Yellow Pages:
Information, Connections and Firm Performance*

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Abstract

We study the impact of a low-cost intervention to reduce information frictions in rural markets by randomly assigning small and medium enterprises in Tanzania to be listed in a telephone directory. The listed firms expand their communication networks, increase sales, and make greater use of mobile money, with positive spillovers to firms in the same village. Estimated effects are larger and more statistically precise for firms that are more productive at baseline, consistent with consumers seeking out better firms when search costs fall. We find no evidence of negative between-village spillovers on unlisted firms.

Keywords: mobile phones; directories; small and medium enterprises; information frictions; Tanzania.

JEL Codes: O13, Q16, I15

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1. Introduction

The diffusion of mobile phones through low-income countries has been 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 facilitated arbitrage in agricultural markets (Jensen, 2007; Aker, 2010). More recent work evaluates mobile-based services that provide curated information to phone users across a variety of sectors (Fafchamps and Minten, 2012; Morten et al., 2012; Jamison et al., 2013; Hall et al., 2014; Aker et al., 2016; Buntaine et al., 2018; Angrist et al., 2020; Grossman et al., 2020; Bergquist et al., 2021; Hasanain et al., 2023), and examines the implications of the changing ICT landscape for economic and political organization (Hjort and Poulsen, 2019; Manacorda and Tesei, 2020; Brunnermeier et al., 2023).

Small and medium-enterprises (SMEs) should, in principle, benefit from this transformation. Prior work has shown that information frictions are substantial for SMEs in both input and output markets (Allen, 2014; Jensen and Miller, 2018; Bergquist et al., 2021; Rudder and Dillon, 2023). Mobile phones can reduce search costs and integrate markets, with potentially large benefits to firms that navigate the transition. Yet most firms in sub-Saharan Africa remain small and unproductive, and serve primarily local markets. One potential factor constraining the impact of mobile phones is the lack of a complementary information service—such as a phonebook or Google search engine—to help phone users search for new contacts. Mobile phones reduce communication costs within *existing* social networks, but they do not necessarily help phone users *expand* their networks.

In this paper we study the importance of this particular information friction—the friction related to finding and contacting new businesses—for firms in Tanzania. Through a randomized controlled trial (RCT), we developed and distributed a telephone directory ("Yellow Pages") that listed information about SMEs that are relevant to agricultural households (retail, transport, skilled trades, production-related services, and others). The experiment had two main components. First, we randomized which SMEs were listed in the directory, in order to better understand how publicizing contact information could affect customer contact and business outcomes. Second, we randomized the distribution of the directory to households, to understand how their choices would be impacted by better access to firm information, and how that in turn would impact firm outcomes.[1] We evaluate these experiments using multiple rounds of surveys conducted primarily over one year. Our aims are to test whether the constraint on contact information is binding, and, if so, to examine how and why firm outcomes change when that constraint is relaxed.

Our analysis leads to three main sets of results. First, relative to firms in villages with no treated firms, the firms listed in the directory expand their communication networks, with increases in both calls and text messages with customers. Estimated effects are large in magnitude, ranging from 21-68% of the mean values for untreated firms. These communication-related findings are robust to an exercise in which enumerators manually verified the reported number of business-related calls by checking the respondent's phone history. We find no advantage to being listed first in a directory subsection, suggesting that directory users took time to read the listing descriptions before choosing whom to contact.

Second, directory firms see increases in the number of sales (by 47%), sales revenue (by 104%), and use of mobile money (by 31%), again relative to firms in villages with no treated firms. The effect on sales revenue is surprisingly large. Although this estimate remains similar across a series of robustness checks, we take a cautious approach and focus our interpretation on the lower bound of the confidence interval (which is 20-221%). We find no treatment effects on employment or stocking purchases, suggesting that firms were not pushed to expand capacity during the study period.

Third, we find evidence of *positive* spillovers to unlisted firms that operate in the same villages as listed firms. Relative to a Pure Control group (firms in villages from which no firms were listed), unlisted firms in villages with treated firms increased SMS traffic (by

[1] See Appendix A1 for a discussion of the ethical considerations of this experimental design.

80%), mobile money usage (by 26%), and the number of sales (by 40%). These positive spillovers are consistent with the directory inducing more foot traffic to treated villages, thereby increasing demand to unlisted firms.

After establishing these main results, we turn to mechanisms. There are many ways that relaxing an information constraint for customers could affect firms. We provide evidence on three possible dimensions of adjustment. First, we use the independently randomized distribution of the directories to show that the benefits to firms are similar regardless of whether directories were distributed nearby. This suggests that some directory recipients were induced to search and trade outside of their villages. We also find in household surveys that rates of extra-village search and purchasing are greater for the households randomly selected to receive directories.

If the effects we document are mediated in part by search, that raises the question of whether consumers were able to find better firms. We address this question by testing whether effects are larger for more productive firms. Using baseline data on employment and sales to construct a measure of labour productivity, we show that both the direct and spillover effects of the directory are larger in magnitude and more precise for firms with above median productivity (for most outcomes, we cannot reject null effects on the low productivity firms). This aligns with a key result in Jensen and Miller (2018), who show that when information frictions fall in India, only the higher quality firms benefit.

Finally, we consider whether the widespread drop in search costs led to net increases in economic activity, or simply reallocated business away from unlisted villages and to the treated firms. In two suggestive analyses, we find no evidence of negative between-village spillovers. Three-year survival rates are identical for Treatment, Control, and Pure Control firms. Furthermore, outcomes for Pure Control firms do not vary meaningfully with the number or the share of treated firms in their subdistrict and sector.

Our research makes three contributions. First, we speak to the role of mobile phones in reducing search costs in rural markets. Prior work has found that phones can make rural markets more efficient (Jensen, 2007; Aker, 2010), and phone-based interventions can in some cases increase input usage and productivity (Fafchamps and Minten, 2012; Nakasone et al., 2013; Aker et al., 2016; Fabregas et al., 2024). Our study differs from most phone-intervention studies in that we seek to facilitate search by providing information that complements phones, rather than use phones as a vehicle for sharing third party information (e.g., prices). In this respect our work connects to a broader literature on the economic consequences of reductions in search costs (Stigler, 1961; Sutton, 1991; Goldfarb and Tucker, 2019; Abebe et al., 2020; Bergquist et al., 2021; Bandiera et al., 2023; Bai, 2024).

We also contribute to the literature on rural market structure and relational contracts. A widespread concern in this literature is the uncompetitiveness of rural markets, due to entry barriers and information asymmetries (Atkin and Donaldson, 2015; Dillon and Dambro, 2017; Bergquist and Dinerstein, 2020). Yet, theoretical and empirical papers on relational contracts highlight the importance of trading with known agents when institutions for contract enforcement are weak, calling into question whether rural households would utilize a directory of mostly unknown firms (Greif, 1993; Banerjee and Duflo, 2000; Baker et al., 2002; Brown et al., 2004; Fafchamps, 2004; Macchiavello and Morjaria, 2015; Casaburi and Reed, 2019; Startz, 2018; Ghani and Reed, 2022; Rudder and Dillon, 2023). We build on a long line of work showing the importance of networks in household choices and outcomes, and show that these networks can expand if information barriers are addressed (Foster and Rosenzweig, 1995; Munshi, 2004; Bandiera and Rasul, 2006; Conley and Udry, 2010; Maertens, 2017; Cai et al., 2015; Kondylis et al., 2014; Magruder, 2018; Beaman and Dillon, 2018; BenYishay and Mobarak, 2018; Beaman et al., 2021; Comola and Prina, 2021).

Finally, our paper adds to the growing evidence base documenting reasons for the poor performance of SMEs in low-income countries. Prior work in this area explores the barriers created by entry costs (Ayyagari et al., 2007) and access to finance (Beck and Demirguc-Kunt, 2006; De Mel et al., 2008, 2012), and highlights the benefits to SMEs from switching technologies (Atkin et al., 2017) and improving management practices (Bloom et al., 2013;

Beaman et al., 2014). A related set of papers emphasizes the importance of information costs to SMEs (Allen, 2014; Aggarwal et al., 2018; Startz, 2018; Jensen and Miller, 2018; Rudder and Dillon, 2023). Our results highlight how reducing one particular information friction—the cost of finding new contacts—can have a meaningful impact on SME performance.

2. Research Setting and Experimental Design

2.1 The Kichabi Directory

In order to understand how information constraints affect the economic outcomes of SMEs, we created and distributed a mobile phone directory in rural Tanzania, called *Kichabi*.[2] The experiment took place in the Dodoma and Manyara regions, covering approximately 5,000 square miles (see Appendix Figure A1). The area is primarily agricultural, with one growing season from January to May. A broad set of formal and informal enterprises provide services to farmers, including input supply shops, pharmacies, transporters, mechanics, and others. These SMEs sell their goods and services directly to farmers in villages, or at weekly markets.

We partnered with the Institute of Rural Development and Planning (IRDP) to conduct a census of all firms in specific sectors across 49 villages.[3] The census collected basic data (name, location, sector, phone number(s), areas of specialization) from firms in eight sectors relevant for agriculture: wholesale trade, retail trade, transport, rentals, agricultural processing, agricultural services (e.g., mechanics or hired labour), non-agricultural services (e.g., pharmacies) and financial services. Firms were not incentivized to participate in the census. Out of the 2,100 firms visited, 71% (1,506) agreed to be interviewed and provided their information. The most common reason for refusal was potential exposure to tax authorities. After removing firms with missing information, we have a sample of 1,495 enterprises.

[2] *Kichabi* is short for *kitabu cha biashara*, or "business book" in Swahili.

[3] The villages were chosen based upon their size (e.g., having 4,000 or more inhabitants) or their function as the sub-district capital. Each village is comprised of a number of sub-villages, which represent distinct administrative areas. Seven of the villages are separate administrative areas of the two small cities in the study area, Babati and Dodoma, which function like separate villages. For details, see Appendix A2.

Using the information collected through the census, we created the *Kichabi* phone directory. The directory was printed as a folded A4 booklet (Figure 1A). Within the directory, firms were listed alphabetically by village, sub-village, sector, and name (Figure 1B). A description column in the booklet, listing areas of specialization collected in the census, allowed for differentiation between otherwise similar firms.

2.2 Experimental Design

To test the importance of frictions related to contact information, we conducted two separate but related experiments. On the supply side, we randomized which firms were *listed* in the directory; on the demand side, we randomized which households *received* a printed copy of the directory. The experiments were randomized independently. This paper primarily focuses on the firm side of the experiment, although we take advantage of the randomized distribution of directories to study mechanisms behind the observed effects.

For the firm experiment, we first stratified by district, and randomly assigned villages to either be eligible for treatment or not.[4] In the treated villages, we further stratified by village and sector, and randomly assigned subvillage-sector groups to either Treatment or Control (Appendix Figure A2). The firms assigned to Treatment were listed in the directory, whereas Control firms were not.[5] We thus have treated firms in treated villages (Treated), Control firms in treated villages (Control) and control firms in control villages (Pure Control).

For the distribution experiment, we identified a sample of 99 villages in the study area. Stratifying by ward (subdistrict) and village size, we randomly assigned villages to the distribution treatment (receiving directory booklets) or distribution control. In both sets of villages we held meetings to introduce the directory and explain its potential uses. In the distribution treatment villages, we gave directory booklets to 70 meeting attendees, prior to

[4] Villages in Tanzania are divided into subvillages, each of which has a chairperson that works with the village leaders. There are typically 2-4 subvillages in a village, with a 5-20 minute walk between subvillage centers. Many subvillages have their own commercial centers.

[5] All firms were listed in new directories that we issued at the end of the experiment, to ensure no long-run disadvantage to unlisted firms. See Appendix A1.

the start of the cultivation season. In control villages, we distributed booklets eight months later, after the end of the season (see Appendix A4 for more details). A total of 29 villages were treated in both experiments.

2.3 Data

Our analysis is based primarily on surveys that we conducted with a random subset of study firms before and after the intervention. Among the 1,495 firms in the census, we randomly selected 440 for five rounds of data collection: a baseline in-person survey (beginning in September 2014); a midline in-person survey (beginning in March 2015); two phone surveys that were short and focused on business-related communications (May-July 2015); and an endline in-person survey (beginning in September 2015). In 2017 we conducted a brief follow-up survey with all firms, to measure any long-term differences in survival across treatment groups (see Appendix Figure A3 for a study timeline). The baseline occurred prior to treatment; all other surveys took place after.

The baseline, midline, and endline surveys covered a range of topics including communication, customer contact, mobile money, sales transactions, revenues, and employment. Attrition for in-person surveys was low, ranging from 2% to 15%, depending upon the survey round. There was no differential attrition between the Treatment, Control, and Pure control groups. We use these data to estimate the impact of being listed in the directory on firms' communication and business outcomes.

Appendix Table A1 provides summary statistics from the baseline enterprise survey. Firm characteristics are generally balanced between the Treat, Control and Pure Control groups. Out of 27 variables tested, there are three that are statistically different across groups at 90% confidence (only one at 95%), which is consistent with random chance. In Appendix Table A2 we show that our results are unchanged if we control for the variables that exhibit slight baseline imbalance.

2.4 Empirical Strategy

To estimate the impact of being listed in the directory on firm-level outcomes, we use analysis of covariance (ANCOVA) regressions of the following form:

$$Y_{ivdt} = \beta_0 + \beta_1 Treat_{ivd} + \beta_2 Control_{ivd} + \beta_3 Y_{ivd0} + \lambda_t + \psi_d + \epsilon_{ivdt} \tag{1}$$

where Y_{ivdt} is the value of the outcome for firm i, in village v, district d, and survey round t; $Treat_{ivd}$ is a binary variable equal to 1 if the firm is listed in the directory; $Control_{ivd}$ is a binary variable equal to 1 if the firm is unlisted, but located in a village with some listed firms; λ_t are survey round fixed effects; ψ_d are fixed effects for districts, which are the randomization strata; and ϵ_{ivdt} is the error term. The excluded category is the Pure Control group, consisting of firms located in villages where no firms were listed. The coefficient $\hat{\beta}_1$ estimates the impact of of the directory listing on treated firms, which we refer to as the intent-to-treat effect (ITT) following Baird et al. (2018). The coefficient $\hat{\beta}_2$ estimates the within-village spillover effect on unlisted firms. In extensions to the main analysis, we estimate models in which we include controls for variables unbalanced at baseline, restrict attention to the 80% of respondents that allowed enumerators to verify phone activity, estimate heterogeneous effects using interactions, or estimate the total effect of the treatment using the variable $AnyTreat_{ivd} = \max\{Treat_{ivd}, Control_{ivd}\}$. Because there are 49 villages (treatment clusters) across the three groups, and 7 Pure Control villages, inference in all regressions is based on p-values calculated with the wild cluster bootstrap (WCB) of (Cameron et al., 2008).

We focus our analysis on four communication-related outcomes (total business calls, incoming business calls, total business text messages, and incoming business text messages), and six economic outcomes (using mobile money, employing any workers, number of workers, number of sales, sales revenue, and number of stocking purchases). Full definitions of these variables are provided in Appendix A3.

3. Results

3.1 Direct and Spillover Effects of Directory Listings

Direct Effects on Communication. Columns 1-3 of Table 1 present our estimates of the direct effects of directory listings on firm-level communication outcomes. Compared to Pure Control firms, being listed in the directory significantly increases a firm's total number of business calls and text messages (Panel A, column 1). The impacts are large in magnitude, with effects ranging from 21-68% of the Pure Control mean.

Although communication outcomes were self-reported, enumerators verified the number of calls by asking the respondent if they could look through the phone history together. Approximately 4 out of 5 respondents allowed the enumerator to check the phone. In Panel B of Table 1 we show that results are larger in magnitude and more precisely estimated when we focus on firms that allowed verification. Taken together, these results suggest that being listed in the directory made it easier for customers to communicate with firms.

Direct Effects on Business Outcomes. The increase in business communication led to changes in other business-related outcomes (Table 2). Compared to the Pure Control, firms listed in the directory made 4.9 more sales in the previous two operating days (46% increase), and saw an average increase of 137,936 TSH in sales revenue (104% increase; Panel A, column 1).[6] The large average effect on sales revenue is due in part to a small number of outliers that persist after winsorizing. In quantile regressions, we find statistically significant effects from the 30th-70th percentiles, ranging in magnitude from 12,348-65,500 TSH (Appendix Figure A4, Panel A). The effect at the median is 28,444 TSH (p-value = 0.06), representing a 21.4% increase in revenue over the Pure Control mean.

We find no effects of the directory listing on the extensive or intensive margin of employment, and no effect on stocking purchases. These null results suggest that the increases

[6] The large effect on sales revenue is not due to differences in survey timing. Firms in all three groups were interviewed in the same survey effort, and results are unchanged if we include the survey date as an additional control (Appendix Table A3).

in sales did not push firms to increase capacity, at least over the study period. The sales-related impacts were also associated with an increase in mobile money usage: firms listed in the directory were 16 percentage points (28%) more likely to use mobile money than Pure Control firms. All of these findings are similar for the verified firms (Table 2, Panel B).

Within-Village Spillovers on Communication. Our experimental design allows us to test for spillover effects to control firms in villages where some firms were listed. The direction of this spillover is ambiguous: if listed firms take business from unlisted firms, unlisted firms would see fewer customers and sales. On the other hand, if the directory crowded in business to treated areas or prompted new investment by reducing search costs, unlisted firms might benefit from the additional foot traffic.

The communication-related results in Table 1 (Column 4, Panel A) are more consistent with the latter story. None of the coefficients are negative and statistically significant; most are positive and large in magnitude. Estimated impacts on text messages are statistically significant and similar in magnitude to the effects on listed firms.[7] These spillover results are largely unchanged, though less precise, when we restrict attention to the 80% of firms that allowed verification (Panel B).

Within-Village Spillovers on Business Outcomes. We also find positive spillover effects on business outcomes for Control firms. These spillover effects are similar in magnitude and significance to the direct effects on listed firms. Relative to Pure Control, Control firms are more likely to use mobile money, make more sales, and have higher sales revenues (Table 2, Panel A).[8] The sales results are slightly smaller in magnitude and less precise when we restrict attention to the verified firms (Panel B). As with the average direct effect on revenue, the large magnitude of the average spillover effect on revenue is due in part to outliers

[7]Finding a communication spillover on text messages but not calls is not surprising, based on anecdotes from the study site. New customers did not have access to the phone numbers of Control firms until they met in person, while doing business in treated villages. Ongoing communication with someone met recently is more likely to occur via text.

[8]In general, we do not observe statistically significant differences between Treated and Control firms.

that survive winsorizing. Quantile regressions show that the statistically significant spillover effects on sales revenue are primarily in the middle of the distribution, as they were for the listed firms (Appendix Figure A4, panel B). The spillover effect at the median is 26,622 TSH (p-value = 0.13); spillover effects at the 40th and 60th percentiles are 13,010 TSH (p-value = 0.097) and 38,361 TSH (p-value = 0.054).

Total Effect on Communication and Business Outcomes. Following (Baird et al., 2018), we also estimate the total effect of the treatment on communication and business-related outcomes. This effect is identified by replacing the Treatment and Control dummies in equation (1) with an "Any Treat" dummy variable, which is equal to 1 for members of both the Treatment and the Control groups. The estimated total effects are presented in Appendix Table A4. The total effects closely match those presented in Tables 1 and 2, with positive and significant effects on phone calls, texts, mobile money usage, number of sales, and sales revenue.

3.2 Mechanisms

We have shown that the telephone directory led to substantial increases in customer contact, mobile money usage, and sales for listed firms, and to positive spillovers on unlisted firms in the same villages. Next we explore the mechanisms behind these effects. Specifically, we analyze household use of the directory, study heterogeneity in impacts, and try to understand whether the positive effects we document came at the expense of firms in untreated villages.

Household Use of the Directory. When we distributed the directory, we selected distribution villages at random from across the study area (see Section 2.2). In follow-up surveys conducted with 831 households roughly 7-8 months after directories were distributed, we find that just under 30% of the directory recipients reported calling at least one listed firm. A large majority of recipients reported sharing or lending the directory to others. In our analysis of household data we find effects that align with those on the firm side: households that received the directory spent more on phone credit, made greater use of mobile money, and increased investment on their farms (see Appendix A4). We find it reassuring that data collected from households corroborates the information reported by firms.

We also test whether effects are larger for firms that are listed first in their directory subsections, relative to those listed later (firms were listed in alphabetical order). We find no significant differences by listing order (Appendix Table A5). We take this as evidence that households took the time to read the firm descriptions and choose whom to call.

Heterogeneity By Firm Type. The average treatment effects reported above may disguise heterogeneity in how firms benefit from the listing. We first examine heterogeneity by sector, where the most natural division is between retail and non-retail firms.[9] These estimated effects are reported in Panels C of Tables 1 and 2. The overall pattern is that both direct and spillover effects are larger in magnitude for retail firms. Effects on calls, texts, and sales revenue are statistically significant for retail firms, but not for non-retail firms. Because the non-retail effects are imprecise, we cannot reject the equality of effects across sectors. Nevertheless, these findings suggest that the directory was more impactful for firms selling goods rather than services.

Perhaps more interesting is the possibility that the directory allowed consumers to search more broadly and switch their business to more productive firms. While we do not have data to estimate total firm productivity at baseline, we can calculate a measure of labour productivity (revenue per worker) using baseline data on employment and sales. In Table 4 we observe that the statistically significant effects of the directory are almost entirely concentrated among firms with above median baseline labour productivity. This finding is consistent with the directory lowering search costs, allowing customers to find more productive firms. This result aligns with Jensen and Miller (2018), who find that when information frictions fall in Kerala boat markets, benefits accrue only to the higher quality boat builders.

[9] We define the retail group to include any firm that primarily sells goods (including wholesalers), while the non-retail group includes transporters, processors, financial services, veterinarians, skilled tradespeople, and others.

Heterogeneous Effects from Directory Distribution in a Firm's Village. In randomizing where directories were distributed, we created exogenous variation in whether directories were received by households in the same villages as listed firms. This makes it possible to test whether treatment effects on firms depend on whether nearby households received copies of the directory. We might expect this interaction to be negative, if demand were fixed and access to the directory allows households to shop outside their local neighborhood.[10] However, the interaction could be positive if the reduction in search costs led to a net increase in investment and expenditure by recipient households. Hence, the net effects of local distribution on firms are ambiguous *ex ante*.

Table 3 reports heterogeneous effects by whether directories were distributed in the firm's village. We find no meaningful differences in magnitudes, no clear pattern of larger point estimates for one group, and no statistically significant differences. While this pattern admits multiple interpretations, the average firm in our setting does not appear to be made worse off when the local customer base experiences a reduction in search costs. This finding also suggests that the directory induced more inter-village exchange. If it had not, then positive effects to listed firms would be concentrated in the villages where distribution occurred.

Business-Stealing, or Economic Growth? Given the evidence of positive within-village spillovers, a key question is whether there were spillovers between treated and untreated villages. While the study was not designed *ex ante* to test for between-village spillovers, two pieces of evidence suggest that Pure Control firms were not negatively affected by the directory. The first comes from a follow-up phone survey that we conducted in 2017, three years after our initial contact with study firms, to assess whether firms were still reachable. The response rate was highest for Pure Control firms, and of those that we reached, 95.8% were still operating (compared to 94.8% and 95.7% for the Treat and Control groups). The implication is that there were no between-village effects on firm survival more than two years

[10] The key idea here is that the randomized distribution operates like a separate experiment, one in which treatment makes it easier for previously captive consumers to find firms outside their village.

after the launch of the directory.

The second piece of evidence comes from testing whether outcomes for Pure Control firms vary with the intensity of treatment in their same ward and sector. The listing randomization created quasi-experimental variation in the share of between-village competitors that could potentially steal business from Pure Control firms. In Appendix Table A7 we report estimated coefficients from regressing outcomes for the Pure Control firms on the number or share of their competitors (defined as firms in the same ward and sector). The main takeaway is that we find no evidence of negative or positive between-village spillovers, as none of the estimated coefficients are statistically significant. An important caveat is that because the sample sizes are not as large as in the main analysis, these null effects are not especially precise.[11]

If the increases in customer contact and sales that we document did not come at the expense of Pure Control firms, where did they come from? One possibility is that, as a result of the decrease in search costs, the expected value of harvests may have increased for directory recipients (e.g., from anticipating better sales outcomes via easily searching for buyers), which in turn could lead to net increases in investment and expenditure. In the household survey data we see that relative to non-recipients, directory recipients increased farm investment, engaged in more search outside their village, used their phones more for search, and received weakly higher prices for the crops that they sold (Appendix Table A6).[12] Our research team is currently engaged in a large-scale follow-up experiment to better understand how receiving a directory with firm contact information affects household investment and choices.

[11] For example, the bootstrap 95% confidence interval for the effect on total business calls from a 1 s.d. increase in the number of treated competitors ranges from -35.7 to 22.8, bounds that are an order of magnitude larger than the direct effect of treatment, which is 2.93. The analogous confidence interval for the between-village spillover effect on sales revenue is [-200207, 218227], endpoints that are roughly 50% greater in magnitude than the direct effect on sales. Hence, we cannot rule out positive or negative between-village spillover effects that are as large or larger than the estimated direct effects of the treatment.

[12] We have not emphasized the treatment effects on households in the main analysis, because although the directory distribution was randomized and all households were interviewed in the same month, treatment and control households were recruited 6-8 months apart.

4. Conclusion

The results of our field experiment highlight the importance of information frictions for small and medium enterprises in rural Tanzania. In an economy recently transformed by mobile phones, we find that the dissemination of contact information facilitates communication between firms and customers, and improves business outcomes of listed firms. There is evidence of heterogeneity, as positive impacts are concentrated among more productive firms and firms in the retail sector. We do not find evidence of negative spillovers, either to unlisted firms in the same villages as listed firms, or to nearby villages where no firms were listed.

More broadly, these findings highlight the importance of the information frictions still facing many SMEs in developing economies, and the potential for low-cost interventions to reduce those frictions. The spread of mobile phones has dramatically reduced the cost of communication within existing social networks. But without a complementary information service to facilitate new connections, those networks may be slow to evolve, and may not reach their full productive potential.

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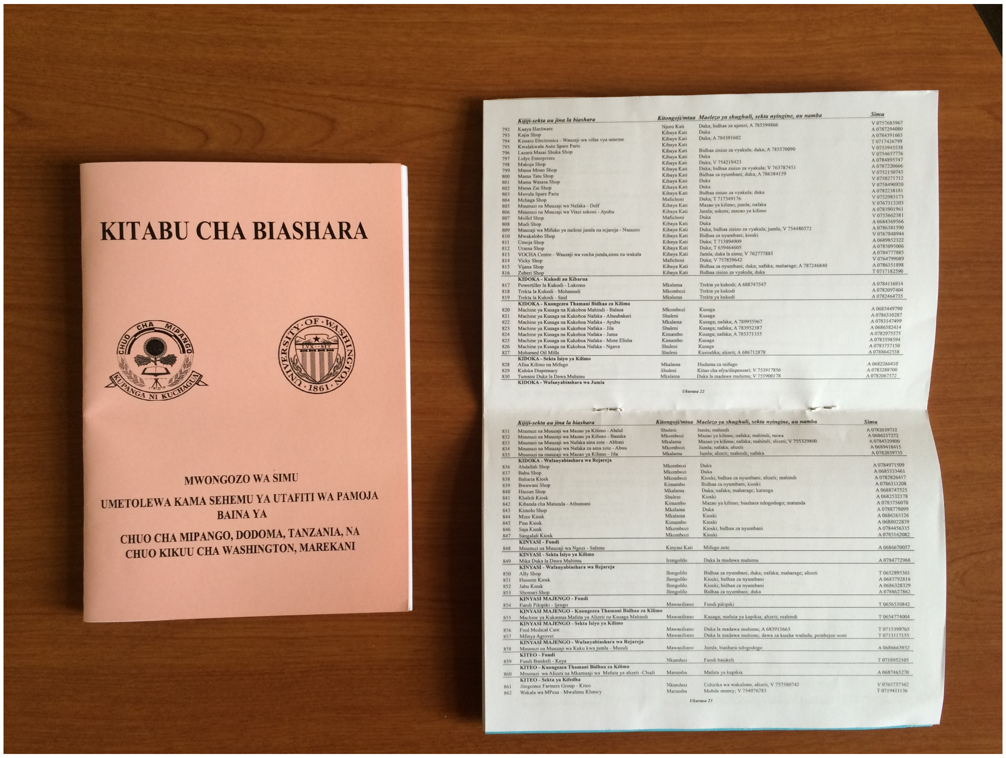
Figure 1: The *Kichabi* Directory



(a) The directory booklet

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

(b) Example directory entries

Notes: Panel A shows the front cover with the Swahili title "Kitabu Cha Biashara" ("business book") alongside a page from the directory. Panel B shows a snapshot from the printed 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 (A/T/V) 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*).

Table 1: Communication-related effects of being listed in the directory

	——Direct effect——			———Spillover———				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Treat	p clust.	p WCB	Control	p clust.	p WCB	N	PC mean
A. All firms								
Total business calls	2.93**	0.01	0.03	1.67	0.34	0.36	1560	13.67
Incoming business calls	1.76	0.14	0.20	0.75	0.63	0.63	1558	11.57
Total business texts	1.15**	0.02	0.04	1.35**	0.02	0.03	825	1.69
Incoming business texts	0.53*	0.05	0.10	0.65*	0.03	0.07	807	1.00
B. Phone history verified								
Total business calls	3.94***	0.00	0.01	1.27	0.35	0.36	1225	13.19
Incoming business calls	2.64*	0.01	0.05	0.63	0.61	0.57	1223	11.04
Total business texts	1.38**	0.00	0.03	1.02**	0.02	0.04	648	1.73
Incoming business texts	0.65*	0.01	0.07	0.50	0.06	0.11	640	1.02
C. By sector (marginal effects)								
Retail								
Total business calls	4.16*	0.01	0.08	1.67	0.45	0.51		
Incoming business calls	2.90	0.03	0.11	1.01	0.57	0.60		
Total business texts	1.42*	0.02	0.09	2.00**	0.01	0.04		
Incoming business texts	0.65	0.07	0.17	0.87	0.03	0.10		
Non-retail								
Total business calls	1.72	0.29	0.23	1.62	0.45	0.40	1560	13.67
Incoming business calls	0.66	0.68	0.68	0.47	0.82	0.74	1558	11.57
Total business texts	0.96	0.20	0.21	0.77	0.33	0.36	825	1.69
Incoming business texts	0.46	0.22	0.22	0.49	0.26	0.25	807	1.00

Notes: All regressions include fixed effects for survey round, randomization strata, and the baseline value of the dependent variable. Sample size varies across regressions because (i) phone call outcomes were measured four times post-treatment, while other outcomes were measured only twice post-treatment, and (ii) rates of non-response or "I don't know" varied across questions. Total business calls is the sum of incoming and outgoing business calls, as well as missed calls (which are used as a way to request a call back from the business). Panel B is based on the 80% of the sample that allowed enumerators to confirm calls by looking through the phone history together. Panel C marginal effects are based on a single set of regressions with interactions for the retail sector. In Panel C, daggers based on the wild cluster bootstrap indicate whether estimated effects are significantly different between retail and non-retail firms: † † †: significant at 1%; ††: significant at 5%; †: significant at 10%. p-values in columns 2 and 5 are based on standard errors clustered at the village level. p-values in columns 3 and 6 are based on the wild cluster bootstrap of Cameron et al. (2008), where the cluster is the village (49 total clusters). Significance stars based on WCB p-values. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 2: Business effects of being listed in the directory

	——Direct effect——			———Spillover———				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Treat	p clust.	p WCB	Control	p clust.	p WCB	N	PC mean
A. All firms								
Use mobile money (=1)	0.16**	0.00	0.04	0.15**	0.01	0.04	641	0.57
Firm has employees (=1)	-0.00	0.99	0.98	-0.01	0.93	0.95	642	0.37
Number of employees	-0.31	0.21	0.36	-0.21	0.45	0.56	642	1.27
Number of sales, last week	4.94**	0.00	0.04	4.26*	0.06	0.09	523	10.61
Sales revenues (TSH)	137936**	0.01	0.03	177293**	0.02	0.03	580	132998
Number of business purchases	1.27	0.28	0.40	0.83	0.44	0.49	641	1.21
B. Phone history verified								
Use mobile money (=1)	0.18**	0.00	0.03	0.14**	0.01	0.05	520	0.58
Firm has employees (=1)	0.03	0.69	0.78	-0.01	0.88	0.91	521	0.35
Number of employees	-0.18	0.44	0.49	-0.27	0.33	0.42	521	1.08
Number of sales, last week	4.33**	0.01	0.04	2.51	0.34	0.37	421	10.66
Sales revenues (TSH)	147993**	0.01	0.01	155377	0.07	0.13	474	130736
Number of business purchases	1.58	0.30	0.42	1.16	0.40	0.36	520	1.27
C. By sector (marginal effects)								
<u>Retail</u>								
Use mobile money (=1)	0.24	0.00	0.14	0.23	0.01	0.14		
Firm has employees (=1)	0.07	0.37	0.45	0.08	0.40	0.48		
Number of employees	0.10	0.82	0.84	0.29	0.52	0.57		
Number of sales, last week	9.79	0.01	0.12	10.39	0.02	0.12		
Sales revenues (TSH)	204440**,†	0.00	0.02	245712**	0.00	0.04		
Number of business purchases	1.36	0.10	0.15	0.81	0.17	0.14		
<u>Non-retail</u>								
Use mobile money (=1)	0.10	0.20	0.21	0.09	0.32	0.33	641	0.57
Firm has employees (=1)	-0.01	0.89	0.90	-0.05	0.52	0.60	642	0.37
Number of employees	-0.45	0.33	0.50	-0.51	0.31	0.58	642	1.27
Number of sales, last week	-0.85	0.76	0.78	-2.38	0.54	0.56	523	10.61
Sales revenues (TSH)	64910	0.24	0.30	105491	0.20	0.22	580	132998
Number of business purchases	0.90	0.53	0.58	0.66	0.64	0.65	641	1.21

Notes: All regressions include fixed effects for survey round, randomization strata, and the baseline value of the dependent variable. Sample size varies across regressions because rates of non-response or "I don't know" varied across questions. Panel B is based on the 80% of the sample that allowed enumerators to confirm calls by looking through the phone history together. Panel C marginal effects are based on a single set of regressions with interactions for the retail sector. In Panel C, daggers indicate whether estimated effects are significantly different between retail and non-retail firms: † † †: significant at 1%; ††: significant at 5%; †: significant at 10%. p-values in columns 2 and 5 are based on standard errors clustered at the village level. p-values in columns 3 and 6 are based on the wild cluster bootstrap of Cameron et al. (2008), where the cluster is the village (49 total clusters). Significance stars based on WCB p-values. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 3: Heterogeneity by whether directories distributed in same village

	——Direct effect——			———Spillover———				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Treat	p clust.	p WCB	Control	p clust.	p WCB	N	PC mean
A. Areas where directory was distributed (marginal effects)								
Total business calls	2.64*	0.06	0.07	3.03	0.14	0.17		
Incoming business calls	1.53	0.24	0.27	1.87	0.31	0.34		
Total business texts	1.36**	0.01	0.04	1.35**	0.04	0.03		
Incoming business texts	0.65*	0.04	0.08	0.68*	0.07	0.09		
Use mobile money (=1)	0.18**	0.00	0.04	0.17**	0.01	0.02		
Firm has employees (=1)	-0.02	0.81	0.87	0.02	0.75	0.69		
Number of employees	-0.36	0.14	0.31	-0.19	0.50	0.58		
Number of sales, last week	5.71*	0.01	0.05	3.75	0.18	0.20		
Sales revenues (TSH)	135077**	0.02	0.04	200400*	0.06	0.08		
Number of business purchases	1.02	0.31	0.32	0.76	0.44	0.48		
B. Areas where directory was *not* distributed (marginal effects)								
Total business calls	3.68*	0.03	0.07	-0.47	0.80	0.84	1560	13.67
Incoming business calls	2.41	0.12	0.17	-1.01	0.52	0.59	1558	11.57
Total business texts	0.73	0.19	0.20	1.37**	0.01	0.02	825	1.69
Incoming business texts	0.30	0.35	0.34	0.63*	0.05	0.08	807	1.00
Use mobile money (=1)	0.14**	0.01	0.04	0.11	0.14	0.16	641	0.57
Firm has employees (=1)	0.03	0.62	0.67	-0.05	0.59	0.66	642	0.37
Number of employees	-0.21	0.44	0.48	-0.24	0.45	0.51	642	1.27
Number of sales, last week	3.27	0.29	0.33	5.05*	0.08	0.08	523	10.61
Sales revenues (TSH)	149404*	0.04	0.10	146141	0.05	0.13	580	132998
Number of business purchases	1.76	0.24	0.43	0.93	0.43	0.45	641	1.21

Notes: All regressions include fixed effects for survey round, randomization strata, and the baseline value of the dependent variable. Sample size varies across regressions because (i) phone call outcomes were measured four times post-treatment, while other outcomes were measured only twice post-treatment, and (ii) rates of non-response or "I don't know" varied across questions. Total business calls is the sum of incoming and outgoing business calls, as well as missed calls (which are used as a way to request a call back from the business). Marginal effects are based on a single set of regressions with interactions for whether the village was randomly assigned to the household treatment (distribution of directories to households). Daggers indicate whether estimated effects are significantly different between distribution village and non-distribution village firms: † † †: significant at 1%; ††: significant at 5%; †: significant at 10%. p-values in columns 2 and 5 are based on standard errors clustered at the village level. p-values in columns 3 and 6 are based on the wild cluster bootstrap of Cameron et al. (2008), where the cluster is the village (49 total clusters). ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 4: Heterogeneity by baseline labor productivity

	——Direct effect——			———Spillover———				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	Treat	p clust.	p WCB	Control	p clust.	p WCB	N	PC mean
A. Above median labor productivity at baseline (marginal effects)								
Total business calls	4.18*	0.01	0.05	3.38	0.11	0.15		
Incoming business calls	2.43	0.13	0.17	1.78	0.35	0.41		
Total business texts	1.96**,†	0.00	0.04	2.47***	0.00	0.00		
Incoming business texts	0.85*	0.01	0.06	1.20**	0.00	0.02		
Use mobile money (=1)	0.28**	0.00	0.04	0.30**	0.00	0.03		
Firm has employees (=1)	-0.02	0.89	0.93	-0.06	0.62	0.64		
Number of employees	0.23	0.45	0.48	0.43	0.22	0.28		
Number of sales, last week	10.30**,†	0.00	0.04	10.60*	0.03	0.07		
Sales revenues (TSH)	164966*	0.04	0.06	305917**	0.00	0.01		
Number of business purchases	1.68	0.28	0.37	1.19	0.36	0.32		
B. Below median labor productivity at baseline (marginal effects)								
Total business calls	2.61	0.16	0.20	1.65	0.48	0.44	1440	13.36
Incoming business calls	2.00	0.19	0.26	1.14	0.56	0.51	1438	11.39
Total business texts	0.60	0.30	0.25	1.13	0.18	0.19	758	1.45
Incoming business texts	0.48	0.15	0.16	0.69	0.10	0.14	742	0.85
Use mobile money (=1)	0.09	0.32	0.29	0.05	0.63	0.59	579	0.52
Firm has employees (=1)	0.01	0.96	0.96	0.03	0.81	0.80	580	0.37
Number of employees	-0.42	0.21	0.36	-0.34	0.31	0.48	580	0.89
Number of sales, last week	-1.08	0.74	0.75	-1.09	0.79	0.79	471	9.27
Sales revenues (TSH)	131023*	0.01	0.08	71209	0.26	0.28	525	120743
Number of business purchases	1.90	0.13	0.10	1.68	0.20	0.10	579	0.97

Notes: All regressions include fixed effects for survey round, randomization strata, and the baseline value of the dependent variable. Sample size varies across regressions because (i) phone call outcomes were measured four times post-treatment, while other outcomes were measured only twice post-treatment, and (ii) rates of non-response or "I don't know" varied across questions. Total business calls is the sum of incoming and outgoing business calls, as well as missed calls (which are used as a way to request a call back from the business). Marginal effects are based on a single set of regressions with interactions for whether the firms was abobe median labor productivity at baseline, where labor productivity is defined as sales revenue divided by the number of workers. Daggers indicate whether estimated effects are significantly different between above median and below median productivity firms: † † †: significant at 1%; ††: significant at 5%; †: significant at 10%. p-values in columns 2 and 5 are based on standard errors clustered at the village level. p-values in columns 3 and 6 are based on the4wild cluster bootstrap of Cameron et al. (2008), where the cluster is the village (49 total clusters). ***: significant at 1%; **: significant at 5%; *: significant at 10%.