lowest section of Panel A have the lowest response rates (Column 3, Table 1). Almost all of the non-responses for these variables are coded as "Not applicable (NA)." It is possible that many of these would be better coded as "0," to acknowledge that these forms of communication are rare but not theoretically impossible. The "NA" response rates are similar for treatment and control enterprises, and the only difference for balance is that the statistically significant difference for incoming business calls is not significant if we recode "NA" as "0" (the revised p-value is 0.13).

During the endline survey we elicited enterprise WTP to be listed in future directories, using a contingent valuation approach.[13] Enumerators read a standardize script describing a hypothetical distribution of 100 directories per location in 50 nearby locations that had not previously received directories. They then asked a series of yes/no questions about willingness-to-pay X TSH to be listed in the hypothetical directory, where $X \in \{0, 2000, 5000, 10000, 15000, 20000\}$. Like the WTP experiment, the goals were twofold: first to estimate average WTP, to inform future programs, and second to test whether WTP was affected by prior treatment. A positive (negative) treatment effect on enterprise WTP

[13]It was not possible to elicit enterprise WTP using a real-stakes experimental design like that used for households. 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 other study aims and taken us well beyond our budget.

suggests that the value of the listing exceeded (fell short of) expectations. We surveyed an additional 509 randomly selected new enterprise owners at endline, in addition to the 381 re-surveyed enterprises, to improve our estimate of the distribution of enterprise WTP.

In June–July of 2017 we conducted a final brief phone survey with all study enterprises. The goals were to determine whether phone numbers were still active three years after our initial contact, and to ask whether enterprises were still operating. We reached 1,327 of 1,494 enterprises (88.8%) using one of the phone numbers collected during the 2014 census. Of the contacted enterprises, 1,263 were still in operation, representing 84.5% of all enterprises and 95.2% of those that we reached.

2. Recipient Survey. We conducted a single post-treatment survey with the directory recipients (treated villages) and planned directory recipients (control villages). Recipient surveys were conducted in July–August 2015, 1–2 months after the harvest and 7–9 months after distribution of the experiment directories. Surveys in control villages took place in the days after the second set of distribution meetings. Surveys in treatment villages were timed to coincide with the control surveys in the same stratum. We randomly selected 70 villages for the survey, stratifying on treatment status, ward, and village size. In each survey village, 12 respondents were randomly selected from the lists of those who had received the experiment directory (treatment villages) or who were scheduled to receive the complete directory soon (control villages). We successfully completed 831 interviews, 423 in treatment villages and 408 in control villages.

In addition to basic descriptive variables, the survey focused on primary outcomes related to using a mobile phone for search and contacting enterprises, and secondary outcomes related to input use, production, crop sales, and household enterprises. Balance on the time invariant (and slow-to-change) household characteristics is assessed with regressions similar to those used for enterprises (Panel B, Table 1). Households characteristics are well balanced across treatment and control, with the exceptions of gender and years in the village. We have no hypothesis for why these two variables may have differentially influenced attendance at the two sets of distribution meetings, and cannot rule out that these imbalances are an artifact of sampling.[14] We include both variables as controls in all household regressions (dropping them has no substantive effect on findings).

Control respondents have an average of 137.5 contacts stored in their phones. The distribution is skewed, with the 25th, 50th, and 75th percentiles occurring at 45, 100, and 204 contacts. The literacy rates is high: 91 percent of respondents can read Swahili. There are only 16 households in which no one can read Swahili.

3. WTP Experiment. The final analysis data set was gathered from the 690 participants in the WTP experiments conducted in July–August 2016. Two types of data were collected from this group. The first are the responses to a short survey administered before each meeting, covering basic descriptive variables, phone ownership and communication, prior exposure to the *Kichabi* booklet, and measures of geographical remoteness. The second data set consists of the bids made during the WTP elicitation experiment. We dropped eleven households with missing survey or bid data for a final WTP sample size of 679. During the last month, over half (51.4%) of the WTP participants had not placed a phone call to someone outside the village, and 47.4% had not left the village. All but 21.5% have at least one mobile phone in the household. Average WTP for a copy of the directory—which was delivered later that same day, upon receipt of payment—was 836 Tanzania shillings (TSH), equivalent to 0.38 US dollars (USD) at the July 2016 average exchange rate of 2,180 shillings per dollar. Restricting attention to the 62% of participants who bid more than zero, average WTP is 1,346 TSH. The maximim bid was 10,000 TSH.

[14]Although the treatment and control distribution meetings took place at different times of year, we intentionally avoided the harvest period when scheduling the second set of meetings so that the opportunity cost of time would be similar across recruitment periods.

3 Empirical Methods

3.1 Enterprise Outcomes

To estimate impacts on enterprise outcomes we follow McKenzie (2012) and employ the following ANCOVA specification using the enterprise survey data:

$$Outcome_{isr} = \alpha + \beta Treat_{is} + \gamma Outcome_{is0} + \delta M_{is0} + \lambda_r + \phi_s + \epsilon_{isr} \tag{1}$$

where $Outcome_{isr}$ is the value of the outcome for enterprise i in stratum s in round r, and $r = 1 \ldots 4$ for the midline, phone survey 1, phone survey 2, and endline, respectively; $Treat_{is}$ is a binary variable equal to 1 if the enterprise is in the treatment group, and 0 otherwise; $Outcome_{is0}$ is the baseline value of the dependent variable (set to zero if missing); M_{is0} is an indicator for whether the baseline value is missing; λ_r is a set of round dummy variables; ϕ_s is a set of strata dummy variables; and ϵ_{isr} is a statistical error term. For outcome variables that were not collected at baseline, we estimate (1) without $Outcome_{is0}$.[15] Enterprises in the pure control group are excluded from estimation of (1) because there is no within-stratum variation in treatment for this group. With treatment randomization, the estimate of $\hat{\beta}$ from specification (1) is the average treatment effect (ATE) of the directory listing for the population of enterprises in the study sectors willing to be listed in the *Kichabi*.

We estimate (1) for two types of enterprise outcomes. The first are communication outcomes, specifically the number of incoming business-related phone calls, the number of calls from new customers, and the number of missed calls. Missed calls are of interest because they are often used as a request for a call-back (A calls B, but hangs up before B is able to answer). The second set of outcomes includes measures of economic activity that could be influenced by greater incoming communication from customers, such as outgoing communication, mobile money, sales, employment, and revenue.

Positive spillovers to control enterprises could happen through two primary channels:

[15] The two outcomes not collected at baseline are the number of incoming business-related calls from new customers, and the number of missed calls.

Kichabi users could travel to a village because of a newly established relationship with a treated enterprise, and encounter some control enterprises with whom they transact; or, treated enterprises could directly channel new business to nearby control enterprises, either via their own demand for intermediate inputs, or by referring callers to control enterprises for items not in stock. Negative spillovers could occur if the directory diverts activity to treated enterprises at the expense of the control group, rather than generating new business. To estimate spillover effects, we use data from treatment, control, and pure control enterprises to estimate the following OLS specification:

$$Outcome_{ikd} = \alpha + \beta_1 Treat_{ikd} + \beta_2 Control_{ikd} + \gamma Outcome_{is0} + \delta M_{is0} + \lambda_r + \psi_{kd} + \epsilon_{ikd} \quad (2)$$

where $Outcome_{ikd}$ is the value of the outcome for enterprise i in sector k and district d; $Control_{ikd}$ is a binary variable for the control group; ψ_{kd} are district-sector fixed effects; and other variables are as in equation (1). The excluded category is for the pure control group enterprises, which are located in villages where no enterprises are listed. District-sector effects are the lowest level of cross-sectional fixed effects that are identified.[16] The estimate of $\hat{\beta}_1$ from specification (2) is the ATE of the directory listing relative to the pure control group, and the estimate of $\hat{\beta}_2$ is the average within-village spillover effect to the control group. Positive values for $\hat{\beta}_1$ in combination with negative values for $\hat{\beta}_2$ would raise concerns that benefits to the treatment group may have come at the expense of the control group. Positive values of both $\hat{\beta}_1$ and $\hat{\beta}_2$ could indicate differential trends across villages, and would also be consistent with positive within-village spillovers from the *Kichabi*.

To estimate average treatment effects on enterprise WTP for a future directory listing, we estimate equation (2) with WTP_{ikd} as the dependent variable, but without including round effects (λ_r) or variables from the baseline survey ($Outcome_{is0}$, M_{is0}), because enterprise WTP was only collected at endline. These regressions include the additional 509 enterprises that were interviewed once, at endline, to increase power for the WTP analysis.

[16] The second-stage randomization strata, which are subvillage-sector effects, cannot be included because there is no variation in treatment status within those strata in the pure control villages.

3.2 Recipient outcomes

Impacts on outcomes for directory recipients are estimated using the recipient survey data and the following OLS specification:

$$Outcome_{js} = \alpha + \beta Treatment_v + \delta X_{js} + \phi_s + \epsilon_{js} \quad (3)$$

where $Outcome_{js}$ is the value of the outcome for household j in stratum s; the matrix X_{js} includes the two time invariant recipient characteristics that exhibited imbalance, gender and number of years living in the village; ϕ_s is a set of strata dummy variables; and ϵ_{js} is a statistical error term. Because non-compliance is a possibility—due either to errors in recording who received the directories in treated villages, or to individuals in the control villages somehow gaining access to a directory—the estimate of $\hat{\beta}$ from specification (3) is the intent-to-treat (ITT) effect of the directory for the population of rural households who would attend a community meeting advertised as an opportunity to learn about an information service for farming households.

We estimate (3) for two types of household outcomes. The first are communication outcomes related to phones or other linkages outside the village. The second category includes agricultural choices, agricultural outcomes, and outcomes related to non-farm enterprises, all of which may be affected by the realized or expected change in search and communication costs facilitated by the *Kichabi*.

We pre-specified two dimensions of potential heterogeneity in recipient treatment effects.[17] First, we posited that treatment effects would be larger for households with lower levels of wealth, as they are less likely to travel and develop large phone networks, and hence are less able than their wealthier counterparts to make productive use of a phone without a complementary information service. Second, we hypothesized that treatment effects would

[17] This study was not formally pre-registered. However, our January 2014 proposal to the study's primary funder, the BASIS innovation lab at UC Davis, is publicly available at the project page on the BASIS website: https://basis.ucdavis.edu/project/communication-search-and-mobile-phones-a-telephone-directory-intervention-tanzania. The initial proposal document includes a discussion of potential dimensions of heterogeneous treatment effects for recipients, at the top of page 4. The two dimensions of heterogeneity discussed in that document that can be analyzed in this study are those related to wealth and gender.

be larger for female recipients than for male recipients, because men are more likely to travel for trading purposes and hence can gather phone numbers more easily than women. Of the 831 directory recipients, 171 (20.6%) were women.

To estimate heterogeneous treatment effects on recipients, we estimate versions of equation (3) that include a binary variable for above/below median wealth or for being a male recipient, as well as an interaction between that variable and the treatment dummy. All other variables are as in (3). Household wealth is measured as the first principal component from a vector of household assets (Filmer and Pritchett, 2001).

To estimate the effects of treatment on recipient WTP we use the bid data from the experiments conducted in 2016 and the following OLS specification:

$$WTP_{jmv} = \alpha + \beta_1 SmallReturn_v + \beta_2 LargeReturn_v + X_{jmv}\gamma + \epsilon_{jmv} \tag{4}$$

where WTP_{jmv} is the bid of participant i in meeting m in village v; $SmallReturn_v$ is a dummy variable for villages in which directories were distributed, but which were not represented in the census or the *Kichabi*; $LargeReturn_v$ is a dummy variable for villages where we conducted the census and delivered directories; X_{jmv} are participant characteristics that might influence WTP; and ϵ_{jmv} is an error term. The group not represented with an indicator variable in (4) are the new villages, where we had neither distributed directories nor conducted the census. Under the assumptions that participants understood the experiment and had no deceptive intentions, the bid values represent participants' maximum WTP for a copy of the directory. The estimate of $\hat{\beta}_1$ is the ITT from living in a distribution village on WTP, and the estimate of $\hat{\beta}_2$ is the ITT from living in a census and distribution village on WTP. Differences between these two coefficients represent variation in WTP for the directory based on whether it contains information about enterprises in one's own village, as well as any differences in average WTP between residents of larger and smaller villages.

3.3 Inference

In estimation tables based on the RCT we report standard errors clustered at the level of treatment assignment (Abadie et al., 2017). In equations (1) and (2) that is the subvillage-sector level. In equations (3) and (**??**) it is the village level. In equation (4), which uses data from the WTP experiments administered after the RCT, we report standard errors based on the wild cluster bootstrap of Cameron, Gelbach and Miller (2008). There were 23 village-meeting clusters in the WTP experiments, each with a distinct peer group, practice round, and drawn price.

We estimate the main treatment effect equations (1) and (3) for a substantial number of outcomes, most of which fall naturally into categories. To correct for any bias due to multiple testing, we also report q-values (adjusted p-values) adjusted to control the false discovery rate (FDR). The FDR is the share of null hypotheses incorrectly rejected due to multiple testing (Simes, 1986; Benjamini, Yekutieli et al., 2001).

4 Results

4.1 Enterprise Treatment Effects

4.1.1 Average Treatment Effects on Enterprise Outcomes

Table 2 reports the treatment effect estimates of $\hat{\beta}$ from equation (1). The table reports coefficient estimates and standard errors (columns 1 and 2), p-values (column 3), q-values for outcome categories denoted by boldface labels (column 4), sample size for each regression (column 5), the control group mean (column 6), and the percentage difference from the control mean represented by the estimated treatment effect (column 7). Sample sizes are larger in Panel A because those outcomes were surveyed at midline, endline, and in the two phone surveys, whereas outcomes in Panel B were surveyed at midline and endline only.[18]

[18] In Panel B of Table 2, sample size is lower for the mobile money, employment, and sales outcomes, because we only asked about these outcomes for enterprises that were operating (some enterprises temporarily close during slack periods). We asked about business communication even if the enterprise was temporarily closed,

In Panel A, column (1), the first row reports that listed firms on average received an additional 2.46 business-related phone calls over the previous two working days. This 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.

If treated respondents intuited the study goals and exaggerated the number of incoming calls, estimates in the first panel of Table 2 are biased upwards. Demand effects of this nature are a common concern in experimental research based on survey outcomes (De Quidt, Haushofer and Roth, 2018). As a verification step, enumerators asked respondents if they could look through the phone history together. When the analysis is limited to the approximately 75% of respondents who agreed to this at least once during the surveys, estimated treatment effects are larger in magnitude (Panel A, "Phone History checked" sample). The coefficient on number of incoming calls increases to 2.67 and is statistically significant (p-value = 0.01). The effect on the number of missed calls increases almost fourfold, to 0.38, 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 for 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.

Panel B of Table 2 reports impacts on other enterprise outcomes. Treated enterprises are 8 percentage points (12%) more likely to use mobile money, and 11 percentage points (21%) more likely to send outgoing mobile money transfers; both results are statistically significant after correcting for multiple testing. Treated enterprises are 12 percentage points (20%) more likely to have workers other than the owner, and 15 percentage points (51%) more likely to have paid, non-family workers. Both results are significant before correcting for multiple testing, with q-values of 0.103 and 0.137, respectively, after multiple testing

because *Kichabi* recipients may not have known which enterprises were on hiatus, and still called.

adjustments. The estimated effects on other outcomes—outgoing calls, text messages, sales, purchases, and revenue—are positive but imprecise.

4.1.2 Enterprise Spillover Effects

Table 3 reports the estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ from equation (2). These coefficients represent the average treatment effect ($\hat{\beta}_1$) and spillover effect ($\hat{\beta}_2$) relative to the pure control group. The table reports estimates for the same set of outcomes as the main analysis in Table 2. There are three main takeaways from Table 3. First, there are no negative and significant estimates of spillover effects on control enterprises. The benefits to treated enterprises did not come at the expense of neighboring enterprises in the same village. Second, the estimated treatment effect ($\hat{\beta}_1$) on the primary communication outcome—the number of incoming business-related calls (Panel A, first row)—is similar in magnitude and statistical significance to its value in Table 2, while the spillover estimate of $\hat{\beta}_2$ for the same outcome is smaller in magnitude and not statistically different from zero. This is not surprising, as we did not list control enterprises in the experimental directory. Third, relative to the pure control enterprises, both the treatment and control enterprises saw economically and statistically significant increases in text messaging (SMS), use of mobile money, number of sales transactions, and sales revenue (Panel B). The average two-day sales revenue of treatment and control enterprises was approximately twice that of pure control enterprises. The *Kichabi* increased the level of business activity in treated villages, generating benefits even for unlisted enterprises.

How do we interpret the statistically significant spillover effects on SMS messaging and mobile money usage, but not on incoming business-related phone calls? Our many discussions with study participants provide one interpretation. The norm in Tanzania is that text messages are reserved for known contacts. Participants consistently stated that they would prefer to first call a new *Kichabi* enterprise, and only use SMS once a relationship was established. We suspect that spillovers occur either through increases in demand from control enterprises mediated directly by treated enterprises, or through incidental encounters that occur when *Kichabi* users travel to a village to transact with a newly established contact from the treatment group. In either case, a connection with a control enterprise can be formed without an initial phone call. Ensuing contact may then take place via SMS, leading to a spillover effect on texting but not calling.

Our findings raise the possibility that negative spillovers may have occurred between rather than within villages. The design does not allow us to determine whether the benefits accruing to treatment and control enterprises came at the expense of the enterprises in the pure control group. However, in mid-2017 we called all study enterprises to determine whether they were still in operation. We reached 92.3% of the PC group, 90.7% of the control group, and 87.1% of the treated group. Of the enterprises reached, we found the following percentages still in operation: 95.8% for PC, 95.7% for control, and 94.8% for treatment. Enterprise survival rates 1.5 years after the end of treatment are almost identical across study arms. Although this analysis cannot rule out the possibility of between-village spillovers, treatment only could have negatively affected the survival of pure control enterprises if their counterfactual survival rates were greater than those of treated enterprises.

4.1.3 Average Treatment Effects on Enterprise WTP

Figure A2 in Appendix C shows demand curves estimated from the enterprise WTP responses in the endline survey, separately for the treatment, control, and pure control enterprises. Respondents selected their maximum WTP from a list, so these curves represent lower bounds on the true demand curves. At all prices, demand is lowest among pure control enterprises, followed by control and then treated enterprises. Mean willingness-to-pay across all enterprises is 3621 TSH (1.72 USD). Among all groups there is positive demand even at the high price of 20,000 TSH (9.17 USD) per listing.

Table 4 reports coefficient estimates from versions of equation (2) modified to suit the cross-sectional nature of the WTP data. Column 1 shows that treatment increases enterprise WTP by 789 TSH, or 23%, relative to the control and pure control groups. Column 2 confirms the relationships implied by Figure A2: both treatment and control firms are willing to pay more on average than pure control firms, although the control group effect is not statistically significant. We cannot reject equality of the coefficients on treat and control in column 2. In column 3 we pool the treatment and control groups against the Pure Control group. On average, location in a village with previously listed enterprises has a positive, economically meaningful, and statistically significant impact on enterprise WTP.

Treatment led to an upward revision of the prior value placed on a listing by the average treated enterprise. The smaller but positive effects for the control group indicate a learning spillover. These effects are not driven by differences in trust that the survey team would follow through on the hypothetical commitment to the printing, because all respondents received a copy of the complete directory, with their enterprise listed, just before their endline interviews. These WTP estimates confirm that the treatment effects reported in Section 4.1 represent real and recognized improvements for the listed enterprises, validated by their greater WTP for future participation.

4.1.4 Discussion of Enterprise Effects

The *Kichabi* led to large increases in incoming calls to listed enterprises. Using the estimated treatment effect on incoming calls in Table 2, we can construct back-of-the-envelope estimates of the total number of calls induced by the experiment. The main outcome variable measures incoming calls over the previous two operating days. Hence, the treatment effect represents an increase of $2.46/2 = 1.23$ calls per day to each listed enterprise. A cautious estimate of the duration of the enterprise study period is from February 1, 2015, to September 16, 2015, for a total of 228 days. In the baseline survey the mean number of operating days per week is 6.22, which gives $228 \times (6.22/7) = 203$ effective operating days on average. The average listed enterprise received $203 \times 1.23 = 250$ more calls during this period than the average control enterprise. With 853 listed enterprises, that amounts to $250 \times 853 = 213,250$ additional phone calls. Because recipients shared the directories widely, we cannot assume that this effect would scale linearly if we distributed more directories. Community members were clearly quick to re-allocate the directories to those most likely to use them.

The absence of a positive effect on calls from new customers may seem to indicate that the directory enabled communication between previously acquainted parties. An alternative interpretation is that directory users called *Kichabi* firms multiple times, and enterprise owners would have only described the caller as "new" after the first call. The latter interpretation is more consistent with our impressions from the fieldwork. Recipients tended to treat the booklet as a source of entirely new information, and many enterprise owners said that the *Kichabi* was bringing in new business.

Comparisons of outcomes between treatment and control enterprises indicate no significant impacts on sales or revenue. This could be due to a lack of statistical power, to the short time period of the study, or to a genuine lack of significant impacts. However, the spillover analysis in Table 3 shows large, positive, statistically significant effects on both the number of sales and total sales revenue for the treatment and control enterprises, relative to the pure control. The most likely explanation for the null results in Table 2 is that the *Kichabi* stimulated activity for both treatment and control enterprises, making it difficult to detect differential effects within-village. This interpretation is strengthened by our analysis of the effect of treatment on stated willingness-to-pay. Through their greater WTP, treatment (and, to some extent, control) enterprises signal their positive impression of the effect of a directory listing on enterprise performance.

4.2 Recipient Treatment Effects

We turn now to the recipient side of the *Kichabi* experiment. Over the study period, 27% of recipients report contacting one or more enterprises in the directory. The average number of calls to directory enterprises, among the callers, was 1.65.[19] 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 interviews, many recipients described lending the directory to a friend who wanted it specifically for business or trading activities. We did not survey these non-recipients. However, non-recipients must have made

[19] An implementation error during the recipient survey prevents us from knowing which specific enterprises were contacted by each recipient.

a large number of calls to listed enterprises, to generate the substantial treatment effects on incoming calls reported in the previous subsection.

4.2.1 Intent-to-Treat Effects on Recipient Outcomes

1. Communication. Panel A of Table 5 reports ITT estimates of $\hat{\beta}$ from specification (3) for general communication outcomes. Treatment leads to an 11 percentage point (19%) increase in the likelihood of sending an SMS message, an increase of 48 stored phone contacts (35%),[20] a 7 percentage point (20%) increase in the likelihood of receiving a mobile money transfer, a 12 percentage point (36%) increases in the probability of sending mobile money, a 6 percentage point (22%) increase in the likelihood of ordering goods for delivery from outside the village, and an 8 percentage point (43%) increase in the likelihood of using the phone to coordinate a delivery. Treated recipients spend 559 TSH (14%) more on phone credit over the previous two weeks (credit is required to place calls or send texts), although the p-value for a two-sided test of significance is 0.107. There is no extensive margin effect on the likelihood of making at least one phone call, because there is almost no variation—96% of control respondents made a phone call during the last two weeks. There are likewise no impacts on the probability of receiving calls or SMS messages. Overall, the breadth and variety of positive, significant effects suggests substantial pent-up demand for information and communication beyond current networks.

2. Crop Production. Panel B of Table 5 reports ITT effects on crop-related choices and outcomes.[21] The first outcome listed is an index equal to 1 if the farmer used the phone for any crop activity. Directory recipients are 11 percentage points (36%) more likely to use their phone for crop-related activities. Recipients were significantly more likely to use their phones for a wide range of crop activities: searching for inputs (10 percentage points; 75% more likely), acquiring those inputs (9 p.p.; 53%), coordinating with buyers (5 p.p.; 99%),

[20] This finding is effectively unchanged if we limit the analysis to recipients who allowed the enumerator to count the contacts.

[21] Most of the variables in this panel were only collected for maize and sunflower, the dominant local crops.

and searching for output prices, conditional on conducting any output price search(10 p.p.; 61%). These effects remain statistically significant after corrections for multiple hypothesis testing. Treated households were also 4 percentage points (44%) more likely to use the phone to seek general agricultural advice (p-value=0.103). Anecdotal evidence suggests that such advice was often sought from input suppliers or wholesale traders.

Treatment reduced the likelihood of a crop failure for maize by 7 percentage points (27%) and for sunflower by 3 percentage points (23%), although the effect on sunflower is not statistically different from zero. A possible mechanism for improved maize outcomes is better access to inputs: treated recipients were 5 percentage points (33%) more likely to borrow or rent land, and 12 percentage points (23%) more likely to hire labor. We find no statistically significant differences on total input expenditure or on the extensive margin use of other inputs, including fertilizer, pesticides, purchased seeds, and tractors or plow animals. There is less variation in use of these inputs than for hired labor and rented land. Only 1 and 4 percent of control households use pesticides and fertilizer (respectively), while 91% use tractors or plow animals and 92% purchase seeds. Treated farmers were not differentially likely to search or source inputs from inside or outside the village.

Treated households were 13 percentage points (25%) more likely to search crop prices outside their villages, conditional on searching. To analyze effects on the prices received, we pool maize and sunflower sales and estimated the regression in logs, with controls for crop-by-unit fixed effects.[22] Receipt of the directory had a positive but imprecise effect on the price received for crops. The point estimate indicates 7% higher prices for treated recipients, with a p-value of 0.13. This finding is based on an unexpectedly small number of sales. The study area experienced a severe drought during a critical period of crop growth in 2015. Only 271 sales resulted from 1,427 plantings of maize or sunflower. Given the substantial impacts on searching prices by phone and searching prices outside the village, it is possible that in a more typical year we would see a statistically significant impact on prices.

[22] We adopted this strategy *ex post*, after seeing very few sales due to the bad weather year. Estimated effects on crop sales price are not statistically significant if we examine crops individually.

3. Livestock Production. Panel C of Table 5 shows ITT effects for the sale and purchase of livestock. Most of these effects are conditioned on selling/buying or some other behavior, and hence are estimated on smaller samples. There is no statistically significant treatment effect for 7 of the 8 livestock outcomes. However, treated recipients who sold cattle or goats were almost 200% more likely to use their phone to search sales prices. In contrast, not a single farmer who bought cattle or goats reported using the phone to search purchase prices. This pattern is reassuring in one sense: respondents' willigness to report no phone use for some activities suggests that the positive treatment effects on other outcomes are not purely reflective of experimenter demand effects.

4. Non-farm Enterprises. In panel D of Table 5 we report ITT effects on non-farm enterprises (NFEs) run by recipients. Treated households are 16 percentage points (59%) more likely to run an NFE. Conditional on operating an NFE, recipients were not differentially likely to purchase inputs or make sales. They were weakly more likely to use their phones to acquire inputs (15% increase, p-value = 0.12).[23]

We have no other information about the NFEs opened by treated enterprises. Barriers to entry are very low in some agriculture-related sectors, such as retail trade and transport, and we suspect that most new NFE activities are in these areas. This finding reflects a common theme from our discussions with residents of the study area. Small business owners and potential entrepreneurs were extremely enthusiastic about both being listed in and receiving a copy of the *Kichabi.* Although we designed the directory to serve farmers, there is substantial overlap in the networks of farmers and of enterprises in related sectors. Non-farm enterprise owners face high costs to travel and establish new contacts and trading relationships. Entrepreneurs saw obtaining a copy of the directory as a low-cost avenue for expanding their business networks and opening new enterprises.

[23] Control farmers were recruited about half a year after treated farmers, so one might be concerned that the effect on NFE ownership reflects differential selection. However, if anything, this should work against us finding a statistically significant treatment effect. Non-treated enterprise owners clamored 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.

4.2.2 Heterogeneous Treatment Effects on Recipient Outcomes

Before conducting the *Kichabi* experiment, we hypothesized that recipient treatment effects would be larger for low wealth recipients and for women. Our analysis supports neither hypothesis. In Table A1 of Appendix D we report estimates of heterogeneous treatment effects for households above and below median wealth. We do find that some impacts are only statistically significant for one group. For example, effects on using hired labor and renting land are only significant for poorer households, while the point estimate on the crop sale price is effectively zero for poorer households but large, positive, and statistically significant for wealthier households. Yet, for all 45 outcomes we cannot reject the null hypothesis that the treatment effects are the same across wealth subgroups.

We find a similar pattern in our estimates of heterogeneous treatment effects by recipient gender (Appendix D, Table A2). There are many outcomes for which the treatment effect is statistically significant for one gender but not the other. In most of those cases the t-statistic is larger in magnitude for men. This is likely an artifact of power, because there are almost four times as many men as women in the sample. In only 3 out of 45 cases can we can reject with 90% confidence the hypothesis that the treatment effects for men and women are the same. This rejection rate is in line with expectations from random chance. Moreover, the statistically different effects are not clustered on a group of related outcomes, and do not lend themselves to obvious interpretation.

These analyses suggest that the directory had broad and statistically similar effects across both the wealth and gender distribution of recipient households. If wealthy households or men enjoy an advantage in formation of phone networks absent a directory, it is not so substantial as to render the directory differentially effective for them.

4.2.3 Intent-to-Treat Effects on Recipient WTP

Figure A3 in Appendix C shows non-parametric demand curves estimated from participant bids in the WTP experiments, separately by village status in the development and distribution of the *Kichabi*. In the 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 is lowest in the large, return villages. These are places where both experiment directories and complete directories were distributed, and where enterprises are listed in the directory.

Table 6 reports coefficient estimates from equation (4). The point estimate for living in a small, return village is negative, though it is not statistically significant (column 1). Small, return villages are locations where *Kichabi* directories were previously distributed, but which are not themselves represented in the directory. The point estimate for large, return villages is −662.3 (p-value=0.07). This implies that the average resident of a large, return village is willing-to-pay only 36.5% as much as the average resident of a new village. The estimated coefficients are largely unchanged when we add controls for other participant characteristics, in columns 2 and 3. The signs of the coefficients on the other characteristics are in line with expectations: older (wealthier) participants exhibit lower (higher) average conditional WTP, women are willing to pay weakly less than men, and those who have previously seen the directory are willing to pay weakly less than those who have not.

The wide availability of directories for sharing likely played a role in reducing average WTP in the large, return villages. These villages received the greatest number of booklets during the re-distribution of complete directories in late 2015, because the owners of listed enterprises received booklets, along with the 70 residents that had received the experimental directories a year earlier). Many WTP participants reported never having previously seen the *Kichabi*, and prior to the experiment they may not have known that booklets were available from their neighbors. But the experiment involved a meeting attended by 30 village residents. It was easy for participants in return villages to learn on the spot that many copies of the booklets were circulating in the village. Of course, lower WTP in the large, return villages may also be due to structural differences between the communities. Larger villages have greater numbers of enterprises, and residents may have perceived lower benefits to acquiring contact information for enterprises in other locations.

4.2.4 Discussion of Recipient Effects

Receiving a *Kichabi* enriched the productive lives of farming households. Recipients were substantially more likely to use their phones to communicate with agents outside the village. The statistically significant effects suggest some clear stories. Treated farmers identified new trading partners from outside their pre-existing networks, ordered more goods for delivery to the village, and paid for those goods using mobile money. Receiving a directory during the planting period must have changed expectations about future access to inputs or output prices, because recipients were more likely to expand their farms through borrowing or renting land, and were more likely to hire labor. These choices require outlays weeks or months before the benefits are realized. Revisions to expectations were accurate in at least one sense: in a bad year for crop production, agricultural outcomes were better for treated farmers, as indicated by the lower rate of crop failure.

We do not know how the directory affected farm yields or profits. However, it is clear that the *Kichabi* sparked new efforts at income diversification, by prompting the creation of new non-farm enterprises. A lesson from this finding and from our many discussions in the study area is that village residents do not see a distinct line between their agricultural activities and other income-generating activities. Essentially everyone in the community is somehow involved in farming. Households dial up and down their non-farm enterprise investments based on changes in costs and opportunities, and some recipients saw the *Kichabi* as a means to jumpstart a new productive activity.

The positive but marginally insignificant effect on crop sales prices is best understood in relation to prior studies. It is widely believed that information asymmetries allow traders to extract rents from farmers when negotiating over crop prices. Although direct evidence of non-competitive pricing by traders is scant (Dillon and Dambro, 2017), perceptions of trader rent extraction are widespread enough that many interventions have been launched to improve farmers' bargaining positions by providing them with market price information via ICT. Muto and Yamano (2009) study the expansion of the mobile network in Uganda, and find positive effects on farmer sales prices for some crops but not for others. Svensson and

Yanagizawa (2009) find a similar pattern of effects from a radio-based price service in Uganda. Neither of these studies can ensure random access to price information. Using propensity score matching, Courtois and Subervie (2014) find positive effects of an SMS-based service on prices received by farmers in Ghana. Yet, Hildebrandt et al. (2015) evaluate the exact same service using an RCT, and find only a short-term positive effect on one crop, which disappears in the second year of the study. This array of mixed and null results from sub-Saharan Africa is similar to the evidence from other regions, where significant effects of targeted information services on prices received by farmers have proved elusive (Aker, Ghosh and Burrell, 2016). In this context, our marginally insignificant treatment effect of 7% is encouraging. If traders are indeed earning rents due to information asymmetries, the *Kichabi* was the same as, or more effective than, many programs focused solely on providing price information. Moreover, the *Kichabi* effect is from a generalized intervention that provided contact details, rather than a one-way push of market price data from a customized information system. If farmers are able to use directory services to learn prices, while also benefitting in myriad other ways, then complex interventions to curate and share specific, narrow types of information with farmers may represent an inefficient use of public resources.

The results of the WTP experiment indicate that prior *Kichabi* exposure—defined as living in a village where directories were previously distributed—reduces individual WTP for a personal copy of the booklet. We find the opposite effect for enterprises, for whom prior experience increases WTP for a future listing (Table 4). The directory plays a starkly different role for the recipients and listed enterprises. In village communities where resource-pooling is common and residents are unlikely to need the booklet full time, the information in the directory is non-rival. The expected benefit to owning a booklet is decreasing in the number of directories in circulation. In contrast, conditional on the directory effectively generating new business for listed enterprises, the expected benefit to being listed is likely to be increasing in the number of other enterprises listed in the directory. If the *Kichabi* is widely used, failure to list one's enterprise could lead to long-term competitive disadvantage. The differential cost to enterprises and recipients from "missing out" on the directory is one of the reasons that phonebooks for landlines typically charge businesses to advertise, but distribute copies to consumers for free.

5 Conclusion

In this paper we evaluate the effects of a telephone directory for the mobile phone network, *Kichabi*, on listed enterprises and recipient households. The directory was designed, printed, and distributed by our team, in a rural, geographically contiguous area of central Tanzania. We find evidence of substantial pent-up demand for communication. Relative to unlisted enterprises in the same villages, the *Kichabi* led to a 27% increase in incoming calls for listed enterprises, greater use of mobile money, and a higher likelihood of having workers other than the owner. Unlisted enterprises enjoyed positive spillovers if they were located in villages with treated enterprises. Compared to a pure control group, both treatment and control enterprises saw increases in the number of sales transactions and total sales revenue, likely due to increases in foot traffic to the villages listed in the *Kichabi*. Owners of treated enterprises indicated greater willingness-to-pay for a future listing, consistent with their perception of positive benefits.

Kichabi recipients also enjoyed substantial benefits. Treated households made greater use of their phones for farming purposes, were more likely to rent land and hire labor, had lower rates of maize crop failure, and enjoyed weakly higher sales prices. They ordered more goods for delivery from outside the village, and made greater use of mobile money. In an incentivized experiment conducted after the end of the RCT, nearly two thirds of non-recipient participants were willing to pay a positive amount for a directory. Residents of previously treated villages were willing to pay less. Directories were shared widely in treated villages, driving down individual WTP for a personal copy.

There are many reasons that markets may be slow to provide profitable goods or services in rural areas of low-income countries. Yet, it reasonable to wonder why no one is printing directories, given the apparent benefits and the high enterprise willingness to pay. In 2017 we put this question directly to executives of a major mobile network operator in Tanzania. They cited the difficulty of associating phone numbers with specific business entities, the uncertain returns to programming for rural areas, and limitations to their capacity for innovating in many dimensions simultaneously. In the absence of action by the network operators, a start-up firm could gather contact information and issue directories. Back-of-the-envelope calculations suggest that printing directories and charging enterprises to be listed would be profitable in Tanzania, even with free distribution (see Appendix B for details). Of course, it is possible that a private entrepreneur would enjoy less cooperation from enterprise owners than we did. Our partnership with IRDP was helpful in building confidence and gaining the support of community leaders. If a trusted public institution is necessary to generate buy-in by enterprises, then directory services represent an important, missing public good in low-income countries.

The idea motivating this study is that contact information is qualitatively different from other types of information. The mobile phone revolution has prompted the creation of many programs to gather and share carefully curated information related to agriculture, health, finance, or other areas. Many of these programs are likely to be beneficial and successful. Yet, it is possible that in the rush to create nuanced platforms for sharing information that complements specific activities, the development community has overlooked the importance of simply providing contact information. Such services have been ever-present, and under-appreciated, for landlines. Residents of rural villages are savvy at seeking out and making use of valuable information. Mobile phones have lowered the costs they bear for communicating with known entities. Phonebooks represent a straightforward way to enhance the value of phones, thicken local networks, and allow residents and businesses to exchange whatever information they please.

Figures

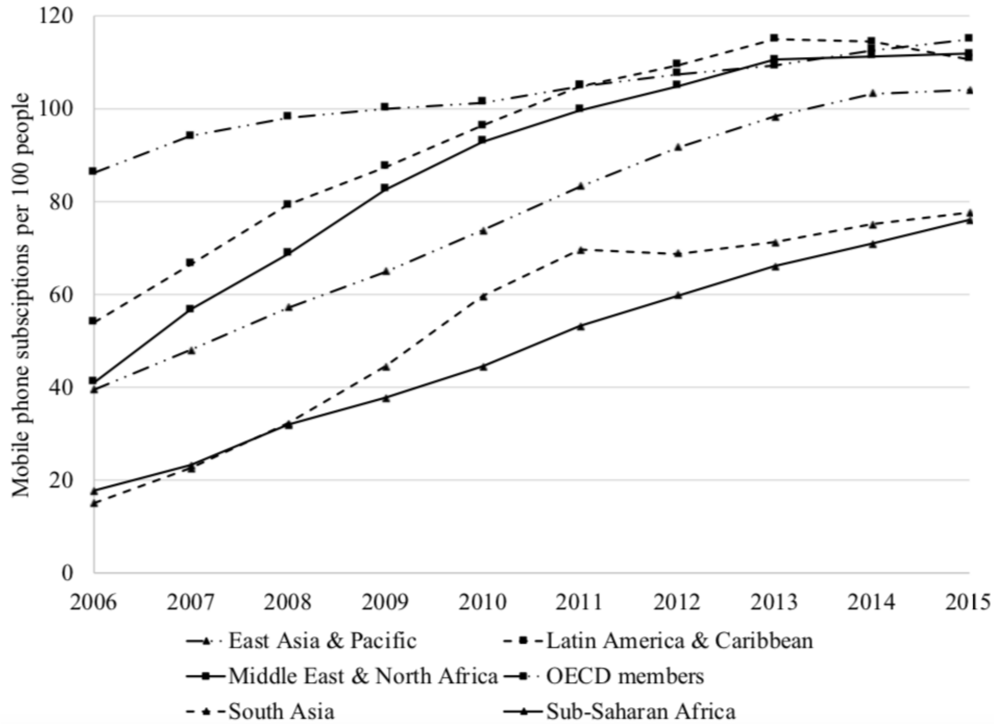

Figure 1: Mobile phone subscriptions per 100 people, by region, 2006-2015

Notes: Source is the World Development Indicators (World Bank, 2020). Authors' calculations.

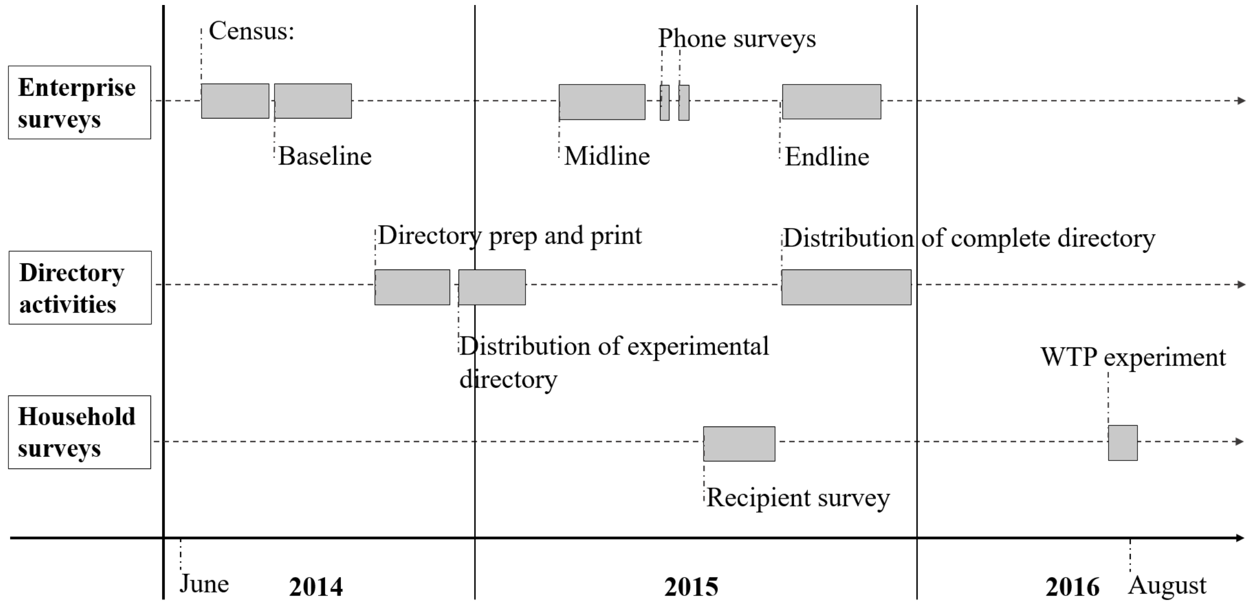

Figure 2: Timeline of the *Kichabi* Telephone Directory Experiment

Notes: This figure shows a timeline for the primary *Kichabi* activities. The agricultural cultivation season runs from roughly January to May. Planting can occur as early as December, and continue through February. Harvest typically occurs in May–July. Not pictured is a follow-up survey activity conducted in July 2017, in which we called all enrolled enterprises to ask if they were still in operation.

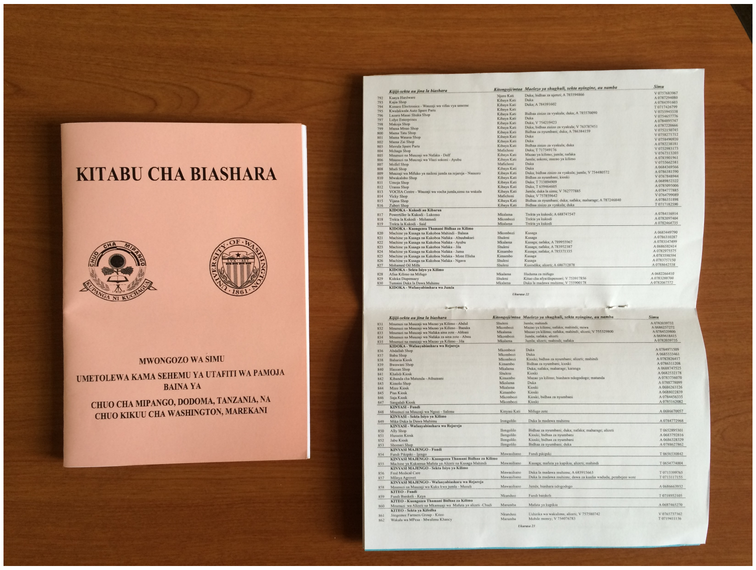

Figure 3: The *Kichabi* Directory

Note: At left is the front cover with the Swahili title "Kitabu Cha Biashara," or "business book." At right is a page from the directory.

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 4: Example entries from the *Kichabi* experimental directory

Notes: Figure shows a snapshot from the printed *Kichabi* telephone directory. The columns from left to right are the enterprise name, sub-village or neighborhood, description field that allows for differentiation and the listing of additional phone numbers, and the primary phone number with a letter code to indicate the mobile network. The entries shown are a subset of those from the villages Mnenia and Mondo. The first three rows are wholesalers from Mnenia (carried over from the previous directory page). The middle group of entries are retailers in Mnenia, 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*).

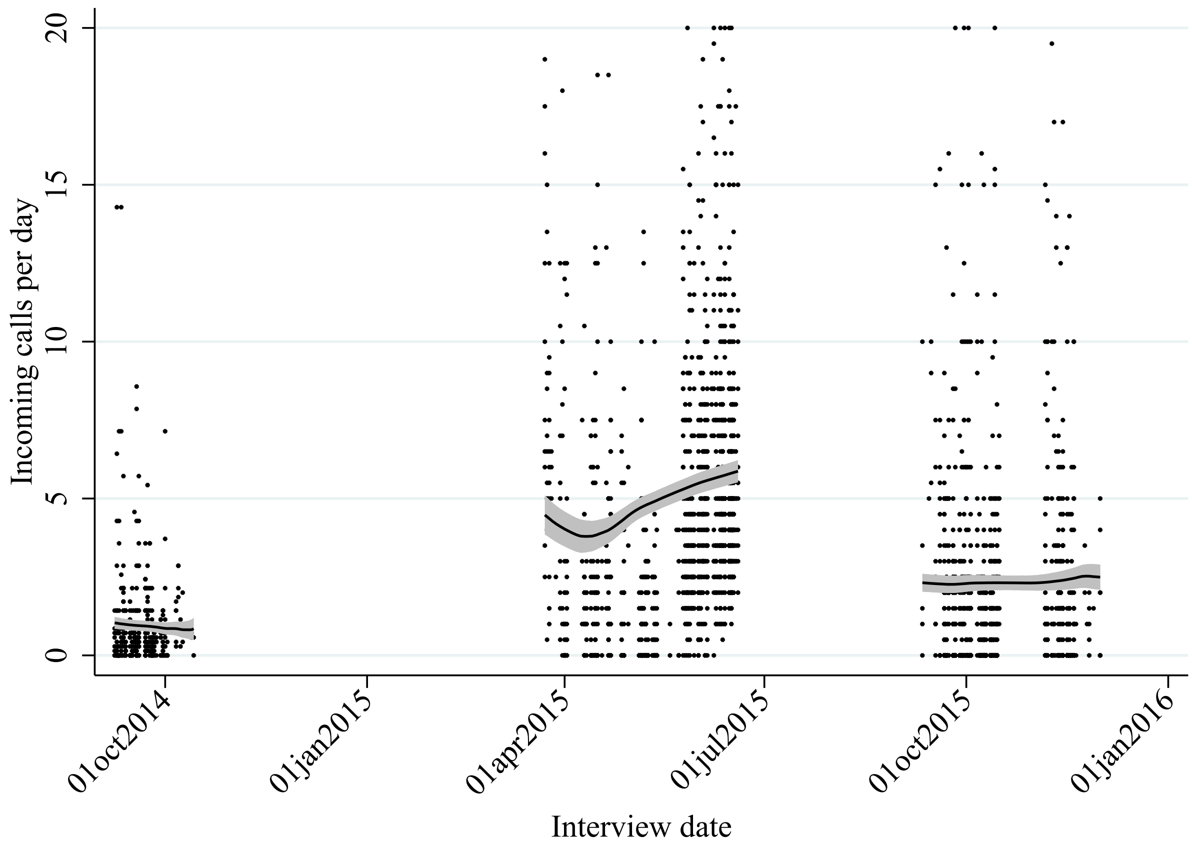

Figure 5: Incoming Business-related Calls Per Day to Study Enterprises

Notes: Authors' calculations from survey data. The horizontal axis shows the survey date, and the vertical axis shows the number of incoming calls per day. Dots indicate each response, and lines show local polynomial regressions using an Epanechnikov kernel with bandwidth set to one quarter the duration of each cluster of interviews. The cluster of responses at left is for the baseline survey, the middle cluster is for the midline and the two phone surveys, and the rightmost cluster is for the endline survey. Incoming calls greater than 20 (top 1%) are not shown, to improve readability.

Table 1: Means of Enterprise and Household Characteristics at Baseline

	Treatment (1)	Control (2)	N (3)	p-value (4)
Panel A: Enterprise characteristics				
Interviewee is male (=1)	0.83	0.84	440	0.66
Interviewee age	38.16	38.51	440	0.83
Interviewee is owner (=1)	0.91	0.87	440	0.98
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	0.40
Electricity access (=1)	0.74	0.77	440	0.60
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	0.74
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	0.42
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	0.46
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
Panel B: Household characteristics				
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 enterprises (Panel A) and post-treatment survey with households (Panel B). Columns 1 and 2 are sample means. Column 4 reports the p-values on the treatment dummy variable in regressions of each variable on a treatment dummy and randomization strata fixed effects, with standard errors clustered at the strata level. In Panel A, the control group includes both control and pure control enterprises.

Table 2: Average Treatment Effects for Listed Enterprises

Dependent variable	Coeff. (1)	s.e. (2)	p-val (3)	q-val (4)	N (5)	Control mean (6)	% change (7)
Panel A. Incoming Phone Calls (last two operating days)							
Full Sample							
Number of business calls received	2.46***	0.86	0.004	0.013	1398	9.11	27.0
Number of calls from new customers	0.28	0.30	0.351	0.518	1276	2.56	11.0
Number of missed calls	0.10	0.16	0.518	0.518	1398	1.28	8.1
Phone History Checked							
Number of business calls received	2.67***	1.03	0.010	0.030	1063	8.83	30.3
Number of calls from new customers	0.25	0.36	0.482	0.482	977	2.49	10.1
Number of missed calls	0.38**	0.18	0.035	0.052	1064	1.12	34.4
Panel B. Other Communication and Business Outcomes							
Communication (last two operating days)							
Number of outgoing business calls	0.33	0.40	0.407	0.787	725	2.72	12.2
Number of incoming business SMS messages	0.05	0.18	0.784	0.787	726	1.43	3.4
Number of outgoing business SMS messages	0.06	0.21	0.787	0.787	726	1.20	4.8
Mobile Money (last month)							
Use mobile money (=1)	0.08*	0.05	0.087	0.087	573	0.67	11.6
.... to receive payments (=1)	0.09*	0.05	0.060	0.087	573	0.57	15.9
.... to send payments (=1)	0.11**	0.06	0.045	0.087	573	0.54	20.5
Employment (last week)							
Any workers besides owner (=1)	0.12**	0.05	0.026	0.103	574	0.60	19.4
Number of workers	0.44	0.29	0.131	0.174	574	1.36	32.4
Any paid, non-family workers (=1)	0.15*	0.08	0.069	0.137	574	0.30	51.3
Number of paid workers	0.22	0.22	0.320	0.320	574	0.86	26.0
Sales and Revenue (last two operating days)							
Number of business purchases	0.37	0.57	0.521	0.690	573	0.80	46.0
Number of sales transactions	1.09	2.72	0.690	0.690	473	18.46	5.9
Sales revenues (TSH)	54037	119357	0.651	0.690	521	293860	18.4

Notes: Authors' estimates from survey data. All regressions include fixed effects for survey round and randomization strata, and regressions other than those for "new customers" and "missed calls" include a control for the baseline value of the dependent variable. Outcomes in Panel A were measured four times post-treatment (midline, phone survey 1, phone survey 2, and endline). The smaller sample size for the "new customers" outcome is due to higher rates of respondents answering "I don't know." The "phone history checked" sample in Panel A includes all respondents who allowed enumerators to confirm calls by looking through the phone history together at least once during the study. Outcomes in Panel B were measured twice post-treatment (midline and endline). In Panel B, sample size is lower for the mobile money, employment, and sales outcomes, because we only asked about these outcomes for enterprises that were operating (some enterprises temporarily close during slack periods). p-values are based on the standard errors reported in the table, which are clustered at the level of treatment assignment. q-values are p-values corrected for multiple hypothesis testing within categories, where categories are indicated by boldface headings. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 3: Spillover Effects to Control Enterprises

Dependent variable	Treat Coeff. (1)	Treat s.e. (2)	Control Coeff. (3)	Control s.e (4)	N (5)	Pure Control mean (6)
Panel A. Incoming Phone Calls (last two operating days)						
Full Sample						
Number of business calls received	1.92**	0.88	0.49	0.98	1576	9.19
Number of calls from new customers	0.32	0.33	0.11	0.36	1444	2.47
Number of missed calls	0.15	0.13	0.17	0.15	1578	1.16
Phone History Checked						
Number of business calls received	1.23	0.86	-0.70	0.99	1224	9.20
Number of calls from new customers	0.32	0.34	-0.02	0.38	1130	2.48
Number of missed calls	0.04	0.16	-0.09	0.16	1226	1.22
Panel B. Other Communication and Business Outcomes						
Communication (last two operating days)						
Number of outgoing business calls	0.77*	0.41	0.68	0.43	815	2.01
Number of incoming business SMS messages	0.53*	0.30	0.59*	0.32	818	1.00
Number of outgoing business SMS messages	0.41*	0.24	0.52*	0.28	818	0.75
Mobile Money (last month)						
Use mobile money (=1)	0.16**	0.06	0.15**	0.07	648	0.57
.... to receive payments (=1)	0.19***	0.06	0.15**	0.07	648	0.44
.... to send payments (=1)	0.14**	0.06	0.12	0.07	648	0.47
Employment (last week)						
Any workers besides owner (=1)	-0.05	0.08	-0.06	0.08	649	0.65
Number of workers	-0.60	0.40	-0.63	0.40	649	1.88
Any paid, non-family workers (=1)	0.01	0.06	-0.04	0.07	649	0.37
Number of paid workers	-0.42	0.37	-0.38	0.37	649	1.27
Sales and Revenue (last two operating days)						
Number of business purchases	-0.72	0.94	-0.92	0.89	648	1.39
Number of sales transactions	7.61***	2.68	8.54**	3.59	529	10.61
Sales revenues (TSH)	149965***	50164	128414*	75638	586	132998

Notes: Authors' estimates from survey data. All regressions include fixed effects for survey round and randomization strata, and regressions other than those for "new customers" and "missed calls" include a control for the baseline value of the dependent variable. Outcomes in Panel A were measured four times post-treatment (midline, phone survey 1, phone survey 2, and endline). The smaller sample size for the "new customers" outcome is due to higher rates of respondents answering "I don't know." The "phone history checked" sample in Panel A includes all respondents who allowed enumerators to confirm calls by looking through the phone history together at least once during the study. Outcomes in Panel B were measured twice post-treatment (midline and endline). In Panel B, sample size is lower for the mobile money, employment, and sales outcomes, because we only asked about these outcomes for enterprises that were operating (some enterprises temporarily close during slack periods). Standard errors clustered by subvillage-sector, the level of treatment assignment. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 4: Average Treatment and Spillover Effects on Enterprise WTP for a Future Listing

Dependent variable: WTP for a listing in a hypothetical re-print of the directory			
	(1)	(2)	(3)
Treat	789.4**	1023.7**	
	(386.9)	(487.3)	
Control		336.5	
		(534.9)	
Treat or Control			814.3*
			(460.4)
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 endline survey data. The excluded category is the Pure Control group. 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 the experimental directory. The dependent variable is the lower bound on the interval containing the maximum WTP to be listed in a hypothetical printing and distribution of new directories in new parts of the study area. Standard errors in parentheses, clustered by subvillage-sector (second level of treatment assignment). ***: significant at 0.01, **: significant at 0.05; *: significant at 0.1.

Table 5: Intent-to-Treat Effects for Recipient Households

Dependent variable	Coeff. (1)	s.e. (2)	p-val (3)	q-val (4)	N (5)	Control mean (6)	% change (7)
Panel A. Communication and Extra-village Linkages (last two weeks, unless noted)							
Outgoing communication							
Made calls (=1)	-0.02	0.02	0.217	0.217	738	0.96	-2.2
Sent SMS (=1)	0.11***	0.03	0.002	0.004	738	0.60	18.7
Spending on phone credit (TSH)	559	342	0.107	0.142	786	4060	13.8
Number of contacts in phone, as of interview	47.95***	11.35	0.000	0.000	683	137.51	34.9
Incoming communication							
Received calls (=1)	-0.01	0.02	0.652	0.652	738	0.97	-0.7
Received SMS (=1)	0.05	0.04	0.216	0.433	738	0.76	6.9
Mobile money							
Sent mobile money (=1)	0.12***	0.03	0.001	0.002	738	0.32	36.3
Received mobile money (=1)	0.07*	0.04	0.051	0.051	738	0.36	19.8
Ordering deliveries, recent agricultural season							
Ordered goods from outside village (=1)	0.06*	0.03	0.052	0.052	831	0.26	22.4
Used phone to order goods (=1)	0.08**	0.03	0.022	0.044	831	0.18	42.7
Panel B. Crop Production (most recent agricultural season)							
Phone use index							
Any phone use for crops (=1)	0.11***	0.04	0.007	0.007	831	0.31	35.8
Components of phone use index							
Used phone to seek general ag advice (=1)	0.04	0.03	0.103	0.103	797	0.10	44.0
Used phone for input acquisition (=1)	0.09***	0.03	0.004	0.011	776	0.18	52.6
Searched for inputs, phone (=1)	0.10***	0.03	0.001	0.004	776	0.13	74.5
Used phone, output price search, if searched (=1)	0.10**	0.04	0.017	0.020	616	0.17	60.9
Used phone to coordinate with buyer (=1)	0.05**	0.02	0.017	0.020	677	0.05	99.0
Used phone to coordinate transport (=1)	0.01**	0.01	0.017	0.020	677	0.00	415.4
Crop failures							
Maize crop failure (=1)	-0.07**	0.03	0.038	0.075	743	0.27	-26.7
Sunflower crop failure (=1)	-0.03	0.03	0.276	0.276	684	0.12	-23.3
Input usage							
Fertilizer (=1)	-0.01	0.02	0.615	0.717	776	0.04	-20.7
Borrowed or rented land (=1)	0.05**	0.02	0.022	0.076	776	0.16	33.4
Pesticides (=1)	0.01	0.01	0.402	0.595	776	0.01	74.2
Purchased seeds (=1)	-0.01	0.02	0.799	0.799	776	0.92	-0.6
Tractors or plow animals (=1)	0.03	0.03	0.304	0.595	776	0.91	3.2
Hired labor (=1)	0.12***	0.03	0.001	0.008	776	0.50	23.1
Total spending on inputs (TSH)	-31107	38782	0.425	0.595	776	436167	-7.1
Input search							
Actively searched for inputs (=1)	0.03	0.02	0.213	0.426	776	0.84	3.7
Searched for inputs, outside village (=1)	-0.01	0.03	0.807	0.807	776	0.22	-3.5
Searched for inputs, within village (=1)	0.04	0.03	0.120	0.426	776	0.79	5.7
Sourced inputs from outside village (=1)	0.02	0.05	0.645	0.807	776	0.45	5.2
Output price							
Log of crop sales price (TSH)	0.07	0.04	0.131	0.131	271	10.22	6.8
Output price search							
Any output price search (=1)	0.04	0.04	0.350	0.350	776	0.74	5.2
Searched outside village, if any search (=1)	0.13**	0.05	0.010	0.020	616	0.50	25.1

Table 5 (continued)

Dependent variable	Coeff. (1)	s.e. (2)	p-val (3)	q-val (4)	N (5)	Control mean (6)	% change (7)
Panel C. Livestock Production (most recent agricultural season)							
Livestock sales							
Sold cattle or goats (=1)	-0.04	0.03	0.175	0.270	831	0.20	-22.0
Searched for prices, cond. on selling (=1)	0.09	0.08	0.270	0.270	178	0.43	20.5
Used phone, sales price search, if searched (=1)	0.43***	0.10	0.000	0.001	79	0.22	197.4
Log of livestock sales price (TSH)	-0.09	0.07	0.211	0.270	87	10.68	-0.9
Livestock purchases							
Bought cattle or goats (=1)	0.00	0.02	0.836	0.836	831	0.10	4.6
Searched for prices, cond. on buying (=1)	0.22	0.15	0.152	0.309	95	0.35	63.3
Used phone, purchase price search, if searched (=1)	–	–	–	–	37	0.00	
Log of livestock purchase price (TSH)	0.20	0.16	0.206	0.309	95	11.42	1.8
Panel D. Non-farm Enterprises (most recent agricultural season, unless noted)							
Has non-farm enterprise as of interview (=1)	0.16***	0.04	0.000	0.000	829	0.28	58.8
Conditional on having business:							
Purchased business inputs (=1)	0.00	0.04	0.998	0.998	297	0.87	0.0
Used phone to acquire inputs (=1)	0.09	0.06	0.117	0.235	297	0.63	14.9
Business made sales (=1)	-0.03	0.04	0.535	0.714	297	0.91	-2.9

Notes: Authors' estimates from a single round of post-treatment survey data. All regressions include strata fixed effects and controls for two time invariant variables that exhibited some imbalance (gender and number of years in village). p-values are based on the reported standard errors, which are clustered at the level of treatment assignment. q-values are p-values corrected for multiple hypothesis testing within categories, where categories are indicated by boldface headings. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 6: Heterogeneity in Individual WTP for Directory, Incentivized Bids

Dependent variable: maximum WTP from BDM elicitation experiment			
	(1)	(2)	(3)
Small return village (distribution, no census)	-251.35	-340.63	-303.64
	(844.21)	(516.75)	(492.79)
Large return village (distribution, census)	-662.32*	-690.66	-766.27*
	(375.24)	(505.14)	(465.20)
Previously seen directory booklet (=1)		-291.76	-253.10
		(222.40)	(227.35)
Respondent is female (=1)		-298.48	-288.19
		(691.19)	(601.48)
Age (years)		-8.84*	-6.76**
		(5.04)	(3.07)
Wealth (asset index value)		243.92**	206.62
		(114.82)	(141.25)
Mean of dependent variable, excluded group	1042	1057	1057
Observations	684	674	674
R^2	0.02	0.05	0.08
Additional controls	No	No	Yes

Notes: Authors' estimates from incentivized willingness-to-pay experiment and associated survey data collected in 2016. Small, return villages are those where we previously distributed directories for the RCT, but which were not themselves represented in the directory. Large, return villages are those that had enterprises listed in the directory, and also received directories during distribution. (There are no locations that were represented in the directory but did not receive distribution, because we distributed copies to all listed enterprises at the end of 2015). The excluded village group consists of new villages, which were randomly selected from the list of villages in the study area that were not involved in any aspect of the RCT. "Additional controls" include dummy variables for primary occupation (farming, government, private sector, self-employed, other) and education of household head. Standard errors are based on the wild cluster bootstrap of Cameron, Gelbach and Miller (2008), where individuals in a cluster are those that attended the same experimental meeting. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

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Appendices – For Online Publication Only

A Study Area and Enterprise Census

Figure A1 in Appendix C shows the study area, with the census towns and villages marked. Dodoma, 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.

B Profitability of Enterprise Distribution

The goal of this section is to estimate the potential profitability of a printed directory service provided by an entrepreneur in an area similar to our study area. Consider first the revenue that could be raised by charging enterprises for a listing. Among the prices listed in the stated preference willingness-to-pay procedure conducted as part of the enterprise endline survey, the revenue-maximizing price is 5000 TSH for all subgroups (treatment, control, pure control). If the target is to generate a directory with 1500 enterprises listed, total expected revenue is 7,500,000 TSH.

Now consider the cost side. The pure control group is the appropriate reference group for this exercise, as these enterprises had no experience with a directory listing at the time that WTP was elicited. At a price of 5000 TSH per listing, 33 pure control enterprises out of every 100 approached are willing to pay to enroll. In large villages and cities, a single enumerator can approach 30 enterprises per average work day, for an average daily yield of 10 enrolled enterprises. At this rate, 150 person-days would be required to enroll 1500 enterprises. The entrepreneur could avoid the cost of tablets and software by working with paper notebooks to collect the basic details necessary for the census. A reasonable goal is to print and distribute 2,000 copies of the directory. The lowest cost way to print the directories would be for the entrepreneur to input the data at an internet cafe, outsource the printing and copying to a stationary shop, and then staple the documents themselves. If approximately 75 enterprises are listed on each side of each page (as in the *Kichabi*), 10 two-sided pages will suffice. A reasonable estimate is that it would cost 800,000 TSH to copy $10 \times 2000 = 20,000$ photocopies in central Tanzania in 2016. An additional 10 person-days should be sufficient to prepare the pages for printing and staple/fold afterwards.

The question of profitability rests on whether $7,500,000 - 800,000 = 6,700,000$ TSH is sufficient to cover both the full travel costs and the opportunity costs of time incurred by the entrepreneur. Travel between large villages and cities is easily accomplished by buses. It would be feasible to travel comfortably and frequently throughout the entire study area at a cost of no more than $250,000$ TSH total. Time spent traveling would add work days.

If we set the number of full days spent traveling to 20, reflecting our experience using buses during fieldwork, the total person-day commitment is 180 days. This amounts to an average daily return of $6,450,000/180 = 35,555$ TSH to cover the entrepreneur's earnings and living expenses.

Average daily earnings in the range of 36,000 TSH is similar to the average daily wage earned by an experienced enumerator, with a bachelor's degree, working for a research firm in Tanzania. During periods away from home, that enumerator would also earn a per diem. So the guaranteed salary and higher wage of an enumerator position likely dominates running a directory start-up. Yet, for every applicant successfully hired as an enumerator, dozens of other candidates are turned away. If the reservation expected wage is less than or equal to 35,555 for any one of those individuals, or for someone else in the community with the capacity to put together a directory, then the entire enterprise of directory provision would be profitable.

There are many caveats to this back-of-the-envelope analysis. A creative entrepreneur could likely find ways to cut costs (e.g., focus on cities and towns first) and increase revenues (allow firms to advertise or bid to be listed first), either of which would increase the expected profits. A small amount of additional experimentation may reveal that the revenue-maximizing price is lower than 5000 TSH (in our WTP survey, the options jumped from 5000 TSH to 2000 TSH). On the other hand, an independent entrepreneur may have difficulty convincing enterprise owners to share their information or pay for the service up front. It seemed clear during our study that our relationship with the Institute of Rural Development Planning was helpful in establishing trust.

Based on this analysis, we think it is highly likely that the expected earnings of a directory start-up are above the reservation wage of many capable Tanzanian entrepreneurs, as long as enterprise demand for listings is comparable to what we measured. If the latter condition is not met, then directory production would be a profitable (or at least self-financing) exercise for a trusted public institution.

C Appendix Figures

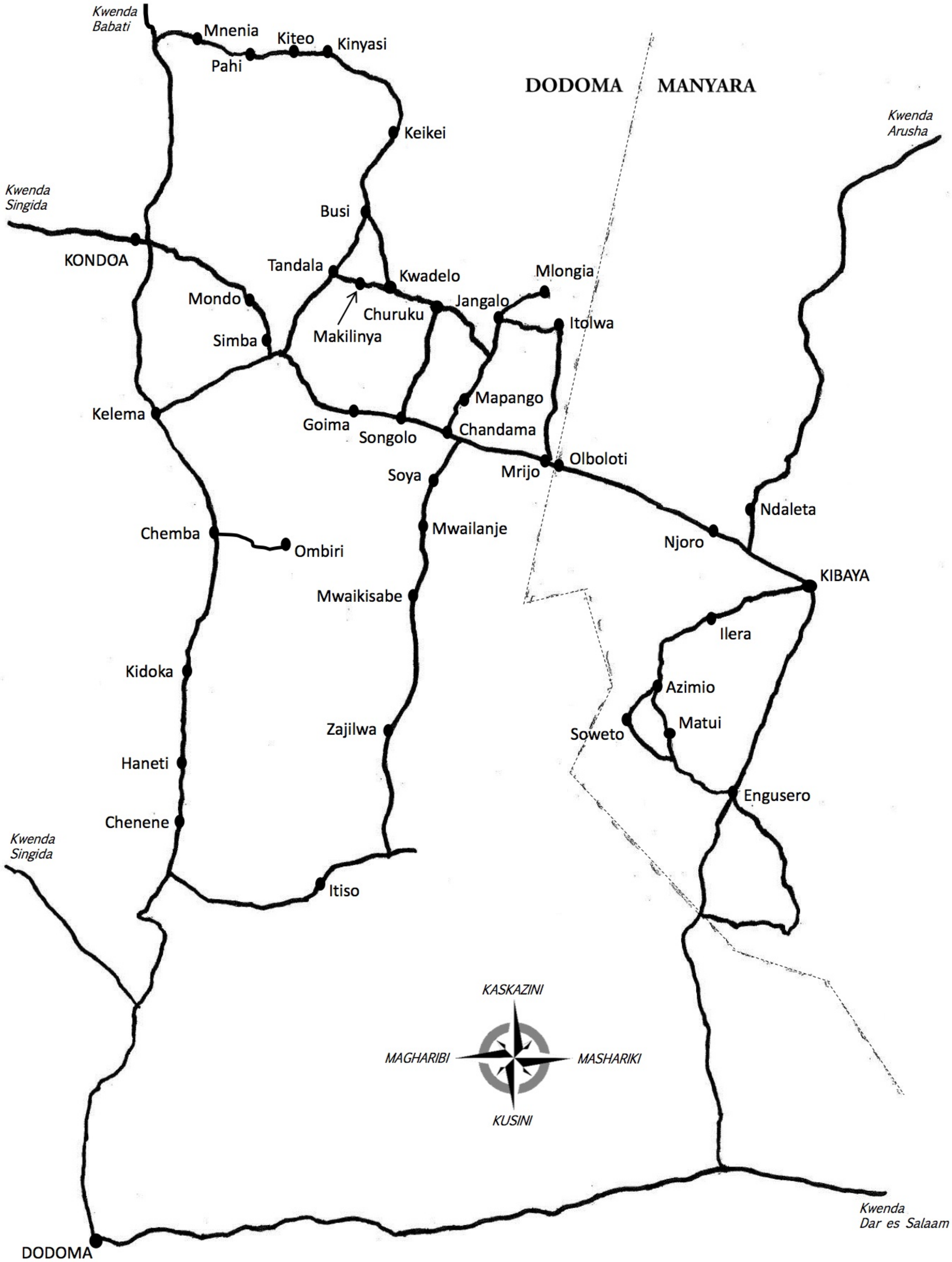

Figure A1: Map of Study Area

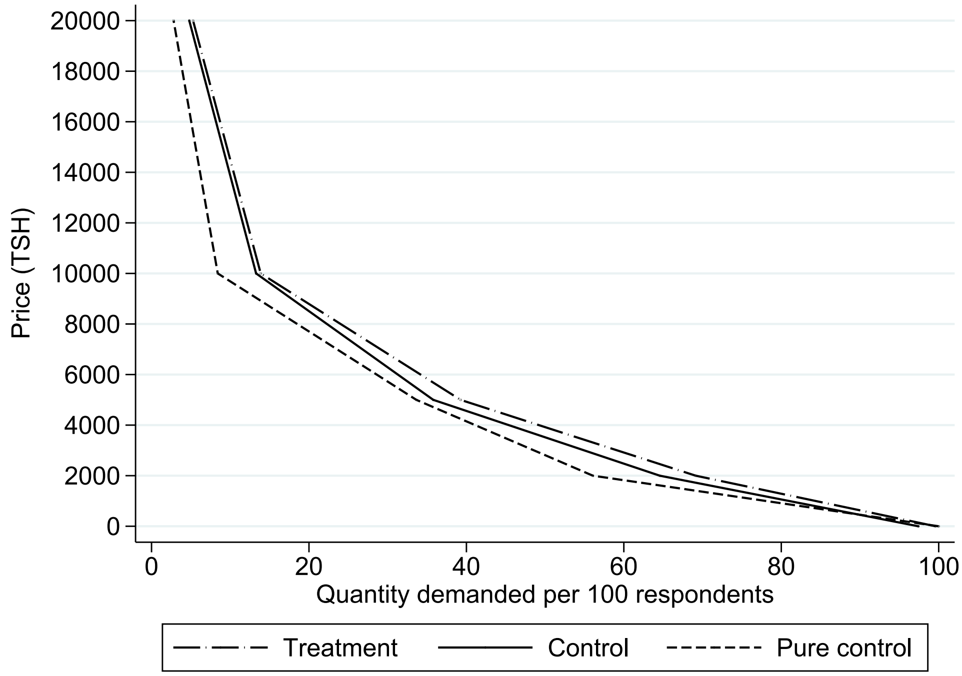

Figure A2: Enterprise Demand for Future Listing, by Study Arm

Figure A3: Individual Demand for a Copy of the Directory, by Village Type

D Appendix Tables

Table A1: Recipients: Heterogeneous Treatment Effects, by Wealth

Dependent variable	Below median wealth: Marginal effect (1)	Below median wealth: Control mean (2)	Above median wealth: Marginal effect (3)	Above median wealth: Control mean (4)	N (5)	Difference (3)-(1) (6)	Difference (p-val) (7)
Panel A. Communication and Extra-village Linkages (last two weeks, unless noted)							
Made calls (=1)	-0.01	0.94	-0.03	0.97	738	-0.02	0.52
Sent SMS (=1)	0.10*	0.64	0.13***	0.56	738	0.03	0.71
Spending on phone credit (TSH)	503.08	3112.56	334.04	5120.79	786	-169.05	0.84
Number of contacts in phone, as of interview	45.11***	126.81	48.71***	147.24	683	3.60	0.84
Received calls (=1)	-0.02	0.97	0.00	0.98	738	0.02	0.30
Received SMS (=1)	0.10*	0.73	0.01	0.78	738	-0.09	0.20
Sent mobile money (=1)	0.11**	0.27	0.11**	0.38	738	0.01	0.94
Received mobile money (=1)	0.12**	0.29	0.01	0.42	738	-0.10	0.20
Ordered goods from outside village (=1)	0.03	0.26	0.08**	0.27	831	0.05	0.41
Used phone to order goods (=1)	0.04	0.18	0.11***	0.17	831	0.08	0.20
Panel B. Crop Production (most recent agricultural season)							
Any phone use for crops (=1)	0.12**	0.27	0.09*	0.35	831	-0.03	0.66
Used phone to seek general ag advice (=1)	0.06*	0.07	0.02	0.14	797	-0.04	0.33
Used phone for input acquisition (=1)	0.10**	0.15	0.08*	0.22	776	-0.02	0.73
Searched for inputs, phone (=1)	0.13***	0.10	0.07*	0.17	776	-0.06	0.31
Used phone, output price search, if searched (=1)	0.10**	0.17	0.10*	0.17	616	-0.00	0.99
Used phone to coordinate with buyer (=1)	0.04	0.05	0.05*	0.05	677	0.02	0.62
Used phone to coordinate transport (=1)	0.01	0.00	0.01	0.01	677	-0.00	0.97
Maize crop failure (=1)	-0.04	0.26	-0.10**	0.28	743	-0.06	0.34
Sunflower crop failure (=1)	-0.02	0.12	-0.04	0.12	684	-0.02	0.73
Fertilizer (=1)	-0.00	0.02	-0.02	0.06	776	-0.02	0.56
Borrowed or rented land (=1)	0.07**	0.20	0.04	0.12	776	-0.03	0.59
Pesticides (=1)	0.01	0.00	0.00	0.02	776	-0.01	0.42
Purchased seeds (=1)	0.01	0.90	-0.02	0.94	776	-0.03	0.50
Tractors or plow animals (=1)	0.00	0.90	0.05*	0.93	776	0.05	0.30
Hired labor (=1)	0.13**	0.38	0.07	0.63	776	-0.06	0.41
Total spending on inputs (TSH)	-32795	288892	-61590	603188	776	-28794	0.75
Actively searched for inputs (=1)	0.00	0.84	0.05	0.84	776	0.05	0.43
Searched for inputs, outside village (=1)	0.04	0.21	-0.05	0.23	776	-0.09	0.20
Searched for inputs, within village (=1)	0.01	0.78	0.07*	0.79	776	0.06	0.37
Sourced inputs from outside village (=1)	0.02	0.40	0.02	0.51	776	0.01	0.95
Log of crop sales price (TSH)	-0.01	10.08	0.12**	10.37	271	0.14	0.11
Any output price search (=1)	0.01	0.76	0.07	0.72	776	0.06	0.29
Searched outside village, if any search (=1)	0.13*	0.50	0.12*	0.50	616	-0.01	0.92
Panel C. Livestock Production (most recent agricultural season)							
Sold cattle or goats (=1)	-0.07*	0.10	-0.05	0.31	831	0.02	0.76
Searched for prices, cond. on selling (=1)	0.04	0.50	0.09	0.40	178	0.04	0.85
Used phone, sales price search, if searched (=1)	0.38	0.15	0.45**	0.25	79	0.07	0.84
Log of livestock sales price (TSH)	-0.24	10.78	-0.07	10.64	87	0.17	0.55
Bought cattle or goats (=1)	-0.02	0.05	0.01	0.15	831	0.03	0.51
Searched for prices, cond. on buying (=1)	0.06	0.45	0.27*	0.31	95	0.21	0.64
Used phone, purchase price search, if searched (=1)	–	–	–	–	37	–	
Log of livestock purchase price (TSH)	0.71	10.98	0.05	11.59	95	-0.66	0.15
Panel D. Non-farm Enterprises (most recent agricultural season, unless noted)							
Has non-farm enterprise as of interview (=1)	0.13**	0.28	0.19***	0.28	829	0.06	0.44
Conditional on having business:							
Purchased business inputs (=1)	-0.02	0.87	0.01	0.87	297	0.03	0.75
Used phone to acquire inputs (=1)	0.04	0.67	0.16*	0.58	297	0.12	0.37
Business made sales (=1)	-0.03	0.90	-0.03	0.92	297	0.00	0.98

Notes: Authors' estimates from survey data. All regressions include strata fixed effects and controls for two time invariant variables that exhibited some imbalance (gender and number of years in village). p-values are based on the reported standard errors, which are clustered at the level of treatment assignment (the village). Column 7 is the p-value for a t-test of the hypothesis that the coefficient on the interaction between treatment and above median wealth is statistically different from zero. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table A2: Recipients: Heterogeneous Treatment Effects, by Gender

Dependent variable	Women: Marginal effect (1)	Women: Control mean (2)	Men: Marginal effect (3)	Men: Control mean (4)	N (5)	Difference (3)-(1) (6)	Difference (p-val) (7)
Panel A. Communication and Extra-village Linkages (last two weeks, unless noted)							
Made calls (=1)	0.05	0.91	-0.04**	0.97	738	-0.09	0.11
Sent SMS (=1)	0.10	0.61	0.12***	0.60	738	0.02	0.83
Spending on phone credit (TSH)	752.09	2609.68	507.11	4535.92	786	-244.98	0.79
Number of contacts in phone, as of interview	48.60**	94.81	47.79***	148.49	683	-0.82	0.97
Received calls (=1)	0.02	0.97	-0.01	0.97	738	-0.03	0.32
Received SMS (=1)	-0.03	0.78	0.07	0.75	738	0.10	0.15
Sent mobile money (=1)	0.03	0.28	0.14***	0.33	738	0.10	0.27
Received mobile money (=1)	-0.06	0.36	0.10**	0.35	738	0.16*	0.07
Ordered goods from outside village (=1)	0.11	0.25	0.04	0.27	831	-0.07	0.38
Used phone to order goods (=1)	0.12*	0.17	0.06*	0.18	831	-0.06	0.41
Panel B. Crop Production (most recent agricultural season)							
Any phone use for crops (=1)	0.09	0.21	0.12**	0.34	831	0.03	0.69
Used phone to seek general ag advice (=1)	0.10**	0.04	0.03	0.12	797	-0.07	0.18
Used phone for input acquisition (=1)	0.07	0.13	0.10***	0.20	776	0.03	0.69
Searched for inputs, phone (=1)	0.10*	0.08	0.10***	0.15	776	-0.00	0.96
Used phone, output price search, if searched (=1)	-0.06	0.13	0.15***	0.18	616	0.20***	0.01
Used phone to coordinate with buyer (=1)	0.04	0.03	0.05**	0.05	677	0.00	0.96
Used phone to coordinate transport (=1)	-0.00	0.00	0.02	0.00	677	0.02	0.14
Maize crop failure (=1)	-0.10	0.32	-0.06	0.25	743	0.04	0.61
Sunflower crop failure (=1)	-0.06	0.19	-0.02	0.10	684	0.04	0.66
Fertilizer (=1)	0.03	0.01	-0.02	0.05	776	-0.05	0.25
Borrowed or rented land (=1)	0.11	0.15	0.04	0.17	776	-0.07	0.36
Pesticides (=1)	0.01	0.01	0.01	0.01	776	-0.00	0.97
Purchased seeds (=1)	-0.01	0.90	-0.00	0.93	776	0.01	0.92
Tractors or plow animals (=1)	0.11*	0.82	0.01	0.94	776	-0.11*	0.08
Hired labor (=1)	0.23**	0.44	0.08**	0.52	776	-0.15	0.17
Total spending on inputs (TSH)	17719	294207	-44748	483818	776	-62468	0.55
Actively searched for inputs (=1)	0.05	0.81	0.03	0.85	776	-0.02	0.82
Searched for inputs, outside village (=1)	-0.03	0.19	-0.00	0.23	776	0.03	0.64
Searched for inputs, within village (=1)	0.06	0.77	0.04	0.79	776	-0.03	0.77
Sourced inputs from outside village (=1)	-0.05	0.40	0.04	0.47	776	0.10	0.37
Log of crop sales price (TSH)	-0.01	9.92	0.09	10.30	271	0.09	0.31
Any output price search (=1)	0.06	0.74	0.03	0.74	776	-0.02	0.83
Searched outside village, if any search (=1)	0.01	0.49	0.16***	0.50	616	0.15	0.16
Panel C. Livestock Production (most recent agricultural season)							
Sold cattle or goats (=1)	-0.10*	0.04	-0.03	0.12	831	0.07	0.25
Searched for prices, cond. on selling (=1)	0.08	0.60	0.09	0.40	178	0.01	0.97
Used phone, sales price search, if searched (=1)	1.01**	0.11	0.36**	0.25	79	-0.65	0.21
Log of livestock sales price (TSH)	-0.20	10.40	-0.09	10.77	87	0.10	0.65
Bought cattle or goats (=1)	0.03	0.08	-0.00	0.10	831	-0.03	0.65
Searched for prices, cond. on buying (=1)	0.74**	0.22	0.10	0.39	95	-0.64	0.11
Used phone, purchase price search, if searched (=1)	–	–	–	–	37	–	
Log of livestock purchase price (TSH)	-0.35	11.47	0.33	11.41	95	0.68	0.23
Panel D. Non-farm Enterprises (most recent agricultural season, unless noted)							
Has non-farm enterprise as of interview (=1)	0.23***	0.36	0.14***	0.25	829	-0.09	0.31
Conditional on having business:							
Purchased business inputs (=1)	-0.07	0.92	0.03	0.84	297	0.11	0.23
Used phone to acquire inputs (=1)	0.10	0.56	0.09	0.66	297	-0.01	0.91
Business made sales (=1)	-0.01	0.95	-0.03	0.89	297	-0.02	0.76

Notes: Authors' estimates from survey data. All regressions include strata fixed effects and controls for two time invariant variables that exhibited some imbalance (gender and number of years in village). p-values are based on the reported standard errors, which are clustered at the level of treatment assignment (the village). Column 7 is the p-value for a t-test of the hypothesis that the coefficient on the interaction between treatment and gender is statistically different from zero. ***: significant at 1%; **: significant at 5%; *: significant at 10%.