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Abstract
We estimate the effects of mobile phone coverage on different measures of economic development. We exploit the timing of mobile coverage at the village level merging it with a village-level panel dataset for rural Peru. The main findings suggest that mobile phone expansion has increased household real consumption by 11 per cent, reduced poverty incidence by 8 percentage points and decreased extreme poverty by 5.4 percentage points. Moreover, those benefits appear to be shared by all covered households regardless of mobile ownership.

Keywords: Mobile coverage; household wellbeing; poverty

JEL: O1; O3; Q1

1) Introduction and context

The last decade has witnessed an explosive growth in the coverage and usage of mobile phones worldwide. This trend has been observed in both industrialized nations and the developing world. In particular, the developing world has exhibit an explosive growth from 10 subscriptions per hundred inhabitants in year 2000 to 67.6 subscriptions per hundred by December 2010.[i] This explosive growth has been viewed as a potential welfare enhancing mechanism. For instance, Khalil *et al*. (2009) suggest that "the development potential of the wireless platform is enormous". In that regard, cross-country evidence suggest a positive relation between information and communications technologies (ICT) expansion and economic development (Roller and Waverman, 2001; Torero and Von Braun, 2006; Djiofack-Zebaze and Keck, 2009). However, empirical evidence showing causal impacts at the micro level is relatively scarce. Accordingly, the aim of this paper is to contribute with the literature towards the provision of evidence related to the development impact that mobile phone access can generate at the household level.

Historically, there has been a sizeable telecommunication infrastructure gap between the developed and developing community. For instance, in year 2000, the ratio of fixed lines per capita in developed with respect to developing countries was about ten. By December 2010, this ratio has been situated in five (a 50% reduction in fixed lines infrastructure gap). However, when considering mobile phones, such ratio has been reduced from eleven in year 2000 to 1.7 by December 2010 (86% reduction in mobile phone infrastructure gap). Therefore, as long as the infrastructure gap is a component explaining why poor countries remain poor, it is expected that such convergence will have an impact on welfare and poverty.

The quick convergence regarding mobile phone coverage is explained by the fact that it can be undertaken without investing in the expensive wired networks necessary for land based phones. Therefore, the introduction of mobile technologies results attractive even under private cost-benefit analyses for places where land phones were unfeasible from a private perspective. As a result, poor rural areas which had no previous access to telecommunications are getting coverage for the first time.

Empirical evidence demonstrating micro economic effects of such explosive growth in mobile phone coverage is relatively scarce. Only few studies have analyzed the introduction of mobile phone technology. For example, Jensen (2007) investigated how mobile phones impact the market for fresh fish in Kerala, India, and Aker (2010) studied the impact of mobile phones on the market for grain in Niger. Both studies focus mainly in how the presence of mobile phones reduced price dispersion across different markets. The studies are consistent in suggesting that price dispersion has been significantly reduced to near perfect adherence to the law of one price after mobile phone coverage.

However, both studies analyze impacts on specific commodities (fish and grains respectively) and no general evidence of an aggregate development impact across different sectors is provided.

Accordingly, this paper provides an analysis regarding the impact of mobile phone coverage on different measures of economic development for rural Peru. We do so by exploiting variation regarding the timing of mobile phone coverage from year 2004 through 2009. As Figure one shows, mobile coverage in rural Peru has increased from 10 per cent in 2004 to 77 per cent in 2009. In addition, cell phone ownership has risen from 2 per cent to 36 per cent during the same period. This contrasts with a flat rate of land phones access situated around 1 per cent over the entire period. Clearly, rural Peru has closed the telecommunications infrastructure gap at a high rate during this period. Therefore, we exploit such event by simulating coverage patterns at the village level and merging it with representative household surveys in order to assess impacts on economic development.

[Figure 1: Mobile Phone Coverage in Rural Peru]

Our findings suggest that mobile coverage expansion has indeed improved household wellbeing. We find that household real consumption has been increased by almost 11 per cent and that poverty incidence has fallen by eight percentage points. Moreover, while we find overall increases in all sub-components of expenditures, investments in health services and transports appear to have increasing effects with respect to coverage exposure time. Finally, we find that coverage not only benefits people that own a cell phone but also benefits non-owners. Such evidence suggests positive spillover effects flowing from owners to non-owners when coverage is present.

We proceed in section two by describing the dataset. We then present the empirical strategy used for the analysis in section three. Results are discussed in section four. Finally, section five summarizes and concludes.

2) The dataset

Prior to year 2000, the Peruvian cell phone market was composed by two mobile phone providers, *Nextel del Peru* and *Telefonica del Peru*. By that time, mobile coverage was present in Lima and other urban areas mostly along the coast where more than half of Peruvians live. However, there was little coverage in less densely populated areas, such as the jungle and highlands. In 2000, a new provider, *TIM Peru* (later sold and rebranded as *Claro Peru*), entered the market. From 2001 onwards, *TIM/Claro* and *Telefonica* began to compete for new clients. Accordingly, both of them began to expand their networks to rural areas that had no previous coverage. Such a dramatic expansion has determined that 77 per cent of rural households were living in covered areas by 2009. We use such expansion in order to assess the impacts of mobile coverage on household wellbeing. In doing so, we rely on different datasets outlined below.

Household level information comes from the Peruvian National Household Survey (*Encuesta Nacional de Hogares, ENAHO*). This is a nationally urban/rural representative survey collected annually by the Peruvian Statistics Bureau (*Instituto Nacional de Estadistica e Informatica, INEI*). We use the ENAHO for years 2004 through 2009 stacking them as a rural village-level panel. The survey provides information on household demographics, expenditures and poverty incidence. In addition, we identify the village in which each household lives and introduce the GPS location for the village

collected during the 2007 National Census. This information will serve us to simulate mobile coverage at the village level.

We also use private administrative information provided by the three private mobile operators in Peru regarding the construction date of their towers. These data also contains tower characteristics like its GPS location, height, transmission power and frequency. Using these characteristics, we simulate mobile coverage at the village level for each year in our sample. Accordingly, using these simulations we determine the year in which each village sampled in the *ENAHO* survey was covered by any mobile provider for the first time. Our final sample comprises 45,401 rural household-year observations.

3) The empirical strategy

To estimate the impact of mobile coverage on household outcomes, we begin running baseline regressions of the following form:

$$Y_{ijt} = \alpha + \phi_t + \beta \cdot Coverage_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt} \qquad (1)$$

where *i* indexes the household, *j* indexes the village, *t* indexes the year. Y is the outcome of interest. ϕ_t is a year fixed effect which controls nonparametrically for aggregate yearly shocks across villages in the sample, for example from a particularly dry or rainy year. X is a vector of controls including household size, indicators for whether the household has electricity, household head sex, age, migratory status, marital status and education. $Coverage_{jt}$ is an indicator taking the value of one if village *j* is within mobile phone coverage in year *t*, while zero otherwise. Estimated standard errors are clustered at the village level in order to allow for heteroskedasticity and serial correlation in estimation errors across households residing in the same village.[ii]

Notice that we focus on mobile coverage rather than mobile ownership or utilization. Therefore, our analysis is framed within an intended to treat (ITT) interpretation. The reason to do so relies on the fact that we exploit an event that is not a household decision (that is village-level mobile coverage) to identify its effect on indicators of economic wellbeing. This because individual ownership is a choice variable that would be correlated with several observable and unobservable characteristics also related with the outcomes of interest. For instance, May (2010) shows that households without mobile ownership exhibit less education and economic assets that those with ownership. Therefore, a comparison between households with and without mobile ownership might yield biased estimates as both groups would differ in observable and unobservable characteristics systematically related to economic wellbeing. Thus we attempt to isolate causality by exploiting the plausibly conditional exogenous timing in mobile coverage at the village level provided by our dataset as follows.

In the context of (1), if coverage timing would had been orthogonal to observable and unobservable characteristics systematically related to economic development, estimates of β would provide consistent measures regarding causal effects of mobile coverage. However, since mobile coverage expansion is a decision undertaken by private companies, we should expect operators to cover zones with higher developmental potential first and leave less developed zones for latter coverage. Therefore, it is likely that results from this strategy will be biased towards finding positive estimated impacts of mobile coverage on variables related to economic wellbeing.

To address previous concerns we exploit the village-level panel nature of our dataset. Intuitively, we exploit the fact that we observe households living in the same village

before and after mobile coverage. Therefore, this allows us to control our estimation for any time-invariant observable and unobservable factors that might have been correlated with coverage timing and economic development at the village level. Formally, we introduce village fixed effects in (1) as follows:

$$Y_{ijt} = \alpha_j + \phi_t + \beta \cdot Coverage_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt} \tag{2}$$

where α_j is a village specific intercept (or fixed effect). The presence of village fixed effects control nonparametrically for any time invariant observable and unobservable characteristics at the village level. Basically, we are purging our estimates from any village level analysis that mobile providers may have done when deciding which villages would be covered earlier or latter. In this model, the identification assumption is that conditional on year fixed effects (which controls for secular time trends in the outcomes), village fixed effects and household characteristics contained in X; coverage timing is orthogonal to unobservable characteristics related to economic development. Therefore, we exploit this timing by comparing outcomes between households living in villages that received relatively early coverage against villages that received coverage latter on. Indeed, estimates of β provide a measure of mobile coverage average effect over the outcomes of interest. Specifically, it provides an estimate of coverage impact in the years after coverage, relative to the mean in the years leading up to the activation of the services (see Angrist and Pischke, 2009; Gertler *et al*., 2011).

While model (2) addresses concerns regarding time-invariant unobservables at the village level; it does not control for potential time-varying unobservable characteristics that might be related to the outcomes of interest. To address such concern, we also introduce differential time trends for each of the seven Peruvian geographical domains as follows:

$$O_{ijt} = \alpha_j + \phi_t + \beta \cdot Coverage_{jt} + X_{ijt}^{'}\gamma + NorthCoast_j \cdot t + CentralCoast_j \cdot t + SouthCoast_j \cdot t$$
$$+ NorthHighlands_j \cdot t + CentralHighlands_j \cdot t + SouthHighlands_j \cdot t + Jungle_j \cdot t + \varepsilon_{ijt}$$

(3)

The advantage of specification (3) is that it separates the impact of the arrival of mobile coverage from other ongoing trends in regional outcomes. This exercise also constitutes a robustness test for our village fixed effects strategy. If estimates of β do not change significantly after the inclusion of regional differential trends, then the potentially endogenous coverage timing is accounted by time invariant village level characteristics rather than by dynamic regional trends.

Finally, we disaggregate the before-after effects previously estimated into year by year effects. Therefore, we add flexibility to model (3) by estimating regression equations similar to:

$$O_{ijt} = \alpha_j + \phi_t + \sum_{-4}^{+8} \beta_p \cdot D_{jp} + X_{ijt}^{'}\gamma + NorthCoast_j \cdot t + CentralCoast_j \cdot t + SouthCoast_j \cdot t$$
$$+ NorthHighlands_j \cdot t + CentralHighlands_j \cdot t + SouthHighlands_j \cdot t + Jungle_j \cdot t + \varepsilon_{ijt}$$

(4)

where D_{jp} is an indicator taking the value of unity for the pth year before or after mobile coverage (that is p=-1 denotes one year before coverage, p=0 is the year in which coverage started, p=1 is one year after coverage and so on) in village j while zero otherwise. We omit the $D_{j,-1}$ indicator from the regression, so our estimates of the coefficients β_p are interpreted as the mean of the outcome variable relative to the year before the village got coverage. All other variables are defined as in (3).

The purpose of this exercise is twofold. First, if our identification strategy is valid, we should not observe anticipatory effects. In other words, we test whether consequences occur after causes and not the other way around. A direct test of this is to observe the significance of β_p estimates for years prior to mobile coverage. To interpret our results as causal, these estimates should be statistically indistinguishable from zero.

Finally, this exercise also allows us to differentiate short and medium term effects. The technique disaggregates the average impact of mobile coverage into year by year effects. Therefore, β_p estimates for years after mobile coverage tell us whether estimated impacts have been concentrated in the short term or if they have exhibited long lasting effects.

4) Results and discussion

4.1. General results

We start our analysis with model (1), which regresses household outcomes on mobile coverage status, year fixed effects and several household characteristics. β estimates from this specification are shown in Table one, column one. Estimated impacts are economically and statistically significant, with strong impacts in cell phone ownership, expenditures, poverty and extreme poverty incidences. However, since mobile coverage is a private decision, operators will more likely cover areas with higher development potential first while deferring coverage of areas with relatively lower potential. In that case, estimates in column one may be biased towards finding positive effects.

[Table 1: Mobile Coverage Estimated Impacts]

Therefore, in order to address previous concerns regarding potentially endogenous placement of mobile towers; we exploit the fact that the household survey interview the same villages across different points in time. Therefore we organize the yearly datasets as a village-level panel and introduce village fixed effects. Accordingly, Table one, column two displays β estimates obtained from model (2). As expected, while estimates remain significant, their magnitudes are materially lower. Furthermore, while the village fixed effects strategy controls for static unobservable characteristics, it does not address the possibility of dynamic unobservables that might still be introducing biases in the estimates. We partially address such concern by introducing differential trends with respect to geographical domains. We show β estimates obtained from the fully saturated model (3) in Table one, column three. Notoriously, these estimates are qualitatively the same as in column two. This evidence suggests that unobservables introducing bias in the estimated effects are mainly static and are well controlled with a village fixed effects strategy.

We first examine how coverage has affected cell phone utilization. At the extensive margin, the survey provides information on whether a household report owning a cell phone. Therefore, our first outcome is defined as a binary variable taking the value of unity if a household report cell phone ownership while zero otherwise.[iii] Column three shows that coverage has impacted self-reported cell phone ownership by an average of nineteen percentage points over the post coverage period. Furthermore, we decompose this average effect into year by year effects using model (4). Figure two below shows β_p estimates along with their 95 per cent confidence intervals.

[Figure 2: Cellphone Coverage and Ownership]

Clearly, the figure shows no anticipatory effects as all estimates for pre coverage periods are statistically indistinguishable from zero (that is zero is included in the confidence interval). It also shows significant and increasing year by year effects for post coverage periods. Indeed, the average impact of nineteen percentage points found earlier is a weighted average of all the post coverage year by year effects reported in Figure two. This decomposition suggests that, after seven years of coverage, virtually every household in the village report owning a cell phone (we have stable impacts of 100 percentage points starting on the seventh post coverage year and thereafter). Evidently, such an explosion in cell phone ownership should have been translated in its utilization. While we don't have data on minutes used, we have real expenditures on mobile phone utilization. Table one; column 3 reports the estimated effect of coverage on real cell phone expenditures. We find an average impact of 1.2 log-points or 231 per cent. Therefore, the presence of coverage more than tripled real expenditures in cell phone utilization.

After showing that coverage has indeed impacted cell phone ownership and utilization, we start assessing coverage effects on measures of economic wellbeing. Estimates reported in column three of Table one display impacts on real expenditures (expressed in natural logarithms). We find that mobile coverage has led to a general increase of 0.1 log-points (10.8%) in total household expenditures. This is in line with the evidence provided by Beuermann *et al*. (2010) where, studying a different time period, the authors report increases of 7.5 per cent in total expenditures as a result of mobile coverage. Therefore, general wellbeing captured by annual real consumption has been positively impacted by mobile coverage.

The mechanisms that could be operating behind the result of increased wellbeing caused by mobile coverage are various. First, the presence of reliable telecommunications greatly decreases the costs associated with searching for information across different markets in order to sell or buy production or inputs in places offering the best prices. Second, by allowing villagers to be informed about the real market price of their products, access to telecommunications increases farmers' bargaining power with traders approaching their villages to buy their production. Third, mobile access may allow villagers to be informed about weather forecasts and incorporate this knowledge into their planting decisions. This could improve efficiency, for example, less fertilizer may be necessary if better weather information allows farmers to plant at a more optimal time. While our dataset does not allow testing directly for these mechanisms, Beuermann (2010) provides evidence that the introduction of public phones among isolated Peruvian rural villages indeed increased poor farmers' profitability by 19.5 per cent.

We further decompose aggregate consumption into different sub-categories. Accordingly, our results show positive average impacts in all sub-categories. The magnitudes of these effects, excluding transport and health expenditures, range between 0.09 and 0.13 log-points (or from 9 to 14 per cent). Impacts are much higher for health (0.21 log-points or 23.7 per cent) and transport (0.18 log-points or 19.2 per cent).

Transport expenditures include fuel, transportation fees and vehicle maintenance. The average impact of a 19.2 per cent increase in this type of expenditures after mobile coverage clearly shows higher physical mobilization. This is consistent with previous evidence in the sense that mobile phones might be used by farmers to get price information for their produce (Aker, 2010). Therefore, it appears that mobilization

towards different markets that might be offering better prices for their products have increased.

The highest impact is concentrated in health expenditures (23.7% increases). Such observation, however, might be reflecting different dynamics. On the one hand, it could suggest higher investments in human capital regarding preventive care among children for example. However, it might also reflect an increased labor supply and, consequently, an increased incidence of health related problems. Unfortunately, our dataset does not provide sub classifications of health expenditures between preventive and corrective care. Therefore, this point is left for future analysis.

We also assess whether mobile coverage has impacted rural poverty. Table one, panel C displays estimates regarding the effects on poverty incidence.[iv] Column three shows that indeed mobile coverage has reduced poverty by eight percentage points. This is a sizeable effect considering that over the study period (2004 – 2009) rural poverty in Peru has dropped by almost 10 percentage points (from 69.8% to 60.3%).[v] Therefore, our estimates corroborate the huge impact that mobile coverage has had in rural welfare. In addition, we estimate that extreme rural poverty has been reduced by five percentage points. Again, a huge effect considering that rural extreme poverty has been reduced by nine percentage points over the study period (from 36.8% to 27.8%).[vi] As discussed earlier, estimates provided in this section reflect the average effect of mobile coverage realized over the time that a village has enjoyed the service. However, these estimates do not provide a sense on whether average effects have been realized over the short or long term. We turn to this issue in the next section.

4.2. Duration of coverage

To investigate if coverage effects have been realized differentially between short and long term horizons, we estimate the flexible model (4). Table two displays estimated effects from this model. First, it is worth noting that estimates for years leading up to coverage are statistically indistinguishable from zero. Indeed, from the 33 pre intervention estimates, only three (9.1% of estimates) are significant at the 10 per cent or lower significance level. This fact corroborates that our results are not an artifact of tower placement directed towards more developed areas (after controlling for unobservable static heterogeneity at the village level embedded in the village fixed effects).

[Table 2: Duration of Coverage - Differential Effects]

Our post-treatment estimates suggest that all of our variables have been affected since the first year in which villages received coverage ("After 0"). However, positive impacts on expenditures have gone beyond the second year of coverage only for health, transport and cell phone expenditures. Indeed, we observe that health related expenditures have increased from 24 per cent in the first year of coverage ("After 0") until 63 per cent after six years ("After 5"). Similarly, transport expenditures increased by 24 per cent in the first year of coverage but they have exhibited higher increases through time. Indeed, such expenditures almost tripled after five years of coverage (an impact of 0.97 log-points or 164 per cent) after which the effects become flat at such level. As expected, cell phone expenditures are the ones showing the highest impacts. They increase consistently over time since the moment of coverage up to seven years after it where expenditures level out with an increase of more than seven times with respect to the pre-treatment year. These

estimates corroborate the unmet demand for telecommunications that existed before coverage. Moreover, estimates show an increasing demand after coverage is provided.

Another interesting observation is that expenditures in leisure first increased by 10 per cent during the first year of coverage. Then, after three years of coverage, they started to decrease at an increasing rate from -13 per cent ("After 3") until -60 per cent ("After 8"). Such evidence shows that households are cutting their expenditures in leisure over the medium and long term after receiving coverage. This finding suggests that, at least over the intensive margin, leisure has become less prevalent. In addition, it appears that extra resources are invested in communicating and in health related issues. Again, the mechanics behind these results are not clear. It might be that households are spending more in human capital via preventive health or that households are increasing their labor supply and, as a result, experiencing adverse health conditions. Therefore, it is important to determine what kind of health issues is driving such results. Future research on these topics is therefore needed to shed light on this.

Impacts on poverty appear to last over the medium term. Table two; column 10 shows that the impact was realized since the first year of coverage ("After 0") with a point estimate suggesting eight percentage points reduction in poverty incidence. This impact has been stable and significant up to four years after coverage. By contrast, extreme poverty has been only impacted during the first year of coverage. Overall, the duration analysis gives support to a causal interpretation for our estimates in the sense that no anticipatory effects are present. Moreover, we provide evidence that economic effects are observed since the first year of coverage and there is no need to wait for longer periods in

order to enjoy observable benefits. In the next section we investigate whether benefits are enjoyed differentially with respect to mobile phone ownership.

4.3. Spillover Effects: Mobile Phone Ownership

Previous estimates have shed light regarding the impact of coverage within an intended-to-treat framework. Accordingly, we have identified the impacts of getting coverage regardless of whether people within covered areas possess a mobile phone. Of course, as Figure one showed, there is a high correlation between coverage and ownership. However, as the same figure shows, not all covered households ultimately possess a phone. As explained before, the decision of acquiring a phone is endogenous and, since our data is not a person level panel, it does not allow controlling for time invariant unobservable characteristics at the individual level. Therefore, a comparison between owners and non-owners will yield biased estimates. In this regard, we exploit the conditionally exogenous timing of coverage to compare owners in covered versus uncovered areas. In addition, we also compare non-owners living in covered villages versus non-owners residing in uncovered areas.

A comparison between owners in different coverage situation sheds light on the additional effect of living in a covered area between persons that have a mobile phone. The question is why should someone living in an uncovered village own a mobile phone if he cannot use it. While this is true within the village where the person resides, it is also true that people travel to markets or other villages with coverage and, therefore, the phone could be used there. Thus people living in uncovered areas but that own phones are

somehow enjoying the benefits of mobile communications. So the question is what additional benefit provides living in a covered village?

Comparing non-owners living in covered areas with non-owners in uncovered areas is of much interest. This because such a comparison sheds light on whether persons that chose not to get a phone (or were unable to get it) are enjoying benefits by simply residing in a covered village. If such benefits exist, they may be interpreted as spillover effects. For example, covered areas start enjoying, in general, better information on market prices and mobile owners share the information with non-owner neighbors. Therefore, non-owners may also increase their bargaining power and profits.

To perform such comparisons, we add two indicator variables to model (4) pointing out whether the household report to possess a mobile phone or not. In addition, we introduce interactions between the indicators and the coverage variable. Estimated coefficients on the interacted terms are shown in Table three below. Interestingly, panel A, column two suggests that effects of coverage are significantly different within non-owners. Total expenses are 5 per cent higher for non-owners after they get coverage. Similarly, other types of expenditures are also significantly higher for non-owners after getting coverage. By contrast, column one shows that owners living in covered villages do not differ significantly from their counterparts residing in uncovered areas. The interpretation is that it appears to be significant spillover effects flowing from owners to non-owners after the village gets coverage.

[Table 3: Spillover Effects – Mobile Ownership]

Panel B of Table three displays estimates with respect to poverty incidence. We first notice that poverty effects are present for both owners and non-owners. Indeed, owners in

covered areas present a reduction of seven percentage points in the fraction of households that live below the poverty line with respect to owners living in uncovered villages. Non-owners in covered areas are found to be three percentage points less likely to be poor after coverage is received with respect to non-owners in uncovered villages. Still we find that spillovers are also present with respect to poverty reduction. Finally, when we look at extreme poverty, we continue finding significant spillover effects. Indeed, non-owners are five percentage points less likely to fall into extreme poverty when mobile coverage arrives with respect to non-owners in still uncovered areas. This analysis shows that benefits of coverage are being enjoyed by everyone, regardless of ownership status through spillover effects.

5) Summary and conclusion

In this paper we have assessed the causal effect of mobile coverage over several measures of households' wellbeing such as consumption and poverty incidence in rural Peru. To do so, we exploited the timing of coverage using a village level panel of rural households obtained from nationally representative household surveys spanning from 2004 through 2009. Our estimates suggest that coverage has increased total real household expenditures by 10.8 per cent. The greatest effects have been observed for health (a real increase of 23.7 per cent), transport (19.2 per cent real increase) and mobile phone expenditures (231 per cent). These effects together imply the existence of a significant unmet demand for mobile services and positive effects associated with the provision of such services. Moreover, we also find sizeable reductions in poverty following mobile coverage

introduction in the order of eight percentage points for overall poverty and five percentage points for extreme poverty.

We showed that estimates can be interpreted as causal by proving that, after controlling for unobserved village level characteristics, no anticipatory effects are present. In addition, we find that positive effects are mainly realized during the first year of coverage. Finally, we showed that mobile coverage has benefited both persons that own and persons that do not own a phone. The latter implies that positive spillover effects flow from owners towards non-owners after the arrival of coverage. While the evidence strongly points out towards positive developmental effects of mobile phones; we still need to understand downstream effects over labor supply, time allocation, investments in human capital, and productivity. Future research in the topic is necessary to understand more precisely how is this technology enhancing aggregate wellbeing.

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[i] Source: International Telecommunications Union, World Telecommunication/ICT Indicators Database.

[ii] In addition, all of our regressions are weighted using the inverse of the sampling probability to reflect survey design.

[iii] When dealing with dichotomous dependent variables, we use linear probability models (LPM) rather than non-linear models for limited dependent variables such as PROBIT or LOGIT. The reason for our choice is twofold. First, we are interested in reporting the marginal impact of coverage on the outcomes of interest, rather than predicting overall probabilities. Accordingly, Angrist (2001) suggests that LPM provide consistent estimators of treatment effects, while correct inference must take into account the heteroskedasticity suffered by construction by LPM. We do the latter by clustering estimated standard errors at the village level. Second, our empirical strategy takes advantage of the panel nature of our dataset to control for unobserved endogeneity through fixed effects. Therefore, we need models that are linear in parameters to remove unobservables using such strategy.

[iv] The dependent variables are defined as indicators taking the value of unity if the household is classified as poor according to the national poverty line, while zero otherwise

[v] Source: Peruvian National Statistics Bureau (INEI). Website: www.inei.gob.pe

[vi] Source: Peruvian National Statistics Bureau (INEI). Website: www.inei.gob.pe

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Table 1: Mobile Coverage Estimated Impacts

	Estimated Effects			Observations
	(1)	(2)	(3)	(4)
Panel A.- Cellphone Utilization:				
Own a cellphone	0.26***	0.19***	0.19***	45401
	(0.01)	(0.01)	(0.01)	
Annual cellphone expenditures (in natural logs)	1.75***	1.17***	1.20***	45401
	(0.05)	(0.07)	(0.07)	
Panel B.- Dependent Variables (Log Expenditures):				
Total	0.23***	0.10***	0.10***	45401
	(0.01)	(0.02)	(0.02)	
Food	0.53***	0.12***	0.12***	44037
	(0.02)	(0.03)	(0.03)	
Clothing	0.11***	0.12***	0.13***	36598
	(0.03)	(0.04)	(0.04)	
Utilities	0.39***	0.09***	0.09***	43691
	(0.03)	(0.03)	(0.03)	
Furniture	0.21***	0.12***	0.12***	44265
	(0.02)	(0.03)	(0.03)	
Health	0.36***	0.20***	0.21***	28132
	(0.03)	(0.06)	(0.06)	
Transport	0.09**	0.17***	0.18***	20124
	(0.04)	(0.07)	(0.07)	
Leisure	0.38***	0.14***	0.13***	36691
	(0.02)	(0.03)	(0.03)	
Panel C.- Impacts on Poverty Incidence:				
Household below poverty line	-0.15***	-0.08***	-0.08***	45401
	(0.01)	(0.01)	(0.01)	
Household below extreme poverty line	-0.14***	-0.06***	-0.05***	45401
	(0.01)	(0.01)	(0.01)	
Household characteristics	Yes	Yes	Yes	
Village fixed effects	No	Yes	Yes	
Differential trends by geographical domain	No	No	Yes	

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. All regressions include year fixed effects. Household characteristics include household size, indicators for whether the household has electricity, household head sex, age, migratory status, marital status and education. * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 2: Duration of Coverage - Differential Effects

	Log Expenditures									Poverty Status	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Total	Food	Clothing	Utilities	Furniture	Health	Transport	Cellphone	Leisure	Poverty	Extreme
Before 4	0.03	-0.04	0.30	0.08	0.05	0.21	0.13	-0.13	0.11	-0.06	0.01
	(0.18)	(0.17)	(0.29)	(0.12)	(0.10)	(0.29)	(0.24)	(0.23)	(0.19)	(0.09)	(0.09)
Before 3	-0.08	0.06	-0.08	0.07	-0.14	0.05	-0.06	-0.28*	0.05	0.10**	0.01
	(0.07)	(0.08)	(0.16)	(0.09)	(0.09)	(0.23)	(0.15)	(0.16)	(0.12)	(0.05)	(0.04)
Before 2	-0.02	-0.05	0.03	0.00	-0.02	0.15**	0.02	0.04	0.02	0.01	0.02
	(0.03)	(0.04)	(0.06)	(0.04)	(0.03)	(0.07)	(0.10)	(0.06)	(0.04)	(0.02)	(0.02)
After 0	0.13***	0.08***	0.11***	0.07**	0.10***	0.24***	0.24***	1.61***	0.10***	-0.08***	-0.04***
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.06)	(0.07)	(0.06)	(0.04)	(0.01)	(0.02)
After 1	0.10***	0.07	0.12*	0.05	0.12***	0.31***	0.35***	2.15***	0.01	-0.07***	-0.04
	(0.04)	(0.04)	(0.06)	(0.05)	(0.04)	(0.09)	(0.11)	(0.10)	(0.05)	(0.02)	(0.02)
After 2	0.03	-0.02	0.10	0.00	0.09	0.29**	0.41***	3.02***	-0.00	-0.07**	-0.01
	(0.05)	(0.06)	(0.09)	(0.07)	(0.06)	(0.13)	(0.15)	(0.14)	(0.07)	(0.03)	(0.03)
After 3	-0.01	-0.04	0.01	-0.07	0.03	0.40**	0.45**	3.72***	-0.13	-0.09*	0.01
	(0.08)	(0.09)	(0.16)	(0.10)	(0.09)	(0.19)	(0.22)	(0.22)	(0.11)	(0.05)	(0.05)
After 4	0.02	0.01	0.02	-0.04	0.10	0.54**	0.63**	4.32***	-0.33*	-0.10	-0.01
	(0.10)	(0.12)	(0.21)	(0.13)	(0.11)	(0.27)	(0.29)	(0.36)	(0.17)	(0.06)	(0.06)
After 5	0.01	0.04	-0.07	-0.05	0.13	0.63**	0.97***	5.37***	-0.32*	-0.07	0.02
	(0.11)	(0.13)	(0.23)	(0.15)	(0.13)	(0.32)	(0.32)	(0.37)	(0.17)	(0.07)	(0.07)
After 6	-0.12	-0.02	-0.46	-0.21	-0.05	0.37	0.95**	6.20***	-0.45**	-0.06	0.08
	(0.14)	(0.16)	(0.28)	(0.19)	(0.17)	(0.39)	(0.40)	(0.46)	(0.23)	(0.09)	(0.08)
After 7	0.00	0.07	-0.30	-0.04	0.23	0.40	0.96**	7.43***	-0.42	-0.12	0.02
	(0.16)	(0.19)	(0.33)	(0.22)	(0.19)	(0.43)	(0.47)	(0.54)	(0.27)	(0.10)	(0.09)
After 8	-0.15	-0.07	-0.37	-0.17	0.05	0.22	1.04**	7.19***	-0.60**	-0.10	0.08
	(0.18)	(0.21)	(0.37)	(0.24)	(0.21)	(0.48)	(0.51)	(0.60)	(0.29)	(0.11)	(0.10)
Observations	45401	44037	36598	43691	44265	28132	20124	45401	36691	45401	45401

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. All regressions include year and village fixed effects. Also include differential trends by geographical domain and household characteristics (household size, indicators for whether the household has electricity, household head sex, age, migratory status, marital status and education). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 3: Spillover Effects – Mobile Ownership

	Estimated Effects		Observations
	(1)	(2)	(3)
	Mobile Owner	Non-Owner	
Panel A.- Dependent Variables (Log Expenditures):			
Total	0.01	0.05***	45401
	(0.08)	(0.02)	
Food	0.13	0.08***	44037
	(0.14)	(0.03)	
Clothing	0.06	0.04	36598
	(0.19)	(0.04)	
Utilities	0.02	0.03	43691
	(0.28)	(0.03)	
Furniture	-0.03	0.06**	44265
	(0.15)	(0.03)	
Health	0.01	0.14**	28132
	(0.27)	(0.06)	
Transport	-0.16	0.08	20124
	(0.32)	(0.07)	
Leisure	-0.19	0.06*	36691
	(0.16)	(0.03)	
Panel B.- Impacts on Poverty Incidence:			
Household below poverty line	-0.07***	-0.03**	45401
	(0.03)	(0.01)	
Household below extreme poverty line	0.07	-0.05***	45401
	(0.06)	(0.01)	

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. All regressions include year and village fixed effects. Also include differential trends by geographical domain and household characteristics (household size, indicators for whether the household has electricity, household head sex, age, migratory status, marital status and education). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

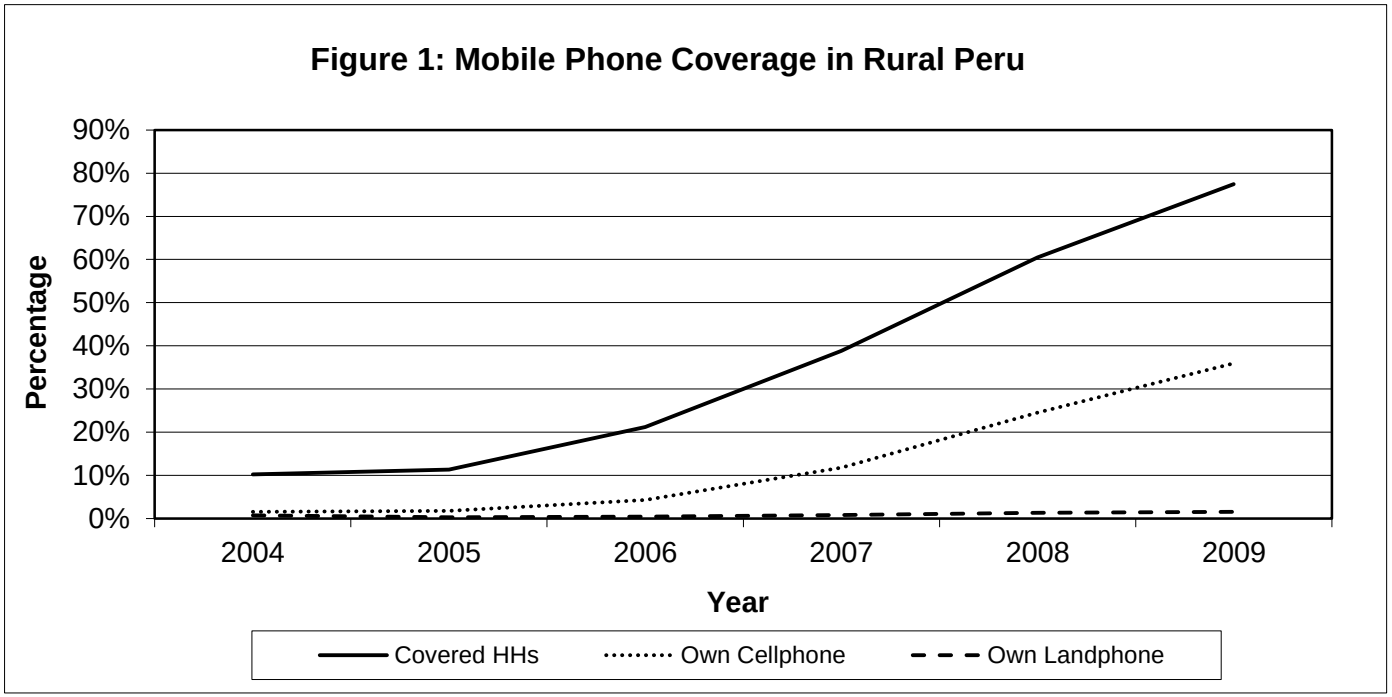
Figure 1: Mobile Phone Coverage in Rural Peru
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
Percentage
2004
2005
2006
2007
2008
2009
Year
Covered HHs
Own Cellphone
Own Landphone

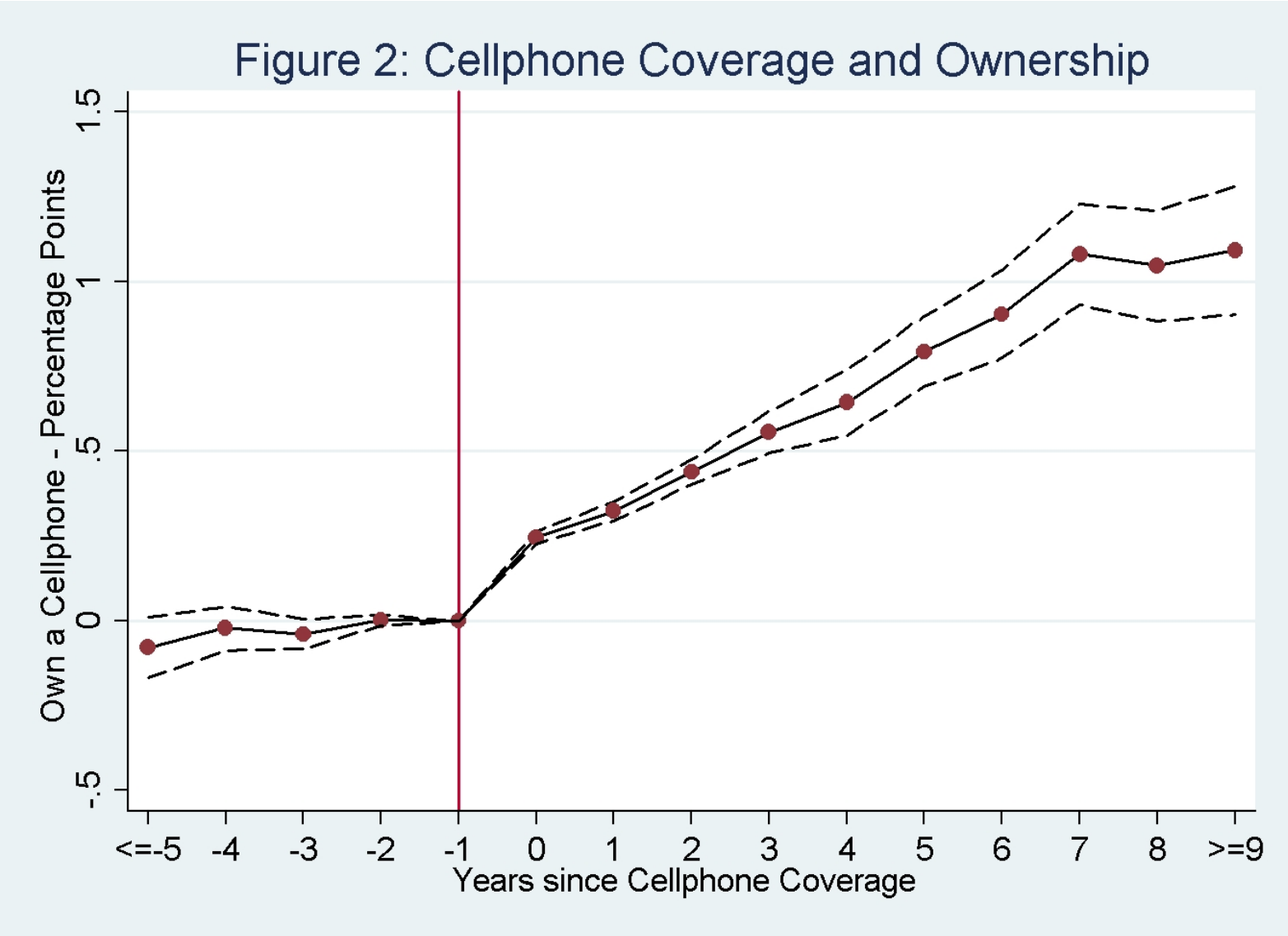
Figure 2: Cellphone Coverage and Ownership
Own a Cellphone - Percentage Points
1.5
1
.5
0
-.5
<=-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
8
>=9
Years since Cellphone Coverage