ABSTRACT

Title of dissertation: ESSAYS ON PUBLIC AND DEVELOPMENT ECONOMICS

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In Chapter 1, we study an intervention in which the Peruvian government provided public payphones to 6,509 rural villages that did not have communication services before. We show that the intervention timing was orthogonal to potential outcomes and exploit it using a panel of treated villages. Findings suggest increases of 16 percent in prices received by farmers for their crops, and a 23.7 percent reduction in agricultural costs. This income shock has been translated into a reduction in child market and agricultural work of 13.7 and 9.2 percentage points respectively. Findings are consistent with a dominant income effect in child labor demand.

In Chapter 2, we exploit a randomized intervention directed towards enhancing local governments' efficiency in three regions of rural Russia (Adyghea, Penza and

Perm) at the onset of a major decentralization reform. We find that satisfaction levels with decentralized services increased only in the region with relatively higher ex-ante experience with decentralized and participatory decision making (Penza). Moreover, we find that settlements with high pre-treatment accountability levels were differentially benefited by the intervention. Our findings suggest that short-term interventions do not translate into higher satisfaction with local public services. Rather, it appears that enhancing local managerial efficiency in delivering public services is a long-term process and that intensive interventions translate into higher satisfaction provided to local governments with relatively longer institutional experience and higher levels of accountability.

In Chapter 3, we use the timing of cell phone coverage in Peru as an exogenous shock to investigate the effects of phone coverage on several measures of economic development. We exploit a unique dataset drawn from information of private cell phone operators regarding the location, date of installation and technical characteristics of their towers from 2001 through 2007. We then merge this information with national household surveys spanning the same period. Estimates suggest an increase of 7 percentage points in the likelihood of self reported cell phone ownership after getting coverage, an increase of 7.5 percent in yearly household expenditures after coverage, and a 13.5 percent increase in the value of assets.

ESSAYS ON PUBLIC AND DEVELOPMENT ECONOMICS

by

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Chapter 1: Telecommunications Technologies, Agricultural Profitability, and Child Labor in Rural Peru

1) Introduction

Economic theory emphasizes the importance of information for the efficiency of markets (Stigler, 1961; Brown and Goolsbee, 2002). Accordingly, reductions in information search costs are expected to enhance market effectiveness. Recent advances in telecommunication technologies (TC) have made information transmission 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 interesting to assess how this improved market performance influences household decisions such as the utilization of child labor and schooling. Accordingly, the purpose of this paper is to shed light on how the introduction of payphones among rural villages in Peru affected agricultural profitability and the utilization 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 that was common before cell phone availability.

In the same vein, Aker (2010) 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 generate these outcomes is a reduction in search costs. Traders who operate in markets with cell phone coverage search over a greater number of markets and sell in more markets, thereby reducing price dispersion.

Recently, Goyal (2010) provides evidence regarding the effects of internet kiosks placement among rural districts in the Indian state of Madhya Pradesh. These kiosks provided real time information of soybean market prices to farmers. The study shows that the kiosks caused an increase of 1.7 percent in the monthly mode price of soy. This result supports the theoretical prediction that the availability of price information to farmers increases the competitiveness of traders in local output markets, leading to an increase in the price of soybean in the intervened districts.

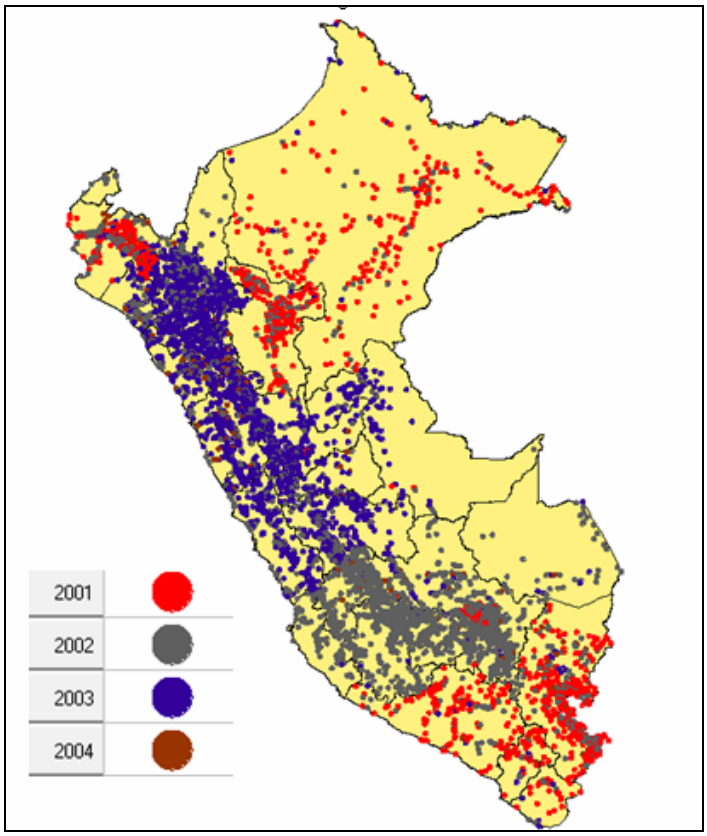
It is worth noting that the intervention studied here differs from the previous studies in that it involves public (satellite) payphones rather than cell phones or internet kiosks. This intervention occurred in places where neither cell phones nor fixed line phones were available. The treated villages were located in zones where cell phone

coverage was technically and economically unfeasible. The satellite technology implemented did not require villages to posses fixed lines or electrical supply in order to enjoy the service. Therefore, phone placement only followed the criteria of being provided to villages without prior access to TC. This coupled with differences in timing for phone placement that were uncorrelated with baseline characteristics, allows us to circumvent concerns common to previous studies regarding endogenous placement of TC with respect to the outcomes of interest.[1]

The specific intervention was carried out by the Peruvian Fund for Investments in Telecommunications (FITEL), which provided at least one public (satellite) payphone, mostly between years 2001 and 2004, to each of the 6,509 targeted villages situated across rural Peru (See Figure 1). None of these villages had any kind of phone services (either fixed lines or cell coverage) prior to the intervention, so these payphones were the first opportunity for villagers to communicate with the rest of the country without having to physically travel or use the mail. According to FITEL's documents, the intervention reduced the average distance from any rural village in Peru to the nearest communication point from 60km. to 5km.[2] I exploit differences in the timing of the intervention across villages to identify the impacts of payphones on agricultural profitability and the utilization of child labor, after showing that these differences in timing were orthogonal to changes in potential outcomes.

[1] This concern comes from the fact that previous studies have exploited differences in the timing of cell phone coverage across markets as if such differences were 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 only in disadvantaged villages, and below we show that the timing of it was orthogonal to potential outcomes and other variables that might be systematically related to them.
[2] This refers to the whole country in aggregate, not only an average across treated villages.

Figure 1: Intervened villages by treatment timing



Previous studies regarding the economic effects of TC concentrate on market outcomes, with a specific focus on price dispersion and market performance. However, none directly address effects of new TC on producers' profitability and how this potentially increased profitability may affect intra-household decisions regarding the utilization of child labor which is very common in rural Peru. This paper, therefore, contributes with new evidence regarding the effects of TC not only on market outcomes such as agricultural profitability but also on intra-household decisions. If TC affects agricultural profitability, the effects on child labor utilization are ambiguous. On the one hand, the substitution effect implies that the opportunity cost of time for a child that is not working becomes higher. Therefore, this effect suggests an increased utilization of child

labor. However, on the other hand, an increased income enjoyed by the household suggests that the utilization of child labor will decrease and, therefore, the child will devote more time to activities representing normal goods for the household (such as leisure or schooling).

In sum, the total impact on child labor will be the net outcome of offsetting income and substitution effects. For instance, the international literature, using different sources of household income variation, has found mixed effects. Some studies find a dominant substitution effect (Duryea and Arends-Kuenning, 2003; Kruger, 2006; and Kruger, 2007). While others suggest a dominant income effect (Beegle et. al., 2006; Dehejia and Gatti, 2005; Dammert, 2008; Del Carpio, 2008; Del Carpio and Marcours, 2009). This paper is the first that uses variation arising from the introduction of TC to identify the impacts of agricultural profitability on child labor.

The main findings suggest that the intervention generated increases of 16 percent in the value perceived for each kilogram of agricultural production, and a 23.7 percent reduction in agricultural costs. This led to an increase of 19.5 percent in agricultural profitability (measured by the financial return to agricultural activities). Moreover, this income shock translated into a reduction in the incidence of child (6 – 13 years old) market work equivalent to 13.7 percentage points and a reduction in child agricultural work of 9.2 percentage points. Overall, the evidence suggests a dominant income effect in the utilization of child labor.

The rest of the paper 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 main results, while section 7 checks the robustness of these results. Finally, section 8 concludes.

2) The FITEL Program

In 1992, the Peruvian government privatized all state-owned telecommunications companies and created a Telecommunications Regulatory Authority (OSIPTEL).[3] In May 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 2006, FITEL was declared an individual public entity ascribed to the Ministry of Transports and Communications.

The specific FITEL intervention studied here provided at least one public (satellite) payphone to each of the 6,509 targeted villages. To do so, FITEL divided the country into seven geographical regions (i.e. north border, north, middle north, middle east, south, middle south, and north tropical forest). The project was executed by granting a 20-year concession to private operators for public telephone 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. It is worth noting that all phones, regardless of which operator wins each region, had to be homogeneous with respect to the technology (i.e. satellite vsat phones). Targeted villages were selected by FITEL prior to the auctioning process following the three-phase procedure described below.

[3] Prior to 1992 the telecommunications sector was state-owned and no private firms existed.

2.1. Village Selection Criteria

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

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

b) Phase II: Villages in the target universe 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 a 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 highest population within the cell or be a central village in the sense that villagers in the cell confluence to that village 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 of field visits to all of the cell centers. The purpose of this field work was to assess the technical viability of installing payphones. In addition, several workshops were conducted in district capitals that were selected as cell centers. These workshops encouraged the participation of district leaders and representatives of local civil society. The purpose of these workshops 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 selected.

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 differences in the timing of the intervention across villages in order to identify causal impacts. This timing is briefly explained below.

2.2. Intervention Timing

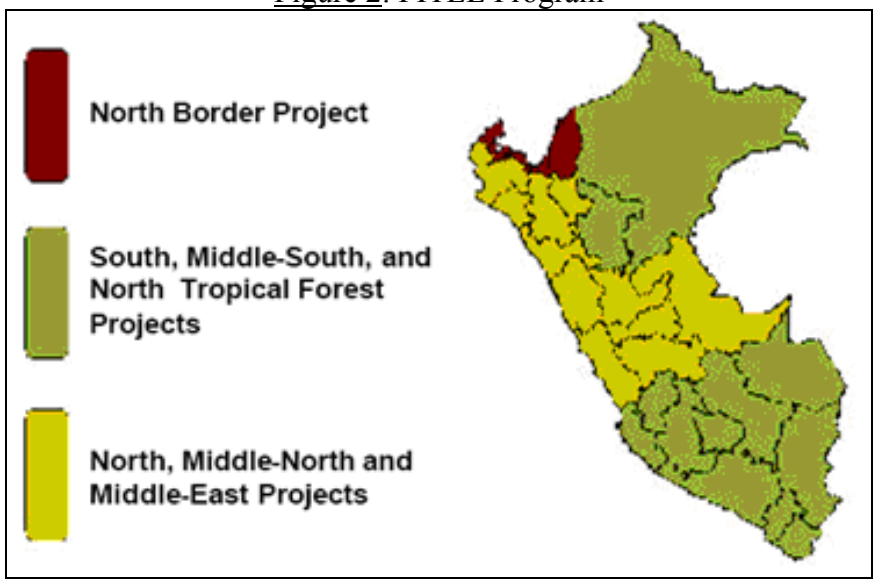
Once targeted villages were selected, FITEL auctioned 20-year concessions for each one of the seven geographical zones: north border, north, middle north, middle east, south, middle south, and north tropical forest. Initially, FITEL planned that all payphones would be operative by the first quarter of 2002. However, delays in the auctioning process determined that the program rollout lasted until year 2004. This timing is detailed in Table 1 and 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 phone rollout.

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		

Accordingly, the identification strategy will exploit differences in the intervention timing at the village level, which as we will show below was orthogonal to baseline outcomes and to variables plausibly related to them. In the empirical analysis, however, we exclude villages treated in 1999 (north border project). These because the 213 villages treated in 1999 were treated first for potentially endogenous reasons, due to their importance as a border with Ecuador (this region is highlighted in Figure 2).[4]

Figure 2: FITEL Program



3) Expected Outcomes

The mechanisms through which access to TC may impact agricultural profitability are diverse. First, the presence of TC greatly decreases the costs associated with

[4] However, results remain qualitatively the same, when these villages are included.

searching for information across different markets in order to sell (buy) agricultural production (inputs) in places offering the best prices. Second, by allowing farmers to be informed about the real market price of their crops, TC increases farmers' 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 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.

The previous mechanisms may coexist, of course, 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 treated households use the technology to search for market information. This is the second most important reason for using the phone (the first was social/family communication, at 95.3 percent). Furthermore, when looking only at households engaged in agricultural production, 38 percent report searching market information as the main usage. In addition, 70 percent of households who report 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 affected agricultural profitability is likely a reduction in search costs. We now present a simple model that formalizes this mechanism.

3.1. Effects on Prices

We assume that farmers derive utility from their agricultural activity through a Bernoulli utility function defined over output and input prices (net of transport costs) as follows:

$$u(P_o, P_i) = v(P_o) - g(P_i) \tag{1}$$

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

In addition, we assume a constant marginal cost C of searching for price information in an additional market. Therefore, if a farmer has already searched for prices in N markets, with O being 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:

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

where $\bar{P}_o$ and $\underline{P}_i$ represent the maximum possible output price and minimum possible input price respectively. $F(.)$ and $G(.)$ are the CDFs of output and input prices respectively. Notice that (2) assumes that if the utility derived from prices found in the N+1 search is below the reservation utility (derived from prices O and I), then the farmer will sell his output at price O and buy his input at price I.[5] So, in that case, the benefit of the N+1 search will be actually a cost of C. This depends on the probabilities of getting better prices. All else equal, as these probabilities fall, will be less attractive to search in another market. Therefore, optimality implies (assuming an interior solution) that the farmer will set his reservation price for output (R) and maximum price paid for the input (M) by equating the expected marginal benefit of the N+1 search to zero. Therefore, the

[5] Notice that this assumes that outputs are sold and inputs purchased in the same market.

reservation price for output and maximum price for the input will be implicitly defined by:

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

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

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

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

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

Clearly, (4)-(5) imply that reservation prices should rise and maximum prices paid for inputs should fall if search costs decrease. The introduction of TC dramatically reduced search costs. In particular, the intervention reduced average distance to the nearest communication point from 60 km. to 5 km. nationwide. Thus, we expect that average reservation prices will rise (prices paid for inputs will fall) and therefore agricultural profitability will rise following the installation of payphones.

3.2. Effects on Child Labor

In the context of rural villages, child labor in farms is very common. Parents decide how to allocate their children's time between school and work. An increase (decrease) in the prices that farmers get for 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. Therefore, the substitution effect implies that an increased opportunity cost of schooling will generate a reduction in its demand and, consequently, an increase in the utilization of child labor.

On the other hand, an increase in per unit profits raises household income and, assuming that schooling is a normal good while child labor an inferior one, the income effect implies that demand for schooling will increase and utilization of child labor will decrease. As a result, the introduction of TC generates offsetting substitution and income effects on child labor. The income effect suggests that a reduction in search costs will decrease child labor, while the substitution effect suggests the opposite. Therefore, the total effect of the introduction of TC on the utilization of child labor is ambiguous.

To formalize the argument, consider a household where the father decides how much time a child will dedicate to school, *S*, and 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:

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

[6] I assume that working in the farm is not an activity that provides human capital to the child.

where $U' > 0$ and $U'' < 0$ for both arguments. The child's time, *T*, is assumed to be allocated between *S* and *F*:

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

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

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

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

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

Alternatively, 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:

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

Clearly, the effect of a decrease in search costs due to the introduction of TC is ambiguous. The substitution effect implies that child labor will increase with the

introduction of TC, while the income effect implies the opposite. The total effect will therefore depend on the relative weights that parents' utility assigns to consumption versus children's human capital and is, ultimately, an empirical question.

4) The Data

The dataset consists on a unique unbalanced panel of treated villages that has been constructed 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, succeeded by the Peruvian National Household Survey (ENAHO) for years 2001 through 2007. The ENAHO replaced the PLSMS and most of their questionnaires mimic those of the PLSMS ones. Both surveys are nationally representative. These surveys contain 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 at which the phone became operative. The third source consists of geo-referenced information from the Peruvian Ministry of Transports and Communications regarding the rural network of roads and rivers. Finally, we used NASA information from the Shuttle Radar Topography Mission to construct a gradient map of Peru at a 90 meter cell precision.[7]

We built the final dataset by coding the PLSMS/ENAHO at the village level and inputting the GPS location of each village using information collected during the 2007 Peruvian census. Then, using the geo-coded information on the communications network and land gradient, we simulated travel time from each surveyed village to the nearest

[7] This dataset is freely available at: http://www2.jpl.nasa.gov/srtm/

FITEL phone using the program *SMALLWORD*.[8] Our sample includes only villages situated within a radius of 30 minutes traveling time to the nearest phone (the mean travel time in the final sample is 6 minutes). Our final sample consists of 15,242 household-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.[9]

Table 2: Household 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 consists of 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.

[8] Smallworld GIS is one of the leading geographical information systems (GIS) designed for the management of complex utility or telecommunications networks. For details regarding the software and its applications see: http://www.gepower.com/prod_serv/products/gis_software_2010/en/index.htm

[9] As an alternative strategy, we also included observations from villages that were never treated and were situated within an interval of two to four hours away from the nearest phone (pure control villages). After this inclusion, results remain qualitatively unchanged and are available upon request. However, we decided to focus our analyses on treated villages given that all of them shared common baseline characteristics; while pure control villages showed some significant differences at baseline. This might has been expected given that treated villages shared several the points outlined in the selection criteria explained in section 2.1.

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 consists of 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.

Figure 3: Sampled villages by treatment timing

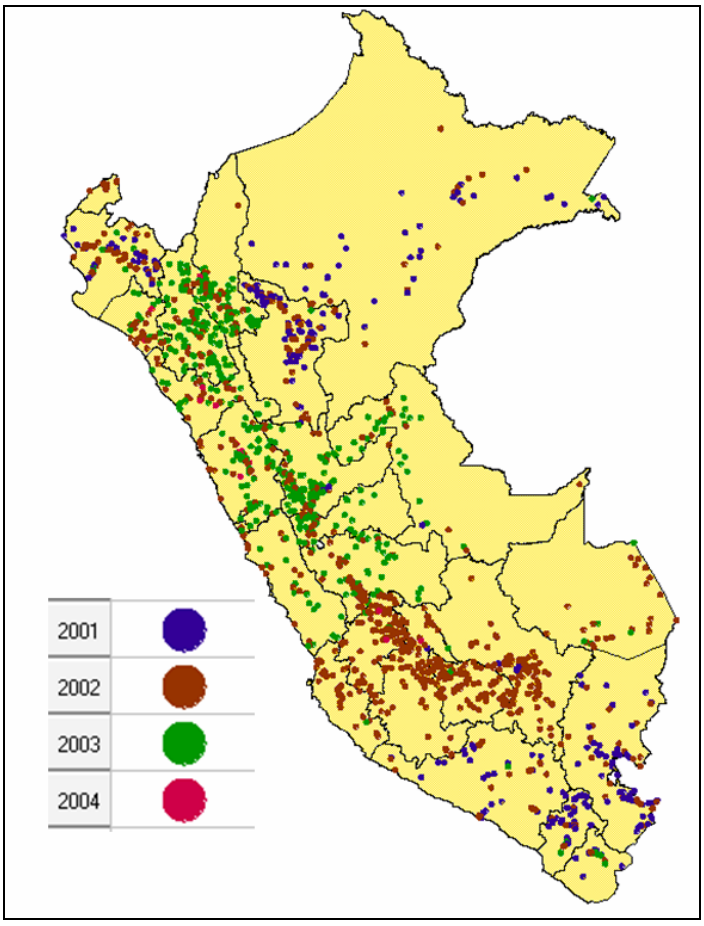


Table 5 displays descriptive statistics at baseline (pooling 1997 and 2000 data). The average age of household heads is 47, with only 36% of them having completed at least secondary education. As expected, the poverty rate in the treated villages is higher than the national average. For instance, 54% of households in the treated villages were considered poor, while the national poverty rate was 44% for the same period. Agricultural profitability, measured by the ratio of total production value over total costs, reached an average of 9.95. The average farmer reported to sell half of the total agricultural production, consuming 30% of it, while using the rest as seeds or for barter.

Children sex ratio was about 1, with 51% of children being male. Child labor amounts to 43% of children reporting market work as their main activity.[10] However, most of them were engaged in agricultural work (35%) as their main activity, while only 8% reported wage work as the main activity.

Table 5: Summary statistics at baseline (1997 – 2000)

	N (1)	*Mean* (2)	*S.D.* (3)	*Min* (4)	*Max* (5)
Panel A: Household head characteristics					
Age	585	46.96	14.62	20	94
High education (1=secondary+)	585	0.36	0.48	0	1
Home ownership	585	0.83	0.38	0	1
Poor	585	0.54	0.50	0	1
Migrant	585	0.28	0.45	0	1
Household size	585	5.37	2.07	1	13
Panel B: Agricultural production					
Annual production (kgs.)	585	4409.72	6451.20	10	35000
Value per kg. sold (in local currency)	482	1.55	7.03	0.01	131.01
Annual costs (in local currency)	585	2195.66	13917.73	1.00	285917.00
Profitability: production (value)/costs	585	9.95	10.38	0.03	49.39
Production sold/total production (kgs.)	585	0.50	0.34	0	1
Production consumed/total production (kgs.)	585	0.30	0.26	0	1
Panel C: Child characteristics					
Age	1138	9.49	2.31	6	13
Gender (1=male)	1138	0.51	0.50	0	1
Market work	1138	0.43	0.49	0	1
Agricultural work	1138	0.35	0.48	0	1
Wage work	1138	0.08	0.26	0	1
School - enrollment	1138	0.95	0.21	0	1
School - main activity	1138	0.57	0.49	0	1

Child labor showed an increased gradient with respect to age. Figure 4 decomposes baseline levels of reported market work by age and sex. The proportion of children that reported market work as their main activity ranges from 28% for age 6, until 55% for age 13. The positive gradient is observed for both boys and girls. However, we observe that for the majority of ages, the incidence of child labor is higher for boys. This observation becomes evident when looking at agricultural work in Figure 5. Here we still observe an increasing gradient of child labor for both boys and girls, but with boys being generally more active until age 11 and then girls catching up at ages 12 and 13. Finally, when observing wage work in Figure 6, we no longer distinguish a sustained increasing

[10] Market work includes wage employment, self-employment, agriculture, helping in a family business, and domestic work in an external household.

gradient. By contrast, we observe an inverted U-shape until age 12. In addition, a distinct feature is that girls are generally more active than boys. This might be explained by the fact that one of the main components of wage work is domestic work in an external household, which is a type of work where girls are preferred.

Figure 4: Child market work at baseline (1997 – 2000)

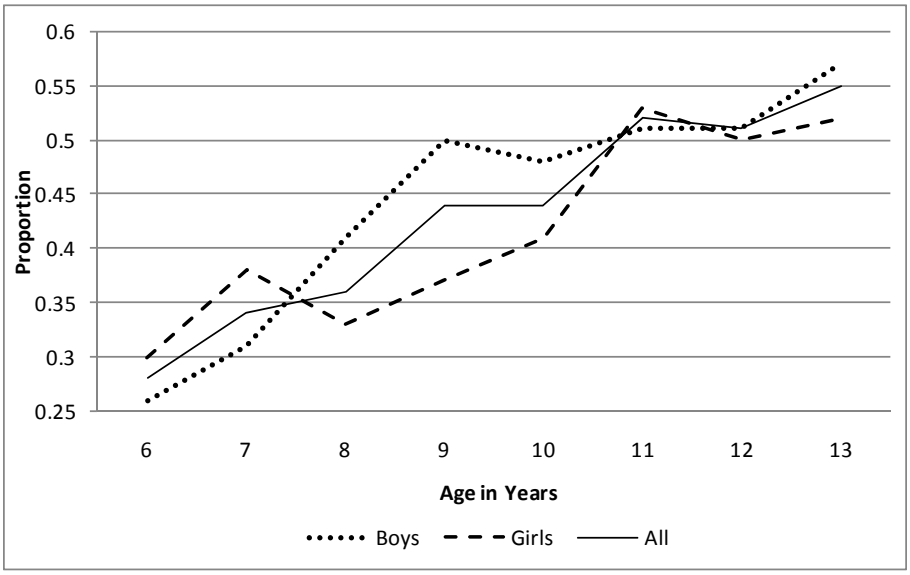


Figure 5: Child agricultural work at baseline (1997 – 2000)

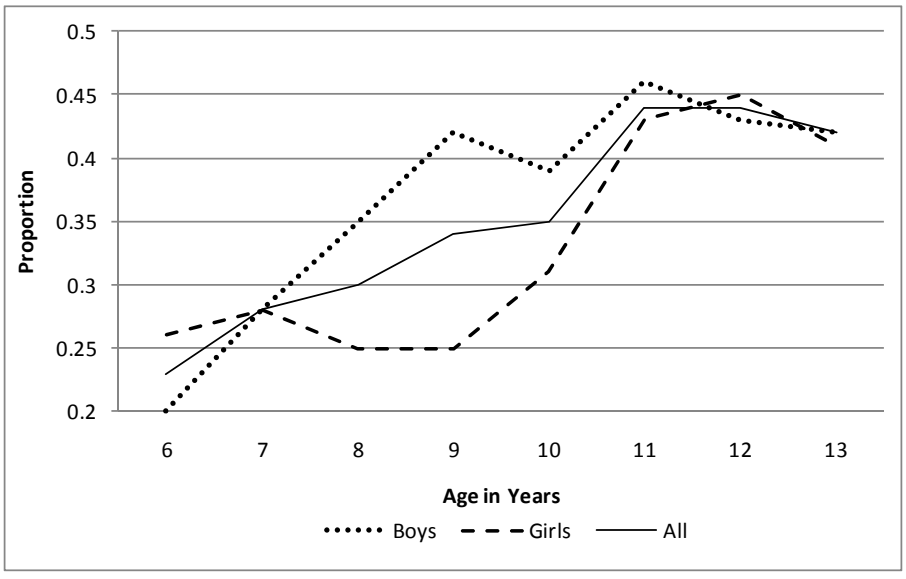
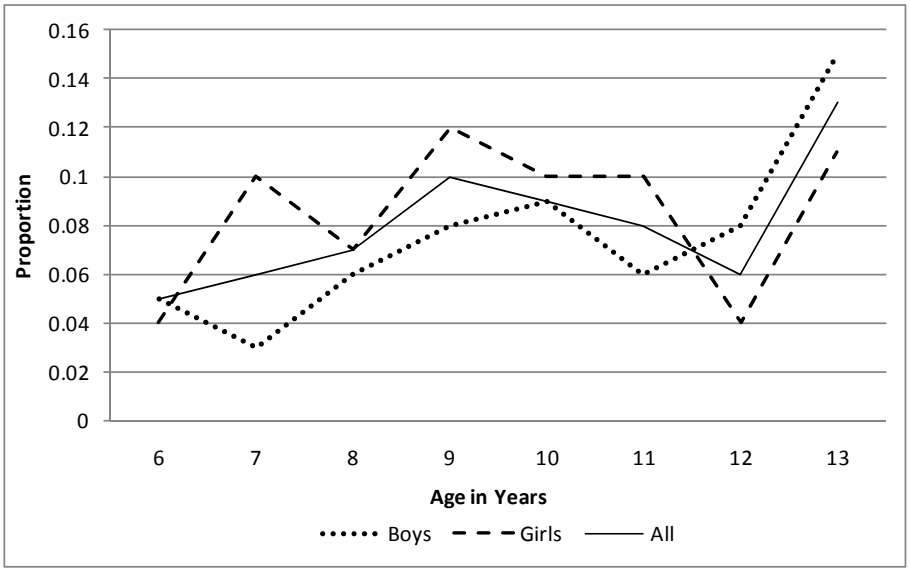


Figure 6: Child wage work at baseline (1997 – 2000)



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 as the difference between mean outcomes before and after the intervention. We estimate regression equations of the following form:

$$O_{ijt} = \alpha_j + \phi_t + \beta_1 \cdot Post_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt} \quad (13)$$

where O_{ijt} is the outcome of interest for household/child *i*, in village *j* in month-year *t*. $Post_{jt}$ is an indicator that takes the value of 1 if village *j* had a phone in month-year *t*, and 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 that in all estimations will be clustered at the village level to account for heteroskedasticity and serial correlation in unobservable characteristics among dwellers living in the same village.

Some aspects of model (13) merit discussion. First, the 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 prior to installation.

To interpret these estimates as causal, the key identifying assumption is that, absent the intervention, villages treated in the first stages of the program and those treated later would have shared the same trends with respect to the outcomes of interest. Moreover, if treatment timing was indeed orthogonal to potential results, differences in outcomes of interest and other characteristics between villages treated early in the program and those treated later evaluated at pre-treatment periods should not exist. Accordingly, Tables 6 and 7 provide evidence showing that baseline differences for households and children treated earlier and later are statistically indistinguishable from zero, where "early" villages are defined as those receiving phones in 2001 and 2002, while "late" villages received phones in 2003 and 2004. This result gives us confidence that treatment timing was unrelated to the outcomes of interest and demographic characteristics.

Table 6: Baseline differences for agricultural households

Survey year:	1997 Late - Early	2000 Late - Early	2001 Late - T2002
	(1)	(2)	(3)
Household head characteristics			
Age	-2.732	0.315	0.028
	(2.270)	(2.271)	(1.068)
High education (1=secondary+)	0.044	-0.095	-0.077**
	(0.065)	(0.065)	(0.031)
Home ownership	-0.045	-0.055	0.005
	(0.083)	(0.041)	(0.027)
Agricultural outcomes (in natural logs)			
Annual production (value)	0.048	-0.005	0.103
	(0.323)	(0.256)	(0.142)
Annual production (kgs.)	-0.073	0.096	0.061
	(0.307)	(0.247)	(0.187)
Value per kg. sold	0.122	-0.193	0.022
	(0.245)	(0.133)	(0.107)
Annual costs	0.068	-0.162	0.013
	(0.353)	(0.337)	(0.172)
Profitability: production (value)/costs	-0.020	0.157	0.091
	(0.267)	(0.265)	(0.130)
Production sold/total production (kgs.)	-0.082	-0.064	0.087
	(0.069)	(0.071)	(0.053)
Production consumed/total production (kgs.)	0.322	0.386*	-0.255
	(0.203)	(0.212)	(0.203)
Observations	254	331	1687

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 10% level; ** Statistically significant at 5% level.

Table 7: Baseline differences for children between 6 and 13 years old

Survey year:	1997 Late - Early	2000 Late - Early	2001 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.

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

$$O_{ijt} = \alpha_j + \phi_t + \beta_1 \cdot Post_{jt} + X'_{ijt}\gamma + Coast_j \cdot f(t) + Highlands_j \cdot f(t) + Jungle_j \cdot f(t) + \varepsilon_{ijt} \quad (14)$$

This specification controls for quadratic trends in outcomes during the study period, and allows 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 or quadratic.

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 about 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.[11] The first row of Table 8 reports estimates of β_1 for prices. Column 1 suggests a 0.157 log-points 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

[11] We take this proxy given that we are interested in the amount of income that farmers receive per unit of production. In that way, the survey provides with the detail of the total value obtained for sold production, expressed in local currency, and the total kilograms of production that was sold.

the reservation prices at which farmers sell their produce. Columns 2 and 3 report estimates coming from specifications in which we add controls such as age, sex and education of the household head, household size, and house ownership status. Our estimates remain virtually unchanged and provide further evidence 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 0.149 log-points (equivalent to 16%).

Table 8: 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*	0.149*	11495
	(0.086)	(0.085)	(0.086)	(0.087)	
Annual production (kgs.)	-0.051	-0.060	-0.058	-0.063	15242
	(0.098)	(0.098)	(0.097)	(0.097)	
Annual costs	-0.232**	-0.236**	-0.235**	-0.213**	15242
	(0.108)	(0.107)	(0.107)	(0.105)	
Profitability: production (value)/costs	0.190**	0.184**	0.182**	0.178**	15242
	(0.089)	(0.089)	(0.089)	(0.089)	
Household characteristics	No	Yes	Yes	Yes	
House ownership status	No	No	Yes	Yes	
Differential quadratic 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-year 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.

Our second exercise 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 agricultural activity. However, as the second row of Table 8 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 has remained constant, the estimated effects on

agricultural costs should mainly reflect effects on input prices rather than quantities. Accordingly, column 1 of the third row of Table 8 shows that TC has reduced annual agricultural costs by 0.232 log-points. Columns 2 through 4 indicate that our estimate is robust to the inclusion of controls and to differential trends by region. The estimated impact in the fully controlled model (column 4) suggests a 0.213 log-point (equivalent to 23.7%) drop 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.

Given that farmers are receiving better prices for their output and paying lower prices for their inputs, profitability of farming activity has increased. The fourth row of Table 8 reports estimates of β_1 for the natural logarithm of the ratio of the value of agricultural production to total costs as our measure of profitability.[12] Our baseline estimate shown in column 1 evidences that TC has increased profitability by 0.19 log-points. This estimate is robust to the inclusion of control variables and differential trends by region. The estimate from the fully controlled model (column 4) remains qualitatively unchanged suggesting an increase of 0.178 log-points (equivalent to 19.5%). 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 average profits of fishermen in Kerala - India as a result of cellphone coverage, while Aker (2010) reports a 29% increase in profits of grain traders in Niger after cellphone rollout. Also, Goyal (2010) reports a 33% net gain in farmers' profits after the

[12] This measure is the continuously compounded annual return to agricultural activities.

introduction of internet kiosks that provided real time information of soybean market prices. Therefore, our estimates are situated in between previous estimated effects.

Our results clearly show that the intervention significantly increased the profitability of farming activities. Therefore, affected households received an exogenous shock to net income per unit of time devoted to agricultural activities. These results are in line with our theoretical predictions and provide an opportunity to test the effects of this shock on households' allocation of their children's time. Accordingly, the next section explores the effect of this intervention on the utilization of 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 utilization of child labor. The ultimate effect will depend on whether the income effect dominates the substitution effect. The dataset provides information about the main activity in which each household member was engaged in the week prior to the survey. Therefore, in order to measure child labor utilization, we compute indicators for market work, agricultural work, and wage work as main activities.[13] Table 9 reports estimated effects of the intervention on these variables, where the unit of observation is now a child-year.

[13] These indicators come from answers to a single question in the survey which asks: "During the previous week, what was your main activity either inside or outside the household?". The possible answers were: a) Helped in the household's or relative's business; b) Domestic work in an external household; c) Helped to elaborate products for sale; d) Helped in the agricultural plot or looking after the cattle; e) Sold products: candy, gum, etc.; f) Transported products, bricks, etc.; g) Other type of work; h) Studying. Therefore, the indicator for Market Work takes the value of one if the child chose any option other than Studying and zero otherwise. The indicator for Agricultural Work takes the value of one if the child chose option d) and zero otherwise. The indicator for Wage Work takes the value of one if the child chose any other option other that Studying or Agricultural Work, while zero otherwise. It is worth noting that from 2002 onwards, the answers included an additional option as "Domestic work inside the household". I still considered this option as Market Work. However, it was included neither in Wage nor in Agricultural Work.

Table 9: 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***	-0.137***	19409
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	
Agricultural work	-0.098**	-0.096**	-0.095**	-0.094**	-0.092**	19409
	(0.041)	(0.040)	(0.040)	(0.040)	(0.041)	
Wage work	-0.024*	-0.022*	-0.022*	-0.022*	-0.021*	19409
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	
School - enrollment	0.005	0.004	0.004	0.004	0.003	19262
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	
Child characteristics	No	Yes	Yes	Yes	Yes	
Household head characteristics	No	No	Yes	Yes	Yes	
House ownership status	No	No	No	Yes	Yes	
Differential quadratic 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-year 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.

Our results clearly suggest a negative effect of the program on the utilization of child labor. For instance, column 1 of row 1 shows that the introduction of TC decreased the likelihood of reporting any market work as the main activity by 14.6 percentage points. This effect is robust to the inclusion of control variables such as sex and age of children, age and education of the household head, and home ownership status (columns 2 through 4). When including differential trends in the specification (column 5), the estimated effect remains robust, suggesting a reduction of 13.7 percentage points in the likelihood of reporting any market work as the main activity. When expressed relative to the baseline proportion of children engaged in market work, the estimated effect implies a 31.9% reduction in the probability of reporting market work as the main activity. Therefore, our results suggest a dominant income effect in the utilization of child labor.

We also evaluate separate 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. Column 1 of row 2 suggests a 9.8 percentage point drop in the likelihood of

reporting agricultural work as main activity following the intervention. This result is robust to the inclusion of control variables, as shown in columns 2 through 4. In addition, column 5 reveals that adding differential trends leaves results practically unchanged, suggesting a 9.2 percentage point reduction in the likelihood of agricultural work. When expressed as a percentage reduction with respect to the baseline level of the outcome, our estimates imply a 26.3% reduction in the probability of reporting agricultural work as the main activity following the arrival of TC.

Wage work has also been affected by the program, with a smaller absolute effect. Our preferred estimate (Table 9 – column 5) suggests a 2.1 percentage point reduction. However, when expressed relative to the baseline level, the estimate implies a 26.3% reduction in the probability of reporting wage work as the main activity. All of our estimates strongly suggest a dominant income effect in the utilization of child labor among Peruvian rural villages. This finding is consistent with Dammert (2008), who reports a 12.3 percentage point increase in child market work among coca-growing regions after a successful coca eradication program during the late 1990's in rural Peru (which decreased net income of coca farmers).

We further investigate whether the reduced probability of reporting work as the main activity has impacted school enrollment. Row 4 of Table 9 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, 95% of children at baseline reported being enrolled in school. However, given that work and school are mutually exclusive categories in the survey question regarding main activity, our finding of a 13.7 percentage point reduction in the likelihood of reporting market

work as main activity directly translates into an equivalent increase in the likelihood of reporting school as main activity. This implies a 24% increase in the probability of reporting school as main activity with respect to the baseline proportion of children that reported school as their main activity. This constitutes a sizeable effect when compared to conditional cash transfer programs that included school attendance as one of the conditions. For instance, Fizsbein and Schady (2009) find that enrollment increased by 3.3 percentage points in the case of PRAF in Honduras (for children aged 6 to 13, from a baseline enrollment of 66%), 7.5 percentage points for Chile Solidário (for children aged 6 to 15, from a baseline enrollment of 61%), and by 12.8 percentage points for the Red de Proteccion Social in Nicaragua (for children aged 7 to 13, from a baseline enrollment of 72%).

6.3. Heterogeneous Effects in Utilization of Child Labor

We next assess heterogeneity in the effects of TC on child labor with respect to gender and age. Columns 2 and 3 of Table 10 reveal that the probability of reporting any market work as the main activity was reduced evenly (in relative terms) for girls and boys. For instance, boys reduced this probability by 31% (0.143/0.46), while girls reduced it by 32% (0.128/0.40). 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 was significantly reduced only for boys while wage work was impacted only for girls. Column 5 suggests that the probability of reporting agricultural work as main activity fell

by 28.7% (0.109/0.38) for boys. Column 9 shows that the probability of reporting wage work as main activity fell by 51.5% (0.036/0.07) for girls.

Table 10: Child labor by gender and age

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)
Panel A: All Children (6 - 13 years old)									
Post	-0.137***	-0.143***	-0.128***	-0.092**	-0.109**	-0.074	-0.021*	-0.001	-0.036**
	(0.041)	(0.048)	(0.048)	(0.041)	(0.048)	(0.046)	(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
Dependent variable mean at baseline	0.43	0.46	0.40	0.35	0.38	0.33	0.08	0.08	0.07
Panel B: Effects by age									
Post (age = 6 - 9)	-0.113***	-0.117**	-0.090	-0.065	-0.076	-0.038	-0.017	0.004	-0.032
	(0.043)	(0.052)	(0.060)	(0.040)	(0.050)	(0.052)	(0.015)	(0.016)	(0.022)
Post (age = 10 - 13)	-0.176***	-0.193***	-0.183***	-0.136**	-0.166**	-0.145**	-0.022	-0.003	-0.032
	(0.052)	(0.072)	(0.064)	(0.053)	(0.071)	(0.063)	(0.015)	(0.020)	(0.024)

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-year 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 quadratic 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.

Therefore, it is clear that the overall impact of TC on the probability of performing some kind of market work was 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 are more active in wage work (mainly composed by domestic work). This pattern was also confirmed by our baseline data (Figure 6) where it was shown that girls were more active than boys for most of the age range.

We further decompose estimated effects by age ranges. Given that the baseline incidence of child labor was different across ages, we should expect that effects might also differ between ages. Accordingly, Panel B of Table 10 presents differential effects by age and sex. For market work (columns 1 to 3), we observe that for both girls and boys effects are stronger from age 10 onwards. This is consistent with the fact that, at baseline, market work had higher incidence at these age ranges (Figure 4). Similarly, agricultural work (columns 4 to 6) has had stronger impacts for boys at ages above 10

(16.6 percentage points), and for girls in that same age range (14.5 percentage points). This is also consistent with the fact that this type of work was more prevalent at these age ranges for boys and girls (Figure 5). Finally, wage work effects vanish when different age ranges are taken into account.

Table 11 – Panel A tests for heterogeneous effects of TC on child labor with respect to parental education. Columns 1 and 2 reveal that reductions in the probability of reporting any kind of market work as the main activity were proportionately 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 was equivalent to 28% (0.129/0.46). However, in households where the head holds a high school or higher degree, this reduction accounted for 40% (0.143/0.36). This evidence shows that parents with relatively higher education take their children out of working activities at a higher rate than their lower educated counterparts. This effect might imply that higher educated parents value human capital accumulation for their children more that their less educated peers. However, it could also be that the introduction of TC has had stronger income impacts among households with relatively more educated heads. Indeed, panel B suggests that households with higher educated parents experienced higher reduction in agricultural costs when compared to their lower educated counterparts (0.375 versus 0.213 log-points).

Table 11: Estimated effects by parental education

Panel A: Child outcomes						
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	-0.129***	-0.143**	-0.105**	-0.035	-0.003	-0.057*
	(0.047)	(0.062)	(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
Baseline mean	0.46	0.36	0.40	0.24	0.06	0.11
Panel B: Agricultural outcomes						
Dependent Variable (in natural logs):	Value per kg. sold		Agricultural costs		Profitability value/costs	
	Low educ.	High educ.	Low educ.	High educ.	Low educ.	High educ.
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.195*	0.020	-0.213*	-0.375**	0.196**	0.131
	(0.100)	(0.140)	(0.115)	(0.184)	(0.094)	(0.165)
Observations	8451	3044	11217	4025	11217	4025
R-squared	0.41	0.46	0.46	0.58	0.43	0.47

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 in Panel A include month-year 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 quadratic trends by natural regions (coast, highlands and jungle). Regressions in Panel B include all previous controls with the exception of child characteristics (sex and age). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

However, when we consider agricultural and wage work separately, (panel A - columns 3 to 6) we find that agricultural work was significantly reduced only among children in households with less educated heads, while wage work was reduced only for children with more educated parents. The probability of reporting agricultural work as main activity fell by 26.3% (0.105/0.40) for children with less educated parents, while the probability of reporting wage work as main activity fell by 51.8% (0.057/0.11) for children with more educated parents.

These findings reflect the fact that agricultural work was much more common at baseline among children with less educated parents. At baseline, 43.5% of children with less educated parents reported agricultural work as their main activity, while only 30.6% of children with more educated parents did so. In addition, as columns 5 and 6 of panel B

show, agricultural profitability has enjoyed a higher impact among households with less educated heads (0.196 log-points). Therefore, the 10.5 percentage point reduction in agricultural work among children in households with less educated parents following the introduction of TC brought the initial proportions nearly into equality.

A similar result holds for wage work. At baseline, 6.2% of children with less educated parents reported wage work as their main activity, while 10.8% of children with more educated parents did so. Therefore, the 5.7 percentage point reduction in wage work among households with more educated parents brought these proportions into near equality.

6.4. Sensitivity Analysis

Column 1 of Table 12 shows estimation results for child labor excluding households living on the coast. Notice that estimated impacts for market and agricultural work are stronger than the estimated effect for the whole country. This is explained by the fact that child labor is much less common on the coast than in the rest of Peru. For instance, at baseline, only 21% of children living on the coast reported having some type of market work as their main activity. By contrast, this figure was 44% in the rest of the country. Similarly, the proportion of children that reported agricultural work to be their main activity at baseline was 19% in the coast and 43% in the rest of the country. Thus, we observe relatively stronger effects of TC in zones where the ex-ante level of child labor was greater.

Table 12: 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	-0.012	-0.040**	-0.001	-0.029	-0.008	-0.029**
	(0.013)	(0.016)	(0.016)	(0.022)	(0.000)	(0.014)
Observations	17193	9317	10074	9274	10117	13254
R-squared	0.17	0.20	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-year 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 quadratic 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.

When looking at wage work, on the other hand, the exclusion of children living on the coast leads to insignificant effects. This is also explained by the fact that wage work among children is more common on the coast than in the rest of the country. At baseline, 11% of children living on 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 were similar for poor and better-off areas of the country. To do this, we merge our data with the 1993 census and classify sample villages according to the district in which they are located. After this, we split the sample into 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 of TC on child market work for poor and non-poor villages (13.5 and 13.3 percentage point reductions

respectively). However, reductions in the incidence of agricultural work are only significant in the poorest districts, while wage work was significantly reduced only in non-poor districts. These findings are consistent with earlier results in that reductions in different types of child labor are stronger in zones with relatively higher baseline incidence of that type of labor. 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.

Next we classified our sample villages by population density at the district level using the 1993 census. Columns 4 and 5 show results for the resulting subsamples. Interestingly, we observe that reductions in the probability of children reporting market and agricultural work as main activities are only significant among villages located in districts above the median density. These results are perhaps not surprising given that denser areas have more potential workers to replace the decreased child labor. By contrast, 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 the household's labor needs.

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 those obtained using the whole sample. This finding suggests that migrant households may need more labor in order to establish some economic security in a relatively new place. Therefore, these households may have used relatively higher levels of child labor over time than non-migrant counterparts. For

instance, 45% of children living in migrant households reported, at baseline, market work as their main activity, while only 37% of children living non-migrant households did so. Similarly, 37% of children living in migrant households reported agricultural work as their main activity, while only 26% of children living non-migrant households did so.

6.5. The Relation Between Profitability and Child Labor

Under the assumption that the only channel trough which TC impacted child labor was agricultural profitability, we could use the exogenous intervention studied here to instrument profitability and recover an estimate of the causal effect of profitability on child labor. To do so, we estimate the following system of equations by 2SLS:

$$\mathrm{P}_{hjt} = \alpha_j + \phi_t + \beta_2 \cdot \mathrm{P}ost_{jt} + X_{hjt}^{'}\gamma + Coast_j \cdot f(t) + Highlands_j \cdot f(t) + Jungle_j \cdot f(t) + \varepsilon_{hjt} \quad (15)$$

$$W_{ihjt} = \alpha_j + \phi_t + \beta_1 \cdot \hat{\mathrm{P}}_{hjt} + X_{ihjt}^{'}\gamma + Coast_j \cdot f(t) + Highlands_j \cdot f(t) + Jungle_j \cdot f(t) + \varepsilon_{ihjt} \quad (16)$$

where P_{hjt} denotes agricultural profitability of household *h* in village *j* at time *t*, and W_{ihjt} denotes the child labor indicator for kid *i* of household *h* in village *j* at time *t*. The rest of variables are defined as in (14).

Equation (15) denotes the first stage regression, while model (16) is the second stage regression where profitability has been instrumented with the indicator for the presence of a phone. We estimate the system using observations of children living in households that reported both agricultural production and costs. Column 1 of Table 13 reports the estimate of β_2, while columns 2 to 4 report estimates of β_1 for the different types of labor.

Column 2 suggests that an increase of 0.1 log-points in agricultural profitability translates into a decrease of 7.5 percentage points in the likelihood of reporting market work as the main activity. Considering that 0.1 log-points is equivalent to 10.5%, and that 7.5 percentage points represent a decrease of 17.4% with respect to the baseline incidence of market work; the implied elasticity of child market work with respect to agricultural profitability is -1.66. This implies that a 1% increase in agricultural profitability translates into a decrease of 1.66% in child market work.

Table 13: Effects of profitability on child labor

Dependent Variable:	Profitability value/costs	Market work	Agricultural work	Wage work
	(1)	(2)	(3)	(4)
Post	0.206**			
	(0.103)			
Instrumented Productivity: log (value/costs)		-0.749***	-0.544**	-0.093
		(0.238)	(0.232)	(0.057)
Observations	15472	15472	15472	15472
R-squared	0.47	0.42	0.42	0.18

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Regression in column (1) is the first stage and has the dependent variable expressed in natural logs and include month-year and village fixed effects, household head characteristics (age and education level), ownership status (indicator for house formal property), and differential quadratic trends by natural regions (coast, highlands and jungle). Regressions in columns (2) to (4) are the second stage for child labor where log productivity has been instumented with the presence of a telephone in the village. * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

Following a similar procedure, column 3 implies an elasticity of child agricultural work with respect to agricultural profitability of -1.48. The interpretation is that a 1% increase in agricultural profitability translates into a decrease of 1.48% in child agricultural work. Finally, column 4 suggests that the relation between agricultural profitability and child wage work is weak. This was somehow expected given that reduced form estimates were not very precise and only significant for girls. This evidence

confirms that agricultural profitability has affected the types of child labor in which it has generated incentives and trade-offs mechanisms between income and substitution effects.

7) Robustness Analysis

7.1. Falsification Test

Next we conduct a falsification test in the spirit of Granger (1969) to verify the causal interpretation of our estimates. We estimate an augmented version of model (14) which incorporates a one year lead and a one year lag of the treatment indicator $Post_{jt}$. The lead indicator represents anticipatory effects. Therefore, if our estimates reflect causal impacts of the program, we expect insignificant estimates for anticipatory effects. The lagged indicator represents post-treatment effects. In that sense, significant estimates would imply that the program had an increasing impact one year after treatment. However, an insignificant effect would imply that program impacts were mainly reflected during the first year of treatment with no significant differential effects thereafter.

Table 14 shows the estimation results. As expected, none of the coefficients representing anticipatory effects are statistically significant at any conventional level. These results give further confidence regarding the causal interpretation of our estimates. In addition, post-treatment effects are also weak. This means that the main impacts of the program have been realized during the first year and have neither been notoriously strengthened or reversed thereafter.

Table 14: Falsification test

Dependent Variable:	Value per kg. sold	Agricultural costs	Profitability value/costs	Market work	Agricultural work	Wage work
	(1)	(2)	(3)	(4)	(5)	(6)
Lead_Post (1 year anticipatory effect)	-0.003	0.066	0.003	0.030	0.044	0.008
	(0.126)	(0.139)	(0.136)	(0.072)	(0.068)	(0.029)
Post	0.131+	-0.208**	0.176**	-0.139***	-0.094**	-0.026**
	(0.097)	(0.105)	(0.089)	(0.041)	(0.041)	(0.013)
Lag_Post (1 year post-treatment effect)	-0.092	0.089	-0.077	0.068*	0.051	0.015
	(0.104)	(0.091)	(0.069)	(0.036)	(0.035)	(0.012)
Observations	11495	15242	15242	19391	19391	19391
R-squared	0.40	0.45	0.40	0.40	0.41	0.17

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Regressions in columns (1) to (3) have dependent variables expressed in natural logs and include month-year and village fixed effects, household head characteristics (age and education level), ownership status (indicator for house formal property), and differential quadratic trends by natural regions (coast, highlands and jungle). Regressions in columns (4) to (6) include all previous controls plus child characteristics (sex and age). +denotes significance at the 18% level; * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

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, cannot ignore the issue that the sampling framework was different for both surveys.[14] 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). Table 15 displays 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.

[14] The PLSMS had their sampling framework in the 1993 Peruvian Census, while the ENAHO (2001-2006) had their sampling framework in a pre-census conducted during 1999-2000. Finally, the ENAHO 2007 had the 2005 Peruvian Census as sampling framework.

Table 15: Estimated effects dropping years 1997 and 2000

Panel A: Agricultural outcomes				
Dependent Variable (in natural logs):	Value per kg. sold	Production in kgs.	Agricultural costs	Profitability value/costs
	(1)	(2)	(3)	(4)
Post	0.121+	-0.068	-0.203*	0.180**
	(0.083)	(0.097)	(0.104)	(0.089)
Observations	11013	14657	14657	14657
R-squared	0.39	0.51	0.43	0.40
Panel B: Child outcomes				
Dependent Variable:	Market work	Agricultural work	Wage work	School enrollment
	(1)	(2)	(3)	(4)
Post	-0.150***	-0.103**	-0.022*	0.002
	(0.041)	(0.041)	(0.012)	(0.017)
Observations	18254	18254	18254	18112
R-squared	0.44	0.44	0.18	0.73

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. All regressions in Panel A include month-year and village fixed effects, household head characteristics (age and education level), ownership status (indicator for house formal property), and differential quadratic trends by natural regions (coast, highlands and jungle). Regressions in Panel B include all previous controls plus child characteristics (sex and age). + significant at the 15% level; * significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level.

7.3. Spillover Effects

As detailed earlier, we are estimating the effects of the program using villages within a range of 30 minutes travel time to the nearest phone. However, the survey provides information coming from villages that are situated farther away. Therefore, we could use these observations to check for possible spillover effects. To do so, we consider observations coming from villages situated within a 2 hour travel time range to the nearest phone. Then we estimate model (14) allowing for differential impacts among villages situated within 30 minutes travel time intervals.

Table 16 shows estimated program effects. Estimates suggest the inexistence of spillover effects. We observe that all effects are insignificant for villages situated in

distances over 30 minutes travel time. This evidences that farmers not living in treated villages don't appear to have travelled to the nearest phone and effectively benefited from it. Therefore, this provides support to believe that the Standard Unit Treatment Value Assumption (SUTVA) holds.

Table 16: Spillover effects

Dependent Variable:	Value per kg. sold	Agricultural costs	Profitability value/costs	Market work	Agricultural work	Wage work
	(1)	(2)	(3)	(4)	(5)	(6)
Post - 30 minutes	0.149*	-0.213**	0.178**	-0.137***	-0.092**	-0.021*
	(0.087)	(0.105)	(0.089)	(0.041)	(0.041)	(0.012)
Post - (30 ; 60] minutes	-0.120	0.200	0.205	0.107	0.116	-0.015
	(0.199)	(0.168)	(0.143)	(0.115)	(0.115)	(0.017)
Post - (60 ; 90] minutes	-0.313	0.182	0.179	-0.044	-0.038	-0.003
	(0.197)	(0.213)	(0.120)	(0.107)	(0.106)	(0.015)
Post - (90 ; 120] minutes	0.298	-0.269	0.343	-0.079	-0.125	0.032
	(0.264)	(0.253)	(0.224)	(0.078)	(0.085)	(0.021)
Observations	18329	24304	24304	29992	29992	29992

Estimated standard errors clustered at the village level in parentheses. Weighted regressions using the inverse of sampling probability to reflect survey design. Regressions in columns (1) to (3) have dependent variables expressed in natural logs and include month-year and village fixed effects, household head characteristics (age and education level), ownership status (indicator for house formal property), and differential quadratic trends by natural regions (coast, highlands and jungle). Regressions in columns (4) to (6) include all previous controls plus child characteristics (sex and age). * denotes significance at the 10% level; ** denotes significance at the 5% level; *** denotes significance at the 1% level.

7.4. Event Studies

In order to disaggregate the before-after effects previously estimated into bimonth-by-bimonth effects, we add flexibility to model (14) by estimating regression equations of the following form:

$$O_{ijt} = \alpha_j + \phi_t + \sum_{-6}^{+7} \beta_p D_{jp} + X_{ijt}^{'}\gamma + Coast_j\, f(t) + Highlands_j\, f(t) + Jungle_j\, f(t) + \varepsilon_{ijt} \quad (17)$$

where D_{jp} is an indicator for the p^{th} bimonth after the phone became operative in village j (where p=0 is the bimonth in which the phone became operative).[15] We omit the $D_{j,-1}$ indicator from the regression, so our estimates of the coefficients β_p are interpreted as

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

the mean of the outcome variable relative to the bimonth before the phone became 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 7. Notice that point estimates bounce around zero before the intervention and are all insignificant. This observation gives further support for the validity of our approach, since we do not see evidence of any trend prior to the installation of the phones. Then, starting in the bimonth the phone became operative, estimated impacts become positive and increasing over time (although power is low). A similar pattern is observed for agricultural profitability (measured as the ratio of total production value to costs) in Figure 8. No significant point estimates are found before phone installation, while positive and increasing impacts are observed after the intervention.

Figure 7: Value per kilogram sold

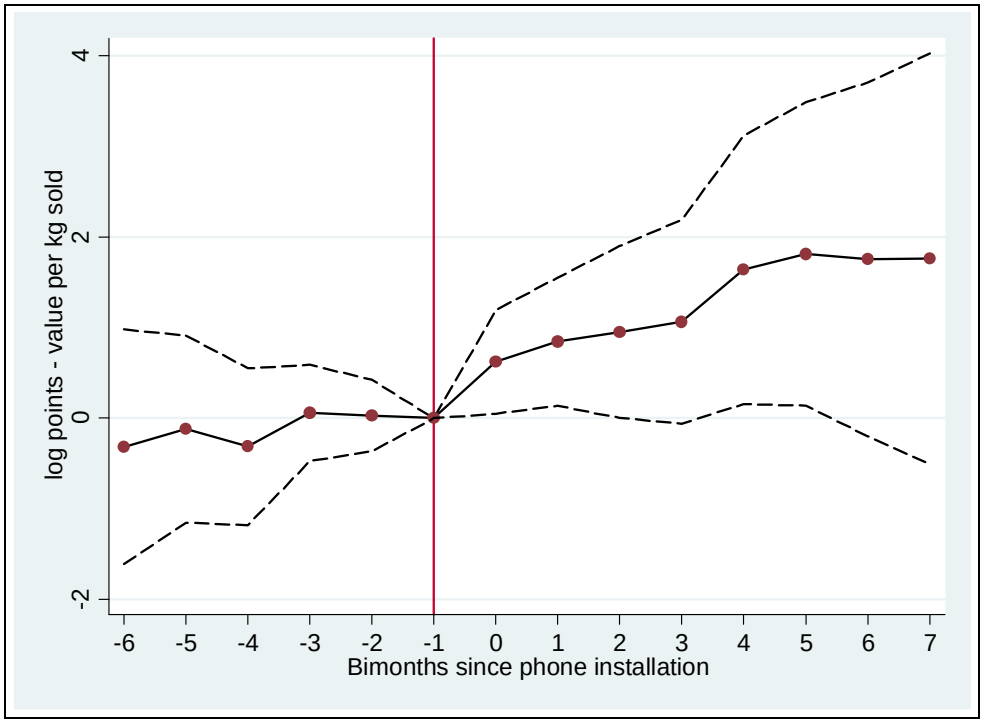
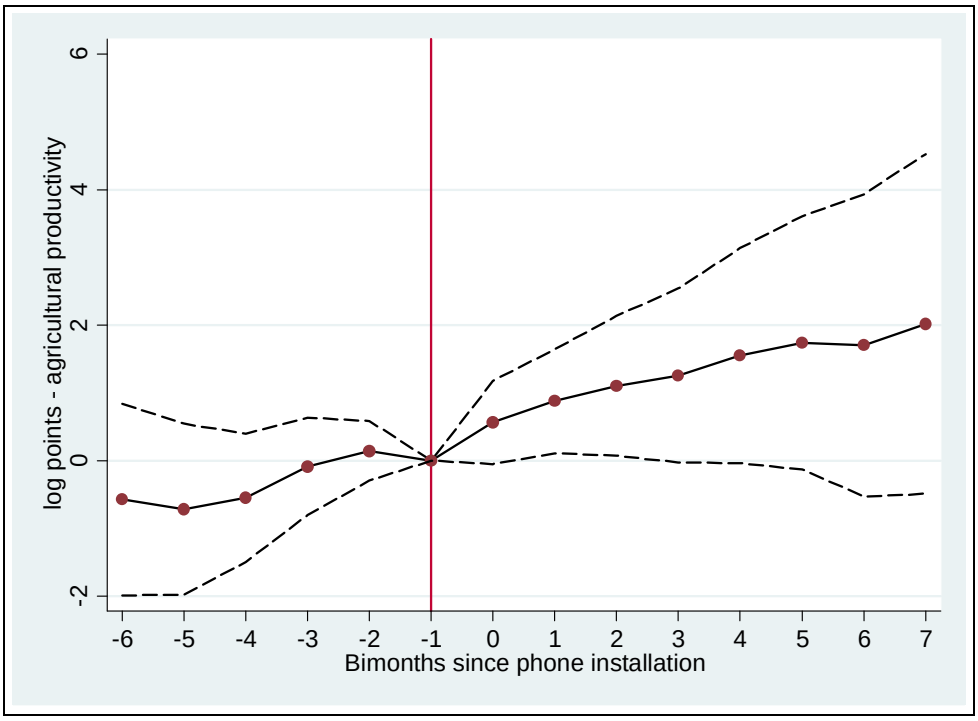


Figure 8: Agricultural profitability



Child labor effects are shown in Figures 9 and 10. Figure 9 plots estimated β_p coefficients for the probability of reporting any type of market work as the main activity. We observe insignificant estimated coefficients prior to the intervention, followed by negative, decreasing and significant estimated impacts starting one bimonth after the phone is installed. Figure 10 shows similar estimated impacts for the probability of reporting agricultural work as the main activity.

Figure 9: Child market work

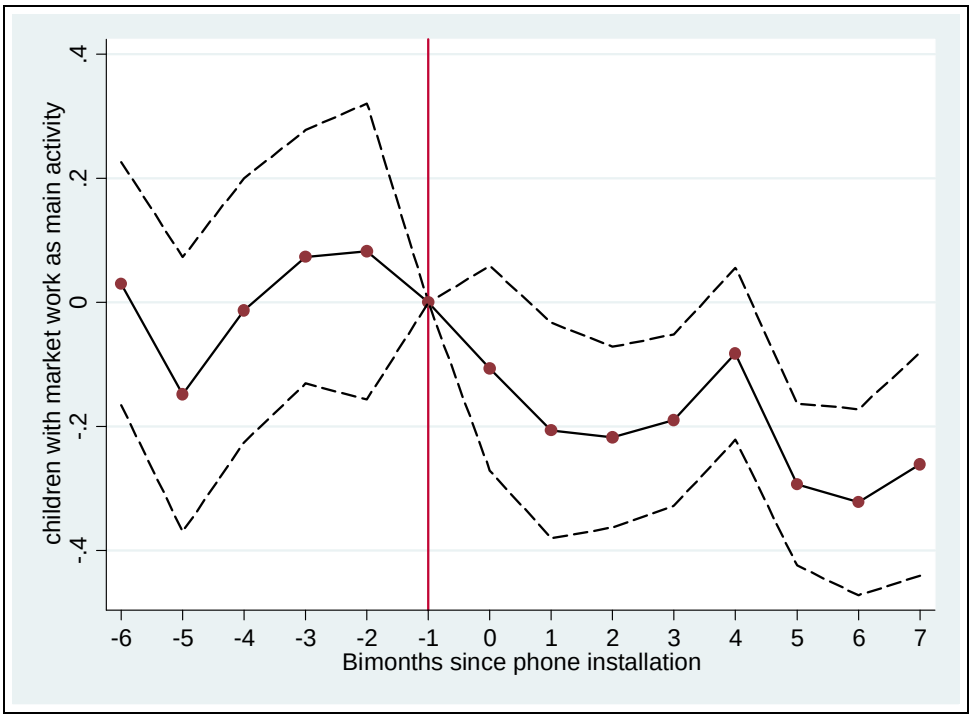
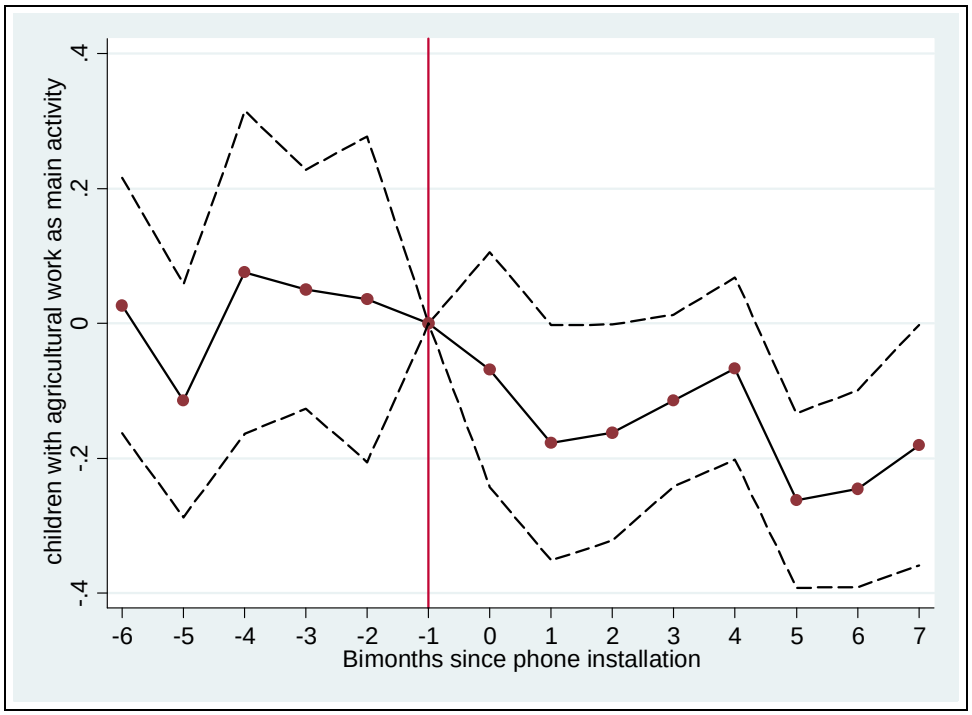


Figure 10: Child agricultural work



8) Summary and Conclusions

This paper examines the impact of provision of public payphones among isolated villages in rural Peru to identify the effects of telecommunication technologies (TC) on agricultural profitability and child labor. The main results suggest that the value received per kilogram of agricultural production increased by 16% following the installation of the phones, while agricultural costs were reduced by 23.7%. These impacts together imply an increase of 19.5% in agricultural profitability. Moreover, this income shock was translated into a reduction of child market work equivalent to 31.9% of baseline labor supply and a reduction in child agricultural work of 26.3%, suggesting a dominant income effect in the utilization of child labor.

A variety of corroborating evidence supports these findings. Results are robust to the inclusion of household characteristics, child characteristics, village fixed effects and differential trends by geographical regions. Differences in effects by population density are also consistent with the notion that areas with higher potential labor supply to substitute for child labor display greater impacts on children's time allocation. There are differential effects by child gender and by education of the head of household, suggesting that child labor is reduced more for groups with higher ex-ante incidence of child labor. I find no impact on the extensive margin of school enrollment, which is not surprising given high school enrollment rates in rural Peru. Finally, event studies analyses show that no pre-existing trends were present with respect to the outcomes of interest and that the estimated impacts became significant as a result of phones introduction.

Overall, these results provide evidence of the potential benefits that TC can offer to poor rural households. By reducing asymmetric information, farmers are able to obtain

better prices for their production and inputs following the advent of TC, thereby increasing their profitability. Moreover, the finding of a dominant income effect in the utilization of child labor suggests that offering cash transfers or subsidies conditional on school attendance may not be necessary for this population. Higher schooling investments after a favorable income shock appear to be incentive compatible among Peruvian rural farmers.

Chapter 2: The Role of Local Governments' Efficiency in Decentralized Public Service Delivery: Evidence from a Randomized Intervention in Rural Russia

1) Introduction

While decentralization in the provision of public services has become an increasingly relied institutional arrangement aimed to improve pro-poor delivery (World Bank, 2004)[16], the effects of decentralization on public service delivery are theoretically ambiguous. On the one hand, decentralization may bring decision-making closer to people, so local preferences can be better reflected in policies (Oates, 1972). On the other hand, decentralization may degrade public service provision when local governments are less efficient than central governments (Smith, 1985). In this regard, the relatively scarce empirical evidence provides findings confirming such theoretical ambiguity.[17] However, the role that the efficiency of newly decentralized local governments plays in the quality of public service delivery remains an important open question.

In the context of a decentralization reform, local governments are assigned with new responsibilities. Therefore, the success of such reform will be in direct relationship with the efficiency with which local governments undertake these responsibilities. This is especially significant in countries that have been centralized for long periods of time and suddenly experience decentralization. In these contexts, there is a high probability that local governments will not be fully capable to take new responsibilities in an efficient way since

[16] Between 1987 and 2006, the World Bank committed about US$ 32 billion to 89 countries through 458 programs, projects, and grants in which decentralization was one of the key themes or classified as an activity.
[17] On the one hand, Humplick and Moini-Araghi (1996) find, in a cross-country study, that unit costs of road maintenance are lower and roads of better quality where maintenance is decentralized. Faguet (2004) finds that decentralization increased the responsiveness of public investment to local needs for several public services in Bolivia. However, on the other hand, Galiani, Gertler and Schargrodsky (2008) show that, while decentralization had an overall positive impact on student test scores in Argentina, these gains did not reach the poor. They conclude that although decentralization may help the good get better, the already disadvantaged may not receive these benefits.

they have never undertaken such duties. Therefore, without an adequate training, it may be that decentralization evaluations yield negative estimated effects. This may happen not because decentralization was not effective, but because local authorities were not fully capable of developing their new responsibilities due to null previous experience.

Accordingly, we investigate the relationship between the efficiency of local authorities and public service delivery outcomes within a context of a decentralization reform. In particular, we ask whether interventions directed towards enhancing the efficiency of newly elected local authorities had an impact on public satisfaction. In addition, we investigate what types of interventions appear to be more effective. Furthermore, we ask whether the effectiveness of these interventions is related to the level of pre-treatment local efficiency which we proxy with democratic experience and accountability.[18]

Isolating causality between these interventions and public delivery outcomes is problematic in the way that some observed intervention may have been assigned to less efficient local governments. In that case, treatment will be negatively correlated with baseline, potentially unobserved, local government efficiency and the estimated effect will be downward biased. On the other hand, if the intervention is given to local governments with better bargaining power, the assignment of treatment may be positively correlated with unobserved capabilities. Therefore, estimated effects will be upward biased. In sum, without having clear exogenous treatment assignments, estimated effects of such programs may be biased in an unknown direction.

To circumvent previous concerns, we rely on a randomized intervention undertaken during a major decentralization reform in rural Russia between 2006 and 2007. The

[18] Following Schedler (1999), we define accountability in our context as the situation in which governments inform their constituencies about their actions and decisions, while taking into account their preferences and priorities in such decisions. See Section 5.3) for the detail on how we measure this concept.

intervention was conducted in the regions of Adyghea, Penza and Perm.[19] We compare the effects of two different interventions intended to enhance the efficiency in public service delivery of newly elected settlement level authorities. The first treatment comprised a non-intensive short-term intervention where local authorities received documents explaining participatory budgeting processes, legal mechanisms applicable to public service delivery, and setting of social and economic priorities. In addition, they received two-week workshops conducted by external consultants.

The second treatment was an intensive long-term intervention where, in addition to the features of the first treatment, each settlement was assigned one full-time Rural Municipal Consultant (RMC) and one Rural Legal Consultant (RLC) for one year. RMCs were local dwellers (teachers, journalists, lower level officials) trained by specialized firms in fiscal planning and participatory budgeting. RLCs were small town lawyers trained by a specialized firm in legislation pertaining to local government and decentralization. In addition, RLCs directed *pro bono* work with the population of the treated settlements. We denote the first treatment as "information", while the second one as "capacity building".

The results from the evaluation show inexistent aggregate effects of the "information" intervention. By contrast, estimated effects of the "capacity building" intervention are found to be positive and significant for most of the services that citizens consider important. These effects, however, are only found in Penza. While this finding may be puzzling, a closer look at the institutional context rationalizes it. Regions in which treatment effects are insignificant (Adyghea and Perm), were decentralized at the settlement level by 2006, while Penza was decentralized at this level since 1995. Therefore, while all regions received the same

[19] Adyghea has a population of 450 thousand inhabitants. Penza population stands at 1,562 thousands people. While Perm population accounts for 698 thousand.

treatments, only Penza exhibited significant and positive impacts. Clearly, significant impacts were only observed when interventions were provided after years of experience with deeper settlement level decentralization which we argue proxies for ex-ante level of local efficiency.

Furthermore, we also analyze heterogeneous effects of the intervention with respect to the pre-treatment level of settlement-accountability. In this regard, we find that settlements with higher accountability (another proxy of ex-ante efficiency) benefited the most from the "capacity building" intervention. In that way, it appears that the improvement of local efficiency is a long-term process and that even intensive interventions to newly decentralized governments may not be translated into public satisfaction in the short-term when ex-ante efficiency is low.

Our findings contribute to the decentralization and experimental literature in two main aspects. First, we provide, to the extent of our knowledge, the first attempt to isolate the role that the efficiency of local governments play in enhancing the decentralization of public services. Second, we show that, consistently with theory, external interventions directed towards enhancing local efficiency convey heterogeneous effects with respect to the pre-treatment level of efficiency. In this regard, we show that experimental impacts greatly depend on the context in which such interventions are conducted and highlight the importance of handling this heterogeneity when conducting an evaluation.

The remaining of the document is organized as follows. Section 2 presents a brief description of the decentralization reform conducted in rural Russia. Section 3 describes the intervention studied in this paper. Section 4 presents the data used and the empirical

approach adopted in the analysis. Section 5 discusses our results, while section 6 investigates the mechanisms driving such results. Finally, section 7 concludes.

2) Decentralization in Rural Russia

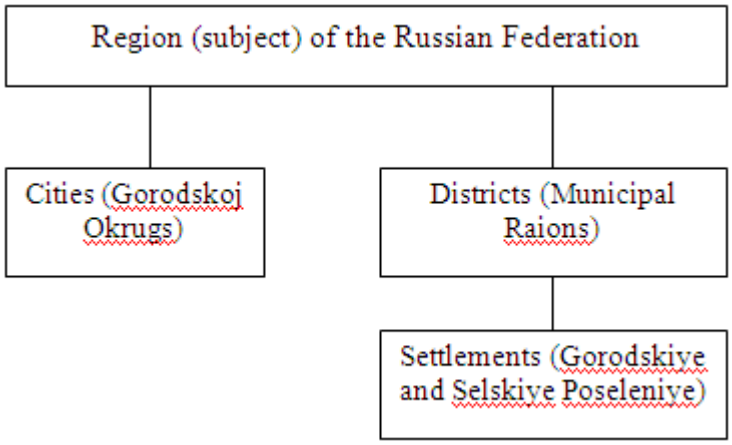
2.1. First Stage

The state of social and economic development of the Russian countryside is one of the major gaps in the post-socialist transition of the Russian Federation. The incidence of poverty in rural Russia is twice that of the urban areas. According to the 2002 census, the share of the poor living in rural areas has doubled since the mid 90s. About 60 percent of the extreme poor are rural Russians. Almost 20 years into the transition, a large share of agricultural producers - the main employers in the countryside - remain unreformed; about 40 percent of them are consistent loss makers. With 27 percent of the population (about 38 million people) living in rural areas, this developmental gap is economically significant. One way in which the Government of Russia (GoR) tried to address this gap was through major decentralization reforms. We provide a brief overview of these reforms.

In 1995, the *Law on the General Principles of the Organization of Local Self-Government* defined the status of local governments and their relations with regional authorities. As shown in Figure 1, in 1995 Russia was politically divided into 89 regions. Regions were subdivided into districts (mainly in rural areas) and cities (in urban areas), constituting local self governing bodies. In addition, districts were sub-divided into settlements. The 1995 local governance legislation allowed regional governments to decide independently on the level of establishing formal self-governance. Accordingly, 10 out of 89 regions opted for settlement level formal local government as the lower level of the two tier

district/settlement self governing structure. We exploit the fact that Penza was one of these 10 regions to investigate how the effects of the intervention to be studied vary with ex-ante decentralized experience.

Figure 1: Structure of local Self-Government in Russia



2.2. Second Stage

Federal Law No. 131 entitled *On the General Organizational Principles of Local Self-Government in the Russian Federation* (the Law), adopted in 2003 and enacted in January 2006, envisaged a universal, two-level model (district/settlement) of local self-governance for all regions of Russia following the structure presented in Figure 1. The country was thus divided into cities *(gorodskoj okrugs)* and municipal districts *(municipal raions)* forming the first tier of local government. Urban and rural settlements *(gorodskiye and selskiye poseleniye)* form the second tier of local government. Regional laws established the boundaries of municipalities in compliance with the requirements set out in the federal legislation. It changed the administrative-territorial division of the country and revised the principles of intergovernmental relations, tax assignment and tax sharing arrangements

among different levels of government and self governing bodies, as well as expenditure and service delivery mandates.

The Law was a major transformation in governance practices and entailed amendments to about 200 federal laws, as well as the Land, Tax, and Labor Codes. According to this Law, every settlement with a population over 1000 inhabitants was legislated to become a formally independent, self-governing unit, complete with an elected head of administration, elected council, and formal budget with assigned revenue sources, own taxes, and mandated responsibilities for services. In 2006, about 10,720 formally independent and self-governing rural settlements were formed, bringing the total number of rural settlements across Russia to about 20,000. Rural settlements now make up over 80% of the total number of settlements (24,200). Following the provisions of the law, local elections were held in October 2005 and newly elected authorities took office in January 2006 for a two to four year terms.

One significant feature of the Law is that it streamlined expenditure responsibilities assigned to local governments of different types. Previous decentralization legislation applied the same list of local government issues to all types of municipalities. As a result, there was a strong imbalance in the assignment of responsibilities: they were assigned in accordance with the ownership of a particular asset (e.g., school, hospital). Under the Law, the specified government authority must provide for a service irrespective of the presence of the appropriate institution (e.g., hospital). If a service cannot be provided by a municipality in its own institution, the municipality may pay for provision of the service at another institution. Thus, the new law guaranteed service provision and determined the tier of local government

(district or settlement) responsible for it. Table 1 displays the main functions assigned to districts, while Table 2 does the same for settlements.

Table 1: Functions assigned to settlements

Construction and maintenance of intra-settlement roads
Intra-settlement public transportation
Recreational and cultural activities
Waste collection
Construction and maintenance of housing for low-income households
Maintenance of cemeteries

Table 2: Functions assigned to districts

Preschool, primary, and secondary education
Health care in general hospitals, maternity care, and ambulance services
Municipal police
Inter-settlement public transportation
Protecting the environment

The Law assigned specific fiscal shares to each level of local governments. Districts were assigned 20 percent of the personal income tax, 90 percent of the single tax on imputed income and 30 percent of the single tax on agricultural enterprises (see Table 3). Settlements were assigned two taxes of their own (land tax and personal property tax). In addition, they were assigned 10 percent of the personal income tax and 30 percent of the single tax on agricultural enterprises. This was a significant change since, for the first time, local governments were assigned their own sources of revenues in order to finance the delivery of public services under their responsibility. Finally, in addition to the assigned taxes and shares, the federal government could transfer additional funds to local governments if their own budgets were insufficient to provide the services under their jurisdictions.

Table 3: Fiscal shares assigned to different tiers of government by Federal Law No. 131

Tax	Determination of		Taxes assigned by Budget Code (%)			
					Local	
	Base	Rate	Federal	Regional	Districts	Settlements
Local taxes						
Personal property tax	F	L				100%
Land tax	F	L				100%
Regional taxes						
Enterprise property tax	F	R		100%		
Transport tax	F	R		100%		
Tax on gambling businesses	F	R		100%		
Federal taxes assigned to regional and local governments						
Enterprise profits tax	F	F	27%	73%		
Personal income tax	F	F		70%	20%	10%
Excise taxes						
On alcohol and alcohol-based products	F	F	50%	50%		
On gasoline and diesel fuel	F	F	40%	60%		
On alcoholic products (except wine)	F	F		100%		
On wine, beer	F	F		100%		
Mineral resource extraction tax						
Hydrocarbons other than gas	F	F	95%	5%		
Common minerals	F	F		100%		
Other minerals	F	F	40%	60%		
Fee for the use of fauna	F	F		100%		
Simplified taxation system for small businesses	F	F	10%	90%		
Single tax on imputed income	F	F	10%		90%	
Single tax on agricultural enterprises	F	F	10%	30%	30%	30%

Note: F in column 1 stands for "Federal"; L in column 2 stands for "Local"; R in column 2 stands for "Regional"

It is worth noting that public budgetary hearings were part of the new legislation and demanded that public hearing are held prior to the approval of local budgets and that the population participates in the decision making about the allocation of local resources. Accordingly, making this formal request part of a living effective practice of budgetary decision making was one of the main operational objectives of the intervention described below.

3) The Intervention

The fact that the Russian decentralization reform represented a major change in the responsibilities for public services delivery from federal to local governments, raises the question as to what extent do these newly conformed units were capable of performing these new duties. In that way, it appears that public service delivery under decentralization may be

sensitive to the local efficiency for delivery and for adequately financing the delivery with the revenues transferred from the federal government. To shed light on these issues and investigate the extent to which local efficiency could enhance the decentralization process; the GoR and The World Bank conducted a pilot program entitled "Local Self-Governance and Civic Engagement in Rural Russia".[20] This pilot experimented with two different methods aimed to enhance public service delivery capabilities to newly elected authorities in three Russian regions (Adyghea, Penza and Perm). We describe this intervention below.

3.1. Intervention Description

The intervention administered two different village-level treatments. Both of the treatments shared a common component in which the newly elected settlement-level authorities and their teams were provided with training materials explaining several aspects of their new responsibilities. These materials included explanations regarding participatory budgeting processes, setting of social and economic priorities and the development of skills in demand driven service provision. This basic treatment also included the organization of workshops in which specialized consultants explained these aspects to new authorities and active community members. We denote this intervention as "information".

The second treatment, in addition to what the first treatment provided, assigned one full-time Rural Municipal Consultant (RMC) and one Rural Legal Consultant (RLC) for a one year period to each treated settlement. These were local dwellers trained in the fundamentals of fiscal planning and participatory budgeting (in the case of RMCs) and local/district lawyers (in the case of the RLCs) trained in the creation of the necessary local

[20] The cost of the pilot was approximately $1.2 million and was funded by the Japanese Social Development Fund. The data collection and analysis were co-funded by the Government of the Russian Federation and the World Bank.

legal documents necessary to enact the provisions of the new law. The consultants were in constant contact with higher level specialists from the Institute of Urban Economics (IUE), Moscow. Since the practice of settlement level participatory budgeting was new to Russia, IUE consultants were, in turn, trained and advised by international consultants from the Urban Institute.

The tasks of the RMCs were to guide the treated settlements through the first post-reform budget cycle, help train public officials and interested citizens from among the population to organize meaningful public budgetary hearings, help identify three achievable budget priorities, and provide consultation assistance in achieving those priorities by mobilizing public and private resources and skills for each particular task. RLCs, in addition to creating the accompanying legal documents (the charter of the settlement, fiscal documentation, etc.), were also providing *pro-bono* consultations to the population on private matters of public significance (e.g., rights for social assistance, cadastre/land/property legislation issues).[21] We denote this treatment as "capacity building".

Each of these treatments was implemented in 22 rural settlements, randomly selected from out of 109 settlements distributed in the regions of Adyghea, Penza and Perm (see Figure 2). Table 4 describes the breakdown of settlements by treatment group and region. Both treatments were conducted during year 2006, after the newly elected settlement-level authorities took office. Monitoring data show that the treatments were well implemented: All of the treated settlements were provided with the information and a full set of courses that were well attended. Public hearings monitored and facilitated by the RMCs took place in all the "capacity building" settlements. All of the treated settlements reported on the achievement of the identified public service priorities. More than 500 *pro-bono* legal

[21] For more detail see the pilot project website at: www.worldbank.org/russia/localgovernance

consultations have been provided, and legal documentation for practical decentralization created.

Figure 2: Geographical Location of Intervened Regions

Table 4: Settlements distribution by treatment status

	Adyghea	Penza	Perm	**Total**
Information	4	9	9	**22**
Capacity Building	4	9	9	**22**
Control	12	27	26	**65**
Total	**20**	**45**	**44**	**109**

3.2. Theoretical Predictions

Following Galiani, Gertler and Schargrodsky (2008) we represent the production technology for public service *i* with a constant elasticity of substitution (CES) function as follows: $P_i = \left[(\eta a_i)^{\alpha} + (\gamma b_i)^{\alpha} \right]^{\frac{1}{\alpha}}$. Where P_i is the output level of public service *i*, a_i is the level of resources invested by the local government, b_i is the level of resources invested by the central government, η is the efficiency of the local government, γ is the

efficiency of the central government, and α is the degree of substitutability between local and central investments in the production of service *i*.

We take central government efficiency (γ) as given and assume that local efficiency is given by the function $\eta = \eta\left[A(t,m,e);T\right]$. Where $A(t,m,e)$ denotes public managerial capacity of the local government. This managerial capacity is assumed to be an increasing function of local democratic experience (t), accountability (m), and a vector of other (potentially unobservable) factors like innate ability, related education and skills, etc; which we denote by e. Finally, T denotes some external intervention aimed to influence local government efficiency. We assume that $\eta\left[A(t,m,e);T\right]$ is increasing in $A(t,m,e)$, while the effect of T is unknown.

In this setting, a local (central) government with $\eta = 0$ ($\gamma = 0$) represents a totally inefficient government. Conversely, a local (central) government with $\eta = 1$ ($\gamma = 1$) represents a totally efficient government. We rule out these extreme cases by assuming that $0 < \eta < 1$ and $0 < \gamma < 1$. In addition, this production function has the following properties with respect to α: $\lim_{\alpha \to 0} P_i = (\eta a_i)^{0.5}(\gamma b_i)^{0.5}$ and $\lim_{\alpha \to 1} P_i = \eta a_i + \gamma b_i$. Therefore, when α=0, both local and central investment complement each other. By contrast, when α=1, both investments are perfect substitutes. We assume that $0 < \alpha < 1$. We use this setting to derive the equilibrium level of public services provided under centralized and decentralized regimes.

- Case 1: Centralized System

In this case it is apparent that $a_i = 0$, since no local investments are made. In a centralized system, local public funds are not administered by some local independent government. Indeed "local governments" are appointed and are part of the central government. Therefore, the central government is the only one providing inputs in the production of public services, i.e. $b_i > 0$.

We further assume that the central government has a cost of investing funds in the provision of local public services denoted by $c^c(b_i)$. This cost function is assumed to be increasing and convex to reflect that as more central resources are devoted to local settlements, marginal costs of monitoring these investments and potential corruption scenarios may be increasing. For simplicity and tractability we parameterize the cost function as $c^c(b_i) = b_i^2$. Therefore, the central government problem simplifies to $\max_{b_i} P_i - b_i^2 = \left[(\eta a_i)^\alpha + (\gamma_c b_i)^\alpha \right]^{\frac{1}{\alpha}} - b_i^2 = \gamma_c b_i - b_i^2$. Optimality yields to the following equilibrium level of public service provision under a centralized system: $P_i^c = \frac{\gamma_c^2}{2}$. Notice that, in this system, every locality receives the same level of public output which is increasing in the efficiency of the central government (γ_c).

- Case 2: Decentralized System

In this case, local governments exist and they decide the amount to invest in local public services (a_i). In addition, the central government continues making transfers to local governments (b_i). However, these transfers are now locally administered. So, the

efficiency in managing these transfers will now be an increasing function of the level of local managerial capacity, $A(t,m,e)$, and the external intervention T. In other words, under decentralization $\gamma_d = \psi\left[A(t,m,e);T\right]$. Where the function $\psi[.]$ has the same properties as function $\eta[.]$ above. Therefore, if $\gamma_d = \psi\left[A(t,m,e);T\right] > \gamma_c$, the local government is more efficient than the central government in delivering public services. While the reverse is true if $\gamma_d = \psi\left[A(t,m,e);T\right] < \gamma_c$

In this setting, the equilibrium level of local public service delivery is the sub-game perfect equilibrium (SPE) of a two stage game. In the first stage, the central government chooses the amount of resources to be transferred to the local government (b_i). In the second stage, the local government chooses the amount of local inputs (a_i), taking b_i as given. In addition, the local government bears a cost of investing in local public services denoted by $c^l(a_i)$. This cost function is assumed to be increasing and linear to reflect that marginal monitoring costs are constant. The intuition for this assumption is that local monitoring may detect misuse of resources quicker than central monitoring. Therefore, the cost function for central government investments in locally administered services is assumed convex, while the cost function for local investments is assumed linear. We parameterize the local managerial cost as $c^l(a_i) = a_i$

The game is solved by backwards induction as follows. In the second stage, the local government solves $\max_{a_i} P_i - a_i = \left[(\eta a_i)^\alpha + (\gamma_d b_i)^\alpha\right]^{\frac{1}{\alpha}} - a_i$. Optimality yields $a_i^d(b_i) = \frac{\gamma_d b_i}{\eta}\left[\eta^{\frac{\alpha}{\alpha-1}} - 1\right]^{\frac{-1}{\alpha}}$. Then, in the first stage, the central government solves

$\max_{b_i} P_i - a_i^d(b_i) - b_i^2 = \left[\left(\eta a_i^d(b_i)\right)^\alpha + \left(\gamma_d b_i\right)^\alpha\right]^{\frac{1}{\alpha}} - a_i^d(b_i) - b_i^2$. Where optimality yields $b_i^d = \frac{\gamma_d}{2}\left[\eta^{-1}\left(\eta^{\frac{\alpha}{\alpha-1}} - 1\right)^{\frac{\alpha-1}{\alpha}}\right]$. Then by replacing b_i^d in $a_i^d(b_i)$ we get $a_i^d = \frac{\gamma_d^2}{2}\eta^{-2}\left(\eta^{\frac{\alpha}{\alpha-1}} - 1\right)^{\frac{\alpha-2}{\alpha}}$. Finally, by replacing a_i^d and b_i^d in P_i we get the SPE level of public service provision under decentralization given by $P_i^d = \frac{\gamma_d^2}{2}\eta^{\frac{2-\alpha}{\alpha-1}}\left(\eta^{\frac{\alpha}{\alpha-1}} - 1\right)^{\frac{\alpha-2}{\alpha}} = \frac{\gamma_d^2}{2}\xi(\eta,\alpha)$.

Furthermore and without loss of generality, we assume that the efficiency that local governments have in managing central transfers, is the same as the efficiency with which they manage their own resources. In terms of the model we assume that $\psi[.] = \eta[.]$, implying that $\gamma_d = \eta = \eta\left[A(t,m,e);T\right]$. Therefore, the equilibrium level of public service provision under decentralization reduces to $P_i^d = \frac{\gamma_d^2}{2}\xi(\gamma_d,\alpha)$. We denote the difference between decentralized and centralized equilibrium level of public service delivery as the Decentralized Additional Supply (DAS). In that way, the DAS is given by $\Delta P = P_i^d - P_i^c = \frac{\gamma_d^2}{2}\xi(\gamma_d,\alpha) - \frac{\gamma_c^2}{2}$.

Notice that the DAS, depending on the efficiency and substitutability parameters, may be positive or negative. If local efficiency is relatively higher than central efficiency, the DAS will be positive; while the opposite is true if central governments are relatively

more efficient than local counterparts. In this way, the model captures the theoretical ambiguity of decentralization on public service delivery set forth in the literature.

In the context of the model, the contribution of an external intervention, *x*, directed towards affecting the efficiency of local governments is given by:

$$\frac{\partial[\Delta P]}{\partial T}=\frac{\partial P_i^d}{\partial T}=\underbrace{\left[\gamma_d\xi(\gamma_d,\alpha)+\frac{\gamma_d^2}{2}\frac{\partial\xi(\gamma_d,\alpha)}{\partial\gamma_d}\right]}_{>0}\underbrace{\left\{\eta\left[A(t,m,e);T=x\right]-\eta\left[A(t,m,e);T=0\right]\right\}}_{sign=?} \tag{1}$$

where the first term is unambiguously positive for $0<\gamma_d<1$ and $0<\alpha<1$. Furthermore, the model predicts that, for any possible combination of local efficiency (γ_d) and substitutability (α), the effects of intervention *x* should be increasing in the baseline level of local efficiency as follows:

$$\frac{\partial^2 P_i^d}{\partial T\partial\gamma_d}=\underbrace{\left[\xi(\gamma_d,\alpha)+\gamma_d\left(\frac{2\partial\xi(\gamma_d,\alpha)}{\partial\gamma_d}+\frac{\gamma_d}{2}\frac{\partial^2\xi(\gamma_d,\alpha)}{\partial\gamma_d\partial\gamma_d}\right)\right]}_{>0}\underbrace{\left\{\eta\left[A(t,m,e);T=x\right]-\eta\left[A(t,m,e);T=0\right]\right\}}_{sign=?} \tag{2}$$

In that way, the model predicts that if intervention *x* affected the DAS through its effect on local efficiency, this effect should be stronger in localities with higher ex-ante local efficiency. Furthermore, if the intervention *x* was orthogonal to unobservables affecting local managerial capacity (e), the effect on the DAS should be stronger in places with relatively higher pre-treatment democratic experience (*t*) and accountability (m). That is, in terms of the model we have:

$$\frac{\partial^2 P_i^d}{\partial T\partial k}=\underbrace{\left[\xi(\gamma_d,\alpha)+\gamma_d\left(\frac{2\partial\xi(\gamma_d,\alpha)}{\partial\gamma_d}+\frac{\gamma_d}{2}\frac{\partial^2\xi(\gamma_d,\alpha)}{\partial\gamma_d\partial\gamma_d}\right)\right]}_{>0}\underbrace{\left[\frac{\partial\gamma_d}{\partial A(t,m,e)}\right]}_{>0}\underbrace{\left[\frac{\partial A(t,m,e)}{\partial k}\right]}_{>0}\underbrace{\left\{\eta\left[A(t,m,e);T=x\right]-\eta\left[A(t,m,e);T=0\right]\right\}}_{sign=?} \tag{3}$$

where k refers to either experience (t) or accountability (m).

In our setting, the region of Penza had, at the time of the intervention, ten more years of local self-governance experience in comparison with the other intervened regions (Adyghea and Perm). Therefore, the model predicts that if the intervention affected the DAS through an effect on local efficiency, these effects should be stronger in Penza. In addition, while the model predicts weaker impacts in Adyghea and Perm, it also anticipates relatively stronger effects in areas with higher pre-treatment accountability. Intuitively, the model states that if some authority has a higher level of public experience or has been exposed to democratic participation for longer time; then we might expect that some of the capacity could have been developed through the empirics of governance in previous years so the marginal effect of the intervention would be greater.

The experimental nature of this intervention addresses concerns regarding potential endogeneity of program placement. Therefore, this intervention provides an ideal environment to test the implications of this theoretical model; while helping us to learn about the effectiveness of these interventions towards enhancing the provision of public services. We describe the dataset used in our empirical work and provide evidence regarding the quality of the randomization process in the next section.

4) Evaluation: Data Collection and Empirical Approach

4.1. Data Collection

The evaluation took place in 109 rural settlements distributed across the regions of Adyghea, Penza and Perm. Figure 2 displays the geographical location of these regions within Russia. Adyghea covers 3,010 squared miles and is situated 1,038 miles southwest

from Moscow. It has a population of 450 thousand inhabitants 48 percent of them rural. Penza is situated 373 miles southeast from Moscow, coverning 16,720 squared miles. The share of rural population is 36 percent out of the total of 1,562 thousands people. Perm covers 62,000 squared miles at an approximate distance of 891 miles northeast from Moscow and has the population of 698 thousand. At 25 percent its share of rural population is slightly below the national average of 27 percent. As mentioned above, regions in Russia are divided into districts. In that way, six districts in Adyghea, nine districts in Penza, and nine districts in Perm were randomly selected to participate in the pilot. Then the pilot settlements were randomly selected from these districts (See Table 4 for the settlement level distribution with respect to treatment status).[22]

In both treatment and comparison settlements, a baseline survey was conducted in February 2006, and the endline survey took place in November 2007. The evaluation instruments included a dataset obtained from a questionnaire applied to 13 to 19 randomly selected households per settlement (depending on the size of the settlement). The questionnaire covered satisfaction levels with public services, priorities for public service delivery, level of trust among villagers and between villagers and authorities, participation levels in public and collective actions, and economic and demographic characteristics. In addition, a similar questionnaire was applied to Public Officials of each settlement.[23] Finally, administrative data with respect to fiscal revenues and expenditures for each settlement during years 2006 and 2007 was collected.

[22] After the pilot districts were selected; two districts in Adyghea, three districts in Penza and three districts in Perm were randomly selected as "information" districts. Also, two districts in Adyghea, three districts in Penza and three districts in Perm were randomly selected as "capacity building" districts. After this selection; four settlements in Adyghea, nine settlements in Penza and nine settlements in Perm were randomly selected from the "information" districts to receive the "information" treatment. Finally, four settlements in Adyghea, nine settlements in Penza and nine settlements in Perm were randomly selected from the "capacity building" districts to receive the "capacity building" treatment.

[23] The surveyed positions included the head of the executive body and the head of the representative body.

As shown in Table 5, the baseline survey was applied to 2,049 households distributed across the three regions and 109 settlements. At endline, the same households were to be interviewed; however, only 1,645 were reached. This constitutes an overall attrition rate of 19.72 percent. Table 5, breaks the attrition rate by region and treatment status. Attrition rates are higher for the "capacity building" settlements; with an overall rate of 23.19 percent within the category. Attrition is a bit lower for "information" settlements, accounting for 17.2 percent. If attrition was unrelated to treatment status, the estimated impacts would still be unbiased and the loss would be reflected in the power of the estimates. However, if attrition was systematically correlated with treatment and the outcomes of interest, estimated impacts would be biased.

Table 5: Attrition patterns across regions

	Households Interviewed		Attrition Rates within Settlement Categories		
Region	**At baseline**	**At endline**	**Overall**	**Information**	**Capacity Building**
Adyghea	395	315	20.25%	10.67%	20.00%
Penza	851	744	12.57%	11.11%	17.44%
Perm	803	586	27.02%	26.71%	30.54%
Total	**2,049**	**1,645**	**19.72%**	**17.20%**	**23.19%**

Therefore, we first test if attrition was related to treatment status by regressing an indicator for attrition on indicators for treatment status and several economic, demographic and geographic characteristics obtained from the baseline survey.[24] Table 6 shows that attrition was only related to the "capacity building" intervention in Penza. Estimates imply, for Penza, that persons living in "capacity building" settlements are six percentage points more likely to leave the sample with respect to persons living in control settlements.

[24] These characteristics include HH head age, education level, sex, marital status, HH income, HH size, distance to the nearest city (in km) and distance to the district center (in km).

Table 6:Relation Between Treatment and Attrition

Dependent Variable = Household was not found in the endline survey								
	All Regions		Adyghea Region		Penza Region		Perm Region	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
Information	-0.022	-0.003	-0.126	-0.080	-0.003	0.017	0.008	-0.023
	(0.032)	(0.026)	(0.083)	(0.061)	(0.016)	(0.020)	(0.050)	(0.048)
Capacity Building	0.038	0.053*	-0.033	0.027	0.060**	0.059**	0.046	0.076
	(0.032)	(0.029)	(0.085)	(0.064)	(0.029)	(0.022)	(0.047)	(0.053)
Controls (at baseline)	NO	YES	NO	YES	NO	YES	NO	NO
Observations	2049	1675	395	322	851	674	803	679

Note: Table reports OLS estimates. Controls include household head age, education level, sex, marital status, income, household size, distance to the nearest city (in km) and distance to the district center (in km). Standard errors clustered at the settlement level in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

The previous relation would imply biases in the estimated treatment effects if attrition were also related to the outcomes of interest. We investigate this by regressing baseline levels of the outcomes to be studied on an indicator for attrition and its interactions with treatment status. Table 7 evidences that attrition was related to only one out of 27 outcomes for Penza at the 10 percent significance level or lower. Likewise, coefficients on the interaction between the attrition indicator and the "capacity building" treatment indicators are significant for only two outcomes in Penza at the 10 percent significance level or lower.[25] Therefore, it appears that attrition was unrelated to the outcomes of interest and the estimated treatment effects will not suffer from systematic biases.

[25] We performed the same analysis for Adyghea and Perm. In Adyghea, only five coefficients on the attrition indicator were significant at the 10 percent level or lower. In Perm, only one coefficient on the attrition indicator was significant at the 10 percent level. Estimates from these regressions are available upon request.

Table 7: Attrition Check - Penza Region

	No Controls			Include Controls		
	Leaver	Leaver x Information	Leaver x C. Building	Leaver	Leaver x Information	Leaver x C. Building
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent Variables - The Process of Public Decision Making						
Made suggestions to local authorities	0.014	0.000	-0.025	0.023	-0.058	-0.034
on settlement budget	(0.054)	(0.089)	(0.091)	(0.064)	(0.099)	(0.088)
Have not gone anywhere to voice	-0.113**	0.003	0.070	-0.119*	0.043	0.064
complaints with municipal leaders	(0.054)	(0.106)	(0.084)	(0.059)	(0.106)	(0.089)
Settlement administration takes	-0.011	0.176	0.067	-0.029	0.239*	0.059
citizens' problems seriously	(0.068)	(0.121)	(0.119)	(0.084)	(0.141)	(0.152)
Number of yearly meetings	-0.184	-0.574	1.590	-0.079	-1.022	1.395
between authorities and citizens	(0.638)	(0.812)	(1.591)	(0.704)	(0.884)	(1.304)
Municipality residents met to address	-0.010	-0.106	0.135	0.014	-0.093	0.083
significant settlement issues	(0.055)	(0.118)	(0.085)	(0.066)	(0.125)	(0.095)
Think that meetings b/w authorities	-0.100	0.204	0.204	-0.087	0.175	0.191
and citizens solve important problems	(0.081)	(0.135)	(0.122)	(0.080)	(0.138)	(0.132)
Willing to take part in local council's	-0.080	0.102	-0.029	-0.098	0.078	0.023
acivities (e.g., garbage collection)	(0.085)	(0.110)	(0.112)	(0.088)	(0.107)	(0.108)
If residents' and authorities' interests	-0.086	0.097	-0.084	-0.053	0.047	-0.077
do not coincide both make concessions	(0.074)	(0.115)	(0.143)	(0.084)	(0.129)	(0.165)
Real power is held by the	-0.039	-0.028	0.117	-0.014	0.022	0.096
settlement administration	(0.055)	(0.134)	(0.148)	(0.073)	(0.144)	(0.127)
Panel B. Dependent Variables - Satisfaction with Settlement Level Services						
Satisfaction index	-0.030	0.136	0.102	-0.023	0.141*	0.128
with roads	(0.038)	(0.082)	(0.080)	(0.044)	(0.077)	(0.085)
Report difficulties to get to school	-0.002	0.035	-0.018	0.004	0.030	-0.016
because of lack of transportation	(0.020)	(0.054)	(0.017)	(0.025)	(0.054)	(0.025)
Satisfaction index with cultural	-0.050	0.128	0.041	-0.007	0.075	-0.014
institutions and recreational areas	(0.044)	(0.082)	(0.086)	(0.047)	(0.072)	(0.074)
Satisfaction index with natural	-0.028	0.091	-0.035	0.041	0.032	-0.091
and cultural monuments	(0.047)	(0.070)	(0.086)	(0.046)	(0.076)	(0.086)
Satisfaction index with collection,	-0.076*	0.251***	0.095	-0.047	0.191**	0.054
removal and utilization of waste	(0.042)	(0.090)	(0.071)	(0.045)	(0.078)	(0.086)
Think that municipality is	-0.079	0.140	0.059	-0.019	0.024	-0.104
clean or rather clean	(0.061)	(0.127)	(0.145)	(0.068)	(0.113)	(0.148)
Satisfaction index with housing	-0.056	0.031	0.174***	-0.024	0.016	0.124**
fund	(0.034)	(0.094)	(0.054)	(0.035)	(0.076)	(0.054)
Satisfaction index with maintenance	-0.073	0.262***	0.032	-0.051	0.205***	0.006
of cemeteries	(0.044)	(0.071)	(0.072)	(0.049)	(0.070)	(0.065)
Panel C. Dependent Variables - Satisfaction with District-Level Services						
Satisfaction index with public	-0.037	0.105	0.032	-0.006	0.119	-0.003
transportation	(0.043)	(0.082)	(0.091)	(0.053)	(0.103)	(0.080)
Satisfaction index with law	-0.053	0.158**	0.045	-0.018	0.138*	0.004
and order	(0.041)	(0.072)	(0.086)	(0.050)	(0.075)	(0.099)
Satisfaction index with general	-0.043	0.066	-0.022	-0.051	0.121*	-0.021
education	(0.042)	(0.077)	(0.077)	(0.042)	(0.068)	(0.071)
Report class cancellations due	0.006	-0.055*	-0.055*	0.014	-0.050	-0.039
to the teachers' absence	(0.030)	(0.029)	(0.029)	(0.034)	(0.037)	(0.034)
Report not enough teachers to	0.012	0.017	-0.036	0.008	0.021	-0.054*
teach the main subjects	(0.023)	(0.051)	(0.024)	(0.023)	(0.053)	(0.031)
Report school facilities in	0.066	-0.002	-0.040	0.073	-0.013	-0.020
poor condition	(0.045)	(0.074)	(0.061)	(0.048)	(0.078)	(0.067)
Satisfaction index with pre-school	-0.057	0.374***	-0.020	-0.083	0.467***	0.074
education	(0.067)	(0.107)	(0.159)	(0.062)	(0.110)	(0.119)
Satisfaction index with level	-0.037	0.180***	0.021	-0.032	0.170**	0.006
of medical treatment	(0.042)	(0.066)	(0.066)	(0.045)	(0.074)	(0.072)
Report very good or somewhat	0.024	0.053	0.079	-0.018	0.009	0.166
good quality of health stop	(0.065)	(0.121)	(0.128)	(0.062)	(0.122)	(0.102)
Number of visits in last year	-0.387	0.149	1.008	-0.793	0.412	1.399
to the rural health stop	(0.586)	(0.981)	(0.935)	(0.653)	(1.004)	(1.028)

Definitions: Leaver is an explanatory variable that refers to whether the household was interviewed at baseline but was not found at endline. Information is an indicator that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention.

Notes: Columns (1) - (3) and (4) - (6) report coefficients from one regression where indicators for Leaver and its interactions with "information" and "capacity building" treatments enter as RHS variables. All regressions use only baseline data. Estimated standard errors clustered at the settlement level are in parentheses. Regressions in columns (4) - (6) include the following controls: baseline value of the dependent variable, household head age, education level, sex, marital status, income, household size, distance to the nearest city (in km) and distance to the district center (in km). * significant at 10%; ** significant at 5%; *** significant at 1%

4.2. Empirical Approach

4.2.1. Randomization Check

We consider three sets or families of outcomes. First, we look at outcomes related to the process of formal public decision making. Then we consider outcomes that proxy for public service delivery under decentralization (i.e. P_i^d in terms of the analytical model). In the second set of outcomes, we assess satisfaction levels with public services administered at the settlement level. Finally, we evaluate satisfaction levels with public services administered at the district level.

The dataset allow us to test the quality of treatment randomization. For this purpose, we first estimate the following regression model:[26]

$$O_{is,2006} = \alpha + \beta^{T1} * T1_s + \beta^{T2} * T2_s + \delta' X_{is,2006} + \varepsilon_{is,2006} \quad (4)$$

where $O_{is,2006}$ is the outcome of interest for household *i* in settlement *s* at baseline. *T1* is an indicator for "information" settlement. *T2* is an indicator for "capacity building" settlement. $X_{is,2006}$ is a *Kx1* vector of economic, demographic and geographic characteristics for household *i* in settlement *s* at baseline.[27] Finally, $\varepsilon_{is,2006}$ is a disturbance term which we allow to be correlated across households within settlements.[28]

Given that the information contained in model (4) was collected before treatment execution; if the randomization quality was appropriate, estimates of β^{T1} and β^{T2} should be statistically indistinguishable from zero. In particular, the share of significant estimates

[26] We estimate the models using the observations corresponding to households that responded the questionnaire in both baseline and endline surveys (1,645 observations in total).
[27] Vector *X* includes HH head age, education level, sex, marital status, HH income, HH size, distance to the nearest city (in km) and distance to the district center (in km). We run the specifications with and without controls. Results were not sensitive to the inclusion of controls.
[28] We cluster estimated standard errors at the settlement level in all of our regressions.

should not exceed the significance level used in the test. These estimations confirm that randomization quality with respect to the first two sets of outcomes was acceptable. For outcomes related to the process of formal public decision making, 5.6 percent of these estimates were significant at the 10 percent level in Perm; while 11.1 percent were significant in Adyghea and Penza. For outcomes measuring satisfaction with settlement level services, 6.3 percent of the estimates were significant at the 10 percent level in Adyghea; while 12.5 percent were significant in Penza and Perm.

Unfortunately, randomization quality deteriorates when looking at the satisfaction level with district services. For this latter set, about 20 to 27 percent of estimated coefficients are significant at the 10 percent level.[29] Therefore, estimated program impacts for these outcomes need to be interpreted with caution as treatment status appears to be correlated with baseline outcomes. To ameliorate these problems, we will include the baseline level of the outcomes as controls in the estimated treatment effects regressions; see the next section for more details.

4.2.2. Program Effect

Given the randomization, the empirical strategy is fairly straightforward. Outcomes are grouped in the sets outlined in the previous section. We regress the endline measure of each outcome on an indicator for each treatment group (the control group is the omitted category), the baseline measure of each outcome, and the baseline values for all of the outcomes in the family or set, i.e.[30]

$$O_{is,2007} = \theta + \lambda^{T1} * T1_s + \lambda^{T2} * T2_s + \kappa' X_{is,2006} + \omega * O_{is,2006} + \mu_{is,2007} \quad (5)$$

[29] Regressions results are available upon request.
[30] We also estimate the model in simple differences, without any control variables. Estimated impacts were not different and are available upon request.

where *i* indexes households, *s* indexes settlement, *T1* and *T2* are the treatment indicators, $X_{is,2006}$ is a *Kx1* vector containing the baseline values for all of the outcomes in the family or set, and $O_{is,2006}$ is the baseline value of the outcome being measured.

The covariates ($X_{is,2006}$ and $O_{is,2006}$) are included to improve estimation precision and to account for chance differences between groups in the distribution of pre-treatment characteristics. The estimated standard errors are clustered at the settlement level (using Huber-White standard errors) to reflect the fact that treatments were implemented at that level. We also run a specification where we pool both treatments together.

As noted in Banerjee et.al. (2008), the large number of outcomes that could have been affected by the intervention constitutes an empirical difficulty in the way that we could choose to emphasize significant or larger effects. Therefore, to circumvent this problem, we follow Katz, Kling, and Liebman (2007) and calculate the average standardized effect over each family or set of outcomes. For each family, we construct a summary index *Z* defined to be the equally weighted average of *z*-scores of its components.[31] The *z*-scores were calculated by subtracting the control group mean and dividing by the control group standard deviation.[32] In that way, each component of the index has mean zero and standard deviation one for the control group.

With these indexes, we run the following regression for each family of outcomes:

[31] When constructing the summary index, the sign of each outcome within a family was oriented so that more beneficial outcomes have higher scores.

[32] If an individual has a valid response to at least one component of the index, any missing values for other components are imputed at the treatment group mean to which the individual was assigned by the random program placement. As Katz, Kling, and Liebman (2007) point out, "this results in differences between treatment and control means of an index being the same as the average of treatment and control means of the components of that index (when the components are divided by their control group standard deviation and have no missing value imputation), so that the index can be interpreted as the average of results for separate measures scaled to standard deviation units".

$$Z_{is,2007} = \theta + \lambda^{T1} * T1_s + \lambda^{T2} * T2_s + \omega * Z_{is,2006} + \mu_{is,2007} \quad (6)$$

where *i* indexes households, *s* indexes settlement, *T1* and *T2* are the treatment indicators, and $Z_{is,2006}$ is the baseline value of the summary index. Notice that, since the absolute magnitudes of the indices are in units akin to standardized test scores; the estimates on the treatment indicators show where the mean of the treatment group is in the distribution of the control group in terms of standard deviation units. We report these estimates in all of our analyses.

5) Intervention Results

5.1. The Process of Formal Public Decision Making

We first analyze the program impact on households' participation in public decision making. Table 8 – Panel A suggests that the "information" treatment within Penza reduced the likelihood of making suggestions to local authorities on settlement budget by 5.4 percentage points. This negative effect is significant at the 10 percent level; implying lower participation of households in "information" areas with respect to budgetary issues. The previous finding was unexpected since the objective of these treatments was to increase the level of households' participation in the public processes. Nonetheless, local budget is a technical issue that may inspire little household participation.

Table 8: Estimated Treatment Effects - Penza Region

	Baseline Mean	Endline Comparison Group Mean	OLS: Impact of Treatment in Endline		
			Information	C. Building	Any Treatment
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Variables - The Process of Public Decision Making					
Made suggestions to local authorities	0.144	0.073	-0.054*	-0.015	-0.033
on settlement budget	(0.016)	(0.013)	(0.031)	(0.034)	(0.027)
Have not gone anywhere to voice	0.943	0.934	-0.035	-0.009	-0.021
complaints with municipal leaders	(0.013)	(0.013)	(0.034)	(0.027)	(0.024)
Settlement administration takes	0.658	0.486	0.027	0.287***	0.168*
citizens' problems seriously	(0.043)	(0.067)	(0.100)	(0.084)	(0.083)
Number of yearly meetings	3.464	1.077	0.051	-0.399	-0.214
between authorities and citizens	(0.325)	(0.201)	(0.542)	(0.299)	(0.320)
Municipality residents met to address	0.727	0.628	-0.036	0.110	0.045
significant settlement issues	(0.033)	(0.046)	(0.081)	(0.106)	(0.081)
Think that meetings b/w authorities	0.563	0.347	-0.063	0.374***	0.174*
and citizens solve important problems	(0.045)	(0.050)	(0.093)	(0.092)	(0.095)
Willing to take part in local council's	0.868	0.898	-0.063	0.028	-0.015
acivities (e.g., garbage collection)	(0.020)	(0.026)	(0.060)	(0.058)	(0.047)
If residents' and authorities' interests	0.401	0.431	-0.026	0.173	0.081
do not coincide both make concessions	(0.033)	(0.057)	(0.110)	(0.122)	(0.096)
Real power is held by the	0.243	0.233	-0.177***	0.386***	0.132
settlement administration	(0.037)	(0.039)	(0.063)	(0.109)	(0.101)
Average over family of			**-0.105**	**0.236****	**0.060**
outcomes (in standard deviations)			**(0.068)**	**(0.103)**	**(0.080)**
Panel B. Dependent Variables - Satisfaction with Settlement Level Services					
Satisfaction index	0.341	0.244	0.157***	0.162**	0.159***
with roads	(0.028)	(0.038)	(0.057)	(0.066)	(0.050)
Report difficulties to get to school	0.019	0.022	-0.029**	-0.021	-0.025*
because of lack of transportation	(0.008)	(0.010)	(0.014)	(0.015)	(0.014)
Satisfaction index with cultural	0.501	0.455	-0.040	0.119*	0.034
institutions and recreational areas	(0.032)	(0.035)	(0.067)	(0.068)	(0.060)
Satisfaction index with natural	0.463	0.416	-0.014	0.047	0.013
and cultural monuments	(0.026)	(0.036)	(0.069)	(0.069)	(0.057)
Satisfaction index with collection,	0.423	0.242	-0.018	0.071	0.024
removal and utilization of waste	(0.030)	(0.028)	(0.056)	(0.056)	(0.045)
Think that municipality is	0.675	0.570	0.086	0.234***	0.160**
clean or rather clean	(0.039)	(0.060)	(0.076)	(0.074)	(0.070)
Satisfaction index with housing	0.499	0.454	0.075*	0.102**	0.089***
fund	(0.026)	(0.020)	(0.044)	(0.040)	(0.033)
Satisfaction index with maintenance	0.658	0.598	0.074	-0.035	0.021
of cemeteries	(0.019)	(0.040)	(0.056)	(0.061)	(0.049)
Average over family of			**0.125**	**0.310*****	**0.216*****
outcomes (in standard deviations)			**(0.098)**	**(0.085)**	**(0.076)**
Panel C. Dependent Variables - Satisfaction with District-Level Services					
Satisfaction index with public	0.471	0.385	0.091	0.087	0.089
transportation	(0.035)	(0.049)	(0.078)	(0.083)	(0.067)
Satisfaction index with law	0.553	0.521	-0.029	0.037	0.005
and order	(0.026)	(0.022)	(0.069)	(0.033)	(0.040)
Satisfaction index with general	0.832	0.791	-0.075**	0.019	-0.026
education	(0.019)	(0.028)	(0.034)	(0.050)	(0.039)
Report class cancellations due	0.048	0.063	-0.063**	-0.023	-0.042*
to the teachers' absence	(0.010)	(0.017)	(0.024)	(0.027)	(0.023)
Report not enough teachers to	0.024	0.040	-0.052**	-0.015	-0.032
teach the main subjects	(0.007)	(0.013)	(0.021)	(0.025)	(0.020)
Report school facilities in	0.041	0.042	0.007	0.016	0.012
poor condition	(0.008)	(0.011)	(0.026)	(0.028)	(0.023)
Satisfaction index with pre-school	0.525	0.484	-0.062	0.036	-0.011
education	(0.050)	(0.064)	(0.097)	(0.083)	(0.074)
Satisfaction index with level	0.699	0.680	-0.039	0.022	-0.007
of medical treatment	(0.018)	(0.025)	(0.047)	(0.046)	(0.040)
Report very good or somewhat	0.397	0.557	-0.223**	0.060	-0.072
good quality of health stop	(0.029)	(0.035)	(0.091)	(0.105)	(0.079)
Number of visits in last year	2.676	3.993	-1.468**	2.331*	0.579
to the rural health stop	(0.245)	(0.750)	(0.578)	(1.181)	(0.791)
Average over family of			**-0.005**	**0.075**	**0.035**
outcomes (in standard deviations)			**(0.069)**	**(0.078)**	**(0.061)**

Definitions: Column (1) reports the average for the entire sample during baseline. Column (2) reports the average in the control group in endline. Information is an explanatory variable that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention.
Notes: Columns (3) and (4) report coefficients from one regression where indicators for "information" and "capacity building" treatments enter as RHS variables, while column (5) reports a coefficient from a separate regression where a dummy for Any Treatment enters as a RHS variable. Estimated standard errors clustered at the settlement level are in parentheses. Regressions include the baseline values of the outcomes in the category as controls. * significant at 10%; ** significant at 5%; *** significant at 1%

However, if treatments affected the overall interaction between citizens and authorities, we would expect more households voicing their concerns with local authorities. Indeed, we find that in Penza (Table 8 – Panel A); the likelihood of not voicing complaints was reduced by 3.5 percentage points in "information" settlements. Therefore, people in "information" settlements within Penza are complaining more than comparison counterparts. This effect reflects positive and negative implications. On the one hand, complaining with local authorities reflects an improved freedom of expression; however, it may also suggest that people are less satisfied in areas were the "information" treatment was administered. The latter implication was not intended by the program and we will further investigate this issue by looking at satisfaction levels in subsequent sections.

We now investigate whether treatment has impacted the seriousness with which settlement administrations take citizens' opinions when solving local problems. Table 8 – Panel A suggests that the "capacity building" treatment increased the likelihood of people perceiving that local administrations are taking their problems seriously by 28.7 percentage points. Therefore, it appears that the consulting component of this treatment; significant part of which was dedicated towards developing more collegial and respectful formats of interaction between the government and the population, made a differential and positive impact. This could have arisen because citizens' bargaining power and technical quality of demands may have increased when consultation was available. Therefore, with more technical backing of demands, local authorities took these suggestions more seriously when making decisions.

Evidence has suggested that consulting impacted positively in the way that local authorities take citizens' problems into account. Therefore, it is worth to explore the impact

of the program on the mechanisms through which citizens' express their suggestions. The "capacity building" treatment included the organization of workshops between citizens and authorities aimed to produce participatory planning. Therefore, it is interesting to measure the impact of treatment on the quantity and quality of the interactions between authorities and constituencies. We first analyze the impact of treatment on the annual frequency of meetings. Table 8 - Panel A suggests that no treatment had an effect on the number of annual meetings within Penza.

However, given that one of the main objectives of these meetings is to solve settlements' problems, one measure of quality will be the extent to which these meetings are being useful to solve problems. In that way, Table 8 - Panel A shows that the likelihood of people reporting that meetings allowed solving important problems went up by 37.4 percentage points in "capacity building" settlements within Penza. Therefore, it is apparent that the consulting component of the "capacity building" intervention is very important in enhancing the quality of meetings.

It appears that the consulting component of the "capacity building" treatment has enhanced the quality of meetings to solve local problems. However, it is not clear whether citizens or authorities or both are making the final decisions. In that way, it is interesting to analyze if treatment had any effect on the bargaining power within settlements. Table 8 - Panel A displays estimated effects of the program on the likelihood that both, authorities and residents, make mutual concessions when their interests do not coincide. Estimates suggest that none of the treatments had a significant effect on this likelihood. Therefore, it appears that the bargaining power between local authorities and citizens within Penza settlements has remained unchanged.

A final issue has to do with the distribution of power between different government tiers. Decentralization empowered settlement administrations formally, but an interesting question is whether the treatments provided additional power to these administrations. With formal training and consulting it may be the case that local public plans increased in quality and, therefore, could provide more power to settlement administrations over higher instances as district administrations. Table 8 - Panel A displays estimated program effects on the proportion of people reporting that the settlement administration exercises real power over any other government tier. Estimated effects suggest that "capacity building" significantly increased the perceived power of settlement administrations by 38.6 percentage points.

The previous finding is particularly striking considering that the change in the legal distribution of power in Penza has been smaller than in other two regions, since settlement level decentralization has existed in the region since 1995. Estimates imply that decentralization was significantly enhanced by "capacity building" as compared to the power that newly decentralized governments exercise over higher government tiers. In particular, the presence of specialized consulting appears to be the most significant feature in empowering local governments in Penza. By contrast, the "information" treatment decreased the perceived power of settlement administrations by 17.7 percentage points. This implies that the provision of information alone may even worsen the position of local governments with respect to higher tiers.

In summary, evidence suggests that Penza is the region that has been impacted more visibly by the program regarding the process of public decision making. This is evidenced by the fact that few statistically significant effects of the changes in the process of public decision making have been found in the regions of Adyghea and Perm (See Panel A in

Tables 9 and 10). For Penza, however, the differential effect of the consulting component of "capacity building" has positively impacted on the quality of meetings in solving relevant problems. In addition, we find that consulting has helped newly conformed settlement level governments to gain in perception of power among their citizens compared to higher levels of government. Moreover, the summary index suggests that the "capacity building" treatment has increased by 0.236 standard deviations the aggregate level of households participation in the process of public decision making within Penza; while no significant aggregate effect is found for any other treatment or region.

Table 9: Estimated Treatment Effects - Adyghea Region

	Baseline Mean	Endline Comparison Group Mean	OLS: Impact of Treatment in Endline		
			Information	C. Building	Any Treatment
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Variables - The Process of Public Decision Making					
Made suggestions to local authorities	0.107	0.107	-0.076	0.010	-0.031
on settlement budget	(0.017)	(0.027)	(0.061)	(0.058)	(0.054)
Have not gone anywhere to voice	0.939	0.904	0.050	0.065	0.058
complaints with municipal leaders	(0.014)	(0.036)	(0.064)	(0.054)	(0.050)
Settlement administration takes	0.552	0.669	0.097	0.033	0.063
citizens' problems seriously	(0.051)	(0.072)	(0.113)	(0.135)	(0.103)
Number of yearly meetings	2.111	1.430	1.134	-0.081	0.496
between authorities and citizens	(0.229)	(0.206)	(0.853)	(0.297)	(0.506)
Municipality residents met to address	0.636	0.574	-0.100	0.038	-0.029
significant settlement issues	(0.059)	(0.100)	(0.180)	(0.213)	(0.166)
Think that meetings b/w authorities	0.447	0.545	0.054	-0.001	0.025
and citizens solve important problems	(0.055)	(0.085)	(0.175)	(0.126)	(0.124)
Willing to take part in local council's	0.825	0.769	0.108	-0.038	0.033
acivities (e.g., garbage collection)	(0.033)	(0.076)	(0.127)	(0.116)	(0.101)
If residents' and authorities' interests	0.399	0.516	-0.094	-0.052	-0.071
do not coincide both make concessions	(0.047)	(0.071)	(0.154)	(0.108)	(0.101)
Real power is held by the	0.160	0.155	-0.033	-0.086	-0.059
settlement administration	(0.037)	(0.063)	(0.113)	(0.097)	(0.095)
Average over family of			**0.100**	**-0.022**	**0.042**
outcomes (in standard deviations)			**(0.107)**	**(0.079)**	**(0.077)**
Panel B. Dependent Variables - Satisfaction with Settlement Level Services					
Satisfaction index	0.456	0.572	-0.109	-0.081	-0.097*
with roads	(0.038)	(0.048)	(0.067)	(0.069)	(0.051)
Report difficulties to get to school	0.032	0.048	-0.034	0.020	-0.010
because of lack of transportation	(0.011)	(0.020)	(0.024)	(0.039)	(0.028)
Satisfaction index with cultural	0.384	0.406	0.025	-0.089	-0.026
institutions and recreational areas	(0.044)	(0.032)	(0.071)	(0.073)	(0.056)
Satisfaction index with natural	0.487	0.545	-0.072	-0.080	-0.076
and cultural monuments	(0.049)	(0.061)	(0.088)	(0.064)	(0.065)
Satisfaction index with collection,	0.396	0.493	0.059	0.005	0.035
removal and utilization of waste	(0.048)	(0.071)	(0.095)	(0.155)	(0.096)
Think that municipality is	0.651	0.885	-0.225*	-0.133	-0.183**
clean or rather clean	(0.052)	(0.031)	(0.109)	(0.086)	(0.081)
Satisfaction index with housing	0.492	0.620	-0.068	-0.112**	-0.087*
fund	(0.044)	(0.046)	(0.064)	(0.049)	(0.049)
Satisfaction index with maintenance	0.739	0.773	-0.013	-0.151**	-0.072
of cemeteries	(0.046)	(0.044)	(0.038)	(0.058)	(0.045)
Average over family of			**-0.129**	**-0.179**	**-0.152***
outcomes (in standard deviations)			**(0.096)**	**(0.104)**	**(0.085)**
Panel C. Dependent Variables - Satisfaction with District-Level Services					
Satisfaction index with public	0.445	0.654	-0.207**	-0.114	-0.169**
transportation	(0.057)	(0.040)	(0.088)	(0.107)	(0.070)
Satisfaction index with law	0.571	0.632	-0.097	0.018	-0.048
and order	(0.046)	(0.044)	(0.058)	(0.057)	(0.049)
Satisfaction index with general	0.886	0.892	-0.108	-0.035	-0.078
education	(0.016)	(0.025)	(0.086)	(0.060)	(0.057)
Report class cancellations due	0.058	0.038	-0.012	0.076*	0.025
to the teachers' absence	(0.013)	(0.018)	(0.031)	(0.038)	(0.029)
Report not enough teachers to	0.045	0.027	-0.006	0.011	0.001
teach the main subjects	(0.014)	(0.012)	(0.019)	(0.036)	(0.023)
Report school facilities in	0.064	0.038	-0.058**	-0.046	-0.053**
poor condition	(0.018)	(0.015)	(0.025)	(0.031)	(0.022)
Satisfaction index with pre-school	0.597	0.746	-0.065	-0.174	-0.111*
education	(0.082)	(0.068)	(0.075)	(0.131)	(0.064)
Satisfaction index with level	0.662	0.665	-0.109	-0.019	-0.071
of medical treatment	(0.034)	(0.047)	(0.086)	(0.058)	(0.063)
Report very good or somewhat	0.365	0.398	0.155	-0.002	0.088
good quality of health stop	(0.049)	(0.061)	(0.098)	(0.085)	(0.078)
Number of visits in last year	2.088	1.800	1.020*	0.012	0.601
to the rural health stop	(0.525)	(0.280)	(0.547)	(0.591)	(0.442)
Average over family of			**-0.088**	**-0.120**	**-0.103**
outcomes (in standard deviations)			**(0.068)**	**(0.100)**	**(0.068)**

Definitions: Column (1) reports the average for the entire sample during baseline. Column (2) reports the average in the control group in endline. Information is an explanatory variable that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention.
Notes: Columns (3) and (4) report coefficients from one regression where indicators for "information" and "capacity building" treatments enter as RHS variables, while column (5) reports a coefficient from a separate regression where a dummy for Any Treatment enters as a RHS variable. Estimated standard errors clustered at the settlement level are in parentheses. Regressions include the baseline values of the outcomes in the category as controls. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Estimated Treatment Effects - Perm Region

	Baseline Mean	Endline Comparison Group Mean	OLS: Impact of Treatment in Endline		
			Information	C. Building	Any Treatment
	(1)	(2)	(3)	(4)	(5)
Panel A. Dependent Variables - The Process of Public Decision Making					
Made suggestions to local authorities	0.086	0.115	-0.010	0.114*	0.056
on settlement budget	(0.015)	(0.016)	(0.042)	(0.065)	(0.044)
Have not gone anywhere to voice	0.948	0.953	-0.007	0.001	-0.003
complaints with municipal leaders	(0.011)	(0.010)	(0.028)	(0.021)	(0.020)
Settlement administration takes	0.544	0.628	-0.063	0.039	-0.010
citizens' problems seriously	(0.031)	(0.043)	(0.123)	(0.083)	(0.082)
Number of yearly meetings	1.424	1.532	1.024**	-0.211	0.352
between authorities and citizens	(0.114)	(0.145)	(0.496)	(0.247)	(0.320)
Municipality residents met to address	0.463	0.541	0.009	0.090	0.054
significant settlement issues	(0.028)	(0.043)	(0.092)	(0.106)	(0.085)
Think that meetings b/w authorities	0.400	0.479	-0.056	-0.027	-0.040
and citizens solve important problems	(0.029)	(0.044)	(0.102)	(0.084)	(0.073)
Willing to take part in local council's	0.894	0.890	0.042	0.010	0.025
acivities (e.g., garbage collection)	(0.014)	(0.019)	(0.033)	(0.032)	(0.029)
If residents' and authorities' interests	0.326	0.348	-0.061	-0.020	-0.038
do not coincide both make concessions	(0.024)	(0.041)	(0.093)	(0.067)	(0.064)
Real power is held by the	0.125	0.180	-0.108*	0.018	-0.040
settlement administration	(0.025)	(0.030)	(0.060)	(0.075)	(0.058)
Average over family of			**-0.018**	**-0.022**	**-0.020**
outcomes (in standard deviations)			**(0.066)**	**(0.058)**	**(0.050)**
Panel B. Dependent Variables - Satisfaction with Settlement Level Services					
Satisfaction index	0.468	0.454	-0.015	-0.038	-0.025
with roads	(0.019)	(0.032)	(0.057)	(0.047)	(0.044)
Report difficulties to get to school	0.029	0.014	0.001	0.028	0.014
because of lack of transportation	(0.009)	(0.006)	(0.011)	(0.019)	(0.012)
Satisfaction index with cultural	0.446	0.492	0.044	-0.093*	-0.021
institutions and recreational areas	(0.018)	(0.023)	(0.036)	(0.046)	(0.039)
Satisfaction index with natural	0.456	0.462	0.001	-0.024	-0.011
and cultural monuments	(0.016)	(0.026)	(0.052)	(0.044)	(0.039)
Satisfaction index with collection,	0.434	0.430	0.023	0.028	0.025
removal and utilization of waste	(0.021)	(0.035)	(0.059)	(0.047)	(0.046)
Think that municipality is	0.660	0.606	-0.161*	-0.090*	-0.128*
clean or rather clean	(0.030)	(0.040)	(0.094)	(0.053)	(0.063)
Satisfaction index with housing	0.354	0.363	0.045	0.012	0.029
fund	(0.018)	(0.033)	(0.043)	(0.044)	(0.036)
Satisfaction index with maintenance	0.538	0.529	0.018	-0.020	0.000
of cemeteries	(0.018)	(0.028)	(0.058)	(0.036)	(0.040)
Average over family of			**0.023**	**-0.115**	**-0.046**
outcomes (in standard deviations)			**(0.095)**	**(0.075)**	**(0.073)**
Panel C. Dependent Variables - Satisfaction with District-Level Services					
Satisfaction index with public	0.521	0.526	0.150***	0.014	0.080*
transportation	(0.018)	(0.032)	(0.056)	(0.052)	(0.047)
Satisfaction index with law	0.504	0.497	0.026	0.040	0.034
and order	(0.013)	(0.020)	(0.030)	(0.028)	(0.025)
Satisfaction index with general	0.715	0.700	0.110*	0.022	0.064
education	(0.015)	(0.028)	(0.059)	(0.042)	(0.042)
Report class cancellations due	0.145	0.115	-0.070**	-0.048*	-0.058**
to the teachers' absence	(0.017)	(0.019)	(0.029)	(0.028)	(0.024)
Report not enough teachers to	0.099	0.074	-0.020	-0.052**	-0.037*
teach the main subjects	(0.015)	(0.015)	(0.025)	(0.023)	(0.020)
Report school facilities in	0.061	0.057	-0.016	-0.033*	-0.025
poor condition	(0.013)	(0.011)	(0.022)	(0.019)	(0.016)
Satisfaction index with pre-school	0.730	0.731	0.060	-0.112	-0.030
education	(0.016)	(0.023)	(0.045)	(0.074)	(0.053)
Satisfaction index with level	0.608	0.617	0.054	-0.009	0.021
of medical treatment	(0.018)	(0.018)	(0.045)	(0.036)	(0.032)
Report very good or somewhat	0.427	0.578	-0.093*	-0.059	-0.075
good quality of health stop	(0.030)	(0.033)	(0.053)	(0.078)	(0.055)
Number of visits in last year	3.118	4.450	-0.246	-0.957	-0.610
to the rural health stop	(0.337)	(0.921)	(1.485)	(1.128)	(1.242)
Average over family of			**0.120***	**-0.012**	**0.053**
outcomes (in standard deviations)			**(0.063)**	**(0.051)**	**(0.046)**

Definitions: Column (1) reports the average for the entire sample during baseline. Column (2) reports the average in the control group in endline. Information is an explanatory variable that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention.
Notes: Columns (3) and (4) report coefficients from one regression where indicators for "information" and "capacity building" treatments enter as RHS variables, while column (5) reports a coefficient from a separate regression where a dummy for Any Treatment enters as a RHS variable. Estimated standard errors clustered at the settlement level are in parentheses. Regressions include the baseline values of the outcomes in the category as controls. * significant at 10%; ** significant at 5%; *** significant at 1%

These findings are consistent with the theoretical model outlined above.[33] The model implied that intervention effects were expected to be stronger in places with higher ex-ante democratic experience. In that way, Penza was one of the 10 regions (out of 89) that chose to have formal self-governance at the settlement level since 1995. Also, these results imply that information and training alone do not equip governments and communities with skills to internalize processes and better implement a decentralization reform. Only interventions with on-the-ground technical assistance yield positive outcomes in local governance efficiency. Moreover, "capacity building" is only assimilated when local governments had previous experience with formal local self-governance (as in Penza). Therefore, the fact that these treatments did not yield statistically significant shifts, do not imply ineffectiveness of treatment; it implies that assistance should be provided over longer periods before results are observed.

5.2. Outcomes of Formal Public Decision Making

The previous section has shown that "capacity building" has had a significant effect on the process of public decision making within Penza. Therefore, an obvious question to ask is in what areas or public services has this increased participation impacted more. We are interested in identifying the public services that have improved more in terms of perceived satisfaction by citizens. If the improved capacity provided by training and consulting services has allowed local governments to produce more sounded public services provision plans, we would expect that the quality of this services will increase.

As mentioned above, some responsibilities were decentralized to settlement administrations; while others were kept by district administrations (see Tables 1 and 2).

[33] That is, if we look at the process of formal public decision making as a public service or as an input to public services delivery.

Therefore, we expect that decentralized responsibilities will be directly impacted by the intervention. However, non-decentralized responsibilities may have an indirect impact arising from more capable settlement level governments. Evidence in the previous section suggested that consulting services have increased the power of settlement governments with respect to higher government tiers (in Penza). In that way, this increased power could be providing local governments with better tools when negotiating or demanding public services for their localities with district administrations. As a result, these settlements could be getting better provisions with respect to public services that are not administered locally.

To investigate the previous contentions we use 5-point scale ratings assigned by households to different public services. In this 5-point scale, "1" means the worst; while "5" represents the best condition.[34] In addition, we normalize this scale to create a satisfaction index as follows: $S = \frac{R-1}{4}$. Were *S* is the satisfaction index and *R* is the self assigned rating. Therefore, *S* ranges from 0 (the worst) to 1 (the best). In that way, we analyze the impact of the program on these indexes for two sets of public services: public services administered at the settlement level and public services administered at the district level.[35]

[34] Specifically, households were asked the following question: "How would you evaluate the condition of the public services that I am going to list in your settlement? Please, evaluate on a five-point scale where "1" means the worst; while "5" the best condition". In this question, the evaluated services included the following: housing fund, communal services, public transportation, roads, law and order, problems of youth, cultural institutions and recreational areas, natural and cultural monuments, collection of waste, availability (access to…) general education, availability (access to…) pre-school education, level of medical treatment, maintenance of cemeteries.

[35] Notice that the theoretical model predictions had the output of the public service as the outcome variable (i.e. P_i^d). However, we don't have a direct measure of output; so we analyze the impact on perceived satisfaction assuming that satisfaction is increasing in the level of output.

5.2.1. Public Services Administered at the Settlement Level

- **Penza**

Table 8 – Panel B displays estimated impacts for public satisfaction with respect to settlements level services. Evidence suggests that perceived satisfaction with cultural monuments and cemeteries was not impacted by any treatment. By contrast, we find that the program significantly impacted on the perceived quality of roads. Estimates suggest that "capacity building" increased the satisfaction index by 0.162 points. This constitutes a 47.5 percent increase with respect to the baseline satisfaction level for this service. In addition, "information" settlements also increased their perceived satisfaction by 0.157 points (a 46 percent increase with respect to the baseline level). Therefore, both treatments positively impacted on satisfaction in almost the same magnitude for this service.

We next consider the proportion of people reporting that it is difficult to get their child's to school because of the lack of transportation. We take this measure as a proxy for the quality of intra-settlement public transportation. Estimates show that "information" reduced the likelihood of reporting difficulties to get to school by three percentage points. No effect is found for "capacity building" settlements. Therefore, "information" was associated with a favorable outcome while "capacity building" had no impact.

Other locally provided services are the cultural institutions and recreational areas. Estimates suggest that "capacity building" increased perceived satisfaction by 0.119 points. This constitutes an increase of 23.8 percent with respect to baseline satisfaction. Therefore, since the "information" treatment had no impact; Penza local administrations appear to increase the quality of cultural institutions only when specialized consulting is available. It may be the case that investing in these services involves planning and organizational

procedures that are differentially accelerated with the availability of specialists. By contrast, when only information is available, local administrations are unable to engage in an efficient execution of these services and perceived quality remains unchanged.

The previous pattern is consistent across other services in this region. Satisfaction with waste management and the housing fund also increased in the "capacity building" settlements; while no effect was found in the "information" ones. Estimates show that the likelihood of citizens reporting that the settlement was clean or rather clean went up by 23.4 percentage points. Finally, satisfaction levels with the housing fund went up by 0.102 points (a 20 percent increase with respect to the baseline satisfaction).

In summary, our findings show that in four (out of seven) public services, "capacity building" increased satisfaction levels. In addition, we find that "information" increased satisfaction levels in only two services and with lower point estimates. Moreover, the summary index for this family of outcomes implies that only "capacity building" had a positive and significant impact on the overall satisfaction with respect to settlement level services. Our aggregate measure suggests that this treatment increased overall satisfaction by 0.31 standard deviations; while no aggregate effect is found for the "information" treatment. These findings suggest that long-term hands-on intensive interventions are useful in increasing public satisfaction with social services. However, the effects of information and training based intervention are weaker. In that way, it appears that public training is a long-term process and not much effect on public satisfaction is expected from short-term attempts to increase local government's efficiency.

- **Adyghea and Perm**

Estimated effects in Table 9 - Panel B show significant impacts for three services in Adyghea: waste management, the housing fund, and cemeteries. In addition, Table 10 - Panel B shows that Perm exhibited significant impacts for two services: cultural institutions and recreational areas, and waste management. However, these estimated effects are all negative; implying that treatment had a detrimental effect in public satisfaction.

In Adyghea, the likelihood of people reporting that the settlement was clean or rather clean decreased by 22.5 percentage points in "information" settlements. In addition, satisfaction levels with the housing fund dropped by 0.112 points in "capacity building" settlements (22.8 percent of baseline satisfaction); while no effect was found in "information" areas. Also, satisfaction levels with the maintenance of cemeteries dropped by 0.151 points in "capacity building" settlements (20.4 percent of baseline satisfaction). In Perm, "capacity building" settlements experienced a decline of 0.093 points in satisfaction with cultural institutions and recreational areas (20.9 percent of baseline satisfaction). In addition, the likelihood of people reporting that the settlement was clean or rather clean decreased by 9 percentage points in "capacity building" settlements and by 16 percentage points in "information" ones.

Obviously, these impacts were not intended by the program; but an intuitive argument may rationalize these results. In contrast with Penza; Adyghea and Perm launched self-governance at the settlement level by January 2006. Therefore, at the time of the intervention, formal collective interaction with elected government officials was a very new experience. In that way, the model presented above implies that program impacts should be lower in these regions. Consistently with this, we find a lower number of significant impacts.

However, the negative sign of these impacts, suggests that the public learning process may not be monotonic in external provision of training.

In this regard, we can think about the population that for the first time has an opportunity to publicly discuss the drawbacks of a system of service provision. This free exchange of opinions about services, often negative, may lead to a more free expression of dissatisfaction with these services, explaining the negative signs not as a reflection of the decrease in the quality of services, but as a manifestation of newly acquired freedom to speak one's mind. In any way, our summary index measures show that none of the treatments had a significant effect in any of these regions with respect to overall satisfaction. So the adverse effects found are specific and not general results.

Notice that the previous observation does not imply that these interventions were not useful in enhancing decentralization. This is demonstrated by the fact that the intervention and in particular the consulting component of it, significantly increased public satisfaction in Penza. However, the findings do suggest that some of the capacities could have been developed through the empirics of governance in previous years so the marginal effect of training and consultations is greater and better targeted when ex-ante self governance experience is higher. This implies that one needs to help such regions not only initially as a one shot deal, but for a number of years to get the process better adjusted and internalized.

5.2.2. Public Services Administered at the District Level

Since both interventions were implemented at the settlement level, impacts of these interventions will only influence outcomes at the district level through indirect channels. Therefore, there are many channels through which the interventions may have affected

district level services provision. These could be the power of local authorities, the level of households' participation or the quantity and quality of meetings between citizens and authorities. These channels may have affected district level delivery by empowering local authorities to bargain with district level officials for better services in their settlements. While household's participation in meetings may have provided local authorities with better knowledge of what are the priorities of their constituencies.

Accordingly, we investigate treatment effects on satisfaction levels with district level services. Panel C of Tables 8, 9 and 10 presents estimated effects for Penza, Adyghea and Perm respectively. First, our summary indexes suggest that none of the treatments had a significant effect on overall satisfaction in neither of the regions. Therefore, it appears that the intervention did not have a general effect with respect to district level services. However, some individual services were affected by the intervention.

In Penza (Table 8 – Panel C), the "information" treatment decreased the satisfaction index with general education by 0.075 points (a 9 percent decrease with respect to baseline satisfaction), decreased the likelihood of reporting good quality of the local health stop by 22.3 percentage points, and decreased the number of yearly visits to the health stop by 1.47. These negative impacts may suggest that while local authorities were providing additional effort in understanding unguided provision of information; they were unable to pay attention to public satisfaction with services beyond their control. By contrast, the "capacity building" intervention had neither negative nor positive individual effects. We only observe a marginally significant increase in yearly visits to the health stop of 2.33. In that way, while no aggregate increment in district level service delivery was observed, it appears that the

consulting component helped local authorities to keep an eye on services that were not their direct responsibility.

In Adyghea (Table 9 – Panel C), we find mixed individual point estimates for the "information" treatment. On the one hand, the satisfaction index with public transportation decreased by 0.21 points (47 percent with respect to baseline satisfaction). On the other hand, the likelihood of reporting schools in poor condition decreased by 6 percentage points, and the number of yearly visits to the local health stop increased by one. By contrast, the "capacity building" treatment had no individual impacts on satisfaction.

Finally, in Perm (Table 10 – Panel C), we find positive effects on some individual outcomes for both treatments. The "information" intervention increased the satisfaction indexes with public transportation by 0.15 points and with general education by 0.11 points (a 29 and 15.4 percent increase with respect to baseline levels respectively). The "capacity building" treatment reduced the likelihood of reporting absence of teachers by 4.8 percentage points, of reporting not enough teachers by 5.2 percentage points, and of reporting school facilities in poor condition by 3.3 percentage points.

In summary, we find no aggregate effects of the intervention on district level services. However, some mixed individual effects are found for the "information" treatment. We found negative impacts in Penza, mixed impacts in Adyghea, and positive impacts in Perm. By contrast, the "capacity building" had no individual impacts in either Penza or Adyghea, and positive impacts on education related categories in Perm. Altogether, these estimates imply null individual impacts of the "capacity building" treatment when the "information" treatment had negative or mixed impacts (Penza and Adyghea); while positive individual impacts of "capacity building" when "information" also had positive impacts (Perm).

In that way, we conclude that the intervention had no significant aggregate impacts on non-decentralized services, but that the "capacity building" intervention showed some either null or positive individual effects; while "information" showed mixed (negative and positive) results. Therefore, it appears that even though neither of the treatments showed a general effect on the family of outcomes; the "capacity building" intervention did not have individual adverse effects, while the "information" one had some unintended effects.

5.3. Heterogeneous Treatment Effects with respect to Pre-Treatment Accountability

So far, we have identified that only the "capacity building" treatment in Penza had an aggregate positive and significant effect on public satisfaction with settlement level services. This finding was consistent with the model presented above because Penza, at the time of the intervention, had ten more years of local governance experience with respect to the other regions. Therefore, under the assumption that governance experience is a component of local efficiency, the intervention was expected to have stronger impacts in Penza.

However, another component that may affect local efficiency has to do with the level of accountability between citizens and local authorities. In that way, if accountability is a component of local efficiency in providing public services, we would expect to see stronger impacts of the interventions in settlements with higher levels of accountability at baseline.

To investigate the previous implications, we exploit the baseline surveys administered to households and local authorities in order to build a settlement-level accountability index. We first rank within settlement households priorities for public services using their responses to the following question: "Imagine that you were allocating budget. What issues would you spend the money on in the first instance? (maximum of three answers possible)". This

question had 17 listed services from which to choose.[36] In that way, we denote by H_{js} the households' rank order of service *j* within settlement *s*.

Similarly, local public officials were asked the following question: "Imagine that the municipal budget receives an additional 10 million rubles. What amount of these 10 millions would you allocate to addressing the following activities?".[37] The response options for this question were exactly the same as the households' question. We aggregate the responses to these questions at the settlement level and rank services according to the amount of money intended to be invested in each service. In that way, we denote by P_{js} the public officials' rank order of service *j* within settlement *s*. We then use both ranks to construct an accountability index for each settlement as follows:

$$A_s = -\sum_{j=1}^{j=17} \left(H_{js} - P_{js} \right)^2 \tag{7}$$

This index measures the distance between citizens and local authorities' priorities with respect to public services provision. We then rank settlements within regions according to our accountability index, A_s, and classify settlements above and below the 50th percentile of each region. Therefore, if accountability is a component of local efficiency in public service provision, the intervention should have stronger effects in settlements above the 50th percentile of the accountability index.

[36] These services are: development of non-agricultural small business; support to the former collective farms; support to private farmers, support to individual small-scale farming; condition of the housing fund; condition of communal services; public transportation; roads; law and order; problems of youth; condition of cultural institutions and recreational areas; preservation of natural and cultural monuments; collection, removal and utilization of waste; availability (access to…) general education; availability (access to…) pre-school education; level (e.g. quality) of medical treatment; and maintenance of cemeteries.

[37] Notice that there is a slight mismatch between the public official's and the household's question. Specifically, the public official's question asks about marginal spending, and the household's question does not explicitly ask about marginal spending (the word additional is not used in the Household Survey). The reason for this discrepancy is that pretests revealed that households often misunderstood the question when phrased in marginal terms and it was reformulated for cognitive/comprehension reasons.

To test the previous contention, we run the following regression model using the summary indexes of our three families of outcomes:[38]

$$Z_{is,2007} = \theta + \lambda^{T1} * T1_s + \beta^{T1} * T1_s * Top_s + \lambda^{T2} * T2_s + \beta^{T2} * T2_s * Top_s + \omega * Z_{is,2006} + \mu_{is,2007} \quad (8)$$

where *i* indexes households, *s* indexes settlement, *T1* and *T2* are the treatment indicators, and $Z_{is,2006}$ is the baseline value of the summary index. Top_s is an indicator which takes the value of 1 if settlement *s* is in the top half of its region with respect to the pre-treatment accountability index and 0 otherwise.[39]

In this model, λ^{T1} and λ^{T2} provide estimated program effects for settlements in the bottom half of the accountability index. While β^{T1} and β^{T2} provide estimates of the additional effect that the treatments had on settlements with high pre-treatment accountability with respect to settlements in the bottom half. In this setting, theory suggests that the latter estimates should be positive and significant with respect to satisfaction with public services.

Table 11 – Panel A presents estimates of model (8) pooling all of the regions together. Consistent with theory, estimates show that the "information" treatment had a higher overall impact in settlements with high accountability. The summary index reveals that these settlements increased overall satisfaction with settlement level services by 0.23 standard deviations more than settlements at the bottom half of accountability. Similarly, the "capacity building" treatment had an additional overall impact of 0.179 standard deviations in settlements situated at the top half of pre-treatment accountability regarding satisfaction

[38] We also run the analogous regression for each component of the different families. These estimates are available upon request.

[39] While treatment assignment was not stratified with respect to the pre-treatment accountability index; the aggregate sample is fairly balanced within both halves of the accountability index. Of all households residing in settlements at the top-half of the accountability index, 52.6 percent were in control settlements, 21.4 percent were in "capacity building" settlements and 26 percent were in "information" settlements. For households in bottom-half accountability settlements, the figures were 69, 16 and 15 percent respectively.

with district level services. Finally, no significant impacts are found with respect to households' participation in the process of public decision making.

Table 11: Heterogeneous Treatment Effects with respect to Pre-Treatment Accountability Index

	OLS: Impact of Treatment in Endline			
	Information	Information x High Accountability	C. Building	C. Building x High Accountability
	(1)	(2)	(3)	(4)
Panel A. Dependent Variables - Summary Indexes for All Regions				
The Process of Public Decision Making	-0.076	0.051	0.071	-0.024
	(0.070)	(0.096)	(0.098)	(0.129)
Satisfaction with Settlement Level Services	-0.098	0.232*	-0.146	0.276
	(0.106)	(0.130)	(0.157)	(0.173)
Satisfaction with District-Level Services	-0.019	0.082	-0.112	0.179*
	(0.097)	(0.109)	(0.082)	(0.105)
Panel B. Dependent Variables - Summary Indexes for Penza Region				
The Process of Public Decision Making	-0.122	-0.006	0.442***	-0.369**
	(0.111)	(0.140)	(0.061)	(0.167)
Satisfaction with Settlement Level Services	-0.109	0.344*	0.438*	-0.214
	(0.136)	(0.171)	(0.219)	(0.246)
Satisfaction with District-Level Services	-0.104	0.172	0.110	-0.060
	(0.142)	(0.159)	(0.122)	(0.168)
Panel C. Dependent Variables - Summary Indexes for Adyghea Region				
The Process of Public Decision Making	0.179*	-	-0.097	0.153
	(0.086)		(0.097)	(0.098)
Satisfaction with Settlement Level Services	-0.055	-	-0.533**	0.575**
	(0.146)		(0.218)	(0.232)
Satisfaction with District-Level Services	-0.073	-	-0.289***	0.389**
	(0.143)		(0.082)	(0.132)
Panel D. Dependent Variables - Summary Indexes for Perm Region				
The Process of Public Decision Making	-0.042	0.086	-0.094	0.139
	(0.084)	(0.126)	(0.067)	(0.107)
Satisfaction with Settlement Level Services	-0.019	0.109	-0.337***	0.363***
	(0.158)	(0.199)	(0.083)	(0.125)
Satisfaction with District-Level Services	0.090	0.057	-0.125	0.192*
	(0.110)	(0.128)	(0.086)	(0.110)

Definitions: Information is an explanatory variable that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention. High Accountability refers to settlements that performed in the top-half of their region with respect to the pre-treatment Accountability Index described in the text.
Notes: Columns (1) to (4) report coefficients from one regression where indicators for "information", "capacity building", whether the settlement performed in the top-half of its region with respect to the pre-treatment "accountability index", and their interactions enter as RHS variables. Estimated standard errors clustered at the settlement level are in parentheses. Regressions include the baseline values of the dependent variable as control. * significant at 10%; ** significant at 5%; *** significant at 1%

- **Penza**

Previously, we found that "capacity building" had an overall positive effect on satisfaction with settlement level services only in the Penza region. While this finding was consistent with theory in the way that Penza had higher pre-treatment democratic experience; it is interesting to assess if the treatments had within region heterogeneous effects with

respect to pre-treatment accountability. This analysis will allow to test if settlements with relatively higher accountability were differentially impacted by the intervention.

Table 11 – Panel B presents estimates of model (8) for Penza. Estimates suggest that households' participation in the process of public decision making increased by 0.442 standard deviations in "capacity building" settlements situated at the bottom half of the accountability index. By contrast, households' participation increased by 0.369 standard deviations less in high accountable settlements due to the "capacity building" treatment. This finding reveals that this intervention had a stronger effect in engaging leaders and constituencies in places where they needed the most. The "information" treatment, however, had no impact at all in households' participation.

However, while no impact of the "information" was found in the process of public decision making; estimates suggest that this treatment increased overall satisfaction with settlement level services by 0.344 standard deviations more in settlements at the top half of pre-treatment accountability. This is interesting because our previous analysis suggested no average impact of the "information" treatment. However, when heterogeneous effects with respect to ex-ante accountability are considered, more accountable settlements exhibit a positive impact. Therefore, this treatment was beneficial only when ex-ante democratic experience and accountability was high.

By contrast, the interaction between the "capacity building" treatment and the high accountability indicator is not significant. This implies that both high and low accountable settlements are equally benefited with this treatment when ex-ante democratic experience is high. Finally, estimated impacts for satisfaction with district level services are all insignificant.

- **Adyghea and Perm**

In the previous analysis, we found no significant impacts at all with respect to our summary indexes in either Adyghea or Perm. This was consistent with theory because neither of these regions had previous local democratic experience at the onset of the intervention. However, under the assumption that accountability constitutes a component of local public efficiency, we would expect to observe more favorable effects in settlements situated at the top of pre-treatment accountability.

Consistent with these expectations, estimated impacts for Adyghea and Perm shown in Panels C and D of Table 11, tell us a similar story. First, no effect at all is found for the "information" treatment.[40] Therefore, while this treatment increased satisfaction with settlement level services in high accountability places with high local democratic experience (Penza); it did not have impact when local democratic experience was low (Adyghea and Perm). This finding suggests that, when resources available for programs aimed to increase local public efficiency are scarce, it may be optimal to administer the "information" treatment only in places with higher local public experience and accountability.

Second, we find that the "capacity building" treatment had an adverse effect on satisfaction with both settlement and district level services when pre-treatment accountability was low. Estimates for Adyghea (Perm) suggests that overall satisfaction with settlement services decreased by 0.533 (0.337) standard deviations in low accountability settlements. Similarly, satisfaction with district services decreased by 0.289 (0.125) standard deviations in Adyghea (Perm) within low accountability areas.

[40] In Adyghea, estimates for the interaction term were not calculated because no household receiving the "information" treatment was situated at the bottom half of the pre-treatment accountability index.

By contrast, this treatment had an additional positive effect in areas situated in the top half of the pre-treatment accountability index. For instance, estimates on the interaction term between "capacity building" treatment and the indicator for high accountability imply an additional positive effect in settlement services satisfaction of 0.575 (0.363) standard deviations in Adyghea (Perm). Similarly, estimates imply an additional positive effect of "capacity building" in high accountability settlements with respect to district services satisfaction equivalent to 0.389 (0.192) standard deviations in Adyghea (Perm).

Therefore, while no average impacts were found for the "capacity building" intervention; when heterogeneous effects with respect to ex-ante accountability are introduced, this treatment appeared to benefit areas with high pre-treatment accountability. Moreover, this treatment was found to impact in negative way settlements at the bottom half of pre-treatment accountability. These findings highlight the importance of considering the pre-treatment heterogeneity in relevant variables when conducting experimental evaluations.

Our evidence suggests that intensive treatments aimed to increase local efficiency, as the "capacity building" one, may not be optimal when both local democratic experience and accountability is low. In such cases, the best thing to do is to direct efforts towards engaging and matching preferences between local leaders and their constituencies before trying to increase leaders' technical capabilities. However, when accountability is high, the "capacity building" treatment provides positive results regardless of the pre-treatment level of local democratic experience.

To sum up, our evidence suggests that when both local democratic experience and accountability are high, the "information" treatment is the most cost-effective intervention. When local democratic experience in high, the "capacity building" treatment provides

positive results regardless of the accountability level. When both local democratic experience and accountability are low, the "capacity building" intervention provides negative results; therefore, in these cases, is recommended to explore alternative mechanisms aimed to increase accountability levels. Finally, when local democratic experience is low but accountability is high, the "capacity building" intervention provides positive results.

5.4. Estimated Impacts and Citizens' Priorities

- **Penza**

We now turn to ask if our estimated impacts had any relation with citizens' priorities for public service delivery. We identify these priorities exploiting answers obtained from the following question asked at baseline: "Imagine that you were allocating budget. What issues would you spend the money on in the first instance? (maximum of three answers possible)". Table 12 summarizes the aggregate percentage of answers ordering services from the highest to the lowest priority. The last column of the table shows which services were impacted by the treatments (T2 stands for "capacity building" and T1 stands for "information"). Immediate observation clearly shows that all of the positive impacts are heavily concentrated in the highest priority services. In addition, the two settlement level services with the highest priorities (Roads and Housing Fund) are the only ones that have been positively impacted by both treatments.

Table 12: Summary of Estimated Impacts and Citizens' Priorities - Penza Region

Public Service	Percentage of HHs	Administered by	Significant Impacts?
Roads	32.16	Settlement	T2 (+) / T1 (+)
Medical treatment	18.00	District	T2 (+) / T1 (-)
Pre-school education	12.18	District	No
Improvement of the condition of the housing fund	8.01	Settlement	T2 (+) / T1 (+)
Collection, removal and utilization of waste	7.35	Settlement	T2 (+)
Condition of cultural institutions and recreational areas	5.71	Settlement	T2 (+)
General education	5.60	District	T1 (-)
Public transportation	5.49	District	No
Law and order	2.63	District	No
Maintenance of cemeteries	1.76	Settlement	No
Preservation of natural and cultural monuments	1.10	Settlement	No

Note: T1 stands for the "Information" treatment; while T2 stands for the "Capacity Building" treatment.

Previous observations show that the positive impacts of the program were not just isolated observations without connection with what citizens prefer. Evidence presented in this section clearly suggests that authorities are concentrating their additional public capabilities in satisfying what the majority of people demand. Therefore, it appears that the program and especially the "capacity building" intervention has been useful to enhance preference matching between authorities and their constituencies. This implies that long-term processes of capacity building not only increases people satisfaction, but also increases it in what people consider important.

- **Adyghea and Perm**

As shown before, aggregate significant impacts in these regions are inexistent. Moreover, most of the few individual significant effects are negative. However, it is also interesting to look if these few negative effects are related with citizens' priorities. Tables 13 and 14 show results for Adyghea and Perm respectively. We observe that these negative effects are situated neither at the top nor at the bottom of the priorities. Therefore, no clear pattern between treatment effects and citizens' priorities is found in these regions.

Table 13: Summary of Estimated Impacts and Citizens' Priorities - Adyghea Region

Public Service	Percentage of HHs	Administered by	Significant Impacts?
Medical treatment	22.93	District	No
Roads	20.53	Settlement	No
Improvement of the condition of the housing fund	13.60	Settlement	T2 (-)
Public transportation	9.60	District	T1 (-)
Pre-school education	7.73	District	No
Collection, removal and utilization of waste	7.47	Settlement	T1 (-)
Condition of cultural institutions and recreational areas	7.20	Settlement	No
General education	4.80	District	No
Law and order	3.73	District	No
Maintenance of cemeteries	1.87	Settlement	T2 (-)
Preservation of natural and cultural monuments	0.53	Settlement	No

Note: T1 stands for the "Information" treatment; while T2 stands for the "Capacity Building" treatment.

Table 14: Summary of Estimated Impacts and Citizens' Priorities - Perm Region

Public Service	Percentage of HHs	Administered by	Significant Impacts?
Roads	23.66	Settlement	No
Improvement of the condition of the housing fund	21.77	Settlement	No
Medical treatment	21.29	District	No
General education	7.26	District	T2 (+) / T1 (+)
Public transportation	5.84	District	T1 (+)
Condition of cultural institutions and recreational areas	5.21	Settlement	T2 (-)
Law and order	5.05	District	No
Collection, removal and utilization of waste	4.26	Settlement	T2 (-) / T1 (-)
Pre-school education	2.68	District	No
Maintenance of cemeteries	1.89	Settlement	No
Preservation of natural and cultural monuments	1.10	Settlement	No

Note: T1 stands for the "Information" treatment; while T2 stands for the "Capacity Building" treatment.

6) Identifying the Mechanisms behind the Results

6.1. Was Treatment Assignment Revenue Neutral?

Some concern arises from the possibility that treatments might have been endogenously assigned to settlements receiving higher revenues. In that case, the estimated impacts on satisfaction may not be due to an enhanced efficiency obtained through the treatments, but would be a simple consequence of investing more resources. Furthermore, even if treatment was not assigned in such way, it may be the case that the central or regional governments wanted this project to succeed and, therefore, transfer more funds to treated settlements after treatment assignment. In such cases, our estimates will not be reflecting the effect of an enhanced public efficiency on satisfaction. Therefore, it becomes important to test if these potential scenarios actually happened. To do this, we use fiscal data containing

settlement level revenues, intergovernmental grants, and expenditures for years 2006 and 2007.

First, to investigate whether treatment assignment was not related to baseline level of revenues or expenditures, we regress the fiscal measure of interest (in natural logs) observed in year 2006 on indicators for treatment assignment. We look at total revenues, own revenues, intergovernmental grants, total expenditures, and expenditures in public services (all expressed in real percapita terms).[41] Results for Adyghea, Penza, and Perm are displayed in columns 1 and 2 of Table 15. Estimates are statistically indistinguishable from zero. This implies that treatment assignment was indeed unrelated to the baseline level of revenues and/or expenditures.

[41] Own revenues refer to the taxes assigned directly to settlements. As detailed in Table 3, these include the totality of the land tax and personal property tax. Fiscal measures were expressed in 2006 rubles using the regional consumer price indexes as deflators.

Table 15: Revenues and Expenditures Trends

	Baseline Data (2006)			Pooling Data for 2006 and 2007		
	Information	C. Building	Any Treatment	Information x Post	C. Building x Post	Any Treatment x Post
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Dependent Variables - Adyghea Region						
Revenue percapita	0.255	0.076	0.178	-0.137	0.108	-0.032
	(0.422)	(0.339)	(0.320)	(0.157)	(0.136)	(0.145)
Own revenue percapita	-0.263	-0.219	-0.388	-0.300	-0.642	-0.383*
	(0.180)	(0.307)	(0.382)	(0.330)	(0.484)	(0.179)
Intergovernmental grants	-0.335	0.226	-0.094	-0.267	0.949	0.254
percapita	(0.245)	(0.204)	(0.198)	(0.143)	(0.896)	(0.458)
Expenditure percapita	-0.296	-0.663	0.074	-0.207**	0.044	-0.099
	(0.369)	(0.411)	(0.239)	(0.075)	(0.082)	(0.096)
Expenditure percapita in	-1.120*	-0.926	0.180	0.173	0.153	0.175
public services	(0.460)	(0.482)	(0.234)	(0.290)	(0.203)	(0.194)
Panel B. Dependent Variables - Penza Region						
Revenue percapita	-0.205	0.154	-0.026	0.008	-0.402***	-0.197
	(0.122)	(0.216)	(0.104)	(0.057)	(0.082)	(0.133)
Own revenue percapita	0.253	0.222	0.145	0.566	-0.368	0.127
	(0.147)	(0.365)	(0.291)	(0.482)	(0.456)	(0.437)
Intergovernmental grants	-0.072	0.254	0.091	0.132	-0.287*	-0.077
percapita	(0.185)	(0.227)	(0.155)	(0.116)	(0.145)	(0.177)
Expenditure percapita	-0.055	0.370	-0.010	0.022	-0.361***	-0.170
	(0.361)	(0.374)	(0.123)	(0.060)	(0.095)	(0.129)
Expenditure percapita in	0.370	0.423	-0.093	0.005	0.121	0.063
public services	(0.419)	(0.497)	(0.112)	(0.067)	(0.096)	(0.046)
Panel C. Dependent Variables - Perm Region						
Revenue percapita	0.090	0.249	0.170*	0.069*	0.025	0.047
	(0.110)	(0.176)	(0.088)	(0.032)	(0.046)	(0.032)
Own revenue percapita	0.074	0.047	0.287	0.248	-0.395	-0.073
	(0.188)	(0.337)	(0.229)	(0.306)	(0.467)	(0.269)
Intergovernmental grants	-0.280	0.199*	-0.041	-0.136	0.167	0.016
percapita	(0.377)	(0.099)	(0.190)	(0.198)	(0.113)	(0.128)
Expenditure percapita	0.509	0.064	0.130	0.035	0.009	0.022
	(0.312)	(0.547)	(0.110)	(0.054)	(0.031)	(0.036)
Expenditure percapita in	0.487*	-0.144	0.118	-0.092	0.142*	0.025
public services	(0.219)	(0.661)	(0.147)	(0.113)	(0.073)	(0.078)

Definitions: Information is an explanatory variable that refers to whether the settlement received the "information" intervention. Likewise, C. Building refers to the "capacity building" intervention. Dependent variables expressed in natural logs of thousands of rubles.

Notes: Columns (1) and (2) report coefficients from one regression where indicators for "information" and "capacity building" treatments enter as RHS variables, while column (3) reports a coefficient from a separate regression where a dummy for Any Treatment enters as a RHS variable. Regressions in columns (1) - (3) are estimated using 2006 budget information only. Columns (4) and (5) report coefficients from diff-in-diff regressions performed by pooling 2006 and 2007 budget data where the indicator "post" takes the value of one for 2007 observations. Column (6) reports a coefficient from a separate diff-in-diff regression where both treatments are pooled in the Any Treatment variable. Estimated standard errors clustered at the district level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Second, to explore if revenues or expenditures trended differentially in treated settlements after treatment assignment, we run the following model using fiscal data for years 2006 and 2007:

$$O_{sdt} = \alpha + \beta_1 * T1_s + \beta_2 * T2_s + \beta_2 * Post + \lambda_1 * T1_s * Post + \lambda_2 * T2_s * Post + \varepsilon_{sdt} \qquad (9)$$

where O_{sdt} is the fiscal measure of interest for settlement *s*, in district *d*, at time *t*. *T1* is an indicator for "information" settlement. *T2* is an indicator for "capacity building" settlement. *Post* is a dummy which takes the value of 1 for observations in year 2007 and zero otherwise. Finally, ε_{sdt} is a disturbance term which we allow to be correlated across time and settlements within districts.[42] In this model, estimates of λ_1 and λ_2 capture the effect of the treatments on the fiscal measure.

Therefore, if revenue and/or expenditures were not impacted by the treatments, we expect these estimates to be insignificant. Columns 4 and 5 of Tables 15 display estimates of λ_1 and λ_2. Panel A suggests that Adyghea did not exhibit any differential trend with respect to revenues. However, we find a significant effect in expenditures suggesting that "information" settlements reduced their overall expenditures by 20.7 percent with respect to control ones between 2006 and 2007. Panel B suggests that "capacity building" settlements in Penza received 40.2 percent lower revenues and 28.7 percent lower intergovernmental grants with respect to control settlements. In addition, these settlements also reduced their overall expenditures by 43.6 percent. Finally, Panel C suggests a marginally significant increase of 6.9 percent in revenues for "information" settlements within Perm.

In summary, we find that in Adyghea and Perm, the flow of revenues was not affected by treatment status across time. However, "capacity building" settlements within Penza observed an economically and statistically reduction in their revenues and expenditures over time. Nonetheless, the expenditures devoted to public services were not affected by treatment status in any Region. Therefore, satisfaction findings of the previous section were not driven by an increase in expenditures devoted towards the provision of

[42] We cluster estimated standard errors at the district level in all of our regressions.

public goods. Furthermore, while "capacity building" settlements in Penza saw a major reduction in their revenues, they did not reduced the expenditures in public services, implying that this revenue shortage was reflected in other type of expenditures.

6.2. Was Increased Efficiency the Mechanism Driving our Results?

A final question relates to the mechanisms through which the program impacted the outcomes of interest. As shown in the previous section, the program was not related to pre-treatment levels of revenues and/or expenditures. It was also shown that the program did not impact the level of expenditures devoted to the provision of local public services through time. Finally, in the region where we found the highest impact of the program (i.e. Penza), treated areas received relatively *lower* levels of revenues with respect to control ones. Therefore, it appears that the positive impacts found were not related to an increase in revenues and/or expenditures among treated settlements.

Accordingly, it appears that an increased level of local efficiency in treated areas was the mechanism driving the estimated program impacts. In that way, to provide further evidence that this was the actual mechanism driving our results, we estimate the following regression model using the summary indexes of our three families of outcomes:

$$Z_{is,2007} = \theta + \lambda * Exp_{s,2007} + \beta^{T1} * T1_s * Exp_{2007} + \beta^{T2} * T2_s * Exp_{s,2007} + \omega * Z_{is,2006} + \mu_{is,2007} \quad (10)$$

where *i* indexes households, *s* indexes settlement, *T1* and *T2* are the treatment indicators, and $Z_{is,2006}$ is the baseline value of the summary index.[43] $Exp_{s,2007}$ represents total expenditures percapita in the provision of settlement-level services in settlement *s* during 2007.[44]

[43] Estimated standard errors were clustered at the settlement level.

In model (10), we estimate a relationship between expenditures and outcomes of public services provision. For example, estimates of λ provide the number of additional standard deviations in the satisfaction index achieved by increasing the expenditures in public services by one thousand Rubles percapita in control settlements. We take this relationship as a proxy for public efficiency. In that way, estimates of β^{T1} and β^{T2} provide us a measure of how more efficient were settlements that received the "information" or "capacity building" treatment with respect to control settlements at endline.[45] Therefore, if the program affected the efficiency of treated settlements, we would expect estimates for either β^{T1} or β^{T2} to be positive and significant.

Our previous results suggested that only the "capacity building" treatment in Penza affected the overall participation of households in the process of public decision making and their satisfaction with settlement level services. Therefore, if these results were driven by increases in efficiency, we would expect positive and significant estimates of β^{T2} only for these two aggregate indexes in Penza; while insignificant coefficients for the rest of indexes and regions.

Table 16 displays estimates of λ, β^{T1} and β^{T2} for the three families of outcomes and the three regions in the study. Consistent with our expectations, estimates of β^{T2} (Panel A – Column 3) are only positive and significant for the process of public decision making and satisfaction with settlement level services in Penza. Our estimates imply that an extra one thousand Rubles in expenditures percapita are associated with 0.293 additional standard

[44] These expenditures are expressed in thousands of Rubles. We also estimated the model using total expenditures, the natural log of total expenditures, and the natural log of expenditures percapita as dependent variables. All of the results were qualitatively the same and are available upon request.
[45] As a check for baseline efficiency differences, we estimated a model analogous to (9) using the summary indexes at baseline as dependent variables and expenditures in 2006 as regressors. Estimated parameters of interest from this model were statistically indistinguishable from zero and are available upon request.

deviations in the process of public decision making index among "capacity building" settlements relative to control ones. Similarly, an additional positive impact of 0.371 standard deviations on the satisfaction index with settlement level services is found in "capacity building" areas with respect to control ones.

Table 16: Expenditures in Public Services and Satisfaction

	OLS: Impact of Treatment in Endline		
	Expenditures in Public Services (1)	Expenditures x Information (2)	Expenditures x C. Building (3)
Panel A. Dependent Variables - Summary Indexes for Penza Region			
The Process of Public Decision Making	0.049	-0.202**	0.293**
	(0.119)	(0.090)	(0.115)
Satisfaction with Settlement Level Services	-0.281*	0.055	0.371***
	(0.142)	(0.152)	(0.118)
Satisfaction with District-Level Services	-0.218**	-0.079	0.089
	(0.104)	(0.080)	(0.095)
Panel B. Dependent Variables - Summary Indexes for Adyghea Region			
The Process of Public Decision Making	0.103	-0.013	0.047
	(0.182)	(0.207)	(0.187)
Satisfaction with Settlement Level Services	-0.127	-0.126	-0.352
	(0.205)	(0.142)	(0.306)
Satisfaction with District-Level Services	-0.107	-0.073	-0.130
	(0.177)	(0.122)	(0.292)
Panel C. Dependent Variables - Summary Indexes for Perm Region			
The Process of Public Decision Making	-0.031	-0.009	0.016
	(0.055)	(0.045)	(0.042)
Satisfaction with Settlement Level Services	-0.054	-0.035	-0.030
	(0.060)	(0.054)	(0.049)
Satisfaction with District-Level Services	-0.050	0.041	0.004
	(0.043)	(0.042)	(0.035)

Definitions: Information is an explanatory variable that refers to whether the household resides in a settlement in which the "information" intervention ocurred. Likewise, C. Building refers to the "capacity building" intervention. Expenditures in Public Services refers to the settlement-level total expenditures percapita (in thousands of Rubles) devoted to the provision of decentralized plublic services (i.e. settlement level services).
Notes: Columns (1) to (3) report coefficients from one regression where indicators for "information" and "capacity building" interacted with the total expenditures percapita invested in public services enter as RHS variables. Estimated standard errors clustered at the settlement level are in parentheses. Regressions include the baseline values of the dependent variable as control. * significant at 10%; ** significant at 5%; *** significant at 1%

Therefore, our estimates confirm that the mechanisms behind our results were indeed related to an increased efficiency of settlement governments rather than an increased level of revenues and/or expenditures in treated areas.

7) Conclusions

The level of local governments' efficiency and the extent to which citizens participate in the processes of public decision making are likely to influence the expected benefits of the decentralization of public services. Whether local efficiency can indeed be enhanced through external interventions, and further, whether it can be effective in improving service delivery, remains an open question on which rigorous evidence is necessary. This paper contributes to the evidence and distils lessons on the elements of intervention design that are likely to be successful. Moreover, our empirical design allowed us to discern how do interventions results differ with respect to ex-ante levels of governance experience and accountability.

We find that the interventions significantly increased civic engagement, quality of public hearings, perceived power of local authorities, and satisfaction levels with public services administered at the settlement level in Penza. However, no positive effects were found in Adyghea or Perm. This is consistent with the theoretical model presented above given that Penza, at the onset of the intervention, had already more than 10 years of settlement level democratic experience. In that way, the 2006 decentralization reform, in the case of Penza, was happening in a much better prepared social and institutional space, so, the transformation was less dramatic than for the other two regions. We also find that the level of pre-treatment accountability matters in the sense that areas with relatively higher ex-ante accountability levels are differentially benefited by the program in terms of general satisfaction with decentralized public services.

With respect to which elements of the intervention design are more effective. Our findings clearly show that effects for the typical program applied to local authorities that we

denoted as "information" provided weak results. For instance, for the case of Penza, only two (out of seven) settlement level services were positively impacted by this treatment. By contrast, the long-term and intensive "capacity building" treatment significantly increased satisfaction levels in four out of the six services situated in the highest priorities of the people. Therefore, it is apparent that translation of training into public satisfaction requires a long-term process with close monitoring between external capacity providers, local authorities and citizens.

Significant results for "capacity building" in Penza, coupled with insignificant results in Adyghea and Perm reflect the fact that effects of training and consultations are greater in a case where institutional adjustments are less dramatic. In addition, it may be that positive results are observed when the participants in the process (both the population and the administration) are better aware of the realm of the possible (i.e. what could be the outcome of formal local government) and, therefore, have realistic expectations and appreciations of the outcomes.

These results are consistent with the view that some of the capacity could have been developed through the empirics of governance in previous years. Therefore, the marginal effect of training and consultations is greater and better targeted. An important implication of our findings is that one needs to help local governments not only initially as a one shot deal, but for a number of years to get the decentralization process better adjusted and internalized.

We conclude by stating that our results strongly suggest that the provision of capacities to local governments is a long-term process. Therefore, it may be the case that even intensive interventions coupled with on-the-ground specialists would not be translated into public satisfaction in the short term. This observation was predicted by the proposed

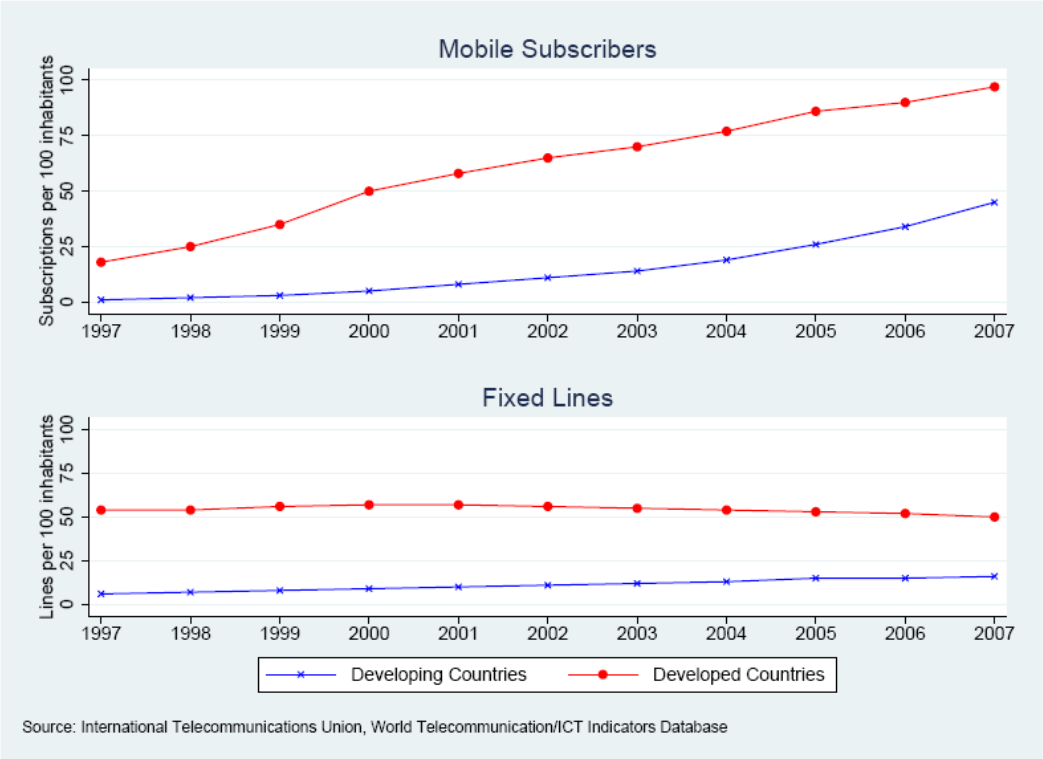
theoretical model and confirmed by the data. Therefore, failure of these exogenous interventions in increasing public satisfaction among newly decentralized settlements should not be interpreted as failure to increase local capacities. By contrast, we interpret these findings as direct evidence that newly decentralized governments are situated in the flat portion of the learning curve and that longer assistance is needed before we observe new capabilities being translated into public satisfaction.

Chapter 3: The Effects of Mobile Phone Infrastructure: Evidence from Rural Peru

1) Introduction

Over the past twenty years, there has been an explosion in the use of mobile telephones worldwide. Initially, this increase was concentrated in industrialized nations, but the remarkable recent trend has been the quick adoption of mobile telephones in the developing world. While mobile phone use was slower to take-off in developing countries (see Figure 1), with only one subscription per hundred inhabitants in 1997, this increased to over one in ten by 2000, and reached almost one in two by 2007. This quick adoption of mobile phones has been viewed with excitement by those in the development community, as there is a widespread belief that mobile phones will lead to income growth in low income countries. A recent World Bank report argues that "the development potential of the wireless platform is enormous" (Khalil et al., 2009), and The Economist asserts that "poor countries have already benefited hugely from mobile phones." (Economist, 2009)

Figure 1: Mobile phone subscriptions and fixed lines per 100 inhabitants, 1997-2007



One reason for the excitement about mobile phones as a tool for development is that low income countries have long lagged behind in infrastructure, including land based telecommunications. For example, in 1997, there were nearly ten times more fixed lines per capita in developed countries than there were in developing countries (see Figure 1). This infrastructure gap has long been identified as a possible explanation for why poor countries remain poor. One advantage of mobile telephone infrastructure is that it can be installed without ever deploying the expensive network of wires necessary for land based phones. As a result, mobile phones often allow developing countries to leapfrog a technology on which they have long lagged, and quickly adopt a better technology. As a result, if the infrastructure gap has indeed been part of the problem for income growth in developing countries, then mobile telephones may be helping these countries to close the gap. This is even more true in rural areas of developing countries, since these areas had

the least access to telecommunications technology prior to the introduction of mobile phones, and are experiencing explosive adoption rates for this new technology.

Despite the prevalent belief that mobile phones are beneficial for developing countries, the research on this topic lags far behind. The exceptions are Jensen (2007), which examines how mobile phones impact the market for fresh fish in Kerala, India, and Aker (2010), which studies the impact of mobile phones on the market for grain in Niger. These papers provide great insight into how mobile phones transform the way that a market operates. At the same time, since both restrict their analysis to the impact of mobile phones on a particular commodity, there remains no systematic evidence of the overall impact of mobile phones on a representative sample of producers and consumers across multiple sectors. To help fill this gap, we set out to provide such evidence, estimating the impact of mobile phone infrastructure on household well-being in rural Peru between 2001 and 2007.

If cell phones improve the well-being of market participants, there is also the question of how these gains are distributed. Jensen (2007) finds the cellphones improve both producer and consumer welfare, while Aker (2010) finds that mobile phones improve trader and consumer welfare, but does not analyze the impact on producer welfare. In the developing country context, where most households are engaged in some form of home farming, it is extremely important to understand how mobile phones impact the profits of home producers before we can assess their overall impact on household well-being. Also, if the distribution of gains differs by commodity, then it is vital conduct this analysis across commodities. This paper does just that, analyzing the impact of

mobile phone coverage on the production and consumption patterns of a representative sample of rural households in Peru.

Another question that has arisen in this literature is whether spill-over benefits accrue to those who do not own phones, but live in areas that have gained mobile phone coverage. Jensen (2007) finds that producers without mobile phones do gain, although not as much as those who own mobile phones. On the consumption side, that paper does not look at heterogeneous effects for owners vs. non owners, which are possible if mobile phone owners are able to find lower prices when making purchases. To shed additional light on this topic, we examine whether there is evidence of spillover effects for those who do not own a phone, both for production and consumption side outcome variables.

In future work, we plan to extend this intuition – that mobile phones may improve household well-being, but that it is also important to understand how these gains are distributed. For example, we will examine whether the benefits are larger in areas that had no access to landlines prior to the introduction of mobile phones. We will also examine whether cell phones benefited the poorest of the poor, or if the gains were concentrated amongst those who were relatively advantaged within these communities.

The paper proceeds as follows. In Section 2, we discuss the theoretical rationale for why mobile phones may impact household well-being and review the existing literature. In Section 3, we provide information on the context of Peru, discussing the mobile phone market and the data sources that we are using to address this research question. In Section 4, we lay out our empirical strategy. In Section 5, we provide our empirical results. In Section 6, we summarize our conclusions.

2) Theory and Literature Review

Before laying out an empirical strategy for measuring the impact of mobile phone coverage on household well-being, we must first provide a rationale for why mobile phones may be welfare enhancing. Jensen (2007), in arguing that mobile phones will have an impact on markets in Kerala, India, points to one of the basic tenets of economics, the First Fundamental Theorem of Welfare Economics. This theorem demonstrates that a competitive equilibrium resulting from utility and profit maximizing agents will always be Pareto efficient. In other words, the invisible hand of the market will always produce an allocation of resources such that it is not possible to make any agent better off without making another agent worse off. It has long been recognized, however, that this result relies on critical assumptions, among which are the existence and efficient operation of markets for all commodities. If these assumptions are not satisfied, then welfare maximizing agents may produce an allocation of resources that is not Pareto efficient. In fact, the Greenwald-Stiglitz Theorem (Greenwald and Stiglitz, 1986), demonstrates that incomplete information can result in deviations from Pareto efficiency. If the introduction of mobile phones enhances the spread of information, then there is the potential to improve the functioning of markets, which would make it possible to make some agents better off without making any agents worse off.

This idea that improved information technology can enhance the functioning of markets, has gained substantial empirical support. This literature has relied on the fact that, if markets are operating efficiently, then the price difference for an identical good being sold at two markets should be no greater than the cost of transporting the commodity from one market to the other. Moreover, if the introduction of better

communications technology reduces the price differences across markets, then this provides convincing evidence that communication technology has improved market efficiency. Garbade and Silber (1978) argue that the introduction of the telegraph and the trans-Atlantic cable reduced price differentials across markets. Jensen (2007) shows that the introduction of mobile phone coverage causes a remarkable reduction in the variability in fresh fish prices, both across markets and over time, and Aker (2010) obtains similar results for grain prices in Niger. Finally, Goyal (2010) provides evidence suggesting that soy price dispersion across markets in Madhya Pradesh, India, decreases following the introduction of Internet kiosks.

While there is a clear theoretical argument for why improvements in communications technology should enhance market efficiency, it is less clear how the economic gains will be distributed across market participants. As stated in Jensen (2007), "How the net welfare gain is shared between the two groups, and whether, in fact, one group gains while the other loses in response to increased arbitrage, is a priori ambiguous." Search-theoretic models can sometimes be used to generate useful predictions, such as when only one side of the market is engaged in search and mobile phones reduce the cost of that search. Predictions, however, become more difficult when both sides of the market are searching, and when mobile phones reduce the cost of search for all agents simultaneously. Consequently, it is difficult to generate predictions regarding the impact of mobile phone infrastructure, since mobile phone coverage is rolled out to entire markets at once, providing simultaneous benefits to farmers, middlemen, and consumers. Given this, the impact of mobile phones on the wellbeing of producers, consumers, and middlemen remains an empirical question. Jensen (2007)

provides evidence that, at least for the fresh fish market in Kerala, India, both fishermen and fish consumers are better off following the introduction of mobile phones. This is because average fish prices decline, generating an increase in consumer surplus. While this same price decrease lowers fishermen profits, this effect is more than offset by an increase in the quantity sold and a simultaneous reduction in the fraction of wasted production (i.e., catch that was going unsold and being dumped back to sea). Looking at grain markets in Niger, Aker (2010) provides evidence that both traders and consumers gain. In that context, traders receive increased profits due to an increase in the sales price, and consumers are better off because the consumer price of grain decreases. The fact that consumer and trader prices move in opposite directions, however, suggest reduced profits for middlemen, and the paper is not able to measure the welfare impact for farmers.

3) Data and Context

In 1998, the cell phone market of Peru was dominated by two mobile phone networks, Nextel del Peru and Telefonica del Peru, and service was available only in Lima and a few other densely populated urban centers. By 2001, these providers had expanded their coverage to most of the urban areas of Peru, particularly along the Pan-American Highway, which runs down the coast and along which more than half of the Peruvian population resides. While coverage was quite pervasive along the coast and in other urban centers by 2001, there was little coverage in less densely populated areas, such as the Peruvian jungle and highlands. In 2000, however, a new license was sold to TIM Peru, and this network was later sold and rebranded as Claro Peru. In 2001, this new provider and Telefonica began aggressively expanding their networks into less densely

populated areas. The extent of this expansion can be seen in Figure 2 and Figure 3, which show the locations of all the mobile phone towers in 2001 and 2007, respectively. This paper exploits this dramatic change in the availability of mobile phone coverage in rural areas between 2001 and 2007 to measure the impact of mobile phone coverage on household outcomes in Peru. In order to conduct this evaluation, we rely on several data sources, which we now describe in detail.

Figure 2: Map of mobile phone towers, 2001

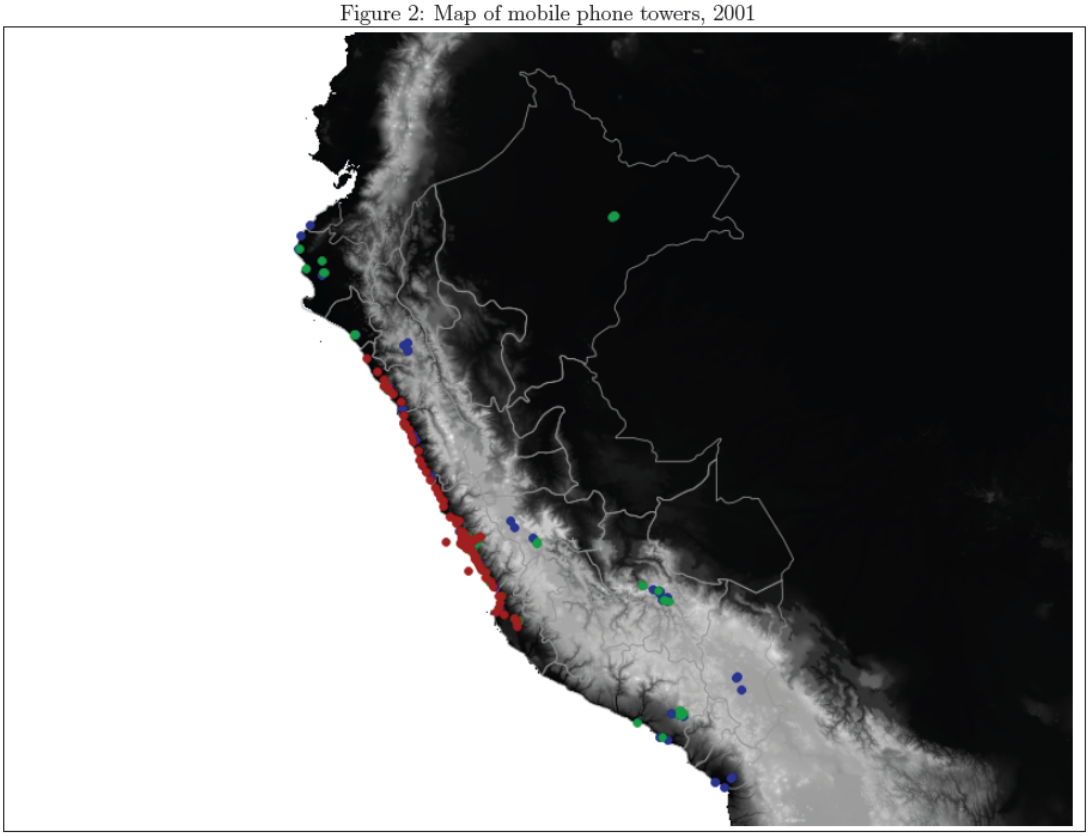
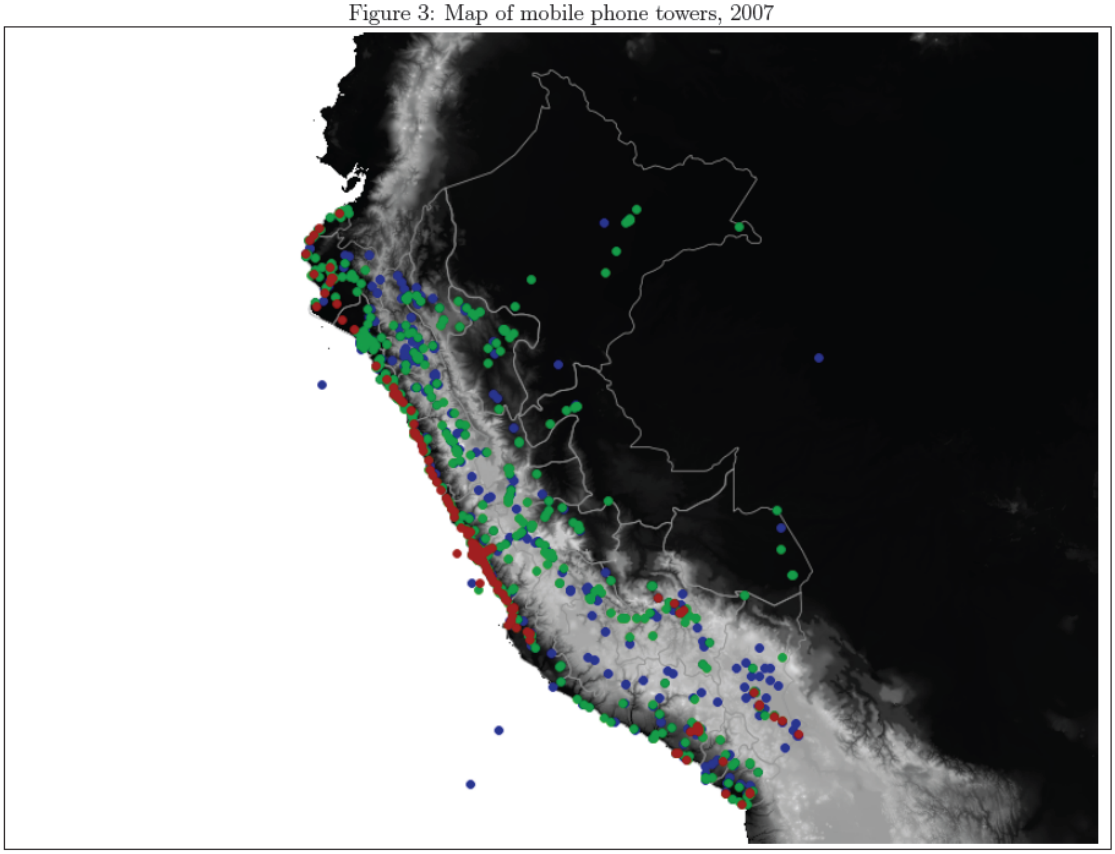


Figure 3: Map of mobile phone towers, 2007



For our household data, we use the Peruvian National Household Survey (Encuesta Nacional de Hogares, ENAHO), an annual household survey collected by the Peruvian National Institute of Statistics and Informatics (Instituto Nacional de Estadistica e Informatica, INEI). This is a comprehensive household survey, providing us with information on household demographics, labor force behavior, home production, and expenditures. This data provides us with a nationally representative repeated cross-section of Peruvian households. From this data, we select only those observations that are living in rural communities. We focus on rural areas because, by 2001, the first year that ENAHO was conducted, mobile phone coverage was already widely available in urban areas, so we do not have the variation in mobile coverage that would be necessary to identify the impact of mobile phones on urban households.

We also use data from the Fund for Investment in Telecommunications (Fondo de Inversion en Telecomunicaciones, FITEL), an independent agency that reports to the Peruvian Ministry of Transportation and Communication (Ministerio de Transportes y Comunicaciones). This data contains the construction date of every mobile phone tower in Peru, as well as detailed tower characteristics, such as its location, height, transmission power and transmission frequency. We use these tower characteristics to simulate mobile coverage areas in each year. This simulation was performed using Radio Mobile, a freely available software package developed by Roger Coud/'e. This software implements the Longley-Rice model, also known as the Irregular Terrain Model (ITM), to simulate signal propagation. The algorithm takes into account geographic terrain (using 90m resolution elevation data from the Shuttle Radar Topography Mission), the curvature of the earth, and tower characteristics, such as transmission strength, antenna type, height, gain and reception limits.

Using these simulated coverage maps, we next determine the year in which each ENAHO village gained mobile phone coverage. Note that we are unable to determine the physical location of some ENAHO villages, so these data are dropped from our analysis. This amounts to 15% of the rural ENAHO households. The extent of missing geographic information varies across years, reaching as high as 56% in 2004 – although our results are robust to dropping 2004 from the analysis. Aside from 2004, the year with the greatest number of lost observations is 2006, where we are unable to determine the location of 10% of rural households.

4) Methodology

We employ several empirical strategies in order to identify the impact of mobile phone infrastructure on household outcomes. As our baseline specification, we simply regress the outcome variable on mobile phone coverage:

$$y_{ivt} = \beta_0 + \beta_1 \cdot Coverage_{vt} + \mu_t + \varepsilon_{ivt} \quad (1)$$

where *i* indexes the household, *v* indexes the village, *t* indexes the year, *y* is the outcome of interest, μ_t is a year fixed effect, and *coverage* is a dummy variable indicating whether the village is within a mobile phone coverage area. Standard errors are clustered at the village level, in order to allow for correlated errors across households in the same village.

If coverage is uncorrelated with ε_{ivt}, This strategy will produce unbiased estimates of the impact of mobile phone coverage. However, since it is in the best interest of mobile phone providers to place their towers in locations that are profit maximizing, it is unlikely that coverage is uncorrelated with ε_{ivt}. More likely, towers are positioned to maximize the number of subscribers per dollar spent on infrastructure, in which case coverage would be more likely in areas with greater population density and higher per capita income. Since these variables may have a direct impact on the outcome variables of interest, the results from this strategy are potentially biased.

We can provide some empirical support for the fact that mobile phone coverage is, indeed, targeted to wealthier villages. Table 1 compares the characteristics of households that are never treated to those that are treated. To side-step the possibility that households with coverage have higher income and expenditures precisely because mobile phones foster economic development, we focus on the comparison between the first

column, which contains means for households in areas that are never covered, with the second column, which contains means for households in areas that will be covered in the future, but were not yet covered. Households that will be covered in the future have consistently higher expenditures and income than households that will never be covered. This provides strong evidence that mobile phone providers are not picking their coverage areas randomly, but are instead targeting service toward particular villages and households.

Table 1: Comparing Covered and Uncovered Households

	Coverage Status		
	Never Covered	Pre-Coverage	Post-Coverage
2001 Expenditures	214.7	302.3	402.5
	(2.9)	(9.2)	(30.8)
2002 Expenditures	214.2	277.4	353.9
	(2.4)	(7.8)	(15.6)
2003 Expenditures	224.2	333.2	362.7
	(3.1)	(10.8)	(16.4)
2004 Expenditures	245.2	361.7	423.7
	(3.9)	(18.7)	(22.8)
2005 Expenditures	224.8	329.1	451.5
	(2.8)	(12.7)	(15.9)
2006 Expenditures	239.9	351.4	433.2
	(2.8)	(14.3)	(13.8)
2007 Expenditures	253.2	.	445.2
	(3.0)	(.)	(11.8)
2001 Income	437.6	634.5	763.3
	(7.7)	(44.7)	(107.8)
2002 Income	428.9	578.0	742.4
	(6.5)	(23.2)	(43.6)
2003 Income	418.2	669.0	746.2
	(6.9)	(80.0)	(39.3)
2004 Income	465.7	615.8	801.5
	(11.7)	(35.7)	(57.9)
2005 Income	440.2	550.9	834.8
	(8.2)	(24.6)	(38.2)
2006 Income	495.6	634.6	946.5
	(8.8)	(34.7)	(105.1)
2007 Income	531.8	.	895.8
	(10.2)	(.)	(31.2)

Standard errors in parentheses

Given the non-random placement of mobile phone coverage, we also employ a second specification with village fixed effects:

$$y_{ivt} = \beta_0 + \beta_1 \cdot Coverage_{vt} + \mu_v + \mu_t + \varepsilon_{ivt} \quad (2)$$

where μ_v is a village fixed effect. This fixed effect controls for any time invariant village characteristics that may influence the mobile phone provider's decision of where to locate their towers.

While this specification eliminates tower placement bias if cell phone providers base their decisions on fixed village characteristics, such as population or per capita income, there is still the potential for bias due to tower placement if mobile phone providers target coverage based on time-varying village characteristics. For example, if mobile phone providers consider the future and maximize the net present value of future subscription revenue, then they may attempt to target villages that are expected to experience higher than average population or economic growth over the coming years. If this is the case, then what appears to be an impact of mobile phones on household outcomes could simply be an artifact of the fact that covered villages have higher than average income or population growth.

In order to assess this concern, we employ a model that allows the impact of coverage to vary with the number of years of coverage:

$$y_{ivt} = \beta_0 + \sum_{k \neq 0} \beta_k \left[years\_covered = k \right]_{vt} + \mu_v + \mu_t + \varepsilon_{ivt} \quad (3)$$

where [*years_covered = k*] is a dummy variable indicating whether the village has been covered exactly *k* years. By including negative years of coverage in this specification, we can assess whether there are trends in the outcome variables leading up to the first year of coverage. If there are no such trends, then it seems unlikely that mobile phone providers

are basing their coverage decisions on time-varying village characteristics, in which case specifications with village fixed effects should provide unbiased estimates of the effects of mobile phone coverage on household outcomes.

5) Results

5.1. Baseline Specification

We begin our analysis of the impact of mobile phones on household well-being with our baseline empirical specification, which simply regresses household outcomes on village coverage status. These results, shown in Table 2, suggest that coverage has a strong positive impact on cell phone ownership, household wage income, assets, and expenditures. The magnitudes of these effects are large, with wage income increasing by 57% and total expenditures increasing by 61%.

Table 2: Regression models

					Log Expenditures			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Has Mobile	Log Income	Has Assets	Log Assets	Total	Food	Non-Food	Durable
coverage	0.151***	0.565***	0.119***	0.711***	0.606***	0.712***	0.503***	0.158***
	(0.010)	(0.030)	(0.009)	(0.048)	(0.026)	(0.027)	(0.031)	(0.041)
N	42335	40093	42335	28508	42148	41248	41850	32689

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

In the methodology section, however, we provide evidence that high income and expenditure villages were more likely to receive coverage. As a result, the correlation between coverage and income in this specification could be a result of reverse causality, with better economic outcomes causing coverage, rather than being evidence that coverage causes better economic outcomes.

5.2. Village Fixed Effects

In order to mitigate concern regarding non-random tower placement, Table 3 implements village fixed effects. This specification is robust to non-random placement of towers, so long as tower placement depends only on time-invariant village characteristics. Relative to the baseline statistical specification, the impacts of coverage are much more muted. Nonetheless, the results suggest that coverage has a substantial impact on mobile phone ownership, which increases by 7%. Coverage does not have a statistically significant impact on wage income or on asset ownership, but is associated with an 8% increase in log total expenditures.

Table 3: Regression models with village fixed effects

					Log Expenditures			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Has Mobile	Log Income	Has Assets	Log Assets	Total	Food	Non-Food	Durable
coverage	0.072***	0.040	-0.029	0.135*	0.075*	0.061	0.089	-0.095
	(0.014)	(0.044)	(0.018)	(0.062)	(0.035)	(0.036)	(0.047)	(0.084)
N	42335	40093	42335	28508	42148	41248	41850	32689

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

5.3. Duration of Treatment

While village fixed effects address the concern that tower placement may depend on time invariant factors, the results may still be biased if tower placement depends on time-varying village characteristics. For example, if mobile phone coverage providers place their towers in areas that are expected to experience faster than average economic growth, than it may erroneously appear that mobile phones are causing economic growth. In order to investigate this concern, we implement a model that allows for heterogeneous impacts by duration of treatment. The results from this specification are shown in Table 4. Note that all the coefficients in this table show effect sizes relative to the omitted group of zero years of coverage.

Table 4: Duration of coverage regression models with village fixed effects

					Log Expenditures			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Has Mobile	Log Income	Has Assets	Log Assets	Total	Food	Non-Food	Durable
before3	-0.013	-0.059	0.005	-0.037	-0.063	-0.036	-0.045	-0.122
	(0.010)	(0.051)	(0.024)	(0.070)	(0.048)	(0.046)	(0.069)	(0.108)
before2	-0.003	0.072	0.005	0.059	-0.009	0.038	0.011	0.055
	(0.013)	(0.071)	(0.034)	(0.106)	(0.063)	(0.057)	(0.084)	(0.129)
before1	-0.006	-0.039	-0.029	-0.092	-0.009	-0.000	-0.007	0.009
	(0.012)	(0.052)	(0.027)	(0.077)	(0.046)	(0.048)	(0.065)	(0.105)
after1	0.046**	-0.013	-0.027	0.082	0.049	0.047	0.066	-0.151
	(0.015)	(0.052)	(0.024)	(0.078)	(0.045)	(0.046)	(0.060)	(0.100)
after2	0.112***	0.145*	-0.043	0.229*	0.105*	0.112*	0.126	0.024
	(0.025)	(0.068)	(0.032)	(0.112)	(0.053)	(0.053)	(0.075)	(0.130)
after3	0.104***	0.069	-0.037	0.233	0.094	0.094	0.155	0.021
	(0.028)	(0.077)	(0.028)	(0.125)	(0.059)	(0.060)	(0.084)	(0.159)
after4	0.198***	0.213*	-0.069	0.156	0.188**	0.114	0.193*	0.317*
	(0.033)	(0.087)	(0.058)	(0.164)	(0.067)	(0.071)	(0.093)	(0.153)
after5	0.182***	0.268**	-0.044	0.430**	0.384***	0.361***	0.290**	0.319*
	(0.031)	(0.092)	(0.046)	(0.150)	(0.073)	(0.074)	(0.098)	(0.158)
after6	0.291***	0.340***	-0.038	0.538**	0.446***	0.369***	0.372***	0.446*
	(0.038)	(0.094)	(0.040)	(0.168)	(0.073)	(0.073)	(0.100)	(0.174)
N	42335	40093	42335	28508	42148	41248	41850	32689

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

The first conclusion to draw from the treatment duration results is that, for all eight outcomes, there are no statistically significant differences prior to treatment. Since there are no statistically significant pre-coverage trends, we find no evidence that mobile phone providers targeted coverage toward areas that were experiencing faster than average income or expenditure growth. Given this, we conclude that the post-treatment effects are not an artifact of tower placement, unless mobile phone providers were somehow able to anticipate future economic growth above and beyond trends that were observable at the time the towers were built.

In addition to providing evidence on the extent of bias due to non-random tower placement, this specification also sheds light on how effect sizes vary with the number of years of coverage. Turning to our results, we find that mobile phone ownership jumps immediately, with an increase of 4.6% in the year after coverage, and rising to 29% six years after coverage. There is also a statistically significant increase in wage income of

15% two years after coverage, and this increase to 34% after six years of coverage. While there is no impact on the number of households that own any assets, the value of household assets increases by 23% two years after coverage, and increases to 54% after six years of coverage. Finally, total household expenditures increase by 10% after one year of coverage, and increase to 45% after six years of coverage. Initally this increase in expenditures appears to be driven by non-durable expenditures, although increases are statistically significant for all components of expenditures after five years of coverage.

5.4. Home Farm and Business Income

Thus far, we have focused on wage income, but most households in developing countries also operate home farms – so in order to pin down the impact of mobile phone coverage on household well-being, it is important to consider the impact on farming profits. These results are shown in Table 5.

Table 5: Home farm outcomes

	(1) Has Home Farm	(2) Production	(3) Expenditures	(4) Profit
before3	-0.053*	-0.410	-0.194	-0.243
	(0.021)	(0.312)	(0.225)	(0.289)
before2	-0.064*	1.154	-0.255	1.337
	(0.030)	(1.357)	(0.389)	(1.259)
before1	-0.014	-0.783	-0.013	-0.744
	(0.019)	(0.560)	(0.272)	(0.541)
after1	0.020	-0.664	0.196	-0.846*
	(0.021)	(0.455)	(0.336)	(0.414)
after2	0.021	-0.915	0.859	-1.706
	(0.029)	(1.220)	(0.664)	(1.175)
after3	0.025	-0.923	2.653	-3.520
	(0.036)	(1.809)	(1.558)	(1.999)
after4	0.073	0.756	2.108*	-1.227
	(0.041)	(1.432)	(0.936)	(1.320)
after5	0.089*	0.711	1.789	-1.012
	(0.040)	(1.481)	(0.948)	(1.313)
after6	0.076	0.539	2.195*	-1.438
	(0.043)	(1.334)	(0.981)	(1.288)
N	42334	33687	33813	33687

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

We find a statistically significant increase in the fraction of households running a home farm for one of the five post-treatment coefficients. In addition, the prevalence of home farming is lower two and three years prior to treatment, suggesting that this effect may simply be picking up a pre-treatment trend in covered areas. There are several possible explanations for such a trend. First, it could be that mobile phone providers targeted areas were farming was becoming more prevalent. Second, it may be that households expected mobile phone coverage would be introduced and, in anticipation of this fact, opened farms. Finally, since this is the only variable for which we find evidence of a pre-treatment trend, it may be that this is just a differential trend between covered and uncovered villages that arose by chance. We plan to explore this finding further to see if we can differentiate between these possibilities. In any event, we find that farm expenditures are higher post treatment, and this effect is statistically significant four and

six years after treatment. The impact of mobile coverage on the value of farm production is not statistically, but the general pattern is a small decline initially, and then small increase. As a result, profits appear somewhat lower after the introduction of mobile coverage, but the effect is only statistically significant in the year immediately after the introduction of mobile phones.

We also look at the impact on home business earnings. These results can be found in Table 6. As with home farming, we find an increase in home businesses following mobile phones, and this effect is statically significant one year and five years after the introduction of coverage. Profits are higher, but the effect is small and statistically insignificant.

Table 6: Home business outcomes

	(1) Has Home Business	(2) Profit
before3	-0.027	-0.019
	(0.019)	(0.034)
before2	-0.034	0.024
	(0.025)	(0.124)
before1	0.017	0.025
	(0.018)	(0.051)
after1	0.040*	0.013
	(0.019)	(0.044)
after2	0.054	0.071
	(0.028)	(0.113)
after3	0.021	0.202
	(0.030)	(0.233)
after4	0.031	0.112
	(0.034)	(0.123)
after5	0.098**	0.154
	(0.038)	(0.132)
after6	0.073	0.169
	(0.039)	(0.132)
N	42334	40817

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

5.5. Robustness Check: Migration

One possible interpretation of the findings presented thus far is that the construction of a mobile phone tower attracts high income individuals to the area. If this is the case, then it may be that mobile phones have no actual impact on household well-being, but simply cream-skim high quality migrants. In order to rule out this possibility, we now look at the impact of mobile phone coverage on the fraction of individuals born in the district. These results are presented in Table 7. Note that there are no statistically significant impacts, regardless of treatment duration. While we have no direct measure of movement within district, it seems unlikely that migrants would be attracted from within the district but not from outside the district. Given this, we do not believe that our results are an artifact of selective migration.

Table 7: Robustness check: migration

	(1) Born in District
before3	0.005
	(0.015)
before2	0.025
	(0.021)
before1	-0.002
	(0.017)
after1	0.004
	(0.017)
after2	0.012
	(0.024)
after3	0.014
	(0.026)
after4	-0.040
	(0.030)
after5	-0.019
	(0.030)
after6	-0.036
	(0.030)
N	187378

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

5.6. Heterogeneous Effects: Mobile Phone Ownership

One final question regarding the impact of mobile phones on household well-being is how these gains are distributed within covered villages. One dimension of heterogeneity that has received attention in the literature is whether benefits go exclusively to mobile phone owners, or whether there are spillover benefits for non-owners. We explore this question in Table 8, where we show the post-coverage effects for non-owners, and the additional effects for those who do own cell phones. From this table, it is apparent that income and asset ownership are relatively flat for non-owners. While we do see a statistically significant increase in total and food expenditures for non-owners, these increases are relatively modest and, for the most part, statistically insignificant until five years after the introduction of mobile phones. The differences between owners and non-owners, however, are large and statistically significant throughout. These results are suggestive that there is little in the way of spillover benefits for those who do not own cell phones. Of course, mobile phone ownership is itself a choice, and we cannot rule out the possibility that there are spillover effects, but that those who benefit use their increased earnings to purchase a mobile phone.

Table 8: Heterogeneous effects: mobile phone ownership

				Log Expenditures			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Log Income	Has Assets	Log Assets	Total	Food	Non-Food	Durable
after1	-0.057	-0.028	-0.042	0.020	0.036	0.010	-0.221*
	(0.055)	(0.024)	(0.084)	(0.050)	(0.050)	(0.066)	(0.104)
after2	0.066	-0.054	0.064	0.075	0.116*	0.065	-0.103
	(0.074)	(0.032)	(0.123)	(0.059)	(0.058)	(0.081)	(0.136)
after3	0.006	-0.043	-0.004	0.057	0.057	0.094	-0.090
	(0.088)	(0.029)	(0.132)	(0.066)	(0.065)	(0.092)	(0.172)
after4	0.068	-0.093	-0.185	0.065	0.029	0.033	0.098
	(0.093)	(0.057)	(0.176)	(0.079)	(0.077)	(0.106)	(0.165)
after5	0.165	-0.062	0.248	0.339***	0.328***	0.206	0.160
	(0.115)	(0.047)	(0.168)	(0.085)	(0.089)	(0.108)	(0.177)
after6	0.118	-0.070	0.031	0.298***	0.252**	0.190	0.142
	(0.102)	(0.041)	(0.178)	(0.082)	(0.083)	(0.108)	(0.192)
own_after1	0.802***	0.104**	1.430***	0.728***	0.528***	0.976***	0.866***
	(0.104)	(0.032)	(0.184)	(0.095)	(0.095)	(0.117)	(0.238)
own_after2	0.710***	0.150***	0.973***	0.469***	0.295***	0.593***	0.893***
	(0.095)	(0.026)	(0.204)	(0.090)	(0.079)	(0.094)	(0.185)
own_after3	0.514***	0.076*	1.280***	0.424***	0.467***	0.504***	0.597*
	(0.123)	(0.034)	(0.182)	(0.120)	(0.091)	(0.147)	(0.264)
own_after4	0.464***	0.093	0.956***	0.496***	0.386***	0.641***	0.674***
	(0.112)	(0.057)	(0.223)	(0.092)	(0.087)	(0.121)	(0.172)
own_after5	0.617***	0.168***	0.534*	0.449***	0.350***	0.535**	0.539*
	(0.126)	(0.041)	(0.227)	(0.101)	(0.101)	(0.168)	(0.240)
own_after6	0.590***	0.092**	1.184***	0.419***	0.327***	0.480***	0.588***
	(0.089)	(0.028)	(0.187)	(0.077)	(0.086)	(0.089)	(0.145)
N	40093	42335	28508	42148	41248	41850	32689

Standard errors in parentheses, clustered at the village (CCPP) level
* p<0.05, ** p<0.01, *** p<0.001

6) Conclusion

There is much excitement in the development community about the potential of mobile phones for fostering increased economic growth and, thus, enhancing the well-being of households in less developed countries. At the same time, while there is convincing evidence that mobile phones enable markets operate more efficiently, there is little empirical work establishing how this increased efficiency impacts the well-being of a typical household. In this paper, we provide evidence on this point, exploiting the roll-out of mobile phone infrastructure in rural Peru between 2001 and 2007 to measure the

impact of cellular coverage on household well-being. We find evidence that mobile phone coverage increases the income and expenditures of rural consumers. Moreover, we find no statistically significant impact on the profits of home businesses, which is important for understanding the overall impact on well-being in an environment such as Peru, where 85% of households operate a home farm.

In future research, we will extend this work in several directions. First, we plan to examine price data to pin down the impact of mobile phones on price variation across villages, and to pin down the impact of mobile phones on the average price paid (on the consumption side) or received (on the production side). We also hope to measure the impact of mobile phone coverage on the diffusion of new technologies. Finally, we will explore the possibility that mobile phones reduce the costs of communication with friends and family outside the village, and thus strengthen the use of extended families as a form of insurance against unanticipated shocks.

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