Research Article

Tracking the Introduction of the Village Phone Product in Rwanda

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Abstract

This paper presents the results from a quantitative impact study of the Grameen/MTN Village Phone in Rwanda, which was conducted between June 2006 and August 2007. We find that the introduction of a Village Phone had a substantial impact on reported access to telecommunications for local entrepreneurs. While the introduction of phones did not follow the intended randomized design, we compare the changes observed in 94 study communities that received the phones to the 284 that did not. We find that the placement of a Village Phone in a community was associated with both an increased use of phones to transmit news and a greater propensity for farmers to arrange their own transit. Despite this improvement in access to telephony, the actual prices received by farmers were not affected. Impacts at the household level were muted by the relatively small size of Village Phone businesses and airtime usage rates, implying that profits must be transferred from other sources to pay off the phone in six months. Reported labor time in household enterprise increased dramatically for Village Phone operators, but positive impacts on consumption or overall business profits were not found.

Introduction

Mobile phones appear to have transformed developing economies, and they are rapidly becoming the most ubiquitous high-tech consumer item in the poor countries of the world. Gauging the real impact of mobile telephony on economic activity is an empirical challenge, however; telecom providers are savvy at building new infrastructure only where demand exists or where growth prospects are high, and hence we tend to see a positive correlation between telephonic and economic expansion even if the relationship is not causal. Only a limited number of studies have been able to use panel data to construct before—after measurements of impacts, and studies such as Jensen (2007) and Aker (2008) typically focus on prices without examining a broad range of community impacts.

This article presents the results of a study intended to measure these broader economic impacts of mobile telephony. We track a group of potential Village Phone operators (VPOs) and the communities in which they reside as the Village Phone (VP) program is introduced in Rwanda. The VP product, known locally as "Tel'imbere," is a result of collaboration between the Grameen Foundation and MTN Rwanda (MTNR), a part of major African mobile phone provider MTN Group. The Village Phone operates in rural areas and links a car battery to a standard mobile handset, which is then connected to an antenna that can be mounted on the roof of a kiosk. This serves as a pay phone for the community, with the operator charging fixed rates and receiving set profits as a function of call desti-

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nation and airtime usage on the phone. Car batteries are easily charged in market centers, and one charge runs the phone for roughly two months. The antenna allows VP to operate beyond the range in which a typical mobile phone gets coverage, up to roughly 30 kilometers from the base station. Collaborations with microfinance institutions (MFIs) provide financing to operators so that they can purchase the phones and pay off the loan with the profit stream from airtime usage. During the study period, the term of the loan offered by MFIs was six months, and the total cost of the loan plus financing was US$261.

The intended design of this study was a randomized controlled trial, in which the phones would be phased in to a group of 380 preselected villages in an experimental fashion. In the end, competitive pressures in the MFI sector, as well as operational glitches in the rollout process, undermined the randomized structure. When we returned to do a follow-up study 15 months after the baseline, we found that 94 communities had received phones, but that the actual rollout bore little resemblance to the intended design. We therefore treat our data as entirely non-experimental, paying careful attention to the actual determinants of the receipt of a phone, and we interpret these impact results with more caution than would be warranted in a randomized evaluation.

We use a detailed household survey as well as an extensive community survey conducted with entrepreneurs in the markets served by VPOs to draw conclusions about the effects of the phones. The household survey covers business outcomes, such as profits, labor inputs, sales, and customer foot traffic at the enterprise level. It also tracks more conventional impact indicators, such as consumption and schooling at the household level. The community survey inquired about a wide range of potential impacts from telephony, including market availability, prices received by local farmers, access to phones, and the means used to transport local goods to national markets and communicate with outside entities, such as police and health care providers.

Our results show clear mechanical effects of the phones at the community level: entrepreneurs report local access rates to any kind of telephony rising from just over 50% to over 90% when the VP is installed. The travel time to reach a phone reported by local entrepreneurs plummets from 56 minutes to 10 minutes in these communities. We see a moderate, but significant, increase in the percentage of local farmers who report arranging for their own transport to market (from 27% to 40%). Local entrepreneurs are also more likely to pass news via cell phone. However, the community analysis shows absolutely no impact of the phones on either trading activity or the availability of goods in local markets, and household-level impacts are repressed by the fact that airtime usage in Rwanda is so low as to indicate that few VPOs realize sufficient profits to pay off their six-month loans from that source alone. The sole significant impact at the household level is an increase in the time reportedly worked in both VP and non-VP enterprises of operators. Therefore, in the context of Rwanda, a densely populated country with moderate pre-existing phone coverage, the actual use of Village Phones by clients is sufficiently low as to prevent VPs from being either highly profitable or drivers of local commerce.

Background

Cross-country evidence on the impacts of information and communication technology (ICT) improvements has tended to show large impacts on economic growth, whether analyzing fixed-line upgrading in developed countries (Röller & Waverman, 2001) or the introduction of mobile phones in developing countries (Waverman et al., 2005; or see Donner [2008] for an excellent review of recent studies). Wallsten (2001) finds that the key determinant of broad-based, low-cost access to telephony in African and Latin American countries is the degree of competition, whereas the effects of deregulation are more muted.[1]

More micro-level studies of the impacts of ICTs have found a decrease in price volatility across markets (Eggleston et al., 2002; Jensen, 2007; Abrahams, 2008; Aker, 2008), decreases in the profits taken by middlemen (Goyal, 2008), and increases in the efficiency of specific institutions using ICTs (Athey & Stern, 2002, for IT in U.S. hospitals).

1. Despite the rapid scale-up of mobile access on an African continent otherwise short on infrastructural success stories, data from the PingER project show that the Internet digital divide between Africa and the rest of the world is large and growing (Zennaro et al., 2006).

The few studies that have looked directly at the impact of mobile telephony on small and medium-sized enterprise (SME) profits in Africa have found divergent effects. Esselaar et al. (2008) find huge effects of ICTs in informal African enterprises, and they criticize Chowdhury and Wolf (2003) for failing to control sufficiently for the degree of informality (informal businesses are more profitable and less likely to use ICTs than formal businesses, and so this may create downward bias). However, the Esselaar study makes no mention of the substantial selection bias in the decision of an informal business to buy a mobile phone, which itself may create substantial *upward* bias. Other impacts of ICT introduction in Africa found in the literature include improved networking with overseas migrants in Senegal (Tall, 2004) and limited improvements in networking between upper-level NGOs in Tanzania (Mercer, 2004). Molony (2007), on the other hand, argues that the continued importance of trust in the overwhelmingly informal commercial interactions on the continent will continue to make face-to-face communication necessary.

The Grameen Village Phone program in its home country, Bangladesh, was the subject of an in-depth analysis by Richardson et al. (2000). The study looked at a broad range of qualitative impacts, focusing particularly on the gender-matching between the VPO and the users of the phone. Also, given the heavy international migration of Bangladeshis to the Persian Gulf and other parts of the world, it found that a predominant use of the VPs was to communicate with overseas diaspora. This issue is less salient in Rwanda, a country that has many external refugees, a few of whom currently reside in places with good telecommunications access (that is, areas like the Kivu region of the Democratic Republic of the Congo). The closest study to ours in terms of research subjects is Donner (2007), who conducts an examination in Kigali of entrepreneurs, much like those studied in our analysis, and suggests that mobile phones create an important avenue through which micro-entrepreneurs, particularly those with no landline phone, can create new networks of customers.

The technical capabilities of the Village Phone suggest that VPs will operate without competition from other phones, thereby generating a marked improvement in the quality of local telephony. Further, this is a study of the pilot of Rwanda's VP program, and therefore, the cases studied here feature virtually no competition from pre-existing VP-program phones. In reality, however, most of the VP rollout in Rwanda occurred in areas that had some pre-existing mobile coverage, and it is not uncommon to see people talking on standard handsets in front of a VP enterprise. In such locations, the introduction of a VP creates no new access to telephony, but it may still cause access to telephony to become both cheaper and more convenient for households that do not own a handset. This effect is likely to be more pronounced in the future if the highly structured pricing of the VP product undercuts high prices for private rental of time on handsets.

Further undermining the magnitude of the "shock" generated by the introduction of VP is a pre-existing product called Tuvugane, which also uses the MTNR network. The Tuvugane handset is the size of an office phone with a small internal battery that can be charged overnight. It lacks the external antenna of the Village Phone product, so it has a more limited ability to extend telephony beyond the boundaries of the standard coverage network. These phones were ubiquitous in urban areas, and they had already penetrated into relatively rural centers as of 2006, when the baseline survey was conducted. Our data present multiple forms of evidence that Tuvugane was the primary competitor of VP's Tel'imbere in Rwanda. Indeed, even *after* the introduction of the VP product, Tuvugane is reported as the nearest public phone by entrepreneurs in more than 60% of Tel'imbere markets. Therefore, we must be careful not to interpret these results as the impact of *access* to a phone, but as the impact of the *improvement* in telephony generated by the VP. While we see some real evidence of the improvement, we do not find the sharp effects that some other studies have found. In the conclusion, we discuss possible reasons for this, and the paper tests for whether impacts are stronger in environments with no previous coverage.

Village Phone Product Design

The VP product consists of a standard Nokia handset plugged into a car battery for power, plus a roof-mounted antenna to extend the range of the phone. The phone is provided to the operator via a six-month loan from local microfinance institutions. The total cost of the loan is US$261, including inter-

Table 1. Village Phone Pricing Schedule, U.S. Dollars per Minute.

		VP Enterprise, First 6 Months		VP Enterprise, After 6 Months	
	Retail Price 1	Cost per min 2	Profit per min 3	Cost per min 4	Profit per min 5
MTN	0.28	0.17	0.11	0.22	0.06
RwandaTel	0.34	0.28	0.07	0.30	0.04
East Africa	0.69	0.48	0.21	0.56	0.12
International	0.96	0.62	0.34	0.81	0.15
SMS	0.10	0.06	0.04	0.08	0.02
SMS International	0.24	0.14	0.10	0.19	0.05
MTN Info +	0.12	0.07	0.05	0.10	0.02

est costs, which translates into an average monthly payment of $43.50. During the period of our study, there were three MFIs providing financing for the VP product: CARE, which makes small loans to relatively poor borrowers in large joint-liability groups, and Vision Finance and Urwego, both MFIs that feature smaller groups and larger loans to wealthier individuals.[2] While more than half the individuals in the study sample were CARE clients, the fraction actually receiving phones is split roughly evenly across the three MFIs.

The loan is supposed to be repaid using profits generated by the difference between the retail price charged to customers and a discounted price charged to the operators by MTNR. The cost to users of the Village Phone (Column 1 in Table 1) is based on rates for pre-paid airtime cards, and it is between standard per-second billing rates and "pay-as-you-go" rates charged to individuals who own their own MTNR handsets. The cost of using a Village Phone depends on both the destination and duration of the phone call. The least expensive rate is for calls to domestic MTNR clients, while the highest rate is for international calls outside East Africa. The cost paid by the VPO to MTNR, however, varies depending on how long the Village Phone has been in operation. MTNR offers a lower cost per unit for the first six months of operation (Column 2, Table 1) to help offset the burden of repaying the loan. After six months, the cost to the VPO rises to those prices listed in Column 4 of Table 1. The profit per unit during each respective time period is also given in the table.

For clients to make phone calls using Village Phone, the VPO must load airtime onto the phone beforehand using MTNR scratch cards. When a call is made, the cost is deducted from the Village Phone account according to the destination, the duration, and whether the phone has been in operation for six months. After the call is made, the VPO retains the full price paid by the client. VPOs have complained that, because the individual phone calls are not prepaid and the calls are not metered, clients do not know the cost beforehand and will often not have the necessary funds after the call is made. To receive proper remuneration, it is the responsibility of the VPO to correctly determine the appropriate rate based on the destination of the call. While there is anecdotal evidence of VPOs charging additional fees to phone users (particularly for receiving calls, which are not charged airtime fees in Rwanda, and for calls that require the VPO to send someone to fetch the user), these fees are not a part of the formal VP product.

Study Design

Rollout of the Village Phones

The intended design of the study was a randomized sequencing of the rollout of the phones. Combined with a baseline survey conducted prior to the rollout and a follow-up conducted two-thirds of the way through, this would have provided us with a clean

2. We find that the average CARE client has annual per capita expenditures of just under $200, while the figures for Vision Finance and Urwego are $280 and $270, respectively. All three are nonprofit organizations.

experiment. We selected our sample during the spring of 2006 by asking the three collaborating MFIs to identify individuals to whom they were willing to make loans who were interested in becoming operators and who belonged to an MFI that would be willing to undertake a VP loan with that individual. The collaborating MFIs had difficulty producing the desired sample size for the study (400 groups), and in the end, we fell slightly short of that number. The sample therefore consists of "likely" clients in acceptable microfinance borrowing groups who had expressed interest in operating a Village Phone at the time of the baseline study.

As discussed in the introduction, the rollout did not proceed as designed. Of the 378 individual/community surveys conducted in the baseline (June 2006), 94 of them had received phones as of the follow-up (August 2007). Nevertheless, the identity of those who received phones bears only a weak resemblance to the intended experimental structure. Intended treatment status was considered as an instrumental variable for actual treatment, but the proportion of designated treatment individuals that actually received a VP was 0.29 versus 0.22 for those designated as controls. Furthermore, the difference between these means was statistically insignificant, diminishing the predictive power of the first stage of an IV regression.[3] Figure 1 maps the actual distribution of treatment and control locations, along with MTNR coverage and the profits of operators: While there is no obvious spatial pattern, the non-randomized nature of the rollout presents several problems that we will now discuss in detail.

From conversations with Grameen and the collaborating MFIs, three main factors drove the collapse of the randomization and the actual provision of the phones. The first of these was an increase in default across the Rwandan microfinance sector driven by the entrance of a host of new, unregulated institutions. Lenders were unwilling to extend Village Phone financing to borrowing groups that had encountered repayment problems, even if they were designated to be treated under the research design. Secondly, a shifting business climate meant that many individuals who had expressed interest in operating a phone at the time of the baseline no longer wanted them at the point at which they were actually offered. Finally, pressure was applied to the MFIs at the time of baseline to provide a sufficient number of potential operators, and some of the individuals put forward may have been improperly vetted. This may have resulted in our drawing a non-representative sample of operators. With these factors causing fewer phones to be rolled out to the treatment than expected, operational pressures led to some control units receiving phones, as well.

The fact that those selecting out of the treatment may have suffered either from repayment shocks or from local business shocks might lead us to think that the operators who actually received the phones were working in more favorable climates than those who did not. In other words, we may expect the bias created by this non-experimental design to be *upward,* meaning that the operators and communities who received phones would appear to be doing better, even had they not received the phones. Given that we find few positive impacts of the phones, particularly at the household level, this upward bias, if anything, would reinforce the overall lack of impact.

The standard method in the impact literature for estimating treatment effects where both baseline and follow-up data are present is a "difference in differences" (DID) regression: this is a comparison of *changes* in the treatment to *changes* in the control. Statistically, we can write the DID test without control variables as:

$$Y_{it} = \beta_0 + \alpha_1 Treat_i + \alpha_2 Per2_t + \delta(Treat_i * Per2_t) + \epsilon_{it} \quad (1)$$

Here Y_{it} is some outcome variable, i indexes the time period (1 or 2), t indexes the individual unit of observation, $Treat_i$ is a binary variable indicating whether a unit will ever be treated, $Per2_t$ is a binary variable for the follow-up period, and the coefficient δ measures the DID; changes are observed only in the treatment and not in the control. To present the data in as transparent a fashion as possible, our tables provide the pre- and post-treatment means of outcome variables, partitioning across communities that did and did not receive phones. The test statistic given for the DID is the t-statistic on δ from above.

3. Note that non-VP communities intended to receive the treatment gained new access to the Tuvugane product in almost the same proportion as those intended to be controls (55% vs. 58%).

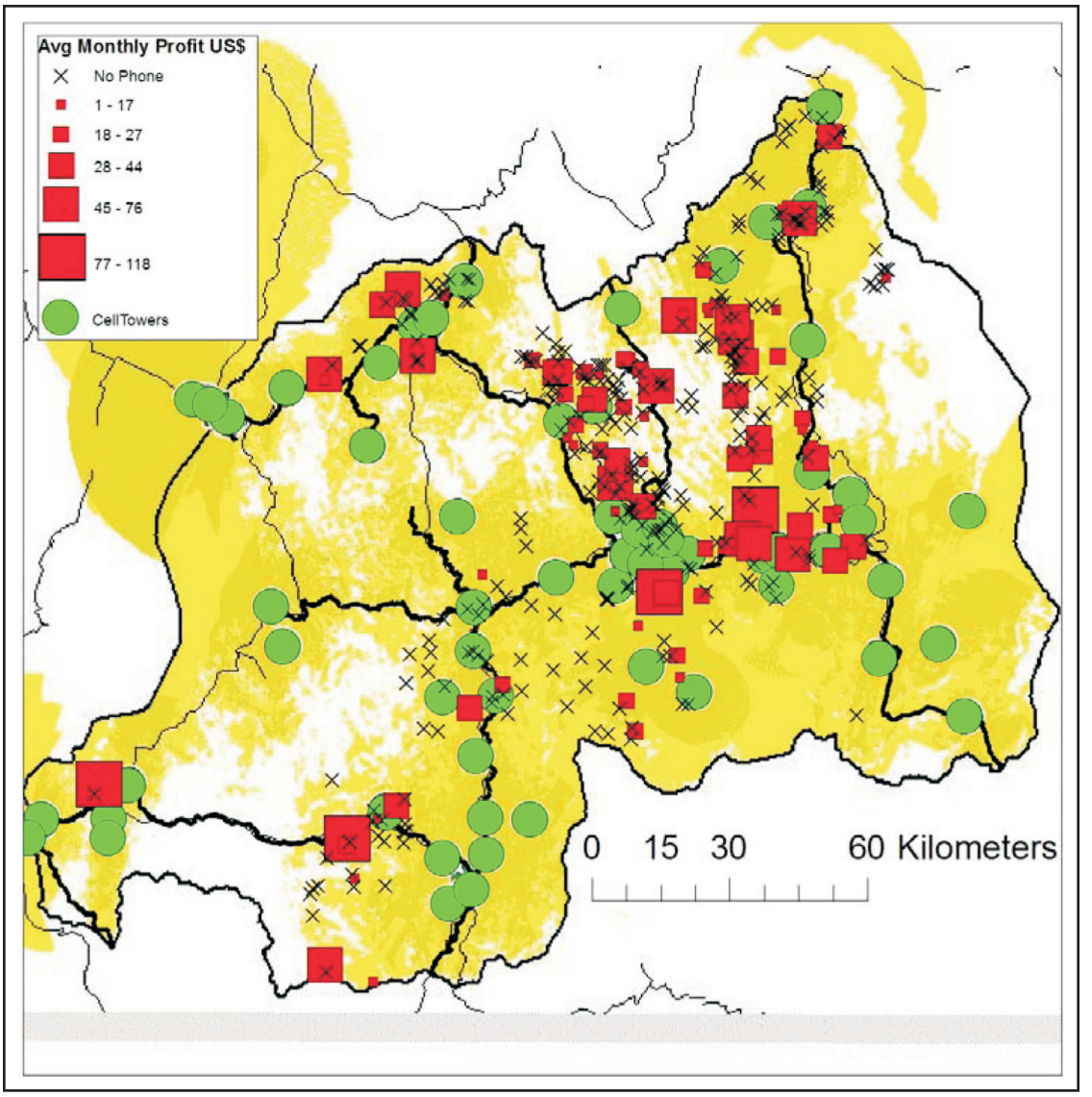

Figure 1. Map of MTNR coverage, cell towers, and the profits of Village Phone Operators.
Squares indicate locations of VPOs, size denotes VPO profit (US$/month). Circles indicate the location of MTNR towers, x gives the location of untreated communities.

One interesting issue becomes tractable due to the failure of randomization, and that is a direct observation of the process through which VPOs are selected. If we take our baseline group as a sample of individuals who were an attractive target market for the product, there is substantial interest in understanding what type of person and what type of village ends up receiving a phone in reality. For this reason, we present all tables with baseline means separately for those who *will* and those who *will not* receive phones, as well as with a t-test of the difference. In this way, we gain insight into the kinds of markets in which uptake for the phones will be highest, although it is important to note that the sample for this study is not representative of Rwanda, but rather it is representative only of the "likely" markets, as identified by the process through which we selected the baseline sample.

Data

The baseline survey was administered in May–June 2006, and the follow-up was conducted in August–September 2007. The survey selected a sample of 378 groups across CARE, Urwego, and Vision Finance. The Operator Survey was administered to the individuals identified as the putative operator in the baseline survey. Wherever possible, the follow-up survey was administered to the same individual. If that individual could not be found in the follow-up and there was no phone in the community, the follow-up team attempted to survey a different member of the same MF group. If the community had received a handset, the follow-up survey tracked the actual operator of the phone in addition to the baseline individual. The Operator Survey collects information on a standard set of household and enterprise outcomes, including consumption, expenditures, schooling, household composition, assets, phone usage, and investment, and cash-flow in household enterprises.

The Community Survey was administered by bringing together two individuals, identified by the surveyors as the most knowledgeable about local market conditions, and asking them to answer a wide variety of community-level questions regarding prices, market availability, local farming activity, and access to local telecommunications.[4] We describe impacts here on all community outcomes except prices, which are considered in a separate paper.

The complete survey includes 764 observations across the two waves. To ensure that we found all relevant operators, we used two methods. First, during the follow-up survey, we not only asked whether our baseline surveyees had received phones, but whether anyone in that community who was not surveyed in the baseline had. This allowed us to correctly identify community-level treatment, even if the operator turned out to not be the person we interviewed in the baseline. Second, we used a list of concurrent rollouts prepared by MTNR to call all VP-program phones introduced during the study period to verify operator identity and location.

In the household-level (and enterprise-level) analysis, we wanted to track the same individual from baseline to follow-up. In this case, we are interested only in individuals who were surveyed in both waves, and so we consider as "treated" only those for whom baseline and follow-up surveys are available and who received phones by the follow-up. This creates a balanced panel dataset on 290 individuals, of which 49 received phones between the two surveys.[5] At the community level, we are not interested in *who exactly* received a phone, but instead whether *anyone* in the community received one. Therefore, for the community-level analysis, we create a dataset where the unit of analysis is the location rather than the individual, and this results in a balanced panel dataset of 378 observations, of which 94 receive the "community" treatment.

The survey data are relatively "noisy," so we use a variety of techniques to dampen variation caused by outliers. We present most of the analysis using simple comparisons of means and difference-in-differences, and we have experimented with trimming outliers (particularly for the self-reported financial calculations), so any results discussed are robust to these changes.

We have three ways to identify operator profits. Following de Mel et al. (2008), we prefer to use self-reported profits to calculate profits mechanically, but we present results using both net revenue minus net costs (with data taken from the household survey) and self-reported profits. In addition to these two methods, the MTNR data provide the phone number, duration, and time of every call made into or out of Village Phones from January–September 2007. In combination with the billing structure, this gives a completely error-free means of calculating direct profits from airtime usage, although this measure does not subtract off the operating costs of battery charging and upkeep of the premises, and it misses potential increased profits from increased

4. In some cases, these community surveys are identical between two respondents, indicating a "consensus" answer, and in other cases they differ. To deal with this, we cluster standard errors at the locality level when running regressions from the community-level questionnaire, which provides answers basically identical to collapsing these surveys to the community/period level.

5. Individuals who do not themselves receive phones, but do reside in communities that receive phones are dropped from the "individual" dataset because they may receive an indirect treatment effect. Results are not sensitive to the inclusion of these observations. We are missing location information on 31 individuals, resulting in fewer observations in regressions that include spatial data.

foot traffic or additional, unofficial charges.[6] This allows us to examine how airtime-driven profits relate to a variety of other characteristics for operators. We also use these raw data files to calculate simple statistics, such as the number of calls per month, average duration of calls, and so on.

We attempted to record the location of all communities with paper maps, from which coordinates were then read as latitude/longitude figures into ArcView for GIS analysis. In the end, we were unable to get definitive locations for 48 surveyees, so the location data are present for 343 of our 391 operators. This can be combined with maps from MTNR that show the location of their cell phone towers at the time of the baseline, and with GIS layers on road networks to calculate distances. Further, we can define variables such as "number of VPOs within 5 km" by drawing circles around the location of each VPO. We will now proceed to an analysis of the data.

Results

Community Level

Comparing the first three columns of Table 2 gives a sense of the selection effects of the introduction of phones during our study period. Communities that would receive a VP by the follow-up were, at the time of the baseline, more isolated from telecommunications than their counterparts that would not receive VPs. The communities that would see the establishment of VP enterprises were not economically isolated, as we see that they were more likely to be in villages hosting markets at the time of the baseline. Thus, the communities that actually receive phones are entrepreneurial centers with poor preexisting telecommunications access. Overall, however, having tested the community-level selection process using a wide battery of characteristics, we find selection effects to be limited. Those baseline characteristics associated with receiving phones are not associated with differential rates of change in the control groups; hence, we suggest that little selection bias will be present in the DID impact estimates at the community level (see Appendix A2 for the selection analysis, and A9 for regressions using these same variables to explain changes in outcomes in the control communities).

The DID impact of the phone is given by a comparison of changes in centers that see the introduction of a VP versus changes in centers that do not. The starkest impacts arise in the reported access to phone service in local markets, and in the distance traveled to reach the closest phone. Reported phone access rises from 51% to 90% at the village level for communities that receive phones. The average travel time for the surveyed entrepreneurs to use a phone fell from 56 minutes to 10 minutes in these communities. The likelihood that the closest reported phone is Tuvugane falls from 86% in the baseline survey, but even after the intervention, we still have 60% of surveyees reporting Tuvugane as closest. Despite this increase in access, the direct price of telephony is not decreased, and the total amount that entrepreneurs report spending on telecommunications also does not change.

An additional interesting impact at the community level is the ways in which local farmers are able to get their goods to the market. The share of producers arranging their own transport in the treatment jumped from 39% to 52% from baseline to follow-up, while in the control group, this share dipped from 46% to 41%. While this effect is significant, it does not translate into better information about prices or into actual farmgate prices being received by farmers (while point values are all positive in the DID, they are not significant). This result is consonant with Molony (2008), who finds that interlinked provision of credit by established buyers prevents farmers from realizing informational gains that would otherwise be generated by mobile telephony. We see that while VPs were attracted to markets, they did not seem to attract markets.

In summary, the phones had sizable impacts on reported access to telecommunications, including travel times and distances, and they also had muted, but detectable, impacts on how news is transferred and how transport is arranged.

Enterprise Level

The enterprise-level analysis is based on a survey module eliciting separate financial details for each different business activity conducted by the household. A new VP creates a new observation at the

6. For example, mobile users in Rwanda do not pay to receive calls, but there are reports that it is not uncommon for VPOs to charge users of the phone for this.

Table 2. Community-Level Analysis.

	Baseline Outcome:			Follow-up Outcome:			
	Non-VPOs	VPOs	T-stat on Baseline Difference	Non-VPOs	VPOs	Difference in Differences	T-stat on Difference in Differences
Phone call from this center possible	0.636	0.512	(1.92)	0.809	0.907	0.222	(3.17)**
Travel time to make a phone call (minutes)	43.218	56.589	(1.52)	27.087	10.568	−29.890	(3.02)**
Nearest phone is Tuvugane	0.799	0.862	(1.34)	0.706	0.616	−0.153	(2.00)*
Amount spent on phone calls	9.650	9.471	(0.10)	10.562	10.871	0.488	(0.15)
Market meets in this center	0.325	0.483	(2.46)*	0.478	0.516	−0.120	(1.63)
# of traders that run business out of this center	5.926	6.684	(0.80)	7.672	9.670	1.240	(0.60)
# of people who live here but trade in different market	0.467	0.391	(1.09)	0.418	0.525	0.183	(1.98)*
Most common method of passing news (3:travel, 1:phone)	1.478	1.651	(1.98)*	1.314	1.247	−0.240	(2.03)*
Producers arrange own transport	0.467	0.391	(1.09)	0.418	0.525	0.183	(1.98)*
Producers know the Kigali price before selling	0.177	0.174	(0.06)	0.175	0.242	0.070	(0.96)
Highest market price received by farmers over last year	0.379	0.348	(0.65)	0.344	0.351	0.038	(0.80)

*Absolute values of t statistics are in parentheses; * significant at 5%; ** significant at 1%; the regressions are estimated via Equation (1).*
Baseline T-tests are conducted on 326 community-level observations, and the DID has 670 panel observations.
For all yes-no questions, the answer is coded as 1=yes and 0=no, so outcome gives the share (out of 1) indicating "yes."
All currency amounts are in US$.

enterprise level, and this makes panel comparison of VP versus non-VP enterprises difficult. We approach this problem in two simple ways. The first three columns of Table 3 use only the follow-up data, and they compare the attributes of VP businesses to the attributes of all other enterprises. This comparison shows clearly that the VP business is smaller than the other enterprises run by these entrepreneurs in every respect but one: the time the entrepreneur reports working in the business. That is to say, the VP enterprise is uniquely time consuming and yet it generates revenues, costs, and profits that are lower than other enterprises. The implicit wage in these enterprises is therefore very low. Clearly, however, "working" at a VP enterprise requires only that the phone be turned on, which therefore likely permits a high degree of multi-tasking.

Whether from the supply side or the demand side of the market, the VP business is likely to be a *second* business. Only five VPOs got phones while not operating a baseline business, a number that represents just 7% of the eventual sample of VPOs. Virtually no VPOs report profits greater than US$90/month, while 20% of non-VPO enterprises make more money than this. While VP businesses have roughly the same modal rate of profit as non-VPOs, the average VPO profit is $20/month, well short of the $48/month reported in the average non-VPO business (see Table 3).[7]

Columns 4–7 of Table 3 give the impact of the entry of a VP enterprise on the *non-VP* enterprise of operators. It is therefore identified from those individuals who start a VP enterprise as a second business, and we analyze only the first business. The large increases in labor time picked up among VPOs show up even in their non-VP businesses, suggesting, for example, that a shopkeeper who also installs a phone keeps longer hours in both enterprises. Reported credit access to this non-VP enterprise also benefits, presumably as a result of the additional line of credit accessed through the VP financing. Reported profits are no higher, however, failing to give evidence of an expected impact of the VP businesses—that increased foot traffic would benefit the VPO's other business, as well.

Analysis of profits in household enterprises is always complicated because of imperfect recall and recordkeeping, as well as fungibility with the consumption side of the household. In our case, the repayment of debt on the phone adds an additional layer of complexity. Even though the MFI loan had already fully been paid off in 80% of our VPO households at follow-up, if the full costs were still being covered, then the household might report lower profits for all enterprises. Rather than relying on survey questions to try to understand how households amortized profits, we use call data from MTNR to calculate the exact profits transferred to the operators. This quantity is a mechanical function of airtime by destination as given in Table 1, and it gives the maximum possible profits directly from the VP enterprise (if there were no costs), ignoring completely the incidental effect on the profits of other enterprises.

The total cost of the six-month loan that finances the VP is US$261, including interest costs. This translates into an average monthly payment of $43.50. We have used the fee schedule per time unit in the VP product, along with exhaustive data from MTNR on call durations and destinations, to calculate revenues and profits from the VP enterprise for operators. The average mechanical profits from operating the phone are $30.50 per month in our sample, meaning that the profits from the phone only cover 70% of the debt-servicing costs.[8] Thus, the average client has to transfer profit into the VP enterprise from other sources in order to make the loan payments. We do not find strong spillover effects of the VP enterprise on profits from other enterprises, meaning that the margin between profits and loan payments likely represents a nega-

7. We had initially suspected that these small reported profits might be due to the fact that clients were deducting debt-servicing costs; meaning that they were mentally subtracting the loan payment out of the reported profit. However, on closer inspection, only 20% of the VPOs who appear in the follow-up survey have had their phones for less than six months, meaning that those still servicing their loan are a minority in the data. Examining revenues instead of profits, the picture is similar: Average monthly revenues are US$62 (VP) versus US$230 (non-VP), and this figure is free of any influence of outstanding debt in reporting.

8. Our mechanical profit estimate does not include costs, such as the need to charge the battery, nor sources of revenue, such as informally billing customers for receiving calls or charging other mobile devices from the Tel'imbere battery.

Table 3. Enterprise-Level Analysis.

	Simple t-test of differences across second-period businesses, VP vs. non-VP.			Impact of the entry of VP businesses on all other enterprises run by VPO, as a DID:			
Outcome (each enterprise is a separate observation):	**Non-VP enterprises, followup data**	**VP enterprises, followup data**	**T-stat on difference**	**Average change 2006–2007 without VP entry**	**Average change 2006–2007 with VP entry**	**Difference in Differences, VP vs. non-VP**	**T-stat on Difference in Difference**
Profit (self-reported)	47.58	19.47	(3.64)**	−12.399	−18.262	−5.863	(0.43)
Rank of business (1–3, 1 is 'most important')	1.15	1.66	(6.65)**	−0.058	−0.052	0.006	(0.06)
Amount of loan invested in this business	40.96	31.76	(0.54)	−63.280	0.201	63.481	(2.32)*
Monthly Costs in enterprise	142.73	35.22	(3.24)**	1.162	−16.315	−17.477	(0.36)
Monthly Revenue in enterprise	231.14	62.18	(4.50)**	−13.409	−87.520	−74.111	(1.18)
Total value of Stock in enterprise	133.90	33.16	(2.76)**	−17.471	−77.514	−60.043	(0.89)
Wage bill in enterprise	123.83	33.41	(2.88)**	−25.514	−50.623	−25.109	(0.50)
Hours worked in this business	43.04	59.59	(3.67)**	7.013	28.911	21.898	(4.29)**

*Absolute values of t statistics are in parentheses; * significant at 5%; ** significant at 1%. All currency amounts are in US$.*
Followup comparisons are conducted on 237 enterprises; the DID has 444 panel observations on non-VP enterprises.

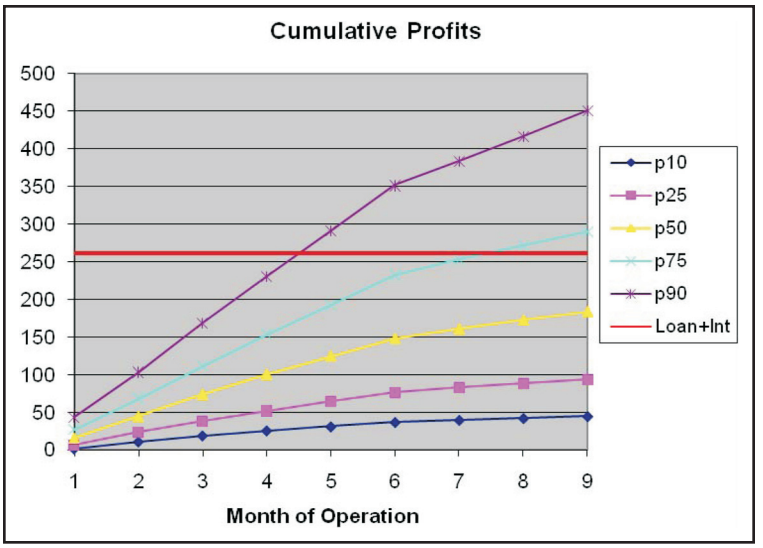

Figure 2. Profitability Analysis, By Distribution of Airtime Usage.

Figures are in U.S. dollars. The red line indicates the total cost, including interest for servicing the loan, and the figure plots cumulative profit over months of operating the phone by percentile of phone usage (p. 50 is the median phone usage rate). When the curves cross the red line, this indicates that phone has paid for itself, according to a simple and conservative measure of profits.

tive net income shock to households during the course of the loan.

After the six months, the household owns the phone outright, and thus it can recoup this investment fully. Figure 2 takes one approach to this accounting problem, showing the trajectory of aggregate profit generated by the phone for different percentiles of phone usage. The red line indicates the threshold at which operators break even, and it shows that, while a client in the 75th percentile of call volume pays off the phone in the seventh month, the median client is still paying off the phone at nine months. We have few phones in our sample with a duration of ownership longer than this, hence we should not expect to see any strong income effects of the phones on the basis of simple profit calculations. Clearly, this has to do with a low average usage of the phones: 15,000 seconds per month sounds like a substantial figure, but this translates into an average monthly airtime use of only 4 hours and 15 minutes (a 40-hour per week business is open approximately 170 hours per month).

Household Level

Turning first to selection effects, Table 4 shows clearly that entrepreneurs who had more businesses, businesses with more clients, higher revenues, and larger business stocks were more likely to take the phones. Using pre-treatment data to make the comparison, the average person who would eventually get a phone had three times the costs, twice the revenue, and more than twice as many customers daily as those who would not eventually get phones. Baseline owners of shops, restaurants, and bars are the most likely to receive phones, and farmers are the least likely.

The relatively small size and low profits of VP enterprises should temper expectations for dramatic impacts on outcomes such as consumption or school enrollment at the household level. Indeed, Table 4 shows such impacts to be non-existent. Results not reported in Table 4 look at each element of the consumption survey separately, as well as at outcomes such as electricity in the home and type of home construction, yet still we find no impacts. The strongest impact seen at the household level echoes the result from the enterprise-level analysis: a dramatic increase in labor time across all businesses, with no corresponding change in profits or the number of employees. The increase in the number of household enterprises is statistically indistinguishable from one, implying that the VP enterprises do not "crowd out" other businesses (had this coefficient been less than one, it would imply that other enterprises were being *closed* as a result of the initiation of a VP business). The amount of debt retired by VPOs is roughly twice that of non-VPOs, illustrating the heavier loan burden and the rapid repayment schedule associated with the operation of a VP enterprise.

The result in labor time is so dramatic as to warrant some additional discussion. First, what it means to be "working" when operating a VP business may be simply that the phone is on, and the operator is

"on call" (the average VPO reports putting 60 hours of work a week into the VP business alone). Second, this result will be driven in part by double-counting, since a major benefit of the VP business is that it can be run jointly with a retail or service business. The possibility of double-counting makes calculation of impacts on implicit wages difficult, but clearly, implicit wages have fallen in VPO households.[9] The overall effect seen in VP and other household enterprises is that operators graduate from running 1.25 businesses, into which they put 28 hours each, to running two businesses, into which they put an average of 50 hours each. The VPO, therefore, far from "crowding out" time in other enterprises, actually "crowds it in." The magnitude of this result falls if we trim outliers, but it remains strongly significant with any reasonable degree of trimming. Figure 3 illustrates this impact in a different way, showing the histogram of total hours worked in VP household enterprises in both the baseline (before they had a phone) and in the follow-up (once they had the phone).

Given the fact that households that would become VPOs were operating larger enterprises in the baseline anyway, we may wonder whether mean reversion is preventing us from seeing an impact. That is, if bigger enterprises would have grown more slowly between baseline and follow-up in the absence of the VP program, our estimates of the "impact" of the phones would be biased *downward*. We check for this by regressing the change in enterprise size on the baseline enterprise size in the control group (where no treatment effects are present), and find that, indeed, such mean reversion is present. We therefore run multivariate impact regressions, controlling for baseline enterprise size in a DID regression. These results (not reported) show that the apparent impacts of the VP on household entrepreneurial outcomes are improved when we control for baseline characteristics (particularly imputed profit, equal to revenues minus costs); in no case, though, do they become statistically significant.

Targeting and Market Analysis

Which Types of Communities and VPOs Are the Most Attractive Markets?

The central targeting question for the VP program relates to the kinds of entrepreneurs, communities, and locations that are the best market for their product. An easy way to answer this is to look at phone usage across the many characteristics captured in our data, and to then examine which attributes correlate with heavy airtime usage.

The results of this analysis found surprisingly few significant characteristics. Perhaps because all our sampled communities were pre-identified as "good" markets (and therefore, we have insufficient variation in the sample), we have found few household or community characteristics correlated with the usage per phone.[10]

Perhaps the most obvious characteristic that we would expect to drive the intensity of phone usage would be the quality of pre-existing mobile coverage (only four communities had landline access as of the baseline, so we focus on mobile access). This is naturally proxied for by distance to the closest MTNR tower. We might expect an inverted-U relationship, whereby those very close to towers see little usage because non-VP mobiles are ubiquitous, and those too far from mobile towers are sufficiently isolated as to have less demand for telephony.

We examine this relationship by regressing the number of calls per VPO per month on the distance from the closest MTNR tower and the square of this distance. While it is true that every VPO with more than 1,200 calls per month lies in the intermediate distance range, the quadratic term is not statistically significant. In the end, we find less evidence of a direct impact of distance from mobile infrastructure on call volume than anticipated.

Do Profits Decay over Time?

One straightforward question that can be asked with the profit data calculated from the MTNR call records is whether we observe "decay" in profits over the course of time as the cell phone network

9. To check this, we calculated implicit wages in two ways: First, we took the total profits across household enterprises and divided them by the number of hours worked by household members in these enterprises. This method assumes no double counting. We then assumed perfect double counting, and instead calculated implicit wages dividing the sum of enterprise profits by the maximum number of hours worked in any enterprise. Both of these measures show a significantly negative DID impact.

10. These results are not presented here, but they are available on request.

Table 4. Household-Level Analysis.

	Baseline Outcome:			Follow-up Outcome:			
Total for Entire Household of:	**Non-VPOs**	**VPOs**	**T-stat on Baseline Difference**	**Non-VPOs**	**VPOs**	**Difference in Differences**	**T-stat on Difference in Differences**
Annual Expenditures per capita	236.196	197.567	(0.81)	246.245	261.488	53.872	(0.68)
# times per week meat is consumed	0.859	0.980	(0.74)	1.250	1.490	0.119	(0.44)
Child enrollment rate	0.795	0.820	(0.40)	0.814	0.800	−0.039	(0.44)
Number of Household Enterprises	0.851	1.266	(3.88)**	0.644	2.001	0.942	(6.46)**
Total Costs per month	161.592	443.848	(2.46)*	108.605	246.289	−144.572	(1.15)
Total Revenue per month	226.685	440.619	(2.64)**	160.305	388.831	14.592	(0.14)
Self-reported monthly profits	50.614	86.740	(1.78)	38.959	68.249	−6.836	(0.27)
Total value of business stock	163.250	461.693	(3.16)**	269.002	285.269	−282.176	(1.03)
# clients on a good day	26.975	64.082	(3.81)**	23.809	70.429	9.513	(0.66)
# paid employees	1.095	1.489	(1.21)	1.211	1.183	−0.422	(0.46)
Total hours across all businesses worked/week	36.794	37.954	(0.34)	47.571	117.563	68.832	(8.27)**
Size of current loan	155.435	251.937	(2.04)*	333.945	418.733	−11.714	(0.12)
Amount paid on current loan	128.478	159.274	(0.71)	259.482	517.892	227.614	(2.04)*

*Absolute values of t statistics are in parentheses; * significant at 5%; ** significant at 1%. All currency amounts are in US$.*
The baseline comparisons are conducted on 290 households; the DID has 580 panel observations.

Figure 3. Hours Worked by Operators per Enterprise/Week, Baseline vs. Follow-Up.

expands. There is a technical issue in calculating these profits, because the pay structure gives smaller payments to operators after six months (the expected duration of loan servicing). This causes the apparent airtime profit to fall discretely at seven months (see Figure 2). This can be dealt with in a simple way by holding the fee structure constant at the rate used in the first six months, in which case the effects of this billing change are eliminated. With this correction made, Figure 4 shows a box-and-whisker plot of revenues to the VPO according to the amount of months the phone has been operating, and it shows no decay over the course of our (admittedly short) time period. While one would want a substantially longer period of observation to make predictions about the future, at least in these early nine months of the Rwanda VP program, there has been no decay in profits over time.

Saturation Effects Between VP Businesses

One area in which local-level characteristics appear to have important impacts on call volume is the "congestion" effect generated by putting too many VPOs in too small of a local area. Since we have the latitude/longitude coordinates of the VPOs, we can examine the density of VPOs to see whether having many VPOs near to each other exerts a "poaching" effect on the client base. (This question is related to the size of the "catchment area" from which a VPO draws clients; if the client base is extremely local, then we expect sharp congestion effects with immediate co-location).

Figure 5 plots the raw relationship between VPO profit and the local saturation of VPOs. We see that having two additional VPOs within five kilometers appears to have no effect on average profits, but

Table 5. Community-Level Determinants of VP Profitability.

	Average Monthly Profit	Number of Calls Per Month
How many months VP has been operating	−1.310	−40.727
	(1.01)	(1.82)
Km to closest MTN tower	0.001	0.013
	(1.68)	(1.55)
Km to closest paved road	−0.001	−0.012
	(1.90)	(2.08)*
Number of VPOs within 5km	−6.180	−112.843
	(2.23)*	(2.36)*
Distance to closest other VPO	−0.002	−0.032
	(2.00)*	(2.08)*

*Absolute values of t statistics are in parentheses; * significant at 5%; ** significant at 1%. Regressions are run on the 85 VPOs for whom all location data are present.*

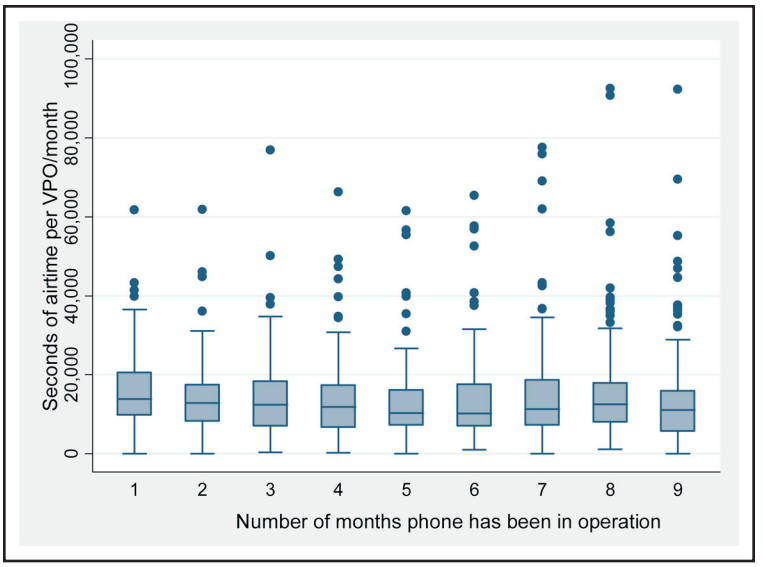

Figure 4. Airtime Usage, by Months Phone Has Been in Operation.

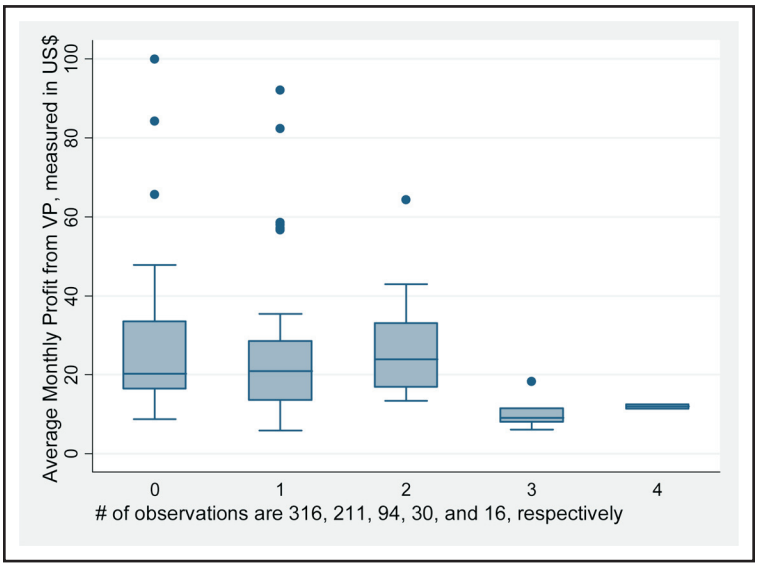

Figure 5. VPO Profits by Number of Other VPOs Within 5 Km.

three or more VPOs results in extremely low averages. The interpretation of this relationship is complicated by the fact that, while being "remote" indicates little competition, it may also indicate an isolated or dispersed market. As a way of trying to disentangle these two effects, Table 5 controls in a linear fashion for the distance to the nearest VPO (which we argue proxies for remoteness) and the number of VPOs within five kilometers (competition). The effects on these two measures are opposite: linear distance from a VPO is a bad thing (remoteness), but it is conditional on this relationship, as having more VPOs located in your immediate areas is also a bad thing. Thus, we see strong evidence of congestion effects in the data.

Does Pre-Existing Telephony Alter the Impact of Village Phone?

A final source of heterogeneity that could be important in mediating the impact of VP is the preexistence of telephony in the community. We might imagine that sharper impacts would be found when VP is introduced into communities that had no prior access to telephones (mobile or landline). To investigate this, we use an interaction analysis to test for differential impacts of the treatment in communities with no baseline access to phones.

The overall decrease in costs and increase in hours worked are both found to be larger in communities where no telephonic access existed. However, consumption in operators' households and profits in their businesses are not found to be significantly different, and since there is no overall effect on these central outcomes, we conclude that the lack of impact is common both to locations with and without access to phones as of baseline.

Conclusion

This paper presents the results of the 2006–2007 study of the Rwanda Village Phone product. We find a substantial improvement in access to telecommunications (reported local access, travel times, use of phones to pass news, and farmers arranging for their own transport to markets), but little evidence that this access translates into improved final outcomes, such as prices received by farmers or opera-

Table 6. Differential Impact in Communities Without Baseline Phone Access.

			Outcome:		
	Expenditures per capita	**Business costs**	**Business revenue**	**Business profit**	**Hours worked**
Round 2 * Treatment	53.087	−144.572	14.592	−6.836	68.867
	(0.67)	(1.16)	(0.14)	(0.27)	(8.30)**
No previous phone access	11.56	−51.306	−19.216	−13.953	−4.282
	(0.36)	(1.00)	(0.46)	(1.33)	(1.06)
No access * Round 2 * Treatment	−60.488	−321.457	−72.073	−31.815	17.539
	(0.75)	(2.56)*	(0.70)	(1.24)	(2.10)*
Round 2	10.057	−52.987	−66.38	−11.655	10.716
	(0.31)	(1.03)	(1.58)	(1.11)	(2.63)**
Treated ever	−65.692	149.536	184.936	23.864	8.824
	(1.00)	(1.43)	(2.15)*	(1.12)	(1.27)
Constant	242.29	134.555	216.559	43.261	34.68
	(8.49)**	(2.97)**	(5.83)**	(4.68)**	(10.27)**
Observations	566	580	580	580	379
R-squared	0	0.04	0.04	0.02	0.37

*Absolute values of t statistics are in parentheses; * significant at 5%; ** significant at 1%.*

tor income. In all, the results may be surprising for the lack of these broad final impacts. While some of the survey data are "noisy" (which would result in the inability to find statistical significance even for large impacts), we fail to find impacts even for many precisely measured outcomes.

The failure of the randomization of the rollout of Village Phone undermines our confidence in the cleanliness with which these impacts are measured. It is likely that groups with repayment problems were less likely to receive phones, and that operators with larger businesses in more attractive markets were more likely to receive them. Both of these factors, however, would lead us to believe that the results presented here are *overestimates* of the real effects, thus this does not seem a likely explanation for the lack of observed impacts. Nevertheless, it is possible that since the areas receiving phones were more likely to have been entrepreneurial centers as of baseline, markets were already functioning well there, and thus they were predisposed to see smaller improvements, even in the absence of the treatment.

At the operator level, the most likely explanation for the lack of individual-level outcomes is the low average phone usage found in our sample. This implies that the median operator earns only about 70% of the total amount due on their loan during the six-month repayment period in Rwanda. If we compare the airtime usage figures to other Village Phone programs in Africa, we see that, indeed, airtime usage is lowest in Rwanda, where the average operator has 8.8 minutes per day of outgoing calls, compared to Grameen data for Cameroon, where that figure is 10.5 minutes, or Uganda, where it is 13 minutes. Therefore, the profits realized by operator households are low both in an absolute sense (compared to the term of the loan) and in a relative sense.

The Grameen Foundation has already responded to this problem in Rwanda, encouraging collaborating MFIs to extend the repayment period for MFI financing from six months to one year, and increasing the margins realized by VPOs after the loan is repaid. In the VP program in Cameroon, changes to the pricing and repayment structure have decreased the break-even daily airtime usage from 14 minutes per day to only six minutes, meaning that one would expect substantially stronger household benefits to operators from the product currently in the field than the product studied in this paper.

At the community level, the relatively small impacts may best be explained by the pre-existence of a similar product (Tuvugane), which is aimed at a

similar segment of the market and shares an almost identical retail pricing structure. The relative ubiquity of Tuvugane in Rwanda suggests that the welfare gains that we expect to accompany the creation of widespread, low-cost access to telephony at the village level had already occurred as of our baseline. Indeed, it is noteworthy that, despite the significant 20% decrease in the number of local entrepreneurs reporting Tuvugane as the closest phone, over 60% of those in communities with Village Phone still report Tuvugane as the closest phone in the follow-up. This, compounded with the small size and relatively good transportation infrastructure of Rwanda, seems to have precluded the sharp impacts of telephony seen in studies such as Jensen (2007). Again, this suggests that the impact of Village Phone at a community level may be larger when it is introduced in one of the many African countries without a similar product already on the market.

Nonetheless, the communities that receive Village Phone do see some demonstrable benefits. Most striking are the improvements in phone access, travel times, and travel distances reported by local entrepreneurs. There is intriguing (if weaker) evidence of a shift toward the use of telephones to transmit news between villages, to arrange for transport of harvests to market, and to contact the police. We therefore find significant evidence of improvements in the intermediate goal of access to telephony, but not in the ultimate outcomes such as incomes or prices received by farmers. In conclusion, airtime usage in Rwanda is sufficiently low as to prevent the VP business from being very profitable for operators. However, communities see both real improvements in telecommunications access and some shifts in local-level behavior arising from this improved access. ■

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Appendixes

Table A1. Basic Comparison of Client-Level Characteristics Across MFIs.

Baseline Outcome:	CARE	URWEGO	VISION FINANCE
Annual expenditures per capita	195.48	268.10	278.57
# times per week meat is consumed	0.66	1.11	1.20
Fraction with electricity	0.01	0.18	0.20
Fraction homeowners	0.92	0.82	0.78
Total value of non-business assets	114.03	472.80	265.04
Annual wages by all household members	191.16	260.99	371.47
Enrollment rate for girls	0.80	0.87	0.85
Enrollment rate for boys	0.81	0.86	0.74
Fraction with landline phone	0.01	0.03	0.02
Fraction with mobile phone	0.17	0.50	0.48
Number of household enterprises	0.67	1.47	1.15
Total costs per month	66.55	295.42	446.73
Total revenue per month	114.95	485.95	449.70
Self-reported monthly profits	26.97	87.47	100.54
Total value of business stock	98.98	457.91	327.14
Purchases of new capital equipment past month	6.15	70.24	183.54
Liquidation of capital equipment past month	1.04	2.00	1.77
# clients on a good day	13.23	52.24	63.49
# clients on a bad day	5.99	20.50	24.35
# paid employees	0.77	2.24	1.44
Total hours across all businesses worked/week	31.66	39.16	42.14
Size of current loan	99.40	323.52	243.67
Amount paid on current loan	91.48	258.20	159.26
Total household savings	418.23	217.48	541.35
Fraction of individuals who received phones	0.11	0.32	0.22

All currency amounts are in U.S. dollars.

Determinants of Selection

Table A2. Household Comparison of Baseline Characteristics (Are those who will receive phones different from those who will not?).

Baseline (untreated) Outcome:	Non-VPOs	VPOs	T-stat on difference
Annual expenditures per capita	236.196	197.567	(0.81)
# times per week meat is consumed	0.859	0.980	(0.74)
Fraction with electricity	0.096	0.063	(0.73)
Fraction homeowners	0.867	0.837	(0.56)
Total value of non-business assets	176.707	349.036	(1.62)
Annual wages by all household members	259.020	228.010	(0.20)
Enrollment rate for girls	0.822	0.840	(0.29)
Enrollment rate for boys	0.795	0.820	(0.40)
Fraction with landline phone	0.012	0.040	(1.39)
Fraction with mobile phone	0.290	0.367	(1.07)
Number of household enterprises	0.851	1.266	(3.88)**
Total costs per month	161.592	443.848	(2.46)*
Total revenue per month	226.685	440.619	(2.64)**
Self-reported monthly profits	50.614	86.740	(1.78)
Total value of business stock	163.250	461.693	(3.16)**
Purchases of new capital equipment past month	42.392	188.945	(1.84)
Liquidation of capital equipment past month	1.266	1.939	(3.47)**
# clients on a good day	26.975	64.082	(3.81)**
# clients on a bad day	11.216	23.755	(2.94)**
# paid employees	1.095	1.489	(1.21)
Total hours across all businesses worked/week	36.794	37.954	(0.34)
Size of current loan	155.435	251.937	(2.04)*
Amount paid on current loan	128.478	159.274	(0.71)
Total household savings	441.087	366.198	(0.25

*Absolute values of t statistics in parentheses; * significant at 5%; ** significant at 1%.*
All currency amounts are in U.S. dollars.

Table A3. Community-Level Comparison of Baseline Characteristics 1 (Are communities that will receive phones different from those that will not?)

Baseline (untreated) Outcome:	Non-VPOs	VPOs	T-stat on difference
Phone call from this center possible	0.636	0.512	(1.92)
# of different places to make call in this center	2.229	2.510	(0.37)
Possible to buy airtime cards in this center	0.468	0.519	(0.63)
Landline connections in this cell	0.171	0.184	(0.23)
# of people with landline phones in this cell	1.80	1.577	(0.19)
# of people with mobile phones in this cell	0.920	0.897	(0.52)
Travel time to make a phone call (minutes)	43.218	56.589	(1.52)
That phone is Tuvugane	0.799	0.862	(1.34)
That phone is landline	0.209	0.116	(1.95)
Distance to reach a Tuvugane phone	48.028	54.061	(0.73)

Table A3. (Continued)

Baseline (untreated) Outcome:	Non-VPOs	VPOs	T-stat on difference
Center on a paved road	0.226	0.190	(0.68)
If not, distance to nearest paved road	116.596	129.672	(1.03)
Travel time to Kigali	191.733	220.640	(1.60)
Travel cost to Kigali	1,348.391	1,442.442	(0.76)
Electricity in this sector	0.309	0.306	(0.05)
Market meets in this center	0.325	0.483	(2.46)*
If not, distance to nearest market	48.742	41.189	(1.29)
How often does nearest market meet	1.885	2.105	(1.24)
# of people opening new business in last year	3.078	3.418	(0.65)
Most common method of passing news	1.478	1.651	(1.98)*
Amount spent on phone calls for business transport	5.029	6.701	(1.11)
Amount spent on phone calls for price inquiries	3.582	3.831	(0.28)
Amount spent on phone calls for market availability	3.389	4.044	(0.49)
Amount spent on phone calls for emergencies	1.572	1.414	(0.47)
Amount spent on phone calls arranging remittences	2.259	2.904	(0.63)
Amount spent on phone calls for social purposes	3.555	3.723	(0.32)
Amount spent on phone calls	9.650	9.471	(0.10)
Surveyed community member owns phone	0.483	0.477	(0.09)
Price charged for friend phone use	0.682	0.595	(0.63)
Price charged for landline phone use	0.614	0.470	(0.74)
Price charged for mobile phone use	0.614	0.470	(0.74)
Price charged for Tuvugane use	0.083	0.137	(0.72)
Price charged for Tel'imbere use	0.065	0.028	(1.08)
Made international calls in last 6 months	0.218	0.206	(0.22)
Number of times household member ill last 6 months	3.653	4.230	(1.30)
# of times HH member been to health clinic 6 months	3.022	3.576	(1.38)
Used phone to contact clinic	0.063	0.068	(0.16)
# of times HH member been hospitalized in 6 months	1.708	1.455	(1.43)
Used phone to contact hospital	1.980	1.979	(0.07)
# of times HH member purchased medication 6 months	3.364	4.710	(1.83)
Used phone to purchase medication	0.025	0.034	(0.40)
Distance to nearest health care facility	52.028	50.043	(0.34)
Nearest health care facility has phone	1.564	1.517	(0.80)
# of crimes in this center in last year	2.349	3.982	(1.80)
How many involved police	1.588	2.616	(1.51)
How were police contacted 1–4	2.091	2.187	(0.55)
If needed, would contact police by landline	0.094	0.050	(1.37)
If needed, would contact police by mobile	0.534	0.400	(2.10)*
If needed, would travel to police station	0.653	0.667	(0.22)

Table A4. Community-Level Comparison of Baseline Characteristics 2
(Are communities that will receive phones different from those that will not?)

Baseline (untreated) Outcome:	Non-VPOs	VPOs	T-stat on difference
Local cooperative in center	0.346	0.414	(1.07)
# of traders for a given commodity	11.114	13.549	(1.16)
# of traders that run business out of this center	5.926	6.684	(0.80)
Producers arrange own transport	0.467	0.391	(1.09)
Producers arrange own transport more than a year ago	0.262	0.269	(0.13)
Cell member used fertilizer or chemicals on this crop	0.709	0.777	(1.25)
Producers know the Kigali price before selling	0.177	0.174	(0.06)
Highest price received by farmers last harvest	0.324	0.304	(0.43)
Lowest price received by farmers last harvest	0.225	0.229	(0.14)
Commodity for sale at local market	0.991	0.994	(0.38)
Highest market price over last year	0.379	0.348	(0.65)
Lowest market price over last year	0.271	0.270	(0.03)
Beef in center	0.646	0.679	(0.55)
Beef in market	1.077	1.128	(1.26)
High price last 6 months	1.500	1.546	(0.89)
Low price last 6 months	1.330	1.356	(0.54)
Airtime cards in center	0.415	0.459	(0.69)
Airtime cards in market	1.278	1.279	(0.02)
High price last 6 months	0.928	0.932	(0.57)
Low price last 6 months	0.917	0.917	(.)
Soda in center	0.966	0.988	(1.36)
Soda in market	1.038	1.023	(0.83)
High price last 6 months	0.348	0.348	(0.11)
Low price last 6 months	0.300	0.306	(1.08)
Condom in center	0.882	0.919	(1.01)
Condom in market	1.068	1.086	(0.56)
High price last 6 months	0.098	0.094	().69)
Low price last 6 months	0.087	0.082	(1.13)

Impacts

Table A5. Household Impacts of Phones, Difference-in-Differences (Are the changes for those who received phones different than the changes for those who did not?).

Magnitude of the Change in Outcome	Average change 2006–2007 among Non-VPOs	Average change 2006–2007 among VPOs	Difference in Differences, VPOs vs. non-VPOs	T-stat on Difference in Difference
Annual expenditures per capita	10.049	63.921	53.872	(0.68)
# times per week meat is consumed	0.391	0.510	0.119	(0.44)
Fraction with electricity	−0.011	0.081	0.092	(1.42)
Fraction homeowners	0.095	0.080	−0.015	(0.23)
Total value of non-business assets	134.443	−59.539	−193.982	(0.79)
Annual wages by all household members	−19.252	219.116	238.368	(1.18)
Enrollment rate for girls	0.026	0.092	0.066	(0.79)
Enrollment rate for boys	0.019	−0.020	−0.039	(0.44)
Fraction with landline phone	0.000	−0.041	−0.041	(1.57)
Fraction with mobile phone	0.086	0.224	0.138	(1.32)
Number of household enterprises	−0.207	0.735	0.942	(6.46)**
Total costs per month	−52.987	−197.559	−144.572	(1.15)
Total revenue per month	−66.380	−51.788	14.592	(0.14)
Self-reported monthly profits	−11.655	−18.491	−6.836	(0.27)
Total value of business stock	105.752	−176.424	−282.176	(1.03)
Purchases of new capital equipment past month	−42.255	−179.209	−136.954	(1.71)
Liquidation of capital equipment past month	414.548	0.551	−413.997	(0.45)
# clients on a good day	−3.166	6.347	9.513	(0.66)
# clients on a bad day	−1.606	4.959	6.565	(1.06)
# paid employees	0.116	−0.306	−0.422	(0.46)
Total hours across all businesses worked/week	10.777	79.609	68.832	(8.27)**
Size of current loan	178.510	166.796	−11.714	(0.12)
Amount paid on current loan	131.004	358.618	227.614	(2.04)*
Total household savings	−89.105	−97.888	−8.783	(0.03)

*Absolute value of t statistics in parentheses; * significant at 5%; ** significant at 1%.*
All currency amounts are in U.S. dollars.

Table A6. Household Impacts, Using Control Variables:
(Controlling for baseline characteristics, are the changes for those who received phones different than the changes for those who did not?).

	Outcome variable:							
Control Variable:	**Self-Reported Profit**	**Imputed Profit**	**Revenue**	**Costs**	**Expenditures per Capita**	**Hours Worked**	**Employees**	**Number of businesses**
Impact of receiving VP	−8.914	217.759	77.309	−140.450	38.397	69.972	−0.316	1.048
	(0.31)	(1.41)	(0.70)	(0.99)	(0.47)	(7.89)**	(0.29)	(7.10)**
Baseline business size	0.044	−0.054	0.127	0.180	0.129	0.001	0.000	0.000
	(5.26)**	(1.16)	(3.85)**	(4.29)**	(5.29)**	(0.41)	(0.56)	(1.77)
Baseline hours worked	0.171	2.517	1.783	−0.734	0.614	0.558	0.018	0.009
	(0.73)	(1.98)*	(1.96)	(0.63)	(0.91)	(6.28)**	(2.04)*	(7.20)**
Baseline number of clients	0.383	−0.267	2.040	2.307	0.662	0.005	0.002	0.001
	(3.80)**	(0.49)	(5.19)**	(4.60)**	(2.28)*	(0.17)	(0.42)	(2.17)*
Distance to MTN cell phone tower (km)	0.662	−3.599	0.835	4.434	−7.340	0.157	0.103	0.009
	(0.58)	(0.58)	(0.19)	(0.77)	(2.22)*	(0.38)	(2.36)*	(1.55)
Distance to road (km)	−2.706	2.816	−9.028	−11.844	−0.827	−0.098	−0.052	−0.017
	(2.64)**	(0.50)	(2.26)*	(2.32)*	(0.28)	(0.27)	(1.35)	(3.15)**
No. of dependents in HH	−0.228	16.838	−1.126	−17.964	−22.402	0.223	0.045	−0.007
	(0.11)	(1.52)	(0.14)	(1.77)	(3.81)**	(0.31)	(0.58)	(0.68)
HH head age	5.417	−8.415	18.820	27.235	19.538	−0.835	0.181	0.027
	(1.64)	(0.47)	(1.46)	(1.65)	(2.04)*	(0.62)	(1.44)	(1.57)
HH head age squared	−0.066	0.063	−0.253	−0.316	−0.207	0.009	−0.002	0.000
	(1.60)	(0.28)	(1.56)	(1.53)	(1.73)	(0.50)	(1.46)	(1.80)
Dummy for followup	−10.050	−10.728	−69.948	−59.220	5.587	14.614	0.202	−0.189
	(0.91)	(0.18)	(1.62)	(1.07)	(0.18)	(3.43)**	(0.48)	(3.29)**
Dummy for ever treated	11.386	−102.73	83.204	185.933	−102.333	−0.532	−0.051	0.233
	(0.56)	(0.92)	(1.05)	(1.83)	(1.74)	(0.08)	(0.07)	(2.19)*
Constant	−61.174	153.738	−147.838	−301.576	−29.654	31.77	−3.541	0.226
	(0.98)	(0.45)	(0.61)	(0.97)	(0.17)	(1.26)	(1.50)	(0.70)
Observations	502	502	502	502	500	320	502	502
R-squared	0.14	0.02	0.18	0.13	0.13	0.43	0.03	0.37

Absolute values of t-statistics are in parentheses.

Table A7. Community-Level Impacts 1
(Are the changes in communities that received phones different from those that do not?)

Magnitude of the Change in Outcome:	Average change 2006–2007 among Non-VPOs	Average change 2006–2007 among VPOs	Difference in Differences, VPOs vs. non-VPOs	T-stat on Difference in Difference
Phone call from this center possible	0.173	0.395	0.222	(3.17)**
# of different places to make call in this center	0.428	2.054	1.626	(1.14)
Possible to buy airtime cards in this center	0.236	0.187	−0.049	(0.53)
Land line connections in this cell	−0.066	−0.023	0.043	(0.63)
# of people with land line phones in this cell	−0.110	−0.369	−0.259	(0.19)
# of people with mobile phones in this cell	0.035	0.092	0.057	(1.17)
Distance to make a phone call	−16.131	−46.021	−29.890	(3.02)**
That phone is Tuvugane	−0.093	−0.246	−0.153	(2.00)*
That phone is Landline	−0.092	0.011	0.103	(1.71)
Distance to reach a Tuvugane phone	−30.834	−38.702	−7.868	(0.79)
Center on a paved road	0.047	0.099	0.052	(0.96)
If not, distance to nearest paved road	4.919	−21.790	−26.709	(1.87)
Travel time to Kigali	153.811	24.696	−129.115	(1.66)
Travel cost to Kigali	36.448	−118.060	−154.508	(1.10)
Electricity in this sector	0.011	0.086	0.075	(1.52)
Market meets in this center	0.153	0.033	−0.120	(1.63)
If not, distance to nearest market	2.928	1.858	−1.070	(0.09)
How often does nearest market meet	0.078	0.073	−0.005	(0.02)
# of people opening new business in last year	0.686	−0.396	−1.082	(1.51)
Most common method of passing news	−0.164	−0.404	−0.240	(2.03)*
Amount spent on phone calls for business transport	−2.189	−4.184	−1.995	(1.15)
Amount spent on phone calls for price inquiries	−1.634	−1.795	−0.161	(0.16)
Amount spent on phone calls for market availability	−1.804	−2.970	−1.166	(0.80)
Amount spent on phone calls for emergencies	−1.249	−1.046	0.203	(0.50)
Amount spent on phone calls arranging remittences	−1.377	−1.515	−0.138	(0.13)
Amount spent on phone calls for social purposes	−0.002	0.844	0.846	(0.83)
Amount spent on phone calls	0.912	1.400	0.488	(0.15)

Table A7. (Continued)

Magnitude of the Change in Outcome:	Average change 2006–2007 among Non-VPOs	Average change 2006–2007 among VPOs	Difference in Differences, VPOs vs. non-VPOs	T-stat on Difference in Difference
Surveyed community member owns phone	0.219	0.257	0.038	(0.49)
Price charged for friend phone use	0.029	−0.314	−0.343	(1.65)
Price charged for landline phone use	−0.122	−0.090	0.032	(0.38)
Price charged for mobile phone use	0.490	0.490	0.000	(.)
Price charged for Tuvugane use	−0.015	−0.023	−0.008	(0.09)
Price charged for Telimbere use	0.024	0.031	0.007	(0.18)
Price charged for other phone use	0.004	0.004	0.000	(.)
Made international calls in last 6 months	0.102	0.135	0.033	(0.40)
Number of times household member ill last 6 months	−0.855	−1.819	−0.964	(1.87)
# of times HH member been to health clinic 6 months	−0.471	−1.413	−0.942	(2.09)*
Used phone to contact clinic	0.099	0.064	−0.035	(0.66)
# of times HH member been hospitalized in 6 months	−1.225	−1.107	0.118	(0.59)
Used phone to contact hospital	−0.022	−0.058	−0.036	(1.12)
# of times HH member purchased medication 6 mos.	−1.014	−2.915	−1.901	(2.53)*
Used phone to purchase medication	0.022	0.016	−0.006	(0.16)
Distance to nearest health care facility	−1.670	−6.923	−5.253	(0.77)
Nearest health care facility has phone	0.172	0.036	−0.136	(1.51)
# of crimes in this center in last year	1.048	−1.229	2.277	(1.94)
How many involved police	1.209	−0.498	−1.707	(1.62)
How were police contacted 1–4	−0.390	−0.278	0.112	(0.45)
If needed, would contact police by landline	0.461	0.431	−0.030	(0.26)
If needed, would contact police by mobile	0.399	0.525	0.126	(1.72)
If needed, would travel to police station	0.167	0.023	−0.144	(1.33)

Table A8. Community-Level Impacts 2
(Are the changes in communities that received phones different from those that do not?)

Magnitude of the Change in Outcome:	Average change 2006–2007 among Non-VPOs	Average change 2006–2007 among VPOs	Difference in Differences, VPOs vs. non-VPOs	T-stat on Difference in Difference
Local cooperative in center	−0.066	−0.109	−0.043	(0.49)
# of traders for a given commodity	1.701	−0.240	−1.941	(0.61)
# of traders that run business out of this center	1.746	2.986	1.240	(0.60)
Producers arrange own transport	−0.049	0.134	0.183	(1.98)*
Producers arrange own transport more than a year ago	−0.066	−0.039	0.027	(0.32)
Cell member used fertilizer or chemicals on this crop	−0.106	−0.058	0.048	(0.60)
Producers know the Kigali price before selling	−0.002	0.068	0.070	(0.96)
Highest price received by farmers last harvest	−0.019	0.011	0.030	(0.65)
Lowest price received by farmers last harvest	0.016	0.029	0.013	(0.41)
Commodity for sale at local market	−0.021	−0.012	0.009	(0.69)
Highest market price over last year	−0.035	0.003	0.038	(0.80)
Lowest market price over last year	0.002	0.016	0.014	(0.41)
Beef in center	0.056	0.036	−0.020	(0.29)
Beef in market	−0.026	−0.084	−0.058	(1.26)
High price last 6 months	0.072	0.039	−0.033	(0.71)
Low price last 6 months	0.048	0.045	−0.003	(0.07)
Airtime cards in center	0.298	0.337	0.039	(0.58)
Airtime cards in market	−0.192	−0.182	0.010	(0.17)
High price last 6 months	0.007	0.006	−0.001	(0.10)
Low price last 6 months	0.000	0.000	0.000	(.)
Soda in center	0.014	0.001	−0.013	(0.60)
Soda in market	−0.030	−0.001	0.029	(1.17)
High price last 6 months	0.025	0.026	0.001	(0.17)
Low price last 6 months	0.068	0.062	−0.006	(0.99)
Condom in center	0.028	0.010	−0.018	(0.46)
Condom in market	−0.015	−0.020	−0.005	(0.10)
High price last 6 months	0.011	0.012	0.001	(0.10)
Low price last 6 months	0.016	0.017	0.001	(0.14)

Table A9. Changes in Non-VPO Households
(Do variables that predict treatment lead to differential changes in the controls?)

	Revenue Change	t-stat	Profit Change	t-stat
Able to make a call from this center	−101.348	−1.31	−27.881	−1.9
Number of household enterprises	−7.976	−0.07	−11.74	−0.53
Total costs per month	0.133	−1.39	0.026	−1.41
Total revenue per month	−0.782	(5.03)**	0.001	−0.02
Self-reported monthly profits	−0.902	−1.47	−1.129	(9.69)**
Total value of business stock	0.004	−0.05	0.011	−0.68
Purchases of new capital equipment past month	−0.127	−0.9	−0.007	−0.27
Liquidation of capital equipment past month	−20.599	−0.4	0.697	−0.07
# clients on a good day	−0.564	−0.36	0.091	−0.31
# clients on a bad day	1.604	−0.44	−0.332	−0.48
# paid employees	0.745	−0.03	−0.427	−0.09
Total hours across all businesses worked/week	5.18	(2.45)*	0.307	−0.77
Size of current loan	0.392	−1.13	0.045	−0.68
Amount paid on current loan	−0.183	−0.5	0.012	−0.17
Total household savings	−0.007	−0.35	−0.001	−0.38
Constant	194.491	−1.55	79.216	(3.32)**
Observations	234		234	
R-squared	0.38		0.62	