The Impact of Mobile Phones: Experimental Evidence from the Random Assignment of New Cell Towers*

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Abstract

We present experimental evidence on the economic impacts of mobile phone access. Our results are based on a randomized control trial in the Philippines, through which 14 isolated and previously unconnected villages were randomly assigned to either receive or not receive a new mobile phone tower. Following a pre-analysis plan, we find that the introduction of mobile phones had large and significant impacts on household income and expenditure, particularly for wage workers. Mobile phone access also increased social connections within and between communities. However, there are no consistent impacts on market access, informedness, or subjective well being. In analysis not pre-specified, we find suggestive evidence that the improved economic conditions are driven by increases in migration, remittances, and self-employment.

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1 Introduction

In the past decade, over two billion individuals in developing countries have started using mobile phones for the first time; a further 710 million subscribers are projected to adopt by 2025 (GSMA, 2019). The vast majority of new mobile phone subscribers live in developing countries, with more than half in the Asia Pacific region (GSMA, 2018).

While observers have noted the "transformative" effect that mobile phones have had in developing economies,[1] there is limited empirical evidence on the economic impacts of this transformation (Aker and Mbiti, 2010; Aker and Blumenstock, 2015). Most prior work has focused on the potential for mobile phones to increase the efficiency of agricultural markets (cf. Jensen, 2007; Aker, 2010) and to provide a platform for rudimentary financial services (Suri and Jack, 2016, cf.). But other key margins of impact are unknown, including how phones affect access to non-price information; migration decisions; income, employment, labor market outcomes; and other aspects of well-being. And the existing evidence base relies exclusively on quasi-random variation in the market-driven expansion of mobile phone networks; to date, no experimental evidence exists on the economic impacts of mobile phone access.

This study presents experimental evidence on the economic impact of first-time access to mobile phone networks. The study is based on a two-stage randomized control trial that involved 14 rural and geographically isolated villages in the Aurora province of the Philippines. In the first stage of the RCT, new mobile phone towers were installed in a randomly selected 7 of the 14 villages.[2] The timing and order of tower installation was also randomly assigned. In the second stage of the RCT, individual households were randomly assigned price promotions that reduced the cost of using the network.

[1] See, for instance, "Mobile phones are transforming Africa", *The Economist*, Dec 10, 2016.

[2] The team at the University of the Philippines had sufficient funding to support only 7 towers. In deploying the towers, they leveraged a new, low-cost technology — the Community Cellular Network (CCN) — which provides low-bandwidth GSM coverage at at one-tenth of the cost of traditional mobile towers. The CCN was explicitly designed for rural settings with intermittent power and limited access to technhical support (Heimerl and Brewer, 2010). See Appendix Figure 5.

We conducted baseline interviews (before any towers were installed) and endline interviews (after all towers were installed) with several thousand households in both treated villages (that received towers) and control villages (that did not). In addition to standard socio-demographic and economic questions, these surveys contained detailed modules about the structure of social connections within and between villages. We then use an ANCOVA specification to measure the intention-to-treat (ITT) effect of treatment assignment, controlling for baseline levels of the dependent variable. Since household-level compliance with treatment assignment was not perfect, we also estimate household-level treatment-on-the-treated (TOT) effects, using treatment assignment as an instrument for household-level cellular access, which we measure at endling using special equipment that detects cellular signal strength.[3] Both the ITT and TOT specifications, as well as the set of outcomes that we test and our multiple testing adjustments, were pre-registered in a pre-analysis plan that was filed before endline data were collected.[4]

Our main analysis highlights three key results. First, as expected, we find that households in villages that were assigned to receive a new cell phone tower were significantly more likely to access and use communications technology. We construct a composite index of *communication access* from several survey questions related to mobile phone use, and estimate that this composite index increased by 0.4 standard deviations in treated villages. This effect is driven by an increase of 43 percentage points in households' reported ability to place a call from their dwelling.

Second, we find that treatment assignment significantly increased households' *social connectedness*, as reported by respondents in the detailed network survey questionnaire. A composite index of *local connectedness* increased by 0.19 standard deviations, and a com-

[3]Compliance was imperfect because while no households had cellular access at baseline, some households in control villages received commercial cellular access after the baseline survey was conducted. Similarly, some households in treated villages did not receive a strong signal from the tower (often by purchasing a signal booster that amplified the signal from a distant commercial tower).

[4]Given our small number of clusters, we conduct inference using wild cluster bootstrapped p-values. We also correct for multiple hypothesis testing. These and other details of our analysis are specified below, and all follow our pre-registered pre-analysis plan.

posite index of *long distance connectedness* increased by 0.08 standard deviations. Positive treatment effects can also be seen in many of the questions from which the composite index is formed, including measures of network centrality, communication frequency, and geographic diversity.

Third, and most importantly, we find that the introduction of a new phone tower led to large and statistically significant increases in household income, expenditures, and food security. Our ITT estimates indicate that treatment assignment increased household income by 17 percent (relative to the control mean); increased household expenditures by 10 percent; and increased food security by 13 percent. The TOT estimates are substantially larger. For instance, we estimates that household income increased by 28 percent among complier households.

The remainder of the paper focuses on understanding the mechanism behind these large and robust effects on income, expenditure, and food security. These effects are not easily explained by the other main outcome indices that we pre-registered in our pre-analysis plan. In particular, we do not find evidence that the mobile phone network increased general informedness, disaster preparedness, market access, migration frequency, risk sharing, or subjective well-being. For many of these outcomes, we observe positive effects, but with the exception of disaster preparedness and market access (which are significant at the 10 percent level), the impacts on these margins are not statistically significant after we account for multiple hypothesis testing.

Instead, we find evidence that the improved economic conditions for households in treated villages are mediated by increases in migration, remittances, and self-employment. Bearing in mind that subsequent analysis of mechanisms was not pre-specified, so should be interpreted as suggestive rather than conclusive, we note statistically significant ITT increases in income from employment outside of the village. This increase accounts for roughly one third of the overall income effect, and is driven by treated household members spending more weeks away (as opposed to earning higher wages while away). We also find treated household members spend significantly more time away from their village visiting friends and family in addition to working. Related, we see that net remittance flows into the household were higher in treated villages, and roughly account for one quarter of the overall increase in income. The remaining income effect can be largely attribted to increased income from self-employment within the village.

These experimental estimates of the impact of cellular access relate to a growing literature on the economic impacts of information and communications technologies. Using cross-country regressions, Roller and Waverman (2001) find evidence of a significant positive causal link between telecommunications infrastructure and growth in 21 OECD countries. In a follow-up study, Waverman, Meschi, and Fuss (2005) find that mobile phones had a similarly positive impact on growth in developing countries. However, Straub (2008) and others highlight the econometric challenges to the identification of the effect of infrastructure on output and productivity.

With few exceptions, prior micro-economic studies of the impact of mobile phones have been non-experimental, instead exploiting quasi-random variation the timing of where and when commercial operators choose to provide coverage. This line of work primarily focuses on how mobile phones increase the efficiency of agricultural markets (Jensen, 2007; Muto and Yamano, 2009; Labonne and Chase, 2009; Aker, 2010). Related work on the expansion of mobile money networks has documented the impact that phone-based financial services have had on consumption smoothing in East Africa (Jack and Suri, 2014; Suri and Jack, 2016; Blumenstock, Eagle, and Fafchamps, 2016; Bharadwaj, Jack, and Suri, 2019).

Most closely related to this paper is a recent project that studies the effect of randomly providing smartphones to poor families in Tanzania (Roessler et al., 2020). Consistent with our estimates of the impact of providing village-level phone access, Roessler et al. (2020) estimate that giving a household a smartphone increases per capita consumption by 20%. In an unrelated field experiment in Tanzania, Jeong (2020) experimentally evaluates the impact of an SMS-based messaging app that connects agricultural workers and employers in

Tanzania, and finds large decreases in wage dispersion in villages where the app was available. Finally, Futch and McIntosh (2009) study the impact of providing village phones in Rwanda, but find no evidence of broader welfare impacts. Relative to these field experiments, our treatment is fundamentally different: we study the effect of village-level mobile phone access, and document the wide-ranging economic impact this has on households in those villages.

More broadly, our results speak to the larger policy debate about the role that information and communications technologies can play in rural development. Increasingly, governments seek to mandate 'universal' access to mobile networks (and internet), often through spectrum licenses that require the provision of last-mile connectivity to rural users (GSMA, 2014). Big tech companies including Facebook, Google, and Microsoft are exploring novel approaches to providing mobile connectivity, including via drones, hot air balloons, and white space in the TV spectrum. Consipicuously absent from this debate is a quantitative understanding of the benefits of such connectivity. We hope these empirical results can help seed this debate by providing rigorous estimates of the benefits of providing mobile phone access to otherwise isolated communities.

2 Background

2.1 The Community Cellular Network

The Community Cellular Network was developed by researchers at the University of California, Berkeley (Heimerl and Brewer, 2010). It is an open-source platform (hardware and software) that (i) provides local coverage (to an approximately 500 meter radius) at one-tenth the cost of traditional cellular towers, (ii) is designed for rural settings with intermittent power, and (iii) is intended to be owned and maintained by local community members with modest technical training. Appendix Figure 5 presents a picture of a CCN deployed in one of our project sites.

2.2 Cellular networks in the Philippines and site selection

The Philippines is an archipelago of 7,641 mountainous islands. Approx. 63% of the population subscribes to a cellular network (GSMA, 2018). Working with researchers at the University of the Philippines Diliman (UPD), we identified candidate sites for CCNs in the Philippines—areas that, as of December 2016, lacked cellular coverage but were not so remote as to make the logistics of research infeasible. We identified 14 candidate sites along the west coast of the largest island in the Philippines, Luzon, depicted in Figure 1. All sites are located in the Province of Aurora. Sites are villages, or 'sitios' using the Philippines term for the lowest-level administrative unit, located near or along the coast. Many of the potential sites are located in sea coves only accessible by boat. Typhoons frequently hit Aurora. A typhoon had badly damaged several potential sites just weeks before our first scoping visit to the area. During typhoon season, coves are often inaccessible for multiple days.

The main regional town in the area, Baler, is up to several hours away by bus or boat from the project sites. Travel to Manila requires upwards of 24 hours. Each candidate site was deemed able to support a CCN. CCN base stations transmit to a 500-meter radius, on average after accounting for variance in terrain. Field teams visited all potential sites to verify eligibility (no cellular connection at that time), determine possible logistics, and meet with local government units (LGUs). While no cellphone signal was present in the selected sites, phone communications were not entirely new to households residing in the sites. Through our baseline survey, we find that 67% of household owned a phone and a sim card. However, people needed to travel long distanced to access phone service.

Figure 2 reports the income distribution for households in our control sites at endline.[5] You can see that the median household is living on roughly 1 USD per day.

[5] We did not collect income at baseline due to concerns about turning off respondents to follow-up surveys.

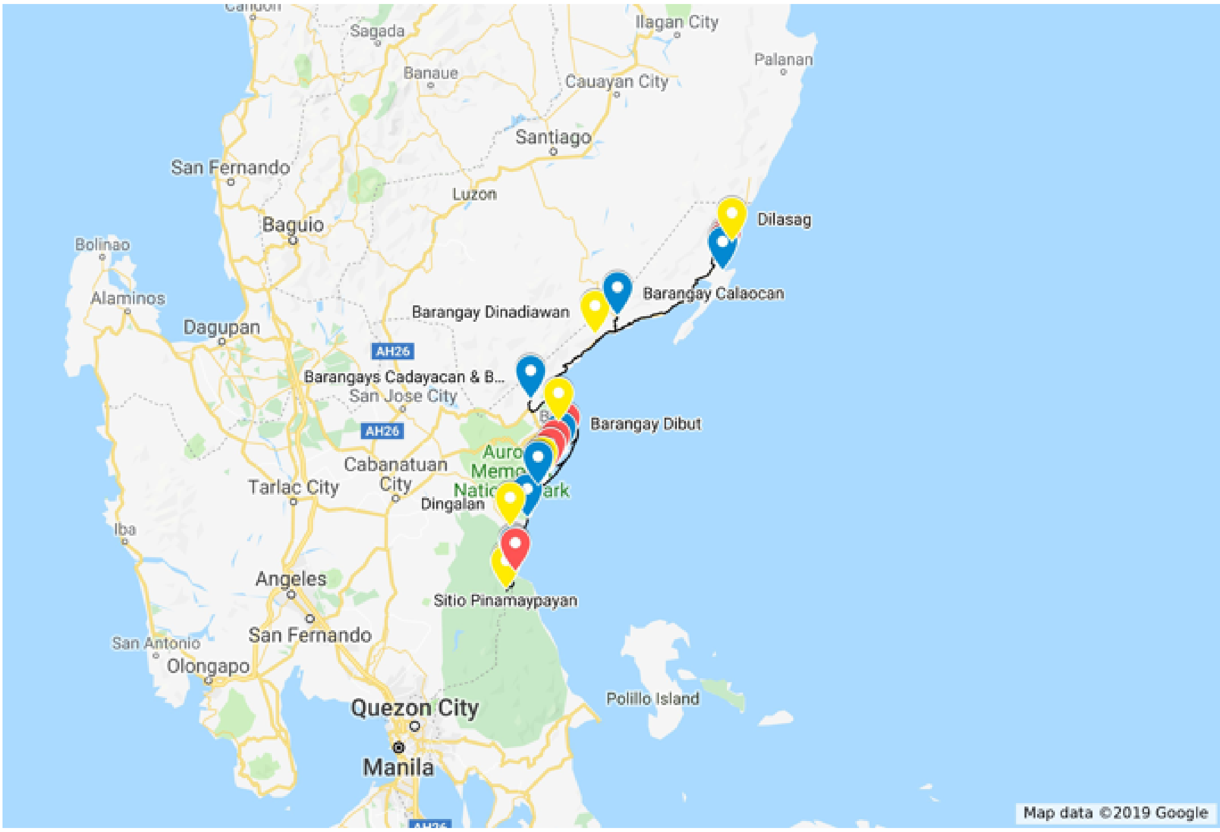

Figure 1: Location of project sites

3 Experimental Design

3.1 Experiment Overview

Our research design consisted of four stages, which are summarized in Table 1. First, we conducted a census of all households in each of the eligible 14 sites. Second, we used a matched-pairs random assignment design to select seven sites that would receive a CCN tower. The University of the Philippines Diliman team led the construction, testing, and network management in each of the seven treatment sites, which were phased in over a 16 month period. Third, we carried out a household-level randomized experiment in all treatment sites in which three promotional treatments were tested: (1) free phone credit, (2) discounts on local calls and text messages, and (3) discounts on long-distance (out-of-network) calls and text messages. We randomly assigned households to one of six experimental groups, as noted

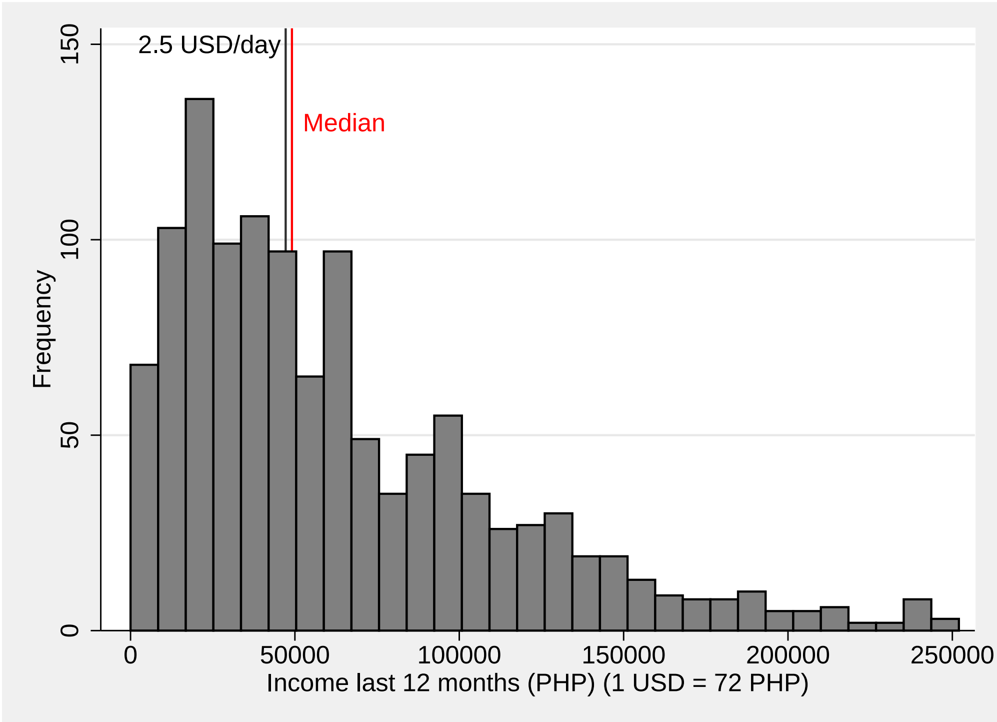

Figure 2: Control household incomes

in the Promotions row of Table 1. Fourth, we conducted endline surveys of all households in the 14 sites.

3.2 Baseline Data Collection

Before the installation of CCN towers, we aimed to complete a full census of each site. To achieve this goal, we leveraged a household listing frame from community health workers and consulted with local leaders to accurately identify all unique households within a site. Across all 14 potential sites, surveyors conducted a total of 2,370 household surveys.[6] While our census surveys were not as detailed as our eventual endline, they included modules about household demographic composition, asset ownership, and economic activity. We collected demographic, education, and labor participation information though a household roster. In

[6]Note, some households were unavailable at the time of the initial baseline. In total, 95% of baseline surveys were conducted before the CCN launch.The remaining 123 baseline surveys were conducted on the day of the launch, only in treatment sites, where we had launch events. All results presented below are robust to exclusion of these 123 households. Results available upon requset.

Table 1: Phases of the Experiment

Phase	Dates	Sample Size
Baseline	Phase 1: 10 sites July-November 2016 Phase 2: 4 sites March-April 2018	N = 14 sites N = 2370 households surveyed N = 6613 adults listed (age 15–107) N = 3351 adult surveys
Implementation	Phased roll out September 2017 - January 2019	N = 7 treatment sites N = 1131 eligible households N = 3064 eligible adults N = 2653 registered accounts
Promotions	Phased roll out September 2018 - March 2019	N = 1131 eligible households N_{T0} = 187 - No promotions N_{T1} = 191 - Free phone credit N_{T3} = 183 - Local discount N_{T3} = 186 - Local discount plus free credit N_{T4} = 191 - Long distance discount N_{T5} = 193 - Long distance discount plus free credit
Endline	May - August 2019	N = 2316 households N = 4113 adult surveys N = 1967 households in panel N = 2475 adults in panel N = 14 community surveys

total, 6,613 adults (15 years or older) were enumerated. All respondents provided voluntary consent to participate in the study.

In each household surveyed, we conducted individual-level surveys with at least one adult per household. Surveyors were instructed to attempt to complete two adult surveys, with priority given to interviews with the head of household and the spouse of the head of the household. All attempts were made to interview one woman and one man per household when at least two members of differing gender were available. The core modules contained within the adult survey were a travel diary and a social network module. In total, 3,351 adult surveys were completed at the time of the baseline. Women comprise 62 percent of the baseline adult survey respondents.

As noted in Table 1, baseline data collection was conducted in two phases. An initial ten sites were identified and surveyed between July and November 2016. Later, in order to increase the number of CCN installations to seven, an additional four sites were identified and surveyed between March and April 2018.

3.3 Randomization and Installation of CCN Sites

3.3.1 Site Selection

Following Greevy et al. (2012), we used Reweighted Mahalanobis distance matching to isolate pairs of sites with similar characteristics. Pairwise distances were calculated using nine site-level characteristics: (i) count of households, (ii) mean household wealth index, (iii) proportion of households that send or receive remittances, (iv) proportion of households that own a cellphone, (v) mean number of social network contacts outside of the barangay, (vi) proportion of households reporting fishing as their main source of income (vii) proportion of adult survey respondents that report receiving information through mobile phone on a monthly or more regular basis (viii) proportion of adults that work or attend school outside of the site, and (ix) municipality where the site is located.

Site characteristics were given equal weight when calculating the Mahalanobis distances.[7]

Since baseline data collection occurred in two phases, we performed the treatment assignment twice. Following Phase 1 of the baseline data collection, we conducted treatment assignment using the initial ten sites surveyed in 2016 (see 3.2). This initial assignment provided four sites to receive CCN tower installations. After Phase 2 of the baseline survey, we re-ran the matching and treatment assignment using the two sites not used in the first treatment assignment plus the four sites surveyed in Phase 2.

Randomization balance is presented in Table 2. We check for balance on 24 variables and find one case of imbalance at 5 percent significance and none additional at 5-10 percent. We take this as evidence that our randomization procedure was properly executed.

3.3.2 CCN Tower Installation

A team of engineers from the University of the Philippines Diliman was responsible for installing, testing, and maintaining the CCNs. The implementation of the CCN was named the VBTS Konekt Network.[8] Barela et al. (2016) provides a thorough description of the VBTS Konekt Network technical design. CCN towers connect to a major Philippine mobile network operator's cellphone towers. We refer to this as the long-distance on-network provider. All calls and texts originating and ending on the VBTS Konekt Network are referred to as local transactions.

We provided the list of selected sites and the ordering of installations to the engineering team at UPD. The UPD team then initiated the process of procuring and transporting VBTS equipment to the treatment sites to install towers. Table 3 provides the dates of installation for each site.

UPD ran network tests to ensure the stability of the network before giving its authorization to launch the tower signal to the customers. In addition, to maximize the likelihood

[7]We used the R package, `nbpMatching`, from Beck, Lu, and Greevy (2016) to calculate distances and perform nonbipartite matching.

[8]VBTS stands for Village Base Transceiver Station.

Table 2: Village randomization balance

	Control sites	Treatment sites	Difference	P-value
Household level variables				
Municipality == San Luis	0.221	0.513	0.292	0.391
	[0.415]	[0.500]	(0.255)	
HH total number of adults	2.864	2.709	-0.155	0.262
	[1.382]	[1.296]	(0.110)	
Rooms in household	1.921	1.786	-0.135	0.339
	[0.857]	[0.809]	(0.101)	
HH owns their land	0.545	0.448	-0.098	0.384
	[0.498]	[0.497]	(0.076)	
HH asset count	2.710	2.239	-0.472	0.584
	[2.220]	[1.980]	(0.515)	
HH has access to electricity	0.813	0.772	-0.041	0.724
	[0.390]	[0.419]	(0.115)	
HH # of cell phones owned	1.462	1.202	-0.260	0.515
	[1.343]	[1.191]	(0.260)	
HH # of sim cards	1.637	1.334	-0.303	0.524
	[1.682]	[1.524]	(0.302)	
HH sent and/or received remittance in last 12 months	0.483	0.441	-0.042	0.809
	[0.500]	[0.497]	(0.080)	
Someone in HH has a bank account	0.166	0.156	-0.010	0.809
	[0.372]	[0.363]	(0.035)	
Fishing is HH's primary income source	0.093	0.242	0.149	0.132
	[0.291]	[0.428]	(0.080)	
Farming is HH's primary income source	0.483	0.346	-0.136	0.023
	[0.500]	[0.476]	(0.051)	
# Observations	1239	1131		
Adult respondent level variables				
Adult respondent is female	0.634	0.651	0.017	0.555
	[0.482]	[0.477]	(0.027)	
Adult respondent age	42.189	40.452	-1.737	0.409
	[14.949]	[15.251]	(1.496)	
Adult respondent is HoH	0.446	0.485	0.039	0.396
	[0.497]	[0.500]	(0.032)	
Adult respondent is spouse of HoH	0.472	0.424	-0.048	0.248
	[0.499]	[0.494]	(0.030)	
Adult respondent visits nearby town ever	0.845	0.878	0.033	0.627
	[0.362]	[0.328]	(0.064)	
Adult respondent learns from mobile phone often	0.347	0.299	-0.048	0.610
	[0.476]	[0.458]	(0.077)	
Adult respondent's total contacts in the sitio	7.524	6.011	-1.513	0.257
	[8.409]	[5.234]	(0.957)	
Adult respondent's total contacts outside the sitio	5.376	3.898	-1.478	0.179
	[7.236]	[5.747]	(0.836)	
Adult respondent voted in the 2016 national elections	0.896	0.864	-0.031	0.625
	[0.306]	[0.343]	(0.035)	
# Observations	1734	1617		
Household roster level variables				
HH member (15+) female	0.472	0.488	0.016	0.187
	[0.499]	[0.500]	(0.012)	
HH member (15+) age	37.156	35.937	-1.219	0.425
	[16.553]	[16.202]	(1.004)	
HH member migrates for work or school	0.332	0.325	-0.007	0.767
	[0.471]	[0.469]	(0.020)	
# Observations	3549	3064		

Standard deviations in brackets. Standard errors in parenthesis. Wild cluster bootstrapped p-values reported.

Table 3: Site Information and Key Dates

Site	Population: Households	Adults	Registered Accounts	Active Accounts	Dates: Baseline End Date	CCN Installation	Account Registration	Promotions Start Date
Site 1 - tower	88	220	98	68	2016-10-23	2017-09-13	2017-09-15	2018-09-01
Site 1 - control	124	341	-	-	2016-10-23	-	-	-
Site 2 - tower	391	1218	990	972	2016-11-06	2017-10-25	2017-10-27	2018-09-17
Site 2 - control	180	500	-	-	2016-09-16	-	-	-
Site 3 - tower	182	502	376	296	2016-11-21	2018-02-01	2018-02-01	2018-10-29
Site 3 - control	150	387	-	-	2016-07-31	-	-	-
Site 4 - tower	176	354	326	300	2016-07-25	2018-05-30	2018-06-02	2018-10-29
Site 4 - control	62	208	-	-	2016-09-03	-	-	-
Site 5 - tower	255	646	471	133	2016-10-23	2018-08-29	2018-09-01	2018-11-27
Site 5 - control	513	1531	-	-	2018-04-26	-	-	-
Site 6 - tower	55	138	137	68	2018-03-19	2018-10-17	2018-10-20	2019-02-22
Site 6 - control	147	409	-	-	2018-04-10	-	-	-
Site 7 - tower	104	255	255	137	2018-03-17	2019-01-25	2019-01-27	2019-02-22
Site 7 - control	63	173	-	-	2018-03-23	-	-	-

of community support and ownership, the UPD team coordinated with a local cooperative as well as retailers who would be responsible for basic maintenance and selling phone credit (referred to as "load" in the Philippines). At that point, UPD facilitated a registration and launch event for the site.

3.3.3 Customer Registration and Tower Launch

In each treatment site, we conducted customer registration events. We advertised to the community at least one week in advance of the events in order to encourage high attendance. At the launch events, University of the Philippines Diliman technicians described the VBTS Konekt Network and the services provided by the network. In addition, all adults living in the site were eligible to collect and register a SIM card under their name. VBTS Konekt Network SIM cards were provided for free and required a GSM 900 or multi-band cellphone. No phones were provided to customers by the research team. SIM cards could be replaced if they were lost or malfunctioned in which case the customer would retain the same phone number in the event of a SIM replacement. Additional SIM cards could be bought for 15 Pesos.[9]

[9] Much more detail about registration is presented in our pre-analysis plan.

Table 4: VBTS Konekt Tariff Schedule

Network Interaction Type	Tariff (PHP)
Call from a Konekt number to another Konekt number	1.00/minute
Call from a Konekt number to a long-distance on-network number	3.00/minute
Call from a Konekt number to an long-distance off-network number	5.50/minute
Text from Konekt number to Konekt number	0.25/message
Text from Konekt number to long-distance on-network number	0.50/message
Text from Konekt number to long-distance off-network number	1.00/message
All incoming calls	FREE
Incoming text messages (on-network local and long-distance)	FREE
Incoming text messages (off-network)	NOT ALLOWED

The VBTS Konekt Network allows for calls to and from other mobile and landline phones within the Philippines. Table 4 provides the schedule of tariffs for all network interaction types. Local calls and texts are the lowest cost, on-network long-distance calls and texts are billed at a higher rate than local interactions, and off-network interactions are the most costly. All incoming calls and texts are free of charge to the customer; however, the calling party for incoming calls and texts are charged at standard long-distance rates. Due to regulatory restrictions, texts from off-network numbers cannot be received. Similarly, international transactions are prohibited on the VBTS Konekt Network.

Customers were informed that they could purchase phone credit through retailers based within the site. Each site had between one and three retailers. To promote the take-up of the network and encourage customers to try the network, all customers that activated their SIM card received five free text messages. Customers were also informed that promotions might be offered to them at a later date.

Following the launch event, UP enabled the cellular network for all activated SIM cards. The network could only works through VBTS Konekt SIM cards. Customers could purchase phone credit directly through retailers.

Table 5: Promotional Treatment Groups

	No Discount	50% Local Discount	50% Long-distance Discount
No Free Load	187	183	191
Free Load	191	186	193

Note: Numbers in cells represent household counts.

3.3.4 Mobile Network Data Collection

Call Detail Records (CDR) — logs of all transactions initiated on or incoming to the CCN — are automatically recorded by the VBTS Konekt Network. All CDR are stored on servers at the University of the Philippines Diliman and shared with the research team following careful IRB protocols.

3.4 Random Assignment to Promotions

After the VBTS Konekt Network was active for at least two months, we offered promotions to customers through a second randomized experiment. Table 5 shows the matrix of treatment assignments. The first dimension was a free credit of 100 Pesos loaded directly to the customer's balance. The second dimension was a type of tariff discount provided to customers. 50% tariff discounts were applied to either local on-network calls or long-distance calls. All customers, including those in the promo control group, received five free long-distance text messages. To account for potential sharing of phones within the household, treatment assignment was done at the household level. Tariff discount promotion lengths varied (they were subject to monthly regulatory approval) by site but were at least 30 days. We stratified promotion treatment assignment by phone use, network size, and wealth.[10]

Customers were automatically enrolled in the promotional group that they were randomly assigned. However, customers were given the option to opt-out. At the start of the promotional period, customers received text messages describing the promotion(s) that

[10] Additional details on randomization are provided in our pre-analysis plan.

they received, in the local language (Tagalog). See Appendix Section 7.4 for text message language.

Table 6 presents promotion randization balance. We check for balance on 24 variables and find none of the joint F-stats of differences across the treatments to be significant.

3.5 Endline Data Collection And Migration

We retruned to all 14 study sites between May and September 2019. Surveyors attempted to interview all households interviewed at the time of the baseline survey. Additionally, if new households moved to one of the sites since baseline, these households were also interviewed. The content of the endline survey was largely the same as the baseline. The household module will be administered with the head of household or the spouse of the household head. As in the baseline, we attempted to survey two adults per household through the adult module. Priority was placed on interviewing the same individuals surveyed at the time of the baseline. Where those individuals are unavailable, a replacement adult of the same gender was selected at random from the same household. All respondents provided voluntary consent to participate in the study.

Table 1 makes note of the number of households surveyed at endline (2692), and the subset of those that were "in panel" (i.e. that were baseline surveyed). We can see that 1967 of 2370 households from the baseline were surveyed at endline (83 percent). Note as our endline was a census of households in these villages, those that attrited likely moved out of our study area. We do not detect differences in this out-migration between treatment and control villages, and we find those households that remain are balanced on the same observables used to verify randomization balance. Also note that there are over 700 new households in our study sample at endline. Again, we do not find evidence of selective in-migration to treatment versus control sites.

Table 6: Promotion randomization balance

	Control HHs	LD only	Local only	FL only	LD + FL	Local + FL	Joint F-stat
Household level variables							
Municipality == San Luis	0.497	0.545	0.492	0.489	0.524	0.528	0.392
	[0.501]	[0.499]	[0.501]	[0.501]	[0.501]	[0.500]	
HH total number of adults	2.765	2.717	2.787	2.591	2.696	2.699	0.477
	[1.204]	[1.339]	[1.277]	[1.416]	[1.241]	[1.296]	
Rooms in household	1.840	1.749	1.738	1.796	1.812	1.782	0.407
	[0.833]	[0.821]	[0.817]	[0.806]	[0.799]	[0.787]	
HH owns their land	0.411	0.476	0.467	0.430	0.445	0.455	0.444
	[0.493]	[0.501]	[0.500]	[0.496]	[0.498]	[0.499]	
HH asset count	2.080	2.267	2.268	2.210	2.267	2.337	0.406
	[1.820]	[2.054]	[1.910]	[2.017]	[2.082]	[1.999]	
HH has access to electricity	0.812	0.733	0.765	0.769	0.753	0.803	0.993
	[0.392]	[0.444]	[0.425]	[0.423]	[0.433]	[0.399]	
HH # of cell phones owned	1.150	1.194	1.180	1.161	1.230	1.295	0.364
	[1.126]	[1.178]	[1.160]	[1.276]	[1.128]	[1.275]	
HH # of sim cards	1.250	1.273	1.324	1.364	1.413	1.379	0.340
	[1.364]	[1.362]	[1.414]	[1.644]	[1.584]	[1.738]	
HH remittance last 12mo	0.471	0.400	0.407	0.452	0.435	0.484	0.884
	[0.500]	[0.491]	[0.493]	[0.499]	[0.497]	[0.501]	
Someone in HH has a bank account	0.155	0.162	0.142	0.146	0.157	0.172	0.166
	[0.363]	[0.370]	[0.350]	[0.354]	[0.365]	[0.378]	
Fishing primary income source	0.205	0.233	0.247	0.269	0.216	0.281	0.901
	[0.405]	[0.424]	[0.433]	[0.445]	[0.412]	[0.451]	
Farming primary income source	0.357	0.370	0.379	0.313	0.368	0.292	1.090
	[0.480]	[0.484]	[0.487]	[0.465]	[0.484]	[0.456]	
# Observations	187	191	183	186	191	193	
Adult respondent level variables							
Resp. is female	0.639	0.650	0.631	0.693	0.652	0.645	0.894
	[0.481]	[0.478]	[0.484]	[0.462]	[0.477]	[0.479]	
Resp. age	40.877	40.444	41.462	39.922	39.207	40.793	0.674
	[15.150]	[16.012]	[15.233]	[15.890]	[13.684]	[15.473]	
Resp. is HoH	0.455	0.498	0.496	0.486	0.470	0.504	0.732
	[0.499]	[0.501]	[0.501]	[0.501]	[0.500]	[0.501]	
Resp. is spouse of HoH	0.448	0.390	0.412	0.432	0.448	0.413	0.957
	[0.498]	[0.489]	[0.493]	[0.496]	[0.498]	[0.493]	
Resp. visits nearby town ever	0.841	0.895	0.888	0.875	0.889	0.880	0.641
	[0.366]	[0.307]	[0.316]	[0.331]	[0.315]	[0.325]	
Resp. learns from mobile phone often	0.303	0.285	0.327	0.315	0.248	0.315	0.957
	[0.460]	[0.452]	[0.470]	[0.465]	[0.433]	[0.465]	
Resp's total contacts in the sitio	6.350	5.758	6.238	6.374	5.821	5.556	1.170
	[7.320]	[3.773]	[5.105]	[6.141]	[4.798]	[3.169]	
Resp's total contacts outside the sitio	4.029	3.707	4.054	4.105	3.900	3.615	0.369
	[4.354]	[5.637]	[7.586]	[6.424]	[6.044]	[3.817]	
Resp. voted in the 2016 national elections	0.874	0.879	0.838	0.836	0.897	0.857	0.810
	[0.333]	[0.327]	[0.370]	[0.371]	[0.305]	[0.351]	
# Observations	277	276	258	256	270	276	
Household roster level variables							
HH member (15+) female	0.462	0.493	0.478	0.519	0.499	0.480	2.129
	[0.499]	[0.500]	[0.500]	[0.500]	[0.500]	[0.500]	
HH member (15+) age	36.520	35.969	36.369	36.195	34.693	35.894	0.900
	[16.302]	[16.659]	[16.696]	[16.445]	[14.917]	[16.155]	
Migrates for work or school	0.346	0.316	0.320	0.336	0.311	0.324	0.311
	[0.476]	[0.465]	[0.467]	[0.473]	[0.463]	[0.469]	
# Observations	517	519	510	482	515	521	

Standard deviations in brackets.

4 Empirical Analysis

4.1 Primary Outcome Treatment Effects

We will use the following specification to estimate our primary outcome treatment effects:

$$Y_i = \beta^{ITT} T_v + \rho Y_i^b + \mathbf{X_i} + \nu_s + \epsilon_i \tag{1}$$

Define Y_i as one of the above outcomes measured at endline for individual i living in household h in village v and Y_i^b as the baseline value of the outcome, if available.[11]

We denote village-level treatment (installation of a cell tower) by T_v, and village level exposure (in months, prior to endline), by E_v. Stratum (i.e. matched pair) dummies are indicated by ν_s. $\mathbf{X_i}$ is an optional control or set of controls, discussed below.

Standard errors and p-values for this and all specifications are discussed below (Section 4.1.3).

4.1.1 Pre-specification of outcomes

First-time access to a cellular network could plausibly impact a wide range of outcomes for households in our sample. Based on our qualitative experiences interacting with villagers, and after a review of the relevant literature, we designed our survey to test four families of hypotheses: (i) access to communications, (ii) social networks, (iii) informedness, and (iv) economic outcomes. Each family contains one or more specific hypotheses, which in turn contains three or more outcomes. Our pre-analysis plan presents details on every outcome, including coding decisions.

[11] In the case that Y_i is a hypothesis index, Y_i^b will be a vector of available baseline values of each of the variables that make up the index. Also, if i was not interviewed at baseline, and if Y_i is a household-level outcome, we replace Y_i^b with Y_h^b.

4.1.2 Controls for precision

Where available, baseline outcome measures are used as control variables when testing for treatment effects on endline measures using an ANCOVA specification. For outcome indices (described below), we do not control for baseline indices but rather each component of the baseline index separately (to allow for the possibility that some variables were not collected at baseline).

In addition, we employ machine-learning techniques to choose a precision-maximizing control set from amongst the set of possible controls for each regression (we do not consider as possible controls outcome variables or variables with more than 5% non-response rates that are not baseline outcome measures). This is consistent with the recommendation of Ludwig, Mullainathan, and Spiess (2019).

4.1.3 Estimating p-values

Equation (1) relies on village-level variation. As we only have 14 villages in our sample, we do not expect standard errors that rely on asymptotic assumptions to be correct. For all hypothesis tests using these specifications, instead, we use the wild bootstrap cluster-t procedure using 1000 simulations (Brooks and Donovan, 2017; Cameron, Gelbach, and Miller, 2008). As a robustness check, we also report p-values derived from randomization inference (we draw all possible counterfactual treatment assignments, estimate treatment effects in each case, and determine what percentage of counterfactual treatment effect coefficients lie above the observed treatment effect coefficient to obtain a p-value) (Fisher, 1960).

4.1.4 Multiple hypothesis test corrections

To address multiple hypothesis testing within each hypothesis, for each hypothesis we create an outcome index that is the z-score average of each of the outcomes associated with the hypothesis, following Kling, Liebman, and Katz (2007). This index serves as our "primary" outcome for each hypothesis. We then correct for multiple hypothesis testing across all of the hypothesis indices within each family by controlling for the False Discovery Rate following Anderson (2008).

4.1.5 Unit of analyses

Some of our outcome variables are measured at the household level, some at the adult respondent level,[12] and some at the household roster level.[13] We always conduct analysis at the most disaggregated level possible. In the case of creating indices when outcomes are at different levels, we aggregate to the most disaggregated level possible by taking means (i.e. if three outcomes are at the household level and one at the adult respondent level for a hypothesis, we will average across two adult respondents' response first to make all outcomes at the household level and then will form the index).

4.2 Results

We present several sets of results in this section. First, we present treatment effects on two pre-specified outcome indices—access to communications and income, expenditure, and food security. We find large, significant increased access to communications in treatment sites relative to controls (i.e. our towers worked). We then find large, significant increases in income, expenditure, and food security for treated households.

Given these very positive treatment effects, the remainder of this section focuses on understanding the link between increased access to communications and increased social welfare. First, we examine pre-specified outcome indices. We find positive, significant impacts on local and long distance social connectedness. We also find no consistent impacts on market access, informedness, or subjective well-being. Second, we examine pre-specified heterogeneity, where we find that those households that were already deriving the greatest portion of their income from migration at baseline benefit the most from receiving a tower. Third, we examine outcomes that were not pre-specified. While speculative, in this section

[12] At baseline, 41.7% of households had 2 adult respondents, 58.3% had two respondents.

[13] The mean number of adults in a household at baseline was 2.78 (mean = 2, mode = 2, max = 12)

we find that increased remittances, increased income from migration, and increased income from self-employment in the village are roughly equally contributing to the overall social welfare gains.

4.2.1 Increased access to communications and income, expenditure, and food security

We find that community cellular networks increased access to communications (Table 7). Increased communications access in treatment areas is an important first result. It is a necessary condition for us to interpret the rest of our results as the impact of cellular communications in particular and not some other simultaneous event or force in treatment areas. Our index measure of communication access increased by 0.416 standard deviations. Column (2) can be used to illustrate compliance in this setting. First of all, we see that, at endline, 37 percent of control households report being able to place a call from their dwelling. This is not a surprise as some of our control sites received intermittent cellular access throughout our study from existing towers that were given more power by telecos. Second, we see this increases by 42 percentage points for treatment sites, to 80 percent. This is more than a doubling, but also we see that we do not reach 100 percent coverage. This is also not a surprise as towers in treatment sites, did go down from time-to-time because of extreme weather events, issues with contracting for VSAT up-links, etc.

In addition to survey measures of access to communication, we collected a direct measure of signal strength using phone handsets equipped with multiple SIMs and an app that constantly records each SIM's network strength. Enumerators walked around each site during our endline surveys with these handsets, recording signal strength. We then aggregate across networks and impute signal strength at the household level using a method known as krigging (CITE). Figure 3 presents the distribution of krigged signal strength for treatment versus control sites. A signal of -90 or above is usually sufficient to place a phone call. We can see that the majority of control households are below such a signal strength and a majority of

Table 7: Increased access to communications

	Hyp. 1 index (1)	Call from dwelling (2)	At least one cell (3)	At least one sim (4)
Sitio treated (=1)	0.416***	0.426***	0.063**	0.092***
	(0.134)	(0.133)	(0.027)	(0.030)
Wild p-val	0.010	0.002	0.051	0.012
RI p-val	0.000	0.000	0.008	0.000
Mean in controls	0.000	0.370	0.782	0.775
# clusters	14	14	14	14
N	2316	2316	2316	2311
R-squared	0.204	0.197	0.164	0.163
Baseline control?	Some	No	Yes	Yes

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016).

treatment households are above it. This corroborates our survey measures.

Next, perhaps most importantly, we present the impact of cellular access on income, expenditure, and food security (Table 8). We find a large, statistically significant increase in our index measure of 0.093 standard deviations. This is driven by an increase in annual income of 17 percent for treated households relative to control, an increase in household expenditure over the last week of 10 percent, an increase in reported adequate food in the last month of 13 percent, and an increase in income earned per HH members while away over the last year of 43 percent.

The impacts on income and expenditure are consistent with one another, suggesting a small potential increase in savings (which we did not measure directly) if expenditure in the last week was average. The increase in expenditure (which includes food) is also consistent with increased food security. We will discuss increased earnings from migration at length below.

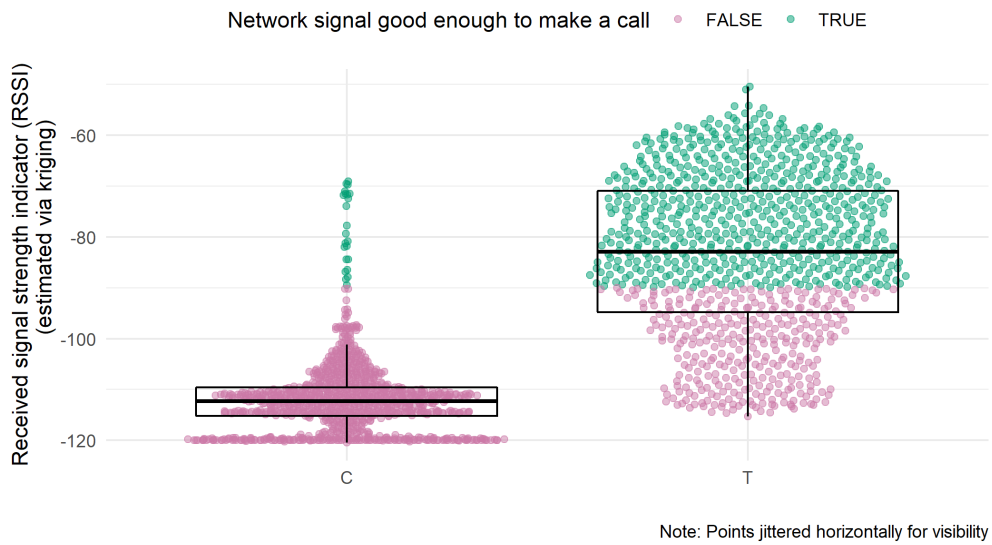

Figure 3: Signal strength and treatment assignment

Table 8: Increased income, expenditure, and food security

	Hyp. 9 index (1)	Total HH income in last 12 months (2)	Total HH expenditure in last 7 days (3)	Adequate food last month (4)	HH member away income (5)	HH member income last 30 days (6)
Sitio treated (=1)	0.093***	14605.019***	992.760*	0.083***	1286.259**	118.870
	(0.017)	(4815.150)	(461.693)	(0.025)	(453.462)	(121.930)
Wild p-val	0.007	0.084	0.205	0.018	0.087	0.418
RI p-val	0.024	0.087	0.276	0.008	0.087	0.457
Mean in controls	0.000	87047.678	9670.330	0.661	2975.775	3034.922
# clusters	14	14	14	14	14	14
N	2316	2292	2182	2313	7216	6988
R-squared	0.032	0.033	0.028	0.023	0.004	0.006
Baseline control?	None	No	No	No	No	No

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.037

4.2.2 Additional pre-specified outcomes—increased social connectedness and no consistent impacts on market access, informedness, or subjective well-being

Most theories that link increased access to communications and increased social welfare would require that households use their increased access to gather valuable information from their social networks.[14] In-line with this, we find that social connectedness increased in treatment areas (Tables 9 and 10), both locally (within the sitio) and at a long distance (outside the sitio). We estimate a treatment effect of 0.19 standard deviations on local connectedness. To understand the magnitude of this impact, we can look to the treatment effect on total contacts in the sitio. It increases from 12.3 on average in control sites to 15.3 on average in treatment sites, an increase of 24 percent. We also see an increase in the intensive margin of communication, the proportion of local contacts communicated with daily, of 10 percent.

Our impacts on long distance social connectedness are smaller though as significant. We estimate a treatment effect of 0.075 standard deviations on long distance connectedness. This is driven by a large increase in communicating frequently with household members when they are away (41 percent) and an increase in social diversity, constructed following Eagle, Macy, and Claxton (2010).

We do not detect consistent impacts on pre-specified outcome indices focused on increased information flows across social networks—informedness, disaster preparedness, and market access. We also do not find an impact on subjective well-being. See Tables 21, 22, 23, and 24 in the appendix.

We also do not find significant impacts on our pre-specified outcome indices related to migration and remittances and risk sharing. See Tables 25 and 26 in the appendix. We will discuss migration and remittances in more depth in the next section, however, including why

[14] An example of a theory that would not require this is that increased access to communications leads to increased smartphone ownership and that smartphones enable increased social welfare directly. In this setting, households were only given 2G access so this theory would not be sufficient to generate the observed results.

Table 9: Increased social connectedness—local

	Hyp. 2 index	Total contacts in the sitio	Prop. contacts communicate daily	Positive eigenvector centrality
	(1)	(2)	(3)	(4)
Sitio treated (=1)	0.186**	3.003**	0.073**	0.054
	(0.074)	(1.234)	(0.027)	(0.032)
Wild p-val	0.106	0.181	0.040	0.199
RI p-val	0.165	0.307	0.055	0.213
Mean in controls	-0.000	12.326	0.708	0.842
# clusters	14	14	14	14
N	4113	4079	4108	4113
R-squared	0.068	0.019	0.046	0.103
Baseline control?	All	Yes	Yes	Yes

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.12

Table 10: Increased social connectedness—long distance

	Hyp. 3 index	Total contacts outside the sitio	Prop. contacts talk about news, politics, weather, jobs, or finances	Count contacts comm. daily	Comm. frequently when away	Feels connected to friends outside sitio(1-5)	Social diversity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sitio treated (=1)	0.075**	-1.058	0.013	0.095	0.156***	-0.016	0.051**
	(0.031)	(0.599)	(0.011)	(0.059)	(0.049)	(0.090)	(0.023)
Wild p-val	0.094	0.188	0.383	0.315	0.016	0.539	0.195
RI p-val	0.134	0.110	0.402	0.378	0.000	0.512	0.197
Mean in controls	-0.011	10.587	0.117	0.881	0.375	3.434	0.243
# clusters	14	14	14	14	14	14	14
N	2316	4099	3821	3825	2513	4111	3793
R-squared	0.123	0.018	0.056	0.035	0.075	0.024	0.041
Baseline control?	Some	Yes	Yes	Yes	No	Yes	Yes

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.12

we speculate there are impacts on these outcomes despite our pre-specified indices detecting no impacts.

4.2.3 Pre-specified heterogeneity—households that were already deriving the greatest portion of their income from migration at baseline benefit the most from receiving a tower

Table 4.2.3 presents heterogeneity in our treatment effect on total household income by baseline primary income source.[15] We find that households whose baseline primary income source was "other wage" (wage work not in farming, fishing, logging, or construction) experience more than double the treatment effects on income as those with other primary income sources. The ITT for these households is 36 percent of the control mean. At the same time, we find those whose baseline primary income source was farming, fishing, or self-employment experience positive but much smaller treatment effects, with the smallest being for farmers. We also find that control income is higher for those households whose baseline primary income source was other wage, though notably when we interact treatment with our baseline wealth index we do not find a significant or large interaction term.

4.3 Understanding the treatment effects on income, expenditure, and food security

Our pre-specified analysis clearly shows that access to a cellular tower increased access to communications, social connectedness, and ultimately income, expenditure, and food security. We are interesting in understanding the mechanism for this ultimate change—that is, what type of behavioral changes did treated households engage in using their cellular connections that afforded them improved economic conditions? To answer this question, we

[15] Note we did not measure income amounts at baseline, only the first, second, and third most important sources of income. Also note we are presenting heterogeneity on income rather than the income, expenditure, and food security index for ease of interpretation. Results are qualitatively the same when the outcome is this index.

Table 11: Heterogeneity by baseline primary income source

	Outcome: Total HH income in last 12 months			
	(1)	(2)	(3)	(4)
Sitio treated (=1)	10644.62**	15839.28**	13712.29**	13704.23***
	(3674.98)	(5411.19)	(4638.40)	(3602.74)
Primary income source other wage	6742.84			
	(3998.52)			
T * primary other wage	11837.31**			
	(5098.97)			
Primary income source farming		-11042.23***		
		(1989.95)		
T * primary farming		-7632.77*		
		(4031.36)		
Primary income source fishing			16183.56***	
			(5033.27)	
T * primary fishing			-3092.43	
			(5783.88)	
Primary income source self-emp.				1980.61
				(4841.59)
T * primary self-emp.				-4713.24
				(6957.49)
Wild p-val	0.126	0.182	0.677	0.519
RI p-val	0.386	0.433	0.866	0.583
Mean in controls	62510	62510	62510	62510
# clusters	14	14	14	14
N	1847	1847	1847	1847
R-squared	0.076	0.083	0.072	0.067
Baseline control?	No	No	No	No

Reported p-values are for interaction term. The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016).

Table 12: Unpacking the income effect

	Income from:									
	TOTAL	Remit.	Farming	Fishing	Self-employ.	Logging	Other wage employ.	Gov't	Relig. or Private	Pension
Sitio treated (=1)	14605.02***	3694.67*	3314.63	2091.87	4643.15***	250.13	437.16	-4.12	-64.00*	138.53
	(4815.15)	(1849.87)	(3071.00)	(1574.95)	(1438.90)	(205.30)	(2124.28)	(570.42)	(33.85)	(222.74)
Wild p-val	0.090	0.237	0.529	0.293	0.048	0.495	0.927	0.998	0.259	0.742
RI p-val	0.087	0.228	0.504	0.425	0.039	0.181	0.929	0.992	0.354	0.614
Mean in controls	87048	7593	17080	3333	18325	0	35997	4350	82	670
# clusters	14	14	14	14	14	14	14	14	14	14
N	2292	2290	2280	2279	2280	2292	2285	2291	2292	2292
R-squared	0.033	0.031	0.042	0.122	0.009	0.012	0.047	0.018	0.004	0.005
Baseline control?	No	No	No	No	No	No	No	No	No	No

Regressions include a control for predicted treatment probability following Wager et al. (2016).

begin by unpacking our measured income effect into all of the categories of income that we captured in Table 12.[16]

We find positive treatment effects on seven of nine income source categories. Interestingly, the only coefficient that is individually significant according to our corrected p-values is that on income from self-employment. We did not measure self-employment activities so cannot speak to this increase other than to say that it is driven by within-sitio gains (96% of those who report self-employment as an occupation also report working in the sitio rather than outside of it), so it is likely a different phenomenon than the other impacts we will discuss.

In addition, the coefficient on remittances is also large and significant, albeit only with clustered standard errors. Given this, we will turn to better understanding treatment effects on remittances.

4.3.1 Speculative analysis finds large treatment effects on remittances

Table 13 presents ITT effects on several remittance outcomes. Column (1) presents results on reported remittance "income" for household members while away, one of the outcomes in our income, expenditure, and food security index. We speculate that this is not large

[16]We measured total income in the last 12 months by first asking households' primary respondent, "In the past 12 months, what is the FIRST most important source of income for your household?" with 10 options that are seen in Table 12. After the respondent selected a source, we asked them to estimate the total income from that source in the last 12 months. We repeated these steps for the top three income sources. Appendix Figure 4 shows the mean reported income from first, second, and third most important sources. While we did not capture all income for every household in this manner, we believe we captured the great majority of income. Note, also, we do not detect treatment effects on which source of income is primary for households.

Table 13: Revisiting remittance effects

	HH member away remittance income	HH received remittance in last 12 months (HH mod.)	HH received remittance in last 12 months (SN mod.)	HH total remittance received in last 12 months (SN mod.)	HH net remittance in last 12 months (SN mod.)
	(1)	(2)	(3)	(4)	(5)
Sitio treated (=1)	116.485	-0.011	0.055***	2347.098***	1694.591***
	(105.498)	(0.023)	(0.010)	(425.912)	(440.916)
Wild p-val	0.409	0.798	0.002	0.004	0.025
RI p-val	0.520	0.795	0.008	0.008	0.024
Mean in controls	644.381	0.458	0.244	2390.492	1728.815
# clusters	14	14	14	14	14
N	7205	2314	3820	4113	4113
R-squared	0.003	0.051	0.015	0.014	0.011
Baseline control?	No	Yes	No	No	No

Regressions include a control for predicted treatment probability following Wager et al. (2016).

because, as we will see shortly, most migration spells for household members are short. This likely implies household members that migrate for work bring increased earnings back directly rather than send it back as remittances. And we do see a significant treatment effect on away income. Column (2) presents a household-level (asked of the primary respondent) catch-all question about whether the household received any remittances in the last 12 months. This is one of the outcomes in our pre-specified hypothesis about remittances and risk-sharing. Column (3) presents an adult respondent-level dummy for whether or not that individual received a remittance in the last 12 months. This was collected by asking the respondent whether each of their named contacts in the social networks module sent a remittance, one at a time. It is puzzling that Columns (2) and (3) do not match up, though at least two reasons why this may be the case are (i) it is possible households receive more remittances from closer contacts (we capped the social network module at 15 contacts) and less from less close contacts, and (ii) differences in measurement error.

Columns (4) and (5) of Table 13 further analyze our social network data to find increased remittances, both gross and net remittance outflows (there is an increase in remittances outflows from treatment). Because there are multiple adults per household and these measures of remittances are at the adult level, the impact on the household will be two or more times larger than the coefficient in Column (5), which would be right in-line with the coefficient on income from remittances in Table 12. We take this as evidence that measurement error is not first order in these social network measures. Taken together, we believe these results, while not pre-specified, present suggestive evidence of increased remittances for treated households of approximately 25 percent of the overall income effect of households.

4.3.2 Speculative analysis finds large treatment effects on earnings from migration

Table 14 presents speculative analysis on earnings from migration, or earnings from family members (defined as those who live primarily in the sitio) while away from the sitio. Column (1) is a repeated from the income, expenditure, and food security hypothesis. Column (2) divides total away income by the total number of weeks spent away by household members, conditional on migrating for at least some time. While this is conditional on a post-treatment outcome, we take the lack of impact as evidence that increased income from migration is driven by more migration, not higher wages while migrating. Column (3) examines the number of times a household member migrated in the last 12 months. The coefficient is large (more than half of the mean in the controls) but it is not significant. We do find significance in Columns (4) through (6), on the total number of weeks spent a way, on a dummy for a household member migrating at all, and on a dummy for a household member migrating for 12 or more weeks.

Taken together, we believe these results support the hypothesis that treatment led to increased migration and increased earnings from migration, driven by longer migration spells rather than increased wages while migrating. Note unlike with remittances, we did not specifically ask about earnings from migration in our annual income measure. We can esti-

Table 14: Revisiting income while away

	HH member away income	HH member away income per week	Times HH member left for a week in last 12 months	Complete weeks HH member spent away in last 12 months	HH member left at all	HH member left for 12+ weeks
	(1)	(2)	(3)	(4)	(5)	(6)
Sitio treated (=1)	1286.259**	-95.501	0.956	1.647**	0.092**	0.081***
	(453.462)	(55.529)	(0.576)	(0.615)	(0.038)	(0.026)
Wild p-val	0.081	0.377	0.256	0.145	0.155	0.090
RI p-val	0.087	0.449	0.268	0.118	0.126	0.087
Mean in controls	2975.775	633.622	1.831	4.249	0.306	0.232
# clusters	14	14	14	14	14	14
N	7216	2522	7216	7216	7216	7216
R-squared	0.004	0.013	0.012	0.013	0.017	0.013
Baseline control?	No	No	No	No	No	No

Regressions include a control for predicted treatment probability following Wager et al. (2016).

mate the impact on total earnings from increased earnings from migration, however. First, note that Column (1) in Table 14 is at the household roster level. There are approximately four adults per household, and so the total effect of treatment per household would be over 5000 PHP. Second, we find income while away correlates with remittance income at 22% (this is not surprising as while we already have suggested most income from migration does not take the form of remittances, surely some does). This means if we are interested in differentiating income from migration and income from remittances, we would need to only count 78% of reported income from migration. 78% of 5000 PHP accounts for approximately 30 percent of total income gains.[17]

We disaggregate the increase in weeks away into activities while away for migrating household members in Table 15. We find large, significant impacts on weeks spent in school, visiting friends and family, and working for a wage not in construction, farming, or fishing. These results are consistent with both the increase in remittances and the increase in earnings while migrating.

[17]Note there is no correlation between reported income from migration and income from self-employment.

Table 15: Activities while away from the village

	Weeks spent away:							
	In school	Visiting friends & family	Working const.	Working farming	Working fishing	Working other wage	Looking for work	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sitio treated (=1)	0.393***	0.487***	0.150	-0.072**	0.053	0.548*	-0.081	0.169
	(0.128)	(0.110)	(0.147)	(0.028)	(0.058)	(0.263)	(0.072)	(0.348)
Wild p-val	0.007	0.017	0.596	0.150	0.735	0.207	0.516	0.752
RI p-val	0.008	0.000	0.606	0.220	0.772	0.173	0.315	0.685
Mean in controls	0.742	0.397	0.448	0.163	0.034	1.035	0.167	1.263
# clusters	14	14	14	14	14	14	14	14
N	7216	7216	7216	7216	7216	7216	7216	7216
R-squared	0.004	0.008	0.004	0.003	0.007	0.003	0.005	0.004
Baseline control?	No	No	No	No	No	No	No	No

Regressions include a control for predicted treatment probability following Wager et al. (2016).

Table 16: Unpacking the income effect—heterogeneity by wage workers

	Income from:									
	TOTAL	Remit.	Farming	Fishing	Self-employ.	Logging	Other wage employ.	Gov't	Relig. or Private	Pension
Sitio treated (=1)	10644.62**	2356.10	4943.24	2603.36	5230.19***	263.59	564.81	855.08	-13.33	133.22
	(3674.98)	(1777.57)	(3830.14)	(1789.33)	(1461.52)	(216.14)	(1783.99)	(562.97)	(9.93)	(162.99)
Primary income source other wage	6742.84	-4333.76***	-11895.71***	-3080.81*	-6227.25***	-41.50	31253.96***	1437.54	-11.84	-11.19
	(3998.52)	(607.98)	(2268.45)	(1655.76)	(1745.99)	(46.21)	(3410.64)	(839.42)	(10.74)	(550.62)
T * other wage primary	11837.31**	8426.56***	-75.06	-2269.46	-3821.80	153.79	5223.43	-2331.42*	15.23	-205.19
	(5098.97)	(2435.63)	(2684.13)	(2293.80)	(2856.73)	(145.99)	(5396.89)	(1182.80)	(12.98)	(578.67)
Wild p-val	0.126	0.005	0.985	0.462	0.350	0.475	0.416	0.136	0.176	0.737
RI p-val	0.386	0.071	0.961	0.370	0.071	0.346	0.528	0.173	0.181	0.567
Mean in controls	62510	6451	17599	3524	17931	0	35334	4415	8	518
# clusters	14	14	14	14	14	14	14	14	14	14
N	1847	1938	1929	1928	1929	1940	1934	1939	1940	1940
R-squared	0.076	0.036	0.055	0.129	0.009	0.015	0.093	0.019	0.009	0.007
Baseline control?	No	No	No	No	No	No	No	No	No	No

Reported p-values are for interaction term. Regressions include a control for predicted treatment probability following Wager et al. (2016).

4.3.3 Speculative heterogeneity analysis supports pre-specified heterogeneity analysis

Finally, we can interact treatment with a household's baseline primary income source being other wage work in our speculative analysis. Table 16 does this for our breakdown of annual income. We find the largest differential treatment effects for these households are on income from remittances and other wage employment. This is consistent with the migration and remittance channels. Interestingly it is not the other wage primary households that are driving the treatment effect on self-employment income.

Table 17: Revisiting income while away—heterogeneity by wage workers

	HH member away income (1)	HH member away income per week (2)	Times HH member left for a week in last 12 months (3)	Complete weeks HH member spent away in last 12 months (4)	HH member left at all (5)	HH member left for 12+ weeks (6)
Sitio treated (=1)	1096.718*	-110.625	0.914	1.493*	0.083*	0.072**
	(528.804)	(69.652)	(0.641)	(0.700)	(0.039)	(0.027)
Wage work primary	68.562	24.878	0.243	0.207	0.023	0.014
	(372.752)	(47.612)	(0.273)	(0.486)	(0.026)	(0.015)
T * wage work primary	3575.102	137.329	0.454	1.803*	0.084**	0.093***
	(2752.552)	(151.103)	(0.515)	(0.850)	(0.037)	(0.028)
Wild p-val	0.383	0.464	0.405	0.131	0.033	0.018
RI p-val	0.441	0.614	0.535	0.197	0.024	0.071
Mean in controls	2790.989	631.174	1.951	4.448	0.305	0.230
# clusters	14	14	14	14	14	14
N	6388	2245	6388	6388	6388	6388
R-squared	0.007	0.014	0.011	0.014	0.022	0.019
Baseline control?	No	No	No	No	No	No

Reported p-values are for interaction term. Regressions include a control for predicted treatment probability following Wager et al. (2016).

Tables 17 and 18 repeat this heterogeneity analysis for our migration results. We find much larger away income for these households in Table 17, driven by more weeks migrating, more migrating at all, and long migration spells. We find this differential increase in weeks away is spent differentially working for other wages in Table 18, which is consistent with what we would expect.

5 Demand for Community Cellular Access

Demand for community cellular access is an important policy outcome that we are well-suited to measure using our random CCN price variation. More specifically, we estimate and report the price elasticity of cellular network usage in our treatment sites. Let U_{it} be the usage, in terms of pesos of expenditure, of individual i during week t. Let $Price_{it}$ be a vector of prices that individual i faces during week t. Given the fact that we will have many zeros in usage, we use an inverse hyperbolic sine transformation to estimate a price

Table 18: Activities while away from the village—heterogeneity by wage workers

	Weeks spent away:							
	In school	Visiting friends & family	Working const.	Working farming	Working fishing	Working other wage	Looking for work	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sitio treated (=1)	0.537***	0.429***	0.018	-0.075*	0.047	0.553	-0.062	0.048
	(0.156)	(0.103)	(0.124)	(0.039)	(0.062)	(0.318)	(0.091)	(0.349)
Wage work primary	0.090	0.067	0.173	0.072	-0.015	-0.031	0.117	-0.265
	(0.371)	(0.058)	(0.150)	(0.109)	(0.019)	(0.174)	(0.088)	(0.176)
T * wage work primary	-0.260	-0.033	0.727	-0.040	-0.039	0.848*	-0.102	0.702
	(0.490)	(0.324)	(0.486)	(0.129)	(0.102)	(0.436)	(0.135)	(0.406)
Wild p-val	0.737	0.899	0.238	0.796	0.776	0.090	0.571	0.151
RI p-val	0.835	0.961	0.276	0.803	0.819	0.291	0.819	0.346
Mean in controls	0.787	0.386	0.478	0.182	0.034	1.038	0.189	1.354
# clusters	14	14	14	14	14	14	14	14
N	6388	6388	6388	6388	6388	6388	6388	6388
R-squared	0.005	0.007	0.007	0.003	0.007	0.005	0.006	0.004
Baseline control?	No	No	No	No	No	No	No	No

Reported p-values are for interaction term. Regressions include a control for predicted treatment probability following Wager et al. (2016).

elasticity, following Bellemare and Wichman (2019). We first estimate the equation:

$$arcsinh(U_{it}) = \beta^{DEMAND} Price_{it} + \nu_s + \gamma_h + \gamma_{wofm} + \gamma_m + \epsilon_{it} \tag{2}$$

where $arcsinh$ is the inverse hyperbolic sine transformation. To improve precision, ν_s are stratification fixed effects as above, γ_h are household fixed effects, γ_{wofm} are week and week of the month fixed effects, and γ_m are month fixed effects. We do not consider in our sample individuals that never purchased a SIM card nor those that purchased a SIM card but never purchased any load.We will directly estimate the elasticity of demand following Bellemare and Wichman (2019) equation (7):

$$\hat{\xi}_{yx} = \hat{\beta} x \frac{\sqrt{y^2+1}}{y} \tag{3}$$

We use β^{DEMAND} for $\hat{\beta}$ and mean values of y and x for our elasticity estimation.

As a secondary specification, we conduct the same regression as above but with price as the dependent variable and we directly report β^{DEMAND} as an elasticity. According to Clemens and Tiongson (2017), regression coefficients on variables transformed with the inverse hyperbolic sine can be interpreted identically to those using the traditional log transformation (as approximating percent changes) for any peso quantity encountered in practice.

Note when we calculate U_{it}, we use pre-promotion prices, not post-promotion prices, to make treatment and control group usage comparable.

Table 19 presents our results. You can see in column (4) that we estimate a price elasticity of -0.56, which is in-line with previous non-experimental estimates in the literature (GSMA, 2008; Rappoport et al., 2003; Goolsbee and Klenow, 2006; Björkegren, 2018). Table 20 disaggregates the results between local and long-distance demand and finds both promotions increase demand along both dimensions.

6 Conclusion

This study presents experimental evidence on the economic impact of first-time access to the mobile phone network. To the best of our knowledge, this is the first study to present experimental evidence on this impact. This is presumably due to the immense level of coordination between academic researchers, government regulators, commercial operators, and local communities that is required to manage a randomized deployment of cellular towers.

The fact that we find such large impacts on social welfare, and that evidence suggests these impacts are mediated through multiple channels simultaneously is quite supportive of the transformational nature of mobile phones and mobile networks.

There are also several clear take-aways for policy. First, we find that Community Cellular Networks can work in a real-world setting. While the technology was known to be functional, navigating the regulatory and context-specific hurdles to successfully launch towers in the Philippines was not easy, but ultimately possible. Second, we are able to present a quantitative understanding of the benefits of CCN connectivity, which is relevant to polices seeking

Table 19: Demand estimation

	Asinh(weekly spending by user)			
	(1)	(2)	(3)	(4)
LD only treatment	0.052			
	(0.120)			
Local only treatment	-0.218*			
	(0.121)			
LD + freeload treatment	0.448***			
	(0.125)			
Local + freeload treatment	0.481***			
	(0.118)			
Either LD treatment		0.238***		
		(0.086)		
Either local treatment		0.163*		
		(0.090)		
Any promotion treatment			0.202***	
			(0.066)	
Weighted promo. tariff				-0.273**
				(0.108)
Week freeload dropped (=1)	0.677***	0.765***	0.764***	0.773***
	(0.106)	(0.108)	(0.108)	(0.108)
# clusters	707	707	707	707
N	47753	47753	47753	47753
R-squared	0.040	0.038	0.038	0.038
Implied price elasticity				**-0.560**

Spending is calculated at pre-promotion rates.

Table 20: Demand for local or long-distance?

	Asinh(Local mins)	Asinh(LD mins)	Asinh(Local sms)	Asinh(LD sms)
	(1)	(2)	(3)	(4)
Either LD treatment	0.085*	0.091	0.167***	0.148**
	(0.044)	(0.088)	(0.059)	(0.064)
Either local treatment	0.121***	0.103	0.083*	0.117*
	(0.046)	(0.097)	(0.049)	(0.065)
Week freeload dropped (=1)	0.279***	0.320***	0.117**	0.330***
	(0.063)	(0.092)	(0.057)	(0.071)
# clusters	707	707	707	707
N	47753	47753	47753	47753
R-squared	0.028	0.052	0.057	0.035

to mandate 'universal' access as well as big tech companie's continuous attempts to bring last-mile connectivity through new technologies similar to the CCN. And finally, our estimates of the elasticity of demand allow for better decision-making around taxing/subsidizing airtime.

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7 Appendix

7.1 Tables and Figures

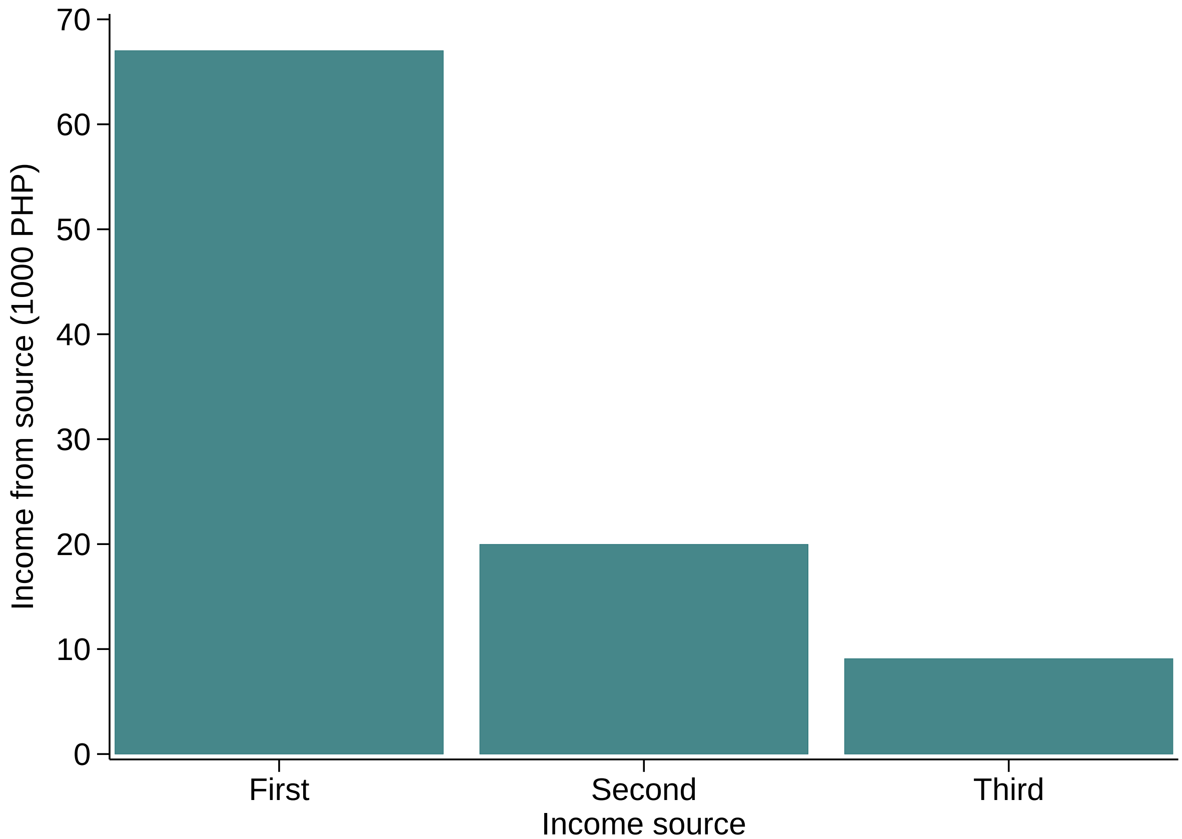

Figure 4: Mean income from first, second, and third most important sources

7.2 Community Cellular Networks

7.3 VBTS Konekt Network

Table 21: No significant impact on informedness

	Hyp. 4 index (1)	Knowledge of current events (2)	Daily news sources (3)	Knows price at market (4)
Sitio treated (=1)	-0.013	0.102	-0.066	-0.075
	(0.074)	(0.124)	(0.094)	(0.051)
Wild p-val	0.556	0.262	0.684	0.823
RI p-val	0.551	0.307	0.669	0.732
Mean in controls	0.003	3.696	2.162	0.739
# clusters	14	14	14	14
N	4111	4111	4111	487
R-squared	0.063	0.038	0.049	0.020
Baseline control?	None	No	No	No

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.66

Table 22: No significant impact on disaster preparedness

	Hyp. 5 index (1)	Feeling of preparedness (2)	Thinks there will be days of advanced warning (3)	Able to evacuate in time (4)
Sitio treated (=1)	0.083*	-0.035*	0.170***	0.003
	(0.041)	(0.018)	(0.049)	(0.019)
Wild p-val	0.198	0.325	0.029	0.461
RI p-val	0.228	0.449	0.047	0.394
Mean in controls	0.003	0.800	0.469	0.895
# clusters	14	14	14	14
N	2316	2300	2309	2276
R-squared	0.037	0.039	0.065	0.041
Baseline control?	None	No	No	No

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.66

Table 23: No significant impact on market access

	Hyp. 6 index	Feels received fair price	Count of contacts talk prices	Count of contacts from which buy goods	Count of contacts to which sell goods
	(1)	(2)	(3)	(4)	(5)
Sitio treated (=1)	0.023	-0.004	0.239***	-0.040*	0.016
	(0.025)	(0.013)	(0.052)	(0.020)	(0.037)
Wild p-val	0.565	0.544	0.008	0.187	0.767
RI p-val	0.512	0.528	0.039	0.142	0.811
Mean in controls	-0.003	0.035	1.098	0.175	0.173
# clusters	14	14	14	14	14
N	4111	926	4108	4108	4108
R-squared	0.019	0.012	0.029	0.013	0.013
Baseline control?	Some	No	Yes	Yes	Yes

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.82

Table 24: No significant impact on subjective well-being

	Hyp. 10 index	Do you see yourself as part of your local community?	Do you feel isolated from the rest of your country?	1-10 life satisfaction	QoL better than 12 months ago	QoL better than 5 years ago	Satisfaction with financial situation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sitio treated (=1)	0.022	0.050	0.072***	-0.019	-0.040	-0.016	-0.070
	(0.024)	(0.038)	(0.016)	(0.116)	(0.023)	(0.021)	(0.111)
Wild p-val	0.438	0.460	0.005	0.907	0.268	0.552	0.669
RI p-val	0.528	0.520	0.024	0.929	0.276	0.583	0.583
Mean in controls	-0.000	0.850	0.452	6.659	0.428	0.552	5.984
# clusters	14	14	14	14	14	14	14
N	4111	4092	4068	4111	4111	4111	4111
R-squared	0.012	0.019	0.027	0.004	0.006	0.007	0.005
Baseline control?	Some	Yes	Yes	No	No	No	No

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.82

Table 25: No significant impact on (pre-specified) migration outcomes

	Hyp. 7 index (1)	Times HH member left for a week in last 12 months (2)	Complete weeks HH member spent away in last 12 months (3)	HH member was away for work (4)	HH member plans to travel in next 12 months (5)	HH member works or is in school outside sitio (6)
Sitio treated (=1)	0.080	0.956	1.647**	0.016	0.016	-0.028
	(0.055)	(0.576)	(0.615)	(0.014)	(0.037)	(0.017)
Wild p-val	0.325	0.252	0.140	0.484	0.786	0.333
RI p-val	0.331	0.268	0.118	0.346	0.756	0.276
Mean in controls	0.021	1.831	4.249	0.111	0.682	0.342
# clusters	14	14	14	14	14	14
N	7262	7216	7216	7216	6241	5277
R-squared	0.011	0.012	0.013	0.005	0.021	0.031
Baseline control?	Some	No	No	No	Yes	Yes

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.82

Table 26: No significant impact on (pre-specified) remittances and risk sharing outcomes

	Hyp. 8 index (1)	HH received remittance in last 12 months (2)	HH sent remittance in last 12 months (3)	HH received loan in last 12 months (4)	HH gave loan in last 12 months (5)	HH member away remittance income (6)
Sitio treated (=1)	0.053	-0.008	0.009	0.000	0.081**	116.485
	(0.039)	(0.022)	(0.018)	(0.038)	(0.034)	(105.498)
Wild p-val	0.454	0.817	0.724	0.991	0.219	0.441
RI p-val	0.449	0.795	0.638	0.976	0.228	0.520
Mean in controls	0.000	0.458	0.240	0.550	0.152	644.381
# clusters	14	14	14	14	14	14
N	2316	2314	2315	2315	2312	7205
R-squared	0.051	0.078	0.031	0.023	0.045	0.003
Baseline control?	Some	Yes	Yes	No	No	No

The index regression controls separately for all available baseline levels of variables used to make the index. Regressions include a control for predicted treatment probability following Wager et al. (2016). Index p-value accounting for MHT: 0.82

Figure 5: A Community Cellular Network Tower

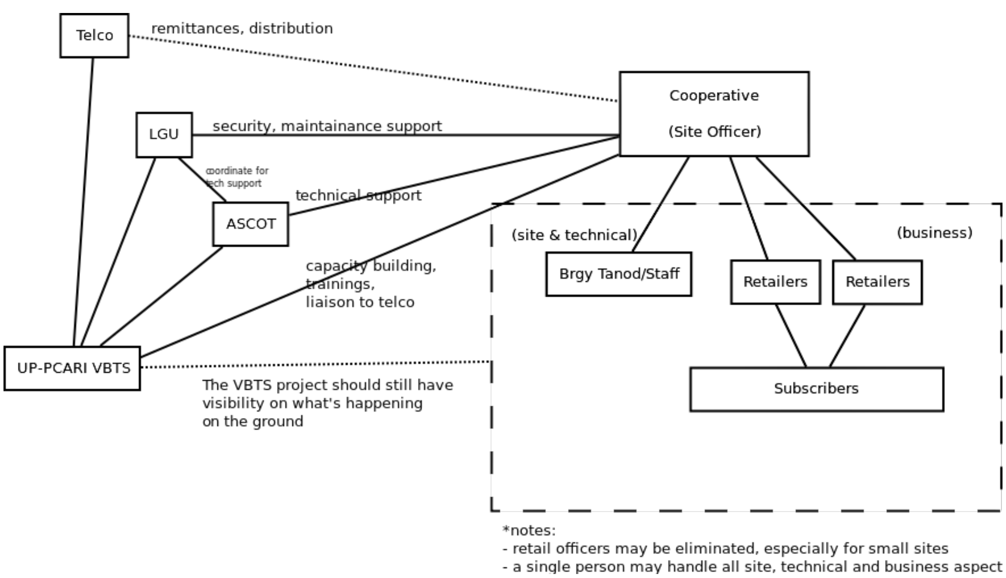

Figure 6: VBTS Konekt Organizational Structure

7.3.1 Promotional Materials

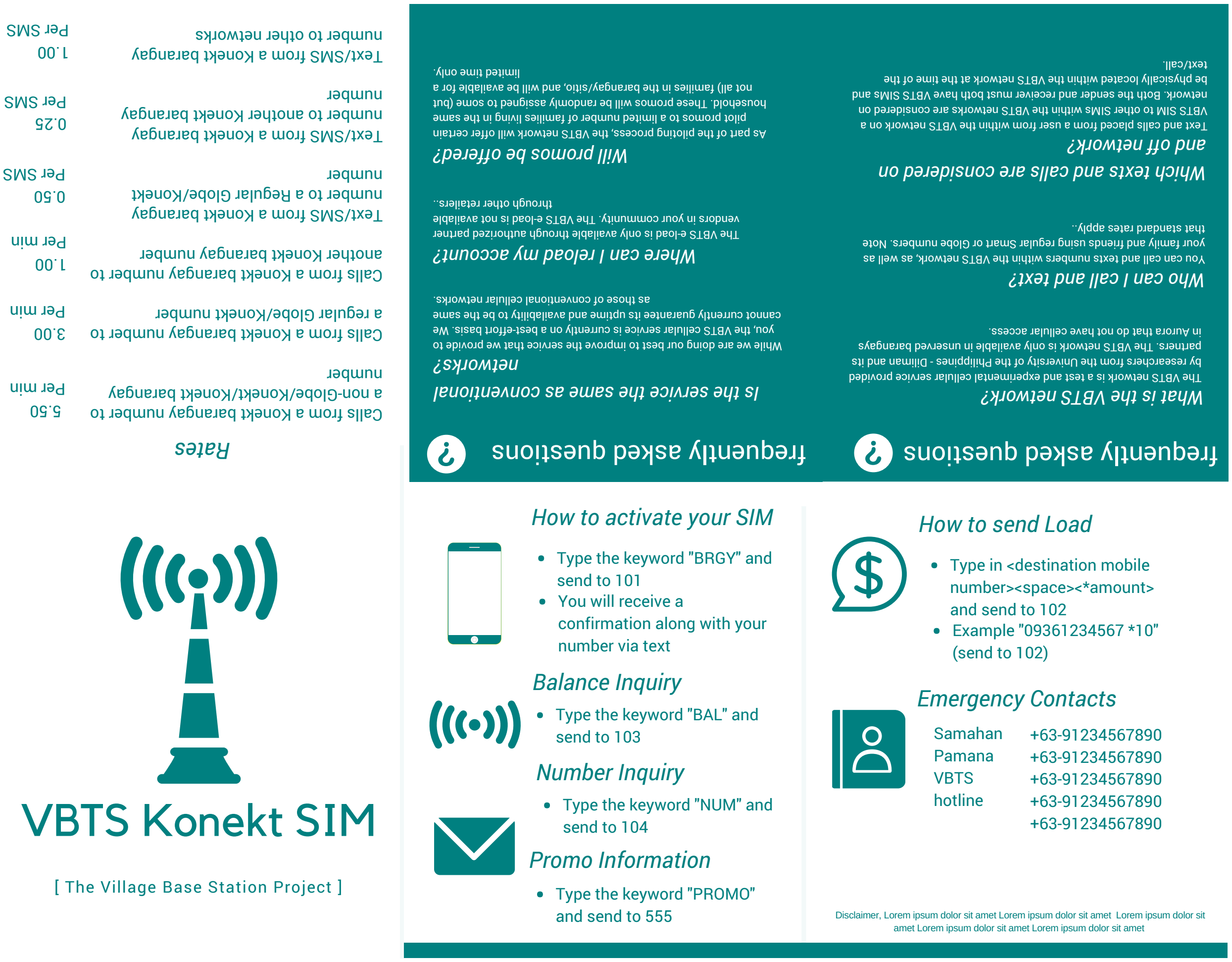

Figure 7: VBTS Konekt Flyer

7.4 Promotion SMS messages

Free 5 text messages *Congratulations! Maari mo nang ma-enjoy ang Free5 promo! Meron kang libreng 5 texts sa Globe at 5 texts palabas ng ibang networks! For more info, i-text ang INFO FREE5 at i-send sa 555. Kung ayaw mong matanggap ang promo na ito, text REMOVE FREE5 at i-send sa 555.*

Free Load: *Bilang pasasalamat sa pagiging VBTS subscriber, ang iyong SIM ay makakatanggap ng libreng Php 100 e-load! Maaring magamit ang load sa pangtawag/text sa kahit anong network. Ang e-load ay matatanggap sa loob ng 72 na oras.*

Figure 8: VBTS Konekt Retailer Poster

Local Discount: *Congratulations! Maari mo nang ma-enjoy ang GoLocal promo! Sa loob ng 30 days, 50% off ang texts at calls mula VBTS to VBTS network. For more info, text INFO to 555. Kung ayaw mong matanggap ang promo na ito, text REMOVE GL at i-send sa 555.*

Long Distance Discount: *Congratulations! Maari mo nang ma-enjoy ang GoLongDistance promo! Sa loob ng 30 days, 50% off ang texts at calls mula VBTS palabas ng ibang network. For more info, i-text ang INFO GLD at i-send sa 555. Kung ayaw mong matanggap ang promo na ito, text REMOVE GLD at i-send sa 555.*

Figure 9: VBTS Konekt Tariff Advertisement