The Effects of Telecommunications Technologies on Agricultural Profits and Child Labor: Evidence from Isolated Rural Villages in Peru

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(This Version: January 2010)

ABSTRACT

This paper provides evidence on the effects that telecommunications technologies have on agricultural profits and human capital investment decisions among highly isolated villages in rural Peru. I exploit a quasi-natural experiment, in which the Peruvian government through the Fund for Investments in Telecommunications (FITEL) provided at least one public (satellite) payphone to 6,509 rural villages that did not have any kind of communication services (either landlines or cell phones) before. The intervention provided these phones mainly between years 2001 and 2004. I show that the timing of the intervention was essentially random and exploit differences in timing using a uniquely constructed (unbalanced) panel of treated villages spanning the years 1997 through 2007. The main findings suggest increases of 14.8 percent in prices received by farmers for their crops, and a 22.6 percent reduction in agricultural costs. Moreover, this income shock has been translated into a reduction in child (6 – 13 years old) market work of 13.6 percentage points and a reduction in child agricultural work of 9.1 percentage points. Overall, the evidence suggests a dominant income effect in the demand for child labor.

Keywords: Telecommunications Technologies; Peru; Child Labor.

JEL classification: O1; O3; Q13; Q16

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I am indebted to Nancy Hidalgo from the Peruvian Statistics Bureau (INEI) for providing the facilities to assembly the database. I am also grateful to Juan Carlos Ames, Gonzalo Ruiz, Carlos Sotelo and Pilar Tejada from FITEL for the provision of the data regarding telephones location and intervention timing. Conversations and comments from Cesar Calvo, Mark Duggan, Javier Escobal, Erica Field, Raymond Guiteras, Judith Hellerstein, Miguel Jaramillo, Melissa Kearney, Jeanne Lafortune, Soohyung Lee, Oswaldo Molina, Eduardo Moron, Christopher McKelvey, Peter Murrell, Eduardo Nakasone, Miguel Paredes, Maximo Torero, Richard Webb, participants in UMD, Universidad de Lima, Universidad del Pacifico, Universidad de Piura – Campus Lima, Universidad San Martin de Porres Applied Micro Seminars substantially improved the paper. Luis Barrantes, Jonathan Chaname and Ricardo Martin provided excellent research assistance. Research support grant from Centro de Investigacion Economica y Social (CIES) of Peru is greatly acknowledged. Any errors and omissions are my own.

1) Introduction

Economic theory extensively recognizes the importance of information on the efficiency of markets. In that way, reductions in information search costs are expected to enhance market effectiveness. Currently, telecommunication technologies (TC) are very advanced and information transmission is extremely cheap in developed societies. However, in the context of isolated communities in developing countries, TC are still far from being universally available. Therefore, interventions providing new access to TC in such societies provide an ideal opportunity to assess the impact of improved information accessibility on market performance. Furthermore, if market effectiveness is improved with new TC, it becomes extremely interesting to assess how market performance influences household decisions such as the demand for child labor and schooling. Accordingly, the purpose of this paper is to shed light on how the introduction of payphones among rural agricultural villages in Peru affected profits, productivity, and the demand for child labor.

Previous literature has studied the effects of TC using the introduction of cell phones as exogenous shocks. For example, Jensen (2007) analyzed the impact of cell phones introduction among fishermen in the Indian state of Kerala. The results show that the adoption of mobile phones was associated with a dramatic reduction in price dispersion, the complete elimination of waste, and near-perfect adherence to the law of one price. The mechanism behind such results is that fishermen started using the cell phones to gather information regarding markets with better prices (in short supply) while in the sea. Therefore, they started to go directly towards these markets to sell their catch and, as a result, prices were equated across markets and market clearing resulted in eliminating the waste coming from unsold fish before cell phone availability.

In the same vein, Aker (2009) analyses the effects of cell phone introduction in Niger. She focuses on grain markets and suggests that cell phones reduced price dispersion across markets by 6.4 percent and intra-annual price variation by 12 percent. Furthermore, the study finds greater impacts in market pairs that are farther away and for those with lower road quality. The study suggests that the main mechanism by which cell phones impact these outcomes is a reduction in search costs. Therefore, traders whom operate in markets with cell phone coverage search over a greater number of markets and sell in more markets thereby reducing price dispersion.

While both of the previous studies concentrate on market outcomes with a specific focus on price dispersion across markets. None of them directly address effects of new TC on producers' net income and how this potentially increased income may affect non-market decisions regarding human capital investments within the family. In that way, this paper contributes with new evidence regarding the effects of TC not only on market outcomes but also on intra-household decisions. In addition, it is worth noting that the intervention to be studied differs from the previous studies in the way that it focuses on public payphones rather than cell phones. The reason is that this intervention was performed in places where neither cell phones nor fixed line phones were available. Furthermore, the treated villages were located in zones where cell phone coverage was technically and economically unfeasible. Therefore, by focusing in this intervention we avoid concerns regarding endogenous placement of TC with respect to the outcomes of interest.[2]

The specific intervention was carried out by the Peruvian Fund for Investments in Telecommunications (FITEL). This consisted in providing at least one public (satellite) payphone between years 2001 and 2004 mainly to each of the 6,509 targeted villages situated across rural Peru (See Figure 1). All these villages shared the common characteristic that none had any kind of phone services (either fixed lines or cell coverage) before. So these payphones were the first opportunity to communicate with the rest of the country without having to physically travel or use the mail. According to FITEL's documents, the intervention has reduced the average distance of access to a phone from 60km. to 5km. at a national level. I exploit the timing of the intervention to identify the impacts on agricultural productivity and the demand for child labor showing that the intervention timing was essentially random.

The main findings suggest increases of 14.8 percent in prices received by farmers for their crops, and a 22.6 percent reduction in agricultural costs. This led to an increase of 18.1 percent in agricultural productivity (measured by the ratio of agricultural production value to costs). Moreover, this income shock has been translated into a reduction in the incidence of child (6 – 13 years old) market work equivalent to 13.6 percentage points and a reduction in child

[2] This concern comes from the fact that previous studies have exploited cell phone coverage timing as if it was as good as random. However, cell phone coverage is a decision of private companies and some concern arises from the fact that these companies may first cover zones with higher economic development potential. By contrast, the intervention to be studied was performed in the more disadvantaged villages and the timing of it was essentially random.

agricultural work of 9.1 percentage points. Overall, the evidence suggests a dominant income effect in the demand for child labor.

The remaining of the document is organized as follows. Section 2 presents a description of the FITEL program. Section 3 presents an analytical framework to understand the expected outcomes of the intervention. Section 4 presents the dataset used for the empirical analysis. Section 5 describes the empirical approach adopted in the analysis. Section 6 discusses our results, while section 7 tests the robustness of such results. Finally, section 8 concludes.

2) The FITEL program

In 1992, the Peruvian government privatized all the state-owned telecommunications companies and created a Telecommunications Regulatory Authority (OSIPTEL).[3] In May of year 1993, OSIPTEL created the Fund for Investments in Telecommunications (FITEL) which began to collect a 1% levy charged on gross operating revenues of telecommunications companies in order to fund rural service expansion. In November of 2006, FITEL was declared an individual public entity ascribed to the Ministry of Transports and Communications.

The specific FITEL intervention consisted in providing at least one public (satellite) payphone to each of the 6,509 targeted villages. To do so, FITEL divided the country in seven geographical regions as shown in Figure 2. The project was executed by granting a 20-year concession to private operators for public telephony services in each geographical region. The selection of the operator for each region was based on an international auction for the lowest subsidy requested from FITEL for the installation, operation and maintenance of these public services. Targeted villages were selected by FITEL prior to the auctioning process following the three-phase procedure described below.

2.1. Village selection criteria

The selection of the rural villages to benefit from the project was based on criteria of prioritization, to maximize the social profitability of the public investment, while minimizing the subsidy. The selection process was composed of three phases as follows:

[3] Prior to 1992 all the telecommunications sector was state-owned and no private participation was present.

a) Phase I: In this phase, FITEL defined the target universe of villages for the intervention. The universe was composed by rural villages with populations between 200 and 3,000 inhabitants that did not have access to TC. Furthermore, for any village to be incorporated in the targeted universe it needed not to be in any future coverage plan of private telecommunications companies. Therefore, targeted villages were neither provided nor expected to be provided access to TC.

b) Phase II: Once obtained the universe of targeted villages, these villages were grouped in cells with average radius of 5km. Cells were formed with the requirement that no village within the cell could either have phone service or be included in the expansion plan of a private operator. Then, one village within each cell (cell center) was pre-selected for treatment (i.e. payphone installation). To be selected as cell center, the village needed to comply with at least one of the following requirements: (i) have a health center; (ii) be accessible (i.e. in connection with rural roads, river crosses or horse paths); (iii) have a high school; and (iv) have the higher population within the cell or be a central village in the sense that villagers in the cell confluence to that village in order to market products or get health services. In addition, district capitals without phone services and that were not included in future expansion plans of private operators were automatically selected as cell centers.

c) Phase III: This phase consisted on field visits to all of the cell centers. The purpose of this field work was to assess the technical viability for the installation of payphones. In addition, several workshops were conducted in district capitals that were selected as cell centers. These workshops counted with the participation of district majors and representatives of the civil society. The purpose was to assess the convenience of the selected cell centers. After this field work, the list of pre-selected villages was updated and the final list of targeted villages was elaborated.

The outlined selection criteria suggest that targeted villages in the different geographical regions of the intervention were similar with respect to several development characteristics. Therefore, the empirical strategy will exploit the timing of the intervention in order to identify causal impacts. This timing is briefly explained below.

2.2. Intervention timing

Once targeted villages were selected, FITEL auctioned the 20-year concessions for each one of the seven geographical zones: north border, north, middle north, middle east, south, middle south, and north tropical forest. Initially, FITEL planned that all payphones would be operative by the first quarter of 2002. However, delays in the auctioning process determined that the program rollout lasted until year 2004. This timing is detailed in Table 1 and it spanned from 1999 through 2004. Provided that the timing of the intervention was not systematically related with the outcomes of interest and/or with variables determining these outcomes; the causal impacts can be identified by exploiting such time variation in phones rollout.

Accordingly, the identification strategy will exploit the intervention timing at the village level which we latter show was essentially random. In the empirical analysis, however, we exclude villages treated in 1999. This because the 213 villages treated then were potentially endogenously treated first due to their importance as border with Ecuador.[4]

3) Expected outcomes

The mechanisms through which access to TC may impact agricultural profits are diverse. First, the presence of TC greatly decreases the costs associated with searching information across different markets in order to sell (buy) agricultural production (inputs) in places offering the more convenient prices. Second, TC allows farmers to be informed about the real market price of their crops and, as a result, increase their bargaining power with traders approaching their villages to buy their production. Third, access to TC may allow farmers to be informed about weather forecasts and incorporate this knowledge into plant timing decisions (for example, perhaps less fertilizer is necessary if you have weather information and thus plant at a more optimal time).

The previous mechanisms may, of course, coexist and the aggregate effect reflects all of them. However, a half program survey conducted by FITEL in 2002 among villages that already had a phone reveals that 19.5 percent of households use the technology to search for market information. This is the second main reason for using the phone (the first was social/family communication with 95.3 percent). Furthermore, when looking only at households engaged in agriculture production, 38 percent of them report searching market information as the main

[4] However, when including them in the analysis, results remain qualitatively the same.

usage. In addition, 70 percent of households reporting using the phone for market information search reveal that the frequency of these searches is either weekly or daily. This evidence suggests that the main mechanism through which the new technologies may have affected agricultural profits is linked to a reduction in search costs. In that way, we present a simple model that formalizes this mechanism.

2.1. Effects on prices

The model assumes farmers with Bernoulli utility function defined over output and input prices (net of transport costs) as follows:

$$u\left(P_o, P_i\right) = v\left(P_o\right) - g\left(P_i\right) \quad (1)$$

where P_o denotes output prices, P_i denotes input prices and $v' > 0, v'' \leq 0, g' > 0$

In addition, we assume a marginal constant cost of searching price information in an additional market of *C*. Therefore, if a farmer has already searched for prices in N markets, being *O* the best offered price for his output and *I* the best price found for his input. The expected marginal utility of the N+1 search is given by:

$$B\left(O, I\right) = \int_{o}^{\overline{P}_o} \int_{\underline{P}_i}^{I} \left[v(P_o) - g(P_i)\right] - \left[v(O) - g(I)\right] dG(P_i) dF(P_o) - C \quad (2)$$

where $F(.)$ and $G(.)$ are the CDFs of output and input prices respectively. Notice that (2) assumes that if the offered price for his output (input) in the N+1 search is below *O* (above *I*), then the farmer will sell his output at price *O* (buy his input at price *I*). So, in that case, the benefit of the N+1 search will be actually a cost of *C*. Therefore, optimality implies (assuming an interior solution) that the farmer will set his reservation prices for outputs (*R*) and maximum prices paid for inputs (*M*) by equating the expected marginal benefit of the N+1 search to zero. Therefore, the reservation price for outputs and maximum prices for inputs will be implicitly defined by:

$$B(R,M)=\int_{R}^{\bar{P}_o}\int_{P_i}^{M}\left[v(P_o)-g(P_i)\right]-\left[v(R)-g(M)\right]dG(P_i)dF(P_o)-C=0 \tag{3}$$

In that way, the effect of a change in C on R can be derived from (3) using the implicit function theorem and Leibnitz rule as follows:

$$\frac{\partial R}{\partial C}=-\frac{\partial B(R,M)/\partial C}{\partial B(R,M)/\partial R}= \\ \frac{1}{-G(M)v'(R)\left[1-F(R)\right]-F'(R)\left[g(M)-E\left(g(P_i)\mid P_i\leq M\right)\right]}<0 \tag{4}$$

Similarly, the effect of a change in C on M can be derived from (3) as follows:

$$\frac{\partial M}{\partial C}=-\frac{\partial B(R,M)/\partial C}{\partial B(R,M)/\partial M}= \\ \frac{1}{G'(M)\left[E\left(v(P_o)\mid P_o\geq R\right)-v(R)\right]+\left[1-F(R)\right]g'(M)G(M)}>0 \tag{5}$$

Clearly, (4)-(5) imply that reservation prices should rise (maximum prices paid for inputs should fall) if search costs decrease. The introduction of TC dramatically reduced search costs. In particular, the intervention to be studied meant that instead of traveling an average distance of 60km in order to be able to communicate with nearby markets or get information; farmers now need to travel a significantly lower mean distance of 5km. In that way, we expect that average reservation prices will rise (prices paid for inputs will fall) and therefore agricultural profits will rise.

2.2. Effects on child labor

In the context of rural villages, child labor in farms is very common. Therefore, parents decide how to allocate their children time between school and work. In that way, an increase (decrease) in the prices that farmers get from their outputs (pay for their inputs) implicitly raises the opportunity cost of schooling. This happens because an additional unit of labor provided to the farm is more valuable when per unit profits are higher. As a result, potential increases in profits derived from the introduction of TC imply two offsetting effects on child labor: income and substitution effects. The income effect suggests that a reduction in transaction costs will decrease child labor, while the substitution effect suggests the opposite.[5] Therefore, the total effect of the introduction of TC on child labor is ambiguous.

To formalize the argument, consider a household where the father decides how much time a kid will dedicate to the school, *S*, and how much time will be dedicated to work in the farm, *F*.[6] There is an increasing and concave human capital production function which depends on *S*, *HK(S)*. Parents derive utility from current consumption, C_c, and human capital of the child. Therefore, parents' utility is given by:

$$U\left[C_c, HK(S)\right] \tag{6}$$

where $U'>0$ and $U''<0$ for both arguments. Total children time, *T*, is assumed to be allocated between *S* and *F*. So the time constraint is given by:

$$T = S + F \tag{7}$$

Parents supply *L* hours of labor inelastically at an hourly profit of *Wp*. So, parents contribution to consumption is *Y=L*Wp*. In addition, each unit of child labor is assumed to contribute a per unit profit of $P_c\left(C, P_o, P_i\right) = R\left(C, P_o, P\right) - M(C, P_o, P)$ towards household consumption. Therefore, the household budget constraint is given by:

$$C_c \leq Y + F * P_c\left(C, P_o, P_i\right) \tag{8}$$

In that way, the household problem is to maximize (6) with respect to C_c and *F* subject to (7) and (8). This maximization yields a Marshallian demand for *F* of the form:

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[5] Here, I assume that schooling is a normal good, while child labor an inferior one.
[6] I assume that working in the farm is not an activity that provides human capital to the child.

$$F(P_c(C,P_o,P_i),Y,T) \tag{9}$$

While minimization of expenditures holding utility at a constant level, $\bar{U}$, yields a compensated demand for F of the form:

$$\tilde{F}(P_c(C,P_o,P_i),\bar{U},T) \tag{10}$$

Therefore, the Slutsky equation implies the following:

$$\frac{\partial \tilde{F}(P_c,\bar{U},T)}{\partial C} = \frac{\partial F(P_c,Y,T)}{\partial P_c}\frac{\partial P_c}{\partial C} - \frac{\partial F(P_c,Y,T)}{\partial Y}\tilde{F}(P_c,\bar{U},T)\frac{\partial P_c}{\partial C} \tag{11}$$

Rearranging (11) provides us with the Substitution and Income effect decomposition:

$$\underbrace{\frac{\partial F(P_c,Y,T)}{\partial C}}_{TotalEffect} = \underbrace{\frac{\partial \tilde{F}(P_c,\bar{U},T)}{\partial C}}_{SubstitutionEffect<0} + \underbrace{\underbrace{\frac{\partial F(P_c,Y,T)}{\partial Y}}_{<0}\underbrace{\tilde{F}(P_c,\bar{U},T)}_{\geq 0}\underbrace{\frac{\partial P_c}{\partial C}}_{<0}}_{IncomeEffect\geq 0} \tag{12}$$

Clearly, the effect of a decrease in transaction costs due to the introduction of TCs is ambiguous. Substitution effect implies that child labor will increase with the introduction of TCs; while the income effect implies the opposite. The total effect will therefore depend on the relative weights that parents' utility assigns to consumption with respect to children human capital.

4) The data

Unfortunately, no primary data collection was conducted with the purpose of evaluating the FITEL program. However, we were able to construct an unbalanced panel of treated villages using several data sources and GIS techniques as follows.

The first data source is the Peruvian Living Standards Measurement Survey (PLSMS) for years 1997 and 2000. Then, I use the Peruvian National Household Survey (ENAHO) for years 2001 through 2007. The ENAHO replaced the PLSMS and most of their questionnaires mimic the PLSMS ones. Both surveys are nationally urban/rural representatives. The survey contains information on demographics, education, income and expenses.

The second source is FITEL's administrative information containing the GPS location of each phone and the date in which the phone became operative. The third source consists in geo-referenced information from the Peruvian Ministry of Transports and Communications regarding the rural network of roads and rivers. Finally, we used NASA information to construct a gradient map of Peru in a 90 meter cell precision.

We built the final dataset by coding the PLSMS/ENAHO at the village level and input the GPS location of each village using information collected during the 2007 census. Then, using all of the previous information regarding communications network and land gradient, we simulated travel time from each surveyed village to the nearest FITEL phone in *SMALLWORD* software. In that way, we took into account villages situated in a radius of 30 minutes traveling time to the nearest phone for the analysis (the mean travel time in the final sample is 6 minutes). After this process we ended up with a final sample of 15,242 households-year and 19,409 children (6 to 13 years old)-year observations distributed across 2,453 village-year observations. Tables 2, 3 and 4 show the distribution of the sample by survey year and treatment timing. In addition, Figure 3 displays the villages included in the sample colored by year of intervention.

5) Empirical strategy

To estimate the causal impact of TC on the outcomes of interest, we follow a village-level panel approach which summarizes the overall impact of the program in the difference between mean outcomes before and after the intervention. This approach involves the estimation of regression equations in the following form:

$$O_{ijt} = \alpha_j + \phi_t + \beta_1 Post_{jt} + X_{ijt}^{'}\gamma + \varepsilon_{ijt} \tag{13}$$

where O_{ijt} is the outcome of interest for household/child *i*, in village *j* in month*year *t*. $Post_{jt}$ is an indicator which takes a value of 1 if village *j* had a phone in month*year *t* while 0 otherwise.

α_j is a village fixed effect. ϕ_t is a month*year fixed effect. X_{ijt} is a vector of controls defined in the results tables. Finally, ε_{ijt} is an error term.[7]

Some aspects of model (13) merit discussion. First, the presence of village fixed effects control nonparametrically for any time-invariant unobservable characteristics across villages. Second, the month*year fixed effects control nonparametrically for aggregate monthly shocks across villages in the sample, for example from a particularly dry or rainy month. In this model, estimates of β_1 provide a measure of the program's average effect over the outcomes of interest. Specifically, it provides an estimate of the program's impact in the years after the installation of the phones, relative to the mean in the years leading up to the activation of the services.

However, to interpret these estimates as causal, the key identifying assumption is that, absent the intervention, both villages treated in the first stages of the program and those treated latter would have shared the same trends with respect to the outcomes of interest. Moreover, if treatment timing was indeed random, differences between the outcomes of interest and other characteristics between villages treated early in the program and those treated latter evaluated at pre-treatment periods should not be significant. Accordingly, Tables 5 and 6 provide evidence showing that baseline differences for households and children treated earlier and latter are statistically indistinguishable from zero. This gives us confidence that treatment timing was indeed random with respect to the outcomes of interest and several demographics.

We also estimate an important variant of equation (13) in which we add region-specific time trends as follows:

$$O_{ijt} = \alpha_j + \phi_t + \beta_1 Post_{jt} + X_{ijt}^{'}\gamma + Coast_j t + Highlands_j t + Jungle_j t + \varepsilon_{ijt} \qquad (14)$$

This specification controls for linear trends in outcomes during the study period, and allow these trends to vary across Peruvian natural regions. The advantage of this specification is that it separates the impact of the arrival of the phones from other ongoing trends in regional outcomes, to the extent that these trends are roughly linear.

[7] In all estimations we cluster the estimated standard errors at the village level.

6) Results and discussion

6.1. Agricultural outcomes

We first look at agricultural outcomes. Specifically, we are interested in testing whether access to TC has led to increases in prices received by farmers for their crops and reductions in prices paid for inputs. However, the survey does not ask directly for prices. Therefore we look at the real local currency value received per kilogram sold of agricultural production as a proxy for prices received by farmers. Table 7 reports estimates of β_1 for several agricultural outcomes. Column 1 suggests a 15.7% increase in the value per kilogram sold of agricultural production as a result of the program. This effect is consistent with the theoretical prediction that a decrease in search costs should increase the reservation prices at which farmers sell their produce. Columns 2 and 3 report estimates coming from specifications in which we add several controls such age, sex and education of the household head, household size, and house ownership status. Our estimates remain virtually unchanged and provide further support suggesting that treatment timing was not correlated with variables that may have affected the outcomes of interest. Finally, column 4 reports estimates from specification (14) which allows for differential trends by region. Again, our results remain qualitatively the same, suggesting that the introduction of TC has increased the value per kilogram sold by 14.8%.

Our second interest is to test whether TC has reduced the prices paid for agricultural inputs. Unfortunately, the dataset does not provide information regarding the quantity of inputs used. It only provides information regarding the total annual costs of the agricultural activity. However, as Table 7 shows, the introduction of TC has not had any affect on the quantity of agricultural production. Therefore, if we assume that the quantity used of inputs remain constant as the quantity produced does so; estimated effects on agricultural costs will be mainly reflecting effects on input prices rather than quantities. Accordingly, column 1 shows that TC has reduced annual agricultural costs by 22.3%. Our estimate is robust to the inclusion of several controls according to columns 2 and 3. Finally, when including differential trends by region, the finding remains robust suggesting a 21.5% reduction in agricultural costs. The estimated impacts are in line with the theoretical predictions in the sense that the reduction in search costs should decrease prices paid for inputs.

Our results show that farmers have been positively affected by receiving better prices for their output and paying lower prices for their inputs. Therefore, profitability of farming activity has increased. In that way, we take the ratio of the value of agricultural production to total costs as our first measure of productivity. Then by taking the natural log to this ratio we have the continuously compounded rate of return to agricultural activity. Our baseline estimate shown in Table 7, column 1 shows that TC has increased this measure of productivity by 19%. This estimate is robust to the inclusion of several control variables. Finally, when including differential trends by region, the result remains qualitatively unchanged suggesting an increase of 18.1% in productivity. It is worth noting that while our estimates may seem large, they are in line with previous literature regarding the effects of TC. For example, Jensen (2007) reports an increase of 9% in fishermen average profits in Kerala - India as a result of cellphone coverage. Aker (2008) reports a 29% increase in grain trader's profits in Niger after cellphone rollout. Therefore, our estimates are situated between previous estimated effects.

We also look at physical productivity measured by the ratio of total production (in kilograms) to total costs. When taking the natural log to this measure we end up with the continuously compounded return (expressed in physical production) with respect to agricultural investments. Table 7, column 1 shows that TC has increased this measure of productivity by 23%. This estimate is also robust to the inclusion of different control variables. When adding differential trends the point estimate remains virtually unchanged suggesting and increase of 22.3% in productivity.

Our results clearly show that the intervention significantly increased the productivity and profitability of farming activities. Therefore, benefited households have received an exogenous shock to the net income perceived for each unit of time devoted to agricultural activities. These results are totally in line with our theoretical predictions and provide an opportunity to test the effects of this shock on the form in which households distribute the time of their children. Accordingly, the next section explores the effect of this intervention on the demand for child labor.

6.2. Child labor effects

As pointed out in the theoretical section, we have no a-priori expectation regarding the direction and size of the program's effect on the demand for child labor. The ultimate effect will depend on whether the income or substitution effect dominates in such demand. The dataset provides information about the main activity in which each household member was engaged in the week leading to the survey. Therefore, in order to measure child labor demand, we compute indicators for market work, agricultural work, and wage work as main activities. Table 8 report estimated effects for such variables.

Our results clearly suggest a negative effect of the program on the demand for child labor. For instance, column 1 evidences that the introduction of TC decreased the likelihood of reporting any market work as main activity by 14.6 percentage points. This effect is robust to the inclusion of several control variables such as sex and age of children, age and education of the household head, and home ownership status. When including differential trends in the specification, the estimated effect remains robust suggesting a reduction of 13.6 percentage points in the likelihood of reporting any market work as main activity. When expressed relative to the baseline proportion of children engaged in market work, the estimated effect implies a 34.4% reduction in the probability of reporting market work as main activity. Therefore, our results suggest a dominant income effect in the demand for child labor.

We also evaluate effects on agricultural and wage work. Given that we are focused on agricultural households, we would expect that reductions in child labor might be concentrated in agricultural work. Our empirical results confirm such expectations. Table 8, column 1 suggests a 9.8 percentage points drop in the likelihood of reporting agricultural work as main activity. This result is robust to the inclusion of several control variables as shown in columns 2 to 4 of the table. In addition, column 5 reveals that adding differential trends leaves results practically unchanged suggesting a 9.1 percentage points reduction in this likelihood. When expressed and a percentage reduction with respect to the baseline level of the outcome, our estimates imply a 23.5% reduction in the probability of reporting agricultural work as main activity.

Wage work has also been affected by the program with a lower absolute effect. Our preferred estimate (table 8 – column 5) suggests a 2.2 percentage points reduction. However, when expressed relative to the baseline level; the estimate implies a 28.9% reduction in the probability of reporting wage work as the main activity. All of our estimates strongly suggest a stronger income effect in the demand for child labor in Peruvian rural villages. This finding is consistent with Dammert (2008) where she reports a 12.3 percentage points increase in child market work among coca-growing regions after a successful coca eradication program during the late 1990's in rural Peru.

We further investigate whether the reduced probability in reporting work as main activity has impacted school enrollment. Table 8 reveals that there has been no impact on school enrollment. This result may seem puzzling but in the context of rural Peru virtually all children are enrolled in some school. For instance, 96.2% of children at baseline reported being enrolled in the school. Therefore, our estimated impacts only capture the fact that more children are now taking school as their main activity with respect to working. This is apparent when we look at the effect in the likelihood of children reporting school as their main activity over any kind of work. This effect is exactly the opposite as the reduction in the probability of reporting market work as the main activity and equivalent to a 13.6 percentage point's increase. This implies a 22.5% increase with respect to the baseline proportion of children reporting school as their main activity.

6.3. Heterogeneous effects

We first assess child labor heterogeneous effects with respect to gender. Table 9, columns 2 and 3 reveal that the probability of reporting any market work as main activity has been reduced evenly (in relative terms) for girls and boys. For instance, boys reduced this probability by 33% (0.144/0.437), while girls reduced it by 35% (0.125/0.356). This finding suggests no gender specific preferences for child labor reductions as a result of an exogenous income shock. However, when market work is disaggregated into agricultural and wage work, we observe that agricultural work has been only reduced for boys while wage work has been only impacted for girls. Column 5 suggests that the probability of reporting agricultural work as main activity has fallen by 25.5% (0.108/0.42.4) for boys. Column 9 shows that the probability of reporting wage work as main activity has been reduced by 48.6% (0.036/0.074) for girls.

Therefore, it is clear that the overall impact on the probability of realizing some kind of market work has been mainly concentrated on agricultural work for boys and wage work for girls. This pattern is consistent with gender differences in the allocation of time found in previous studies of Peru (Dammert 2008; Ersado, 2005; Ilahi, 2001; Levison and Moe, 1998; Ray, 2000). Boys are generally more active in agricultural work while girls more active in wage work (mainly composed by domestic work).

Table 10 displays results for heterogeneous effects with respect to parental education. Columns 1 and 2 reveal that reductions in the probability of reporting any kind of market work as main activity have been greater for children in households where the head has achieved at least a high school degree. For instance, in households where the head did not finish high school, the reduction in market child labor has been equivalent to 29.7% (0.133/0.448). However, in households where the head holds a high school or higher degree, this reduction accounted for 45.2% (0.14/0.31). This evidence suggests that parents with relatively higher education take their children out of working activities in a higher proportion than lower educated counterparts. This effect implies that higher educated parents may value more human capital accumulation in the form of schooling for their children as compared with lower educated peers.

However, these effects are different when considering agricultural and market work separately. Findings in columns 3 to 6 suggests that agricultural work has only been reduced for children in households with lower educated heads; while wage work was only reduced for children with higher educated parents. The probability of reporting agricultural work as main activity fell by 25.1% (0.109/0.435) for children with lower educated parents. While the probability of reporting wage work as main activity fell by 53.7% (0.058/0.108) for children with higher educated parents.

Previous findings reflect the fact that agricultural work was much more common, at baseline, among children with lower educated parents. At baseline, 43.5% of children with lower educated parents reported agricultural work as their main activity; while only 30.6% of children with higher educated parents did so. Therefore, after the 10.9 percentage point's reduction in agricultural work among households with lower educated parents, these baseline proportions have been virtually equated.

Something similar happens for wage work. At baseline, 6.2% of children with lower educated parents reported wage work as their main activity; while 10.9% of children with higher educated parents did so. Therefore, after the 5.8 percentage point's reduction in wage work among households with higher educated parents, these baseline proportions have been virtually equated.

6.4. Sensitivity analysis

Table 11 - column 1 show estimation results excluding households living in the coast. Notice that estimated impacts for market and agricultural work are stronger than the aggregate effect for the whole country. This is explained by the fact that child labor is much less common

in the coast than in the rest of Peru. For instance, at baseline, only 21% of children living in the coast reported having some type of market work as their main activity. By contrast, this figure was equivalent to 44% in the rest of the country. Similarly, the proportion of children that reported agricultural work to be their main activity as baseline was equivalent to 19% in the coast and 43% in the rest of the country. In that way, we observe relatively stronger effects in zones where the ex-ante level of child labor was greater.

When looking at wage work, the exclusion of children living in the coast leads to insignificant effects. This is also explained by the fact that wage work is much more common in the coast than in the rest of the country. At baseline, 11% of children living in the coast reported wage work as their main activity; while only 7% did so in the rest of the country. Therefore, reductions in wage work have also been concentrated in the zone where this kind of labor was more common.

We now explore whether program effects have been similar for poor and non-poor zones. To do this, we merge our data with the 1993 census and classify the observed villages according to the district in which they are located. After this, we split the sample in villages located within districts above and below the median of the 1993 district-level poverty rate distribution. Columns 2 and 3 display estimation results for both sub-samples. Our results suggest virtually the same effect in child market work reductions for poor and non-poor villages (13.5 and 13.3 percentage point's reduction respectively). However, reductions in the incidence of agricultural work are only significant in the poorest districts with an effect equivalent to 12 percentage points. In addition, wage work has only been reduced in non-poor districts with an estimate of 4.2 percentage points. These findings are consistent in the sense that reductions in different types of child labor are stronger in zones with relatively higher incidence of it. For instance, at baseline, 47% of children living in the poorest districts reported having agricultural work as their main activity; while only 36% did so in non-poor areas. Similarly, 9% of children living in non-poor areas reported wage work as their main activity at baseline; while only 6.5% did so in poorer districts.

We also classified our sample by population density at the district level using the 1993 census. Columns 4 and 5 show these results. Interestingly, we observe that reductions in the probability of reporting market and agricultural work as main activities are only significant among villages located in districts above the median density. These results were somehow expected given that denser areas have more potential workers to replace the decreased child labor. Therefore, in areas with lower density, the incidence of child labor has remained unchanged given the relatively lower external labor supply that may have served to replace children in household's labor demand.

Finally, column 6 shows that when we exclude migrants (defined as children living in households where the head was born outside the district of current residency); estimated effects become stronger than the ones obtained using the whole sample. This finding was also expected in the sense that migrant households may need more labor in order to establish some economic security in a relatively new place. Therefore, these households may demand relatively higher levels of child labor than non-migrant counterparts. In that way, as a result, children in non-migrant households have reduced work incidence relatively more than their migrant counterparts.

7) Robustness analysis

7.1. Falsification tests

We first conduct a falsification test to verify the validity of our estimates. We do so by re-estimating model (14) with a variant in the indicator for treatment status ($Post_{jt}$). Specifically, we falsify the treatment indicator pretending that phones arrived one year before the real date. Therefore, provided that our estimates were causal impacts of the program, we expect that estimated program effects coming from the falsified regression should be statistically indistinguishable from zero. Tables 12 and 13 show the estimation results. As expected, none of the coefficients of interest are statistically significant at any conventional level. These results give further confidence regarding the causality of our estimates.

7.2. Survey design issues

As mentioned earlier, we built a panel dataset at the village level using the PLSMS for years 1997 and 2000 and the ENAHO for years 2001 through 2007. Although both surveys are representative at the national level and all of our regressions are weighted using the inverse of sampling probability to control for survey design. We can not ignore the issue that the sampling framework was different for both surveys. Therefore, in order to test for the robustness of our results, we re-estimate model (14) using only the observations coming from the ENAHO survey (years 2001 through 2007). Tables 14 and 15 display the estimation results. Estimated impacts using the trimmed sample are virtually the same as the estimated effects coming from the complete dataset. Therefore, it appears that survey design is not an issue of concern in our dataset.

7.3. Event studies

With the purpose of disaggregating the before-after effects previously estimated into bimonth-by-bimonth effects, we add flexibility to model (14) by estimating regression equations similar to:

$$O_{ijt} = \alpha_j + \phi_t + \sum_{-6}^{+7} \beta_p D_{jp} + X_{ijt}^{'}\gamma + Coast_j t + Highlands_j t + Jungle_j t + \varepsilon_{ijt} \qquad (15)$$

where D_{jp} is an indicator for the p^{th} bimonth after the phone was operative (starting at zero, i.e. p=0 is the bimonth in which the phone became operative) in village j.[8] 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 bimonth before the phone becomes operative. All other variables are defined as in (14).

Estimated β_p coefficients for the value per kilogram of agricultural production sold along with their 95% confidence intervals are shown in Figure 4. Notice that point estimates bounce around zero before the intervention being all insignificant. This observation gives further confidence for the validity of our approach, since we do not see evidence of any trend prior to the installation of the phones. Then, starting the bimonth in which the phone is operative, estimated impacts become positive, significant and increasing over time. A very similar pattern is observed for agricultural productivity (measured as the ratio of total production value to costs) in Figure 5. No significant point estimates before phone installation, while positive and significant impacts after the intervention.

Child labor effects are evidenced in Figures 6 and 7. Figure 6 plots estimated β_p coefficients regarding the probability of reporting any type of market work as main activity. Again, we observe insignificant estimated coefficients prior to the intervention bouncing around without a discernible pattern. Then, we start observing negative estimated impacts starting one

[8] Notice that we use observations from households surveyed within a window of one year before and one year after the installation of the phone.

bimonth after the phone is installed. These negative effects show a general decreasing pattern over time. Similarly, Figure 7 shows estimated impacts regarding the probability or reporting agricultural work as main activity. The pattern closely resembles the one for market work, no significant estimates or trends prior to the intervention and negative impacts starting in the first bimonth after the reception of the phone.

8) Summary and conclusions

This paper exploits an exogenous provision of public payphones among isolated villages in rural Peru to identify the effects of telecommunication technologies (TC) on agricultural productivity and child labor. The main results suggest that the value received per kilogram of agricultural production increased by 14.8%. In addition, agricultural costs were reduced by 22.6%. Both of these impacts determined an increase equivalent to 18.1% in agricultural productivity. Moreover, this income shock has been translated in a reduction of child market work equivalent to 34.4% and a reduction in child agricultural work of 23.5%, suggesting a dominant income effect in the demand for child labor.

A range of evidence support these results i) results are robust to the inclusion of household characteristics, child characteristics, village fixed effects and differential trends by geographical regions, ii) differences in effects by population density are also consistent with the notion that areas with higher potential labor supply to substitute child labor have greater impacts in children's time allocation, iii) there are differential effects by child gender and by education of the head of household suggesting that child labor is reduced more for stratus with higher ex-ante incidence of such behavior. Given high school enrollment rates in rural Peru, and the fact that no effect is found in such variable; these results indicate that children are not exiting work completely but that their main focus is shifting towards studying. Finally, event studies analyses show that no pre-existing trends were present with respect to the outcomes of interest and that the estimated impacts started being significant as a result of phones introduction.

Overall, these results provide evidence of the great development potential that TC can offer to poor rural households. By reducing asymmetric information, farmers are able to obtain better prices for their production and inputs, thereby increasing their productivity. Moreover, findings regarding a dominant income effect in the demand for child labor, may suggest that any schooling related conditionality imposed to get some cash transfer or subsidy is not necessary.

This because the unintended effect of potential increased demand for child labor as a result of a higher value per unit of time devoted to work appears not to be dominant in rural Peru. By contrast, higher schooling investments after an income shock appear to be incentive compatible among poor Peruvian rural farmers.

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Figures

Figure 1: Intervened villages by treatment timing

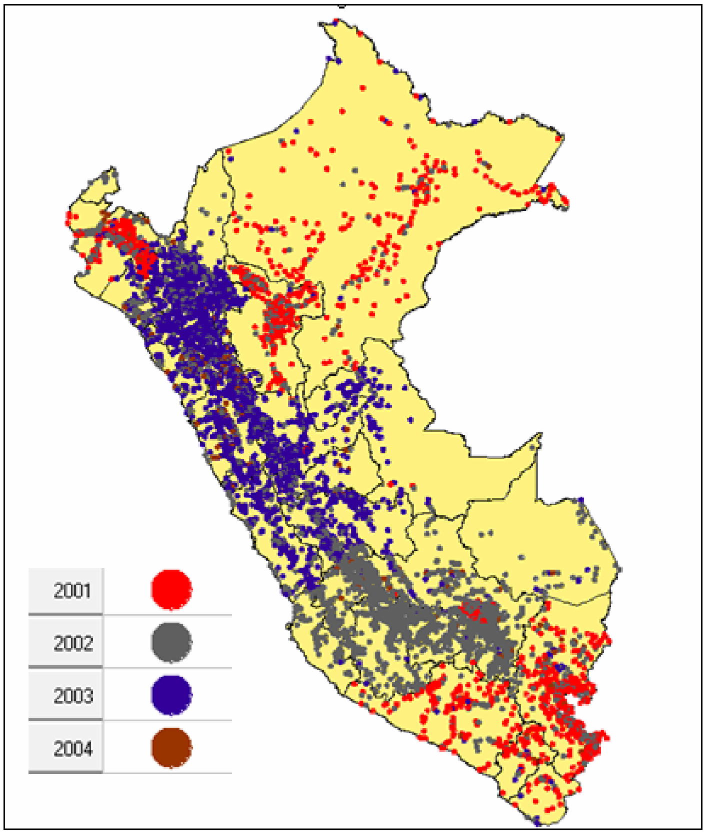


Figure 2: FITEL Program

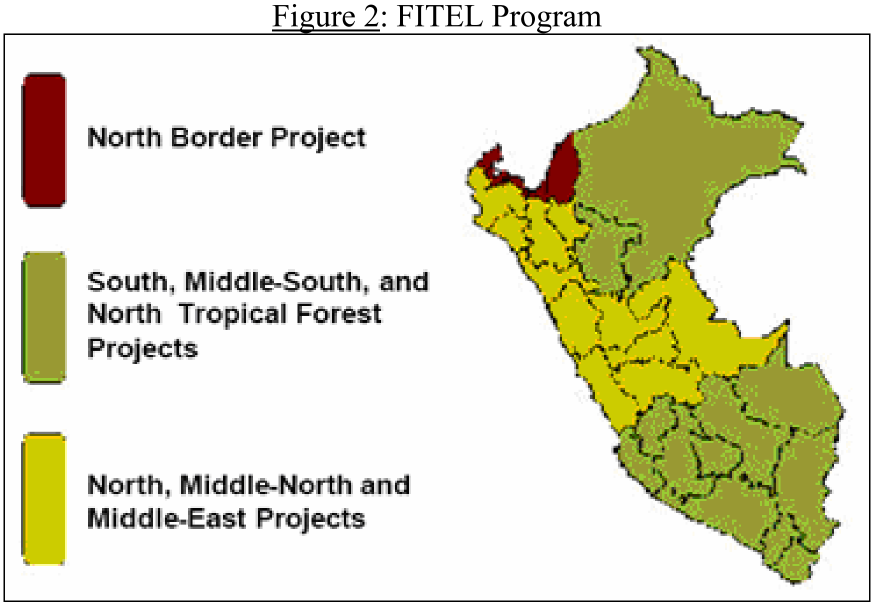
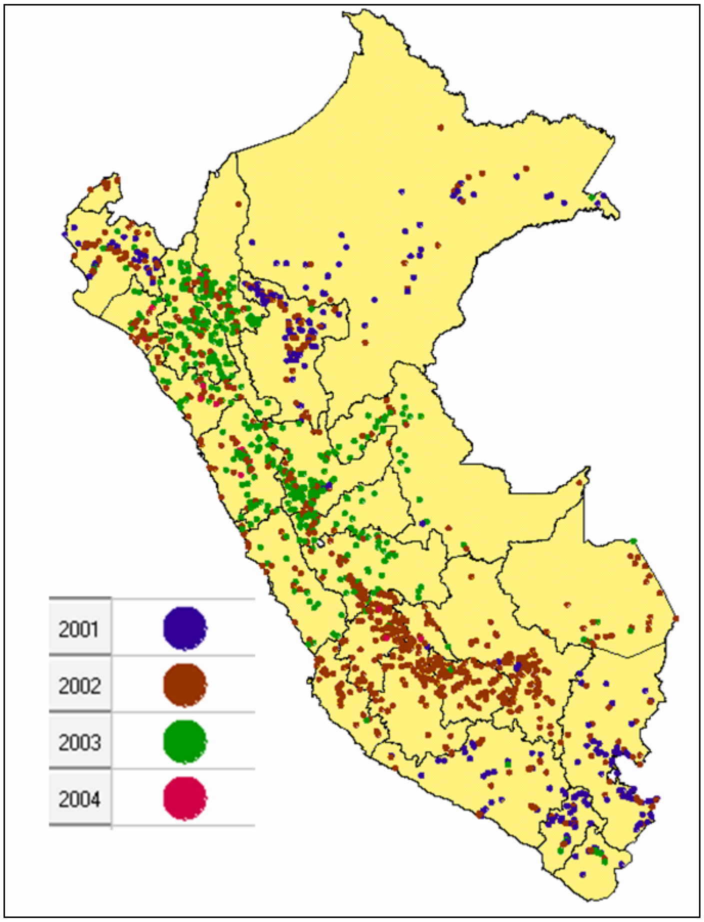


Figure 3: Sampled villages by treatment timing



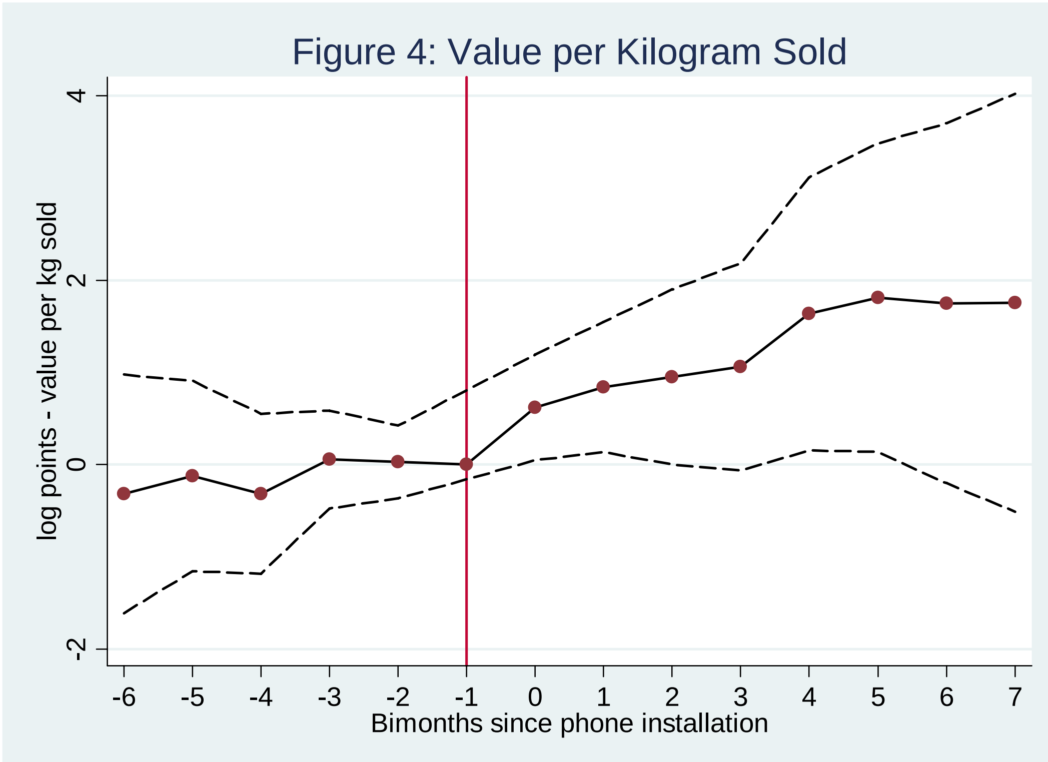


Figure 4: Value per Kilogram Sold
log points - value per kg sold
-2
0
2
4
-6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7
Bimonths since phone installation

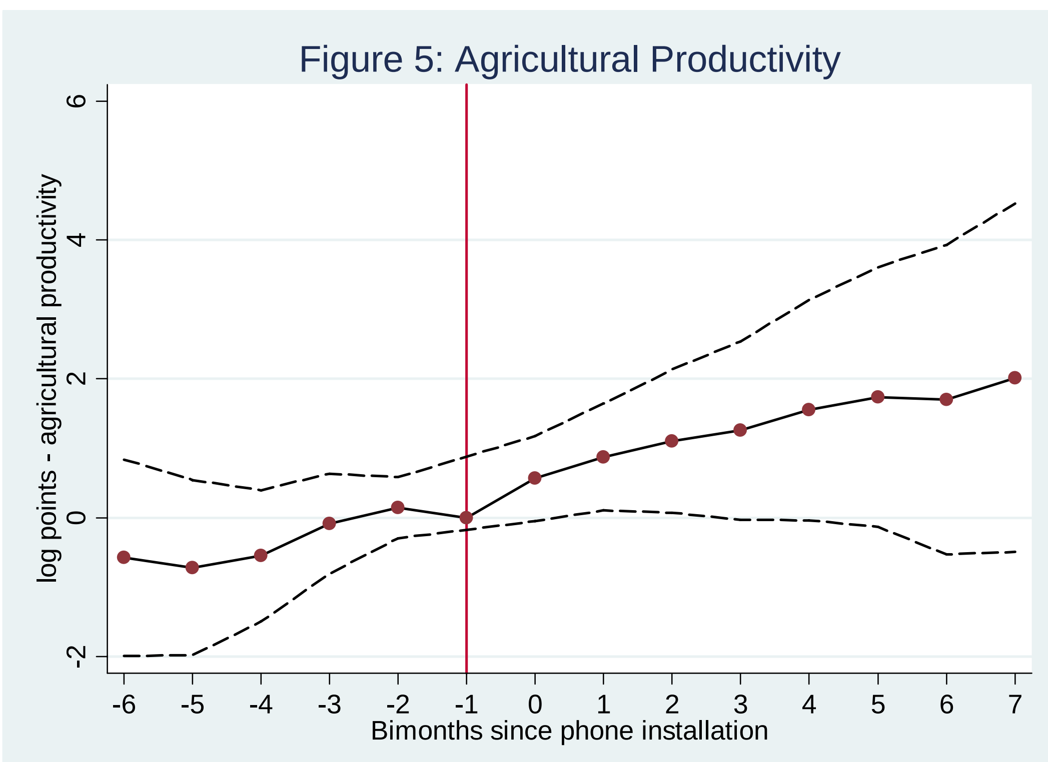


Figure 5: Agricultural Productivity
log points - agricultural productivity
-2
0
2
4
6
-6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7
Bimonths since phone installation

Figure 6: Child Market Work

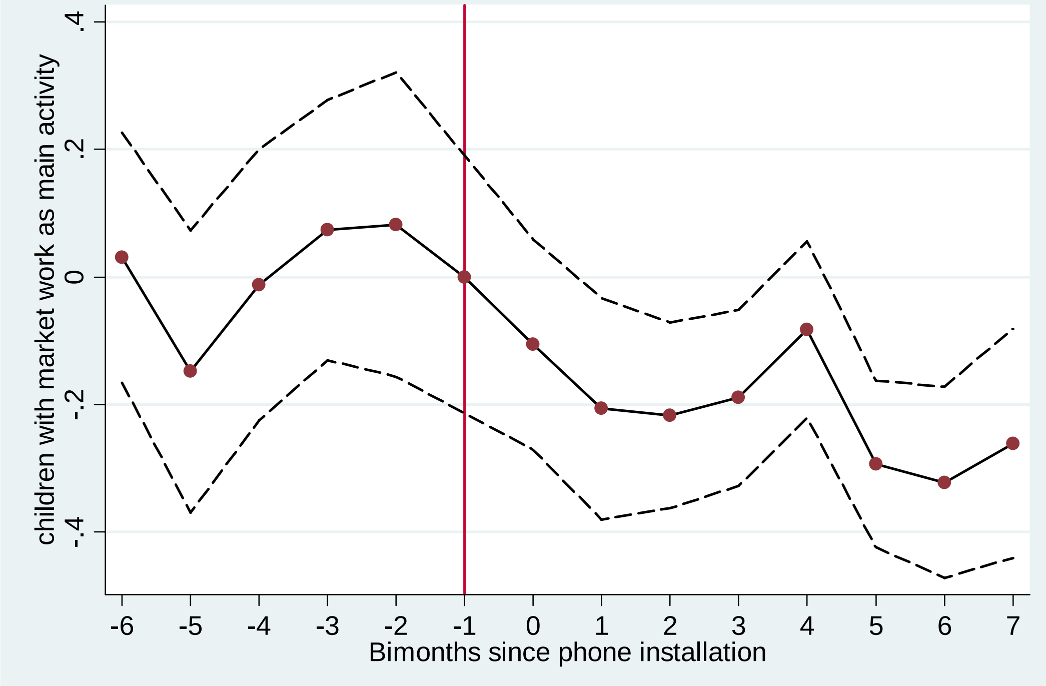
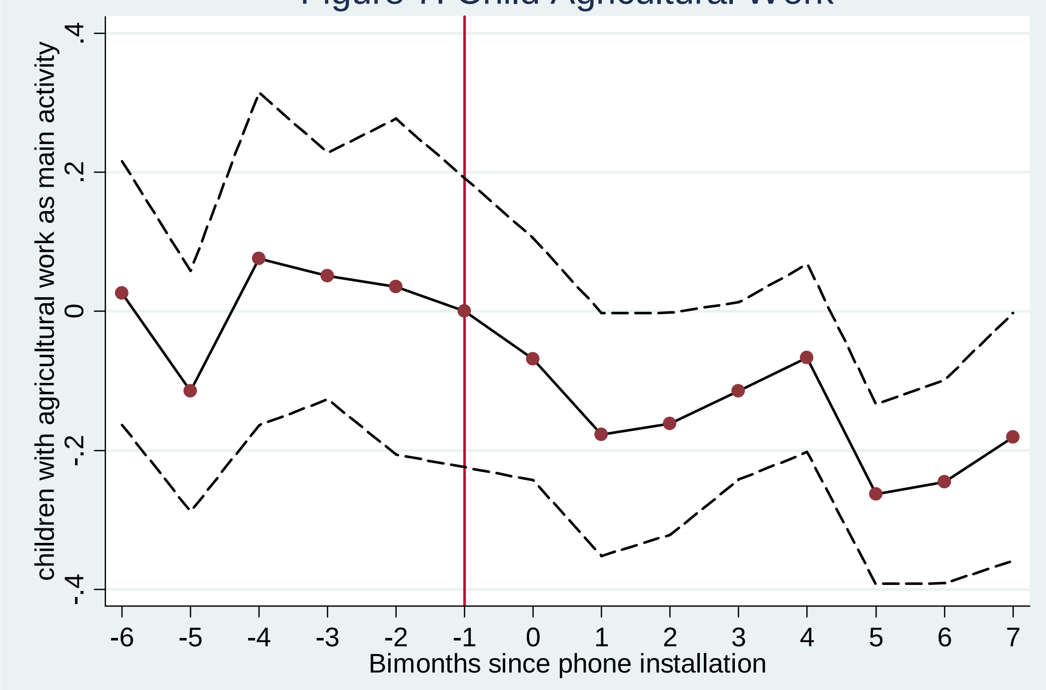


Figure 7: Child Agricultural Work



Tables

Table 1: Timing of FITEL intervention

Year of Treatment	Number of Treated Villages	Percent	Cummulative
1999	213	3.27	3.27
2001	1,184	18.19	21.46
2002	2,666	40.96	62.42
2003	2,368	36.38	98.80
2004	78	1.20	100.00
Total	6,509		

Table 2: Households sample size by survey year and treatment timing

Survey year	Treated early	Treated late	Total sample
(1)	(2)	(3)	(4)
1997	161	93	254
2000	224	107	331
2001	1,132	767	1,899
2002	1,409	666	2,075
2003	1,127	572	1,699
2004	615	393	1,008
2005	1,604	916	2,520
2006	1,610	862	2,472
2007	2,047	937	2,984
Total	9,929	5,313	15,242

The sample refers to households reporting both agricultural production and costs. Treated early refers to households in villages that received a phone between 2001 and 2002. Treated late refers to households in villages that received a phone between 2003 and 2004.

Table 3: Children sample size by survey year and treatment timing

Survey year	Treated early	Treated late	Total sample
(1)	(2)	(3)	(4)
1997	353	157	510
2000	423	205	628
2001	1,605	1,001	2,606
2002	1,923	903	2,826
2003	1,433	729	2,162
2004	872	469	1,341
2005	2,061	1,161	3,222
2006	1,858	979	2,837
2007	2,122	1,155	3,277
Total	12,650	6,759	19,409

The sample refers to children between 6 and 13 years old. Treated early refers to children in villages that received a phone between 2001 and 2002. Treated late refers to children in villages that received a phone between 2003 and 2004.

Table 4: Village sample size by survey year and treatment timing

Survey year	Treated early	Treated late	Total sample
(1)	(2)	(3)	(4)
1997	30	17	47
2000	40	19	59
2001	149	93	242
2002	232	108	340
2003	187	90	277
2004	102	59	161
2005	264	150	414
2006	264	139	403
2007	343	167	510
Total	1,611	842	2,453

The sample refers to villages receiving a phone. Treated early refers to villages that received a phone between 2001 and 2002. Treated late refers to villages that received a phone between 2003 and 2004.

Table 5: Baseline differences for agricultural households

Survey year:	1997	2000	2001
	Late - Early	Late - Early	Late - T2002
	(1)	(2)	(3)
Household head characteristics			
Age	-1.668	-2.015	-0.275
	(1.963)	(2.653)	(1.047)
High education (1=secondary+)	0.058	-0.062	-0.075*
	(0.062)	(0.054)	(0.030)
Home ownership	-0.051	-0.031	0.009
	(0.073)	(0.043)	(0.026)
Agricultural outcomes (in natural logs)			
Annual production (value)	0.011	-0.113	0.117
	(0.293)	(0.281)	(0.141)
Annual production (kgs.)	-0.103	-0.007	0.079
	(0.310)	(0.256)	(0.185)
Value per kg. sold	0.175	-0.203	0.027
	(0.237)	(0.126)	(0.105)
Annual costs	0.068	-0.162	0.013
	(0.353)	(0.337)	(0.172)
Productivity 1: production (value)/costs	-0.020	0.157	0.091
	(0.267)	(0.265)	(0.130)
Productivity 2: production (kgs.)/costs	-0.142	0.258	0.049
	(0.263)	(0.271)	(0.139)
Production sold/total production (kgs.)	-0.049	-0.060	0.088
	(0.075)	(0.075)	(0.053)
Production consumed/total production (kgs.)	0.237	0.348	-0.269
	(0.192)	(0.204)	(0.198)
Observations	323	410	1759

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Late refers to villages treated during 2003 or 2004. Early refers to villages treated during 2001 or 2002. T2002 refers to villages treated during 2002.
* Statistically significant at 5% level.

Table 6: Baseline differences for children between 6 and 13 years old

Survey year:	1997	2000	2001
	Late - Early	Late - Early	Late - T2002
	(1)	(2)	(3)
Child characteristics			
Age	0.060	-0.180	-0.033
	(0.182)	(0.181)	(0.116)
Gender (1=male)	-0.081	-0.074	-0.029
	(0.053)	(0.043)	(0.027)
Child outcomes			
Market work	-0.056	-0.054	-0.045
	(0.103)	(0.072)	(0.058)
Agricultural work	-0.045	-0.056	-0.037
	(0.104)	(0.072)	(0.059)
Wage work	-0.011	-0.006	-0.008
	(0.006)	(0.007)	(0.022)
School - enrollment	0.031	-0.020	-0.043*
	(0.019)	(0.020)	(0.014)
School - main activity	0.056	0.054	0.045
	(0.103)	(0.072)	(0.058)
Observations	510	628	2314

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Late refers to villages treated during 2003 or 2004. Early refers to villages treated during 2001 or 2002. T2002 refers to villages treated during 2002.
* Statistically significant at 5% level.

Table 7: Estimated effects on agricultural outcomes

	Estimated Effects				Observations
	(1)	(2)	(3)	(4)	(5)
Dependent variables (in natural logs):					
Value per kg. sold	0.157*	0.155*	0.158*	0.148*	11495
	(0.086)	(0.085)	(0.086)	(0.087)	
Annual production (kgs.)	-0.032	-0.037	-0.036	-0.042	15742
	(0.101)	(0.101)	(0.101)	(0.100)	
Annual costs	-0.223**	-0.226**	-0.224**	-0.215**	15339
	(0.108)	(0.107)	(0.107)	(0.106)	
Productivity 1: production (value)/costs	0.190**	0.184**	0.182**	0.181**	15242
	(0.089)	(0.089)	(0.089)	(0.089)	
Productivity 2: production (kgs.)/costs	0.230*	0.226*	0.225*	0.223*	15242
	(0.118)	(0.118)	(0.118)	(0.118)	
Household characteristics	No	Yes	Yes	Yes	
House ownership status	No	No	Yes	Yes	
Differential trends by natural region	No	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 month and village fixed effects. Household characteristics include household size, as well as sex, age and education level of the household head. Ownership status is an indicator for house formal property. The natural regions are coast, highlands and jungle. * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 8: Estimated effects on children's outcomes

	Estimated Effects					Observations
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables:						
Market work	-0.146***	-0.142***	-0.140***	-0.140***	-0.136***	19391
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	
Agricultural work	-0.098**	-0.096**	-0.095**	-0.094**	-0.091**	19391
	(0.041)	(0.040)	(0.040)	(0.040)	(0.040)	
Wage work	-0.024*	-0.022*	-0.022*	-0.022*	-0.022*	19391
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	
School - enrollment	0.005	0.004	0.004	0.004	0.004	19250
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	
School - main activity	0.146***	0.142***	0.140***	0.140***	0.136***	19391
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	
Child characteristics	No	Yes	Yes	Yes	Yes	
Household head characteristics	No	No	Yes	Yes	Yes	
House ownership status	No	No	No	Yes	Yes	
Differential trends by natural region	No	No	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 month and village fixed effects. Market work includes wage employment, self-employment, agriculture, helping in a family business, domestic work in an external household, among others. Child characteristics include sex and age. Household head characteristics include age and education level. Ownership status is an indicator for house formal property. The natural regions are coast, highlands and jungle. * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 9: Child labor by gender

Dependent Variable:	Market work			Agricultural work			Wage work		
	All	Boys	Girls	All	Boys	Girls	All	Boys	Girls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post	-0.136***	-0.144***	-0.125***	-0.091**	-0.108**	-0.072	-0.022*	-0.005	-0.036**
	(0.041)	(0.048)	(0.048)	(0.040)	(0.047)	(0.045)	(0.012)	(0.013)	(0.018)
Observations	19391	9721	9670	19391	9721	9670	19391	9721	9670
R-squared	0.40	0.46	0.44	0.41	0.46	0.44	0.17	0.25	0.26

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 month and village fixed effects, child characteristics (sex and age), household head characteristics (age and education level), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 10: Child labor by parental education

Dependent Variable:	Market work		Agricultural work		Wage work	
	Low educ.	High educ.	Low educ.	High educ.	Low educ.	High educ.
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.133***	-0.140**	-0.109**	-0.031	-0.003	-0.058*
	(0.047)	(0.063)	(0.046)	(0.064)	(0.013)	(0.031)
Observations	13196	6195	13196	6195	13196	6195
R-squared	0.43	0.52	0.44	0.52	0.23	0.30

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Low education refers to household head with primary or lower education. High education refers to household head with secondary or higher education. All regressions include month and village fixed effects, child characteristics (sex and age), household head characteristics (age and education level), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 11: Child labor sensitivity analysis

	Excluding coast	%Poor<median	%Poor>median	Low population density<median	High population density>median	Without migrants
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variables:						
Market work	-0.153***	-0.133***	-0.135**	-0.043	-0.161***	-0.176***
	(0.046)	(0.051)	(0.065)	(0.054)	(0.062)	(0.052)
Observations	17193	9317	10074	9274	10117	13254
R-squared	0.40	0.40	0.42	0.44	0.40	0.43
Agricultural work	-0.118***	-0.061	-0.120*	-0.003	-0.122**	-0.119**
	(0.046)	(0.049)	(0.063)	(0.051)	(0.062)	(0.053)
Observations	17193	9317	10074	9274	10117	13254
R-squared	0.40	0.41	0.42	0.43	0.40	0.43
Wage work	-0.013	-0.042***	-0.000	-0.029	-0.008	-0.031**
	(0.013)	(0.016)	(0.016)	(0.022)	(0.013)	(0.014)
Observations	17193	9317	10074	9274	10117	13254
R-squared	0.17	0.19	0.17	0.21	0.15	0.19

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Market work includes wage employment, self-employment, agriculture, helping in a family business, domestic work in an external household, among others. All regressions include month and village fixed effects, child characteristics (sex and age), household head characteristics (age and education level), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). Percentage of poor people and population density refer to the district of residency (data from the 1993 Peruvian Census). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 12: Falsification test – agricultural outcomes

Dependent Variable (in natural logs):	Production in kgs.	Sell value per kg.	Agricultural costs	Productivity value/costs	Productivity kgs./costs
	(1)	(2)	(3)	(4)	(5)
Post	0.109	-0.024	0.031	0.023	0.075
	(0.144)	(0.115)	(0.144)	(0.136)	(0.165)
Observations	15242	11495	15242	15242	15242
R-squared	0.50	0.37	0.45	0.40	0.44

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 month fixed effects, village fixed effects, household characteristics (household size, as well as sex, age and education level of the household head), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 13: Falsification test – children outcomes

Dependent Variable:	Market work	Agricultural work	Wage work	School enrollment	School is main activity
	(1)	(2)	(3)	(4)	(5)
Post	0.010	0.027	0.006	0.023	-0.010
	(0.071)	(0.066)	(0.029)	(0.021)	(0.071)
Observations	19391	19391	19391	19250	19391
R-squared	0.40	0.41	0.17	0.70	0.40

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 month and village fixed effects, child characteristics (sex and age), household head characteristics (age and education level), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

Table 14: Estimated effects dropping years 1997 and 2000 – agricultural outcomes

Dependent Variable (in natural logs):	Production in kgs.	Sell value per kg.	Agricultural costs	Productivity value/costs	Productivity kgs./costs
	(1)	(2)	(3)	(4)	(5)
Post	-0.077	0.121+	-0.221**	0.179**	0.220*
	(0.097)	(0.083)	(0.106)	(0.089)	(0.118)
Observations	14657	11013	14657	14657	14657
R-squared	0.51	0.39	0.43	0.40	0.46

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 month fixed effects, village fixed effects, household characteristics (household size, as well as sex, age and education level of the household head), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). + significant at the 15% level; * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Table 15: Estimated effects dropping years 1997 and 2000 – children outcomes

Dependent Variable:	Market work	Agricultural work	Wage work	School enrollment	School is main activity
	(1)	(2)	(3)	(4)	(5)
Post	-0.149***	-0.103**	-0.023*	0.003	0.149***
	(0.041)	(0.040)	(0.012)	(0.017)	(0.041)
Observations	18254	18254	18254	18112	18254
R-squared	0.44	0.44	0.18	0.73	0.44

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 month and village fixed effects, child characteristics (sex and age), household head characteristics (age and education level), ownership status (indicator for house formal property), and differential trends by natural regions (coast, highlands and jungle). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.