CONTRACTING THE ROAD TO DEVELOPMENT: EARLY IMPACTS OF A RURAL ROADS PROGRAM[1]

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Abstract

We study here the early impacts of the Peruvian rural roads program (RRP), characterized by the contracting of private local firms for the rehabilitation and maintenance of rural roads with local supervision by community leaders setting incentives that favour prevention activities and a sustainable and timely maintenance of rural roads. The analysis is based on a quasi-experimental approach through which control roads are defined prior to the intervention based on key observable characteristics of the road and the villages they connect. Diff-in-Diff estimates are reported to control for biases associated to time-invariant unobservables. We find that this institutional innovation improved road transitability which in turn led to significant changes in employment patterns, increased investments in education and health, but not to higher household labor income. We cannot discard that lack of income effects can be explained by the short time allowed for this evaluation (2 years from baseline), as we do observe individuals in treated motorized roads (MRs) increase waged employment while females in non-motorized tracks (NMTs) increase work at the family farms. In turn, increases in household investments in human capital are strong and robust in treated MRs. School attendance increases, although some gender inequities remain as older girls (12-18) do not show such improvements. Reduced morbidity for children under five and improved access to early childhood development programs is also observed in treated MRs. Thus, despite the lack of income effects, the results of this early evaluation are encouraging about the impacts of the Peruvian RRP. The contracting of local private firms for the rehabilitation and maintenance of rural roads in developing countries may quickly improve rural roads and change economic and social opportunities for rural households. Still, further studies are required to determine whether lack of income effects result from lack of time, or from the need of complementary investments.

JEL codes: H54, L33, L92, O12, O13

June, 2009

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[1] Author's e-mail: jvaldivi@grade.org.pe. The author wishes to thank officials from PROVIAS DESCENTRALIZADO for the permission to use the data for this study and to the PEP Research Network, CIES and CAF for financial support. This version has benefited from insightful and helpful comments by Sonia Laszlo, Sam Morley and participants of seminars at CAF, NEUDC, PEP, GRADE and CEA. The author also acknowledges the valuable support of Vanessa Cheng, Valerie Koechlin, Paola Vargas and Rosa Vidarte at different stages of the project, in the supervision of the data collection of the 2006 round and with the data analysis. Usual disclaimer applies.

1. Introduction

The economic literature has been increasingly reporting mechanisms through which improved roads can create opportunities for economic growth and poverty reduction in rural economies (Binswanger, Khandker and Rosenzweig, 1993; among others). Through the reduction of transportation costs, improved roads can increase productivity and demand for labor in farm and non-farm activities thus leading to increased income and consumption. Although often ignored until recently, improved roads can also have meaningful social impacts, in particular those associated to household investments in health and education (van de Walle, 2002).

Nevertheless, macroeconomic adjustments and local governance issues have led to underinvestment in this kind of infrastructure (World Bank, 2005). Moreover, rural transport projects have focused on building new roads or upgrading their condition, while disregarding the need to establish an institutional arrangement to guarantee timely rehabilitation and maintenance of roads (Malmberg Calvo, 1998). In that sense, the focus of the Peruvian Rural Roads Project (RRP) on an institutional innovation that focuses on the rehabilitation and permanent maintenance of already existent rural roads, for which local private firms are promoted and contracted, makes it particularly important to be analyzed. That is, public funds are provided not only for a one-time rehabilitation but also for permanent maintenance of treated roads, and payments to contracted local firms require a satisfactory report from PROVIAS and community supervisors. To my knowledge, there does not exist a study with a careful evaluation of the impacts of a road program that focus on a similar institutional innovation.

The analysis of this kind of interventions is particularly relevant in the current wave of decentralization in the provision of infrastructure in the developing world. Decentralization to local governments combined with community participation can increase quality in service provision by increasing accountability of providers (World Bank, 2004). However, more recent studies have been more cautious about the ultimate effects of decentralization and community participation on quality of public services and the provision of infrastructure. Local capture and corruption can make provision of infrastructure worse under decentralized mechanisms (Bardhan and Mookherjee, 2006; Olken, 2007). More empirical evidence is needed to see which of the trends end up dominating under different contexts.

Focusing on the Peruvian RRP, this paper attempts to contribute to the literature by evaluating the impacts of a unique program that focuses on an institutional innovation to improve road rehabilitation and permanent maintenance. Most of the interventions reported in the literature, especially in Africa and Asia, include building new roads or upgrading them, for instance, by paving them, while disregarding regular maintenance. Indeed, the Peruvian RRP does not include pavement upgrades as in the Blangladesh case analyzed by Khandker et. al. (2006) nor includes building new roads as in the Vietnamese case studied by Mu and van de Walle (2007). Considering these activities are not banned in other roads, the impacts we report here are associated to an improved efficiency in road rehabilitation and maintenance with the program rather than to the absolute lack of these efforts on the part of other public agencies such as local governments, or others. To my knowledge, there is no published study that focuses on such institutional innovation.

Methodologically, we use a quasi-experimental approach that allows controlling for time-invariant unobserved characteristics of villages and households. We use a longitudinal dataset that enables us to measure impacts on a wide variety of socio-economic, institutional and environmental characteristics. Furthermore, we also explore the heterogeneity in the impacts by individual, household and village characteristics, as well as conditioning community factors for the realization of benefits. In particular, we analyze the extent to which poorer households, smaller communities, rural women, and other especially marginalized groups, benefit from the enhanced economic environment resulting from the Peruvian RRP. Policy makers would greatly benefit from identifying a conditional factor, either at the level of the community or the household, that spurs the impact of improved rural roads. However, if the key conditional factor varies too much by outcome so that no general pattern can be identified, targeting policy implications would be less clear.

Other unique features of the Peruvian RRP refer to the type of roads targeted and the timing of this evaluation. First, the Peruvian RRP does not only treat motorized roads but also non-motorized tracks. Second, although the Peruvian RRP has been operating since 1998, this study focuses on the impact of the cohort of interventions that started in 2004, and evaluates its impacts after only two years. That is, the estimated impacts we are presenting here need to be interpreted as the very early impacts of the improvement in rural roads generated by the institutional innovation.

This paper is organized in five sections including this introduction. The second section presents the key features of the intervention and discusses the expected effects. Section 3 describes the characteristics of the data and the methodology used for estimating

the impact of the Peruvian RRP. Section 4 presents the estimated impacts on the quality of the roads, and its effects on household income and expenditures, employment, as well as investments in education and health. Section 5 summarizes the results and discusses its limitations as well as some of its potential policy implications.

2. The program and its expected effects

The Peruvian RRP is a large program run by the PROVIAS RURAL, a unit of the Vice-minister of Transport that has been operating since 1995 with the objective to improve transport conditions in rural villages by contracting private local firms to manage and carry out, on a sustainable basis, the maintenance of rural roads in the poorest areas of Peru[2]. The first phase of the RRP was carried out during 1995-2000 in 12 departments that ranked highest in rural poverty within the country. During that first phase, the project improved rural accessibility in 314 districts contracting with 495 local firms in charge of rehabilitating and maintaining about 12,000 kilometers of rural roads and key secondary roads and about 3,000 km of non-motorized tracks (Escobal, et. al., op. cit.)[3].

2.1 The intervention

The RRP program is based on an institutional innovation that focuses on the rehabilitation and permanent maintenance of already existent rural unpaved roads and non-motorized

[2] Currently, the unit running the program is called PERU DESCENTRALIZADO, an indication of the increased role of local governments in the planning and execution of the program, as part of the decentralization process being carried out within the Peruvian State.

[3] The system of district-level rural roads in Peru is estimated in 70,000 kilometers. In the 12 departments the system of rural roads is estimated in 28,000 kilometers.

tracks, for which local private firms are promoted and contracted. Rural roads in Peru are the responsibility of the central government which in turn delegates it to local governments. Thus, when a road is blocked due to floods or other weather shocks, local governments start rehabilitation with technical and financial support from the central government. However, responsibility is less clear for regular maintenance as, for instance, it is not clear where the users can complain to for excessive bumpings and stones in the roads, which in turn increase travel time as well as maintaining costs for public and private vehicles.

The Peruvian RRP commits financing from the central government, through the program, and assigns the responsibility of performing quality rehabilitation and permanent maintenance of selected rural roads to local private firms through contracts that connect regular payments to the quality of the road as periodically assessed by officials from both the program and local governments, as well as other community organizations. This assessment is not limited to checking the materials the local firms use in the rehabilitation activities, as it can also rely on observable final outcomes such as travel time to go from startpoint to endopoint of the road segment, the number of months the road is blocked due to landslides during rainy season, or the bumpiness of the road as it increase maintenance costs for private and public motorized vehicles. Thus, in principle, incentives are set for local contractors to implement a more regular maintenance, including maintenance of road drainage systems as they that could prevent blockages, or other activities that reduce costs of rehabilitation when weather shocks hit transitability of the road.

Nevertheless, the incentive structure can be damaged if the program is captured by local elites as the supervision by local authorities can become a formality. Olken (2007)

reports the case of the Indonesian rural road program affected by corruption, as measured by the discrepancies between reported budgets and the budgets estimated by special supervisors based on an analysis of materials and labor used. However, it is important to notice that the Indonesian RRP, as opposed to the Peruvian RRP, does not include regular maintenance so that incentives cannot be set based on observable qualities of the road, and only by costly monitoring of the actual materials used during the construction, upgrading or rehabilitation.

The institutional innovation in the Peruvian RRP depends critically on the quality of local institutions, including local authorities and firms (Malmberg, 1998). Thus, the RRP promotes local institutional development by providing technical assistance to local governments and small and medium local enterprises for improved planning and management of rural roads and for the development of micro-enterprises formed by groups of beneficiaries for road maintenance. The program has a local office in every department which starts by identifying the provinces in which they will operate in each stage. Once a province is identified, the program starts with the formation of a Provincial Road Institute (PRI) with increasing participation of the provincial municipality and other local authorities. The program's departmental office coordinates with the PRI to select the specific road segments to be rehabilitated and maintained at each stage, with participation of other local institutions through open consultations in different districts.

Once a road has been selected for intervention, local officials from the program and local authorities coordinate to initiate the intervention with an open call for individuals interested in becoming members of the local microenterprise of road maintenance (MEMV,

for its name in spanish) that is going to be in charge of the periodic rehabilitation and maintenance of the selected road (Escobal, et. al., 2005). They are then selected based on their previous experience in road maintenance, time of residence in the locality, as well as characteristics such as education, age, etc. The selected individuals are then trained in the management of the microenterprise and maintenance of the road and the MEMV is legally formed. Next, the program's local officers elaborate the annual plan of activities using a program that allows the calculation of the number of individuals and time required to rehabilitate and maintain the selected road as well as the cost per kilometer[4]. These estimates are based on the characteristics of the selected road such as its location, length, width, traffic and weather conditions. MEMVs then sign a contract with PROVIAS RURAL through which they receive monthly payments, based on the estimated costs and a quality certification issued monthly by the PROVIAS and community supervisors[5]. As mentioned above, these output-based contracts set clear incentives for the local MEMV to invest in prevention activities and provide a sustained and timely maintenance of the contracted rural road (Benavides, 2003).

In terms of the kind of rural roads targeted by the program, it is important to point out that, unlike previous cases recently analyzed in the literature, the Peruvian RRP does not include road paving or the building of new roads. Second, the program not only considers unpaved motorized roads but also non-motorized tracks. The gradual inclusion of non-

[4] Escobal et. al., op. cit., reports that average costs are US $ 17,000 per kilometer rehabilitated and US $ 2,800 per kilometer maintained.

motorized tracks aims at promoting gender equity in the distribution of the intervention's impacts as these tracks were identified as the ones women use most[6].

2.2 Expected effects

Setting the right incentives for contracted local firms should improve quality rehabilitation and maintenance of rural roads treated by the program, by reducing the time they remain blocked when large weather shocks hit, and also by reducing the time required to travel across the different points connected by the selected roads. Thus, we can expect the program to better integrate poorly accessible zones to regional economic centers, reducing transport costs and raising the reliability of vehicular access to expand markets for agricultural and non-farm products and enhancing a more diversified set of employment opportunities for rural households. Improved transportation will also reduce time to reach basic social services such as health, education and justice.

However, it is important to clarify here that our counterfactual is not complete inaction with respect to the rehabilitation of maintenance of control rural roads. Control roads may not have guaranteed financing for rehabilitation and maintenance, nor a specific agent with the clear responsibility and incentives to perform the actual physical works.

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[5] In case of an unsatisfactory maintenance, supervisors give time to the MEMV to repair the deficiencies. If the situation is not solved, the local office applies discounts to the monthly payments, and the contract is dissolved if the deficiencies occur over three consecutive months.

[6] The program identified this fact when collected the opinions of potential beneficiaries through gender-based focus groups organized in several rural communities (see Fort and Menendez, 2005). Additional focus groups after the intervention have confirmed that a large proportion of women see the program's road intervention enabling them to travel farther and more safely, and has also led to increased income.

Thus, control roads may take longer to be rehabilitated after a flood, or have bumps and stones that increase travel time and maintenance costs for vehicles. Still, local governments and other offices of the Ministry of Transport and Communications (MTC) and public agencies such as FONCODES may have performed related activities in control roads, especially when weather shocks blocked them. Thus, rather than the impacts of rehabilitating and maintaining a rural road, this study evaluates the effects associated to the improved efficiency in these rehabilitation and maintenance activities as a result of the reallocation of incentives due to output-based contracts that favor prevention and sustained and timely maintenance by local contractors[7].

Many papers have shown the different mechanisms through which improved rural roads benefit the welfare of households and individuals associated to beneficiary roads (Mu and van de Walle, 2007; Khandker, et. al., 2006; Levy, 2004; Escobal y Ponce, 2002; Jacoby, 2000; among others). We briefly summarize the most important findings of that literature, with some extra comments regarding the sequence of effects as they may affect the time needed for some of these effects to materialize. The most direct effects of the RRP are associated to the transitability of the rural roads, which are often estimated through the travel time needed to go from the initial to end points of the segment of the reference and the time (weeks or months in a year) a road stays blocked due to a climatic shock or alike. Levy (2004), for instance, report such effects in Morocco, emphasizing on the importance

[7] This issue is particularly important in the case of the Peruvian RRP, as local governments have seen a substantial increase in their budgets as a result of the decentralization process, and the RRP intervention does not include road upgrading as in the Blangladesh case analyzed by Khandker et. al. (2006) nor building new roads as in the Vietnamese case studied by Mu and van de Walle (2007).

of the number of months the road remained blocked in the context of rural roads. Other subsequent effects are reduced time that individuals residing in the connected villages take on average to go to key markets, schools, health facilities, depending on the nature of the role of the segment on the local road network. At the same time, the improved transitability may eventually lead to an improved public transportation service that can be measured in terms of an increased frequency of buses or reduced prices for the transportation of individuals and cargo. The latter effects are clearly conditioned on the improved transitability of treated roads, and thus they are likely to take longer to materialize.

The improved rural roads activate a series of mechanisms that transform traditional productive patterns, agricultural and non-agricultural, in the villages associated to the rehabilitated segments. First, reduced travel times help individuals access to extra off-farm employment opportunities both, agricultural and non-agricultural, within and outside the village. Escobal and Ponce (2002) find such result for the first round of interventions by the Peruvian RPP, especially non-agricultural wage employment for more educated individuals. Jacoby (2000) also argues for such effects as he finds a negative correlation between agricultural and non-agricultural wages and the distance from the village to the key markets in Nepal.

Farm productivity and income can also receive a boost as a result of reduced post-harvest crop losses, lower input prices, higher output prices or improved access to financial and non-financial agricultural services, (Biswanger, Khandker and Rosenzweig, 1993). Improved accessibility to markets increases small farmer's bargaining power with local traders. Access to credit and agricultural extension services may take longer and come first

for less poor farmers or in villages closer to larger markets, as they tend to require extra conditions such as mechanisms for agricultural risk management and organization of small local farmers. Access to these services is seen as crucial for small farmers to switch towards high-value crops.

However, these effects have not been found in all cases and often concentrated on less poor farmers, consistent with the fact that complementary investments are required for them to materialize. Moreover, income effects may at least initially be perceived as temporary and consequently households decide to increase savings through increments in livestock rather than increased consumption (Escobal and Ponce, 2002). More important for this study is the fact that many of these effects are conditioned on adjustments in the supply of key agricultural services such as agricultural extension, credit, which may take longer to materialize. Thus, it would not be that surprising if we do not find income effects in this study, especially if we consider that the follow up in which this study is based comes only two years since treated roads started being served by the program.

Improved rural roads cannot only spur productivity and income but also household investments in the human capital of their children. As travel times are reduced, it is less costly for parents to send their younger children to school as they would need to devote less time traveling with them to the school location. Older children would in turn be able to attend school while at the same time being able to help with housework or at the family farm. Levy (2004), for instance, finds increased school attendance, especially among girls, which may imply that improved travel security may also be an important factor for rural girls. These demand-side effects are likely to show up early. But supply-side effects may

also increase household investments in schooling as quality can improve as a result of more effective attendance by teachers or even improved recruiting as a result of reduced travel times to larger villages or the capital of the district. However, the teacher's recruiting effect may be expected to take longer to show up.

Similar mechanisms can be stated to explain improved access to health services. With improved roads, visits to health facilities may take less time for the ill individual or the family member in charge of their care. The attendance and recruiting effects can be raised for doctors and other health professionals, in the same way they were mentioned for school teachers. In addition, improved roads can also help bring social programs based on health facilities closer to the associated villages, increasing access to preventive health programs that could reduce sickness events among children and adults. Qualitative studies in Sub-Saharan Africa and Asia have reported that individuals identify improved access to health services as the key benefit obtained from improved roads (see, for instance, Porter, 2002 and Hettige, 2006).

All these positive effects may significantly alter the socio-economic context in the villages associated to the improved roads so that migration incentives in search of better employment and education opportunities may be reduced. However, at the same time, permanent and temporary migration costs are reduced. Thus, the net effect of improved roads on permanent and temporary migration may go either way.

In sum, we have seen a wide variety of potential impacts of the Peruvian RRPs. However, not all of them are likely to show up in this early evaluation after just two years. It is also likely that the size and time lags of these effects may vary across households and

villages depending on the initial endowments of private and public assets. Poverty in developing countries is strongly concentrated in rural areas, but still there is significant heterogeneity that can lead to differentiated impacts and have important implications for project design. If higher or faster impacts are found in households and villages initially less endowed, policy makers will face a much desired win-win situation. However, if higher impacts concentrate among the initially better endowed, targeting for higher impacts may lead to increased inequalities within rural economies. Khandker et. al. (2006) and Mu and van de Walle (2007) explore the nature of these heterogeneities finding that road improvements tend to be pro-poor in rural Bangladesh and Vietnam, respectively, a very encouraging result. However, peculiarities of the Peruvian RRP demands for us to analyze whether such trend is sustained when the intervention does not include road upgrades, but instead a permanent maintenance component for both unpaved motorized roads and non-motorized tracks.

3. Design, data and methodology

3.1 Quasi-experimental design

For this intervention, treatment and control roads are not chosen randomly from a set of *eligible* roads. Instead, treatment roads are first selected by a departmental committee (program officials and local authorities) that chooses the provinces to be intervened. Then, the PRI and the local program officials picked the specific road to be intervened at a particular round. For this treatment group, a control group was selected prior to any intervention based on similarities in key observable variables such as the longitude and type

of road (rural road or non-motorized track), characteristics of the villages involved such as population size, access to basic public services and infrastructure, altitude[8].

Another important matched variable is the hierarchy of the villages involved, so that if a district capital was involved in the treated road, the control road also connected a district capital. In addition, control roads are also required to have no intersection with a treated road or track to minimize the probability that benefits on treated villages spill over the control villages. Actually, control roads were selected within the same province but from different districts to minimize the possibility they belonged to the same road network as the treated ones but at different stages. This effort was based on information provided by three key databases: the 1999 Pre-census database (INEI), the Population Census of 2005 (INEI) and the Geo-referenced Road Map (MINTRA-MINEDU), which is rarely available in a digital format for use in economics research. Table 3 shows the pre-treatment means for treatment and control groups for many observable variables, showing there are almost not statistically significant differences between these two groups[9].

The selection process described for this intervention has important implications for the interpretation of the impact estimates we present here. First, it implies that our indicator would estimate a treatment on the treated effect. The relationship between our estimates and the average treatment effects would depend on the nature of the prioritization. If the PRI selected roads associated to poorest and most remote villages, and those would be the ones for which the RRP impacts are smallest, then our estimates would be underestimating

[8] Treated and control roads are associated to villages by defining the origin and the end of the road.

the average effects. On the other hand, if roads where selected so that impacts would be largest, and these officials were right in their ex-ante assessments, then our estimates would be overestimating the average treatment effect.

Second, the selection may affect our ability to identify a proper control group. The selection may be so acute that the likelihood of identifying a road similar in all characteristics to the treated ones is rather low. We argue, though, that the size of the intervention in any province and department and the measures taken by our team helped contain such potential problem. A key issue is to avoid choosing as controls roads that are systematically located at different points of the road network. For that, besides similarities in access to key infrastructure, altitude, and population, we argue the hierarchy of the towns is crucial. That is, if a capital of a district is associated to the treated road, we look for another road that connects another capital of district to a similar ending town. In general, for each treated road, we restrict the search to different districts within the same province. However, when one of the towns was sensibly different from the rest available in the province, we looked in the adjacent provinces[10]. Although the described selection process for the control group attempts to maximize the probability the control group be equivalent to the treatment group of roads, we cannot discard the existence of certain time-variant unobservables that can affect our estimates.

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[9] For the outcomes analyzed in this study, Table 4-Table 11 show treatment-control comparisons at baseline. No significant differences are found there.

[10] That was the case, for instance, when a district capital was involved. Recall that treated roads needed to be unpaved so capital of provinces have generally not been directly associated to treatment roads.

3.2 Data requirements and sources

The impact evaluation presented in this paper refers to the cohort of interventions defined for 2004 and uses the last two rounds (2004 and 2006) of a specialized household and community-level survey that includes a wide variety of socio-economic, institutional and environmental indicators[11,12]. The survey questionnaires were for the most part the same for the two rounds, and they all were applied in the fourth quarter of the corresponding years, so that consistent comparisons are allowed. The household survey includes information about the characteristics of the dwelling, health and education of all household members, farm and non-farm entrepreneurial activities, commercialization channels, etc[13]. The community-level survey is applied to key local informants and includes information about the characteristics of the villages in terms of access to public infrastructure and basic services, distance to nearest markets, and other key public facilities. It also includes characteristics of the roads such as the time required to travel from the initial to the final point of the road by the different means, number of public transportation units that use the roads, the number of months the road remain closed over the past year, number of car accidents, maintenance and operation costs for public transportation units offering services in the road. Also, number of students in primary and secondary schools, number of health

[11] The Peruvian RRP also applied a baseline survey in 2000 that would allow the analysis of the impacts of that cohort of interventions, also providing valuable information about the dynamics of the effects of the RRP. That is, we would be able to verify the time lags and sustainability of effects. However, such analysis is postponed for a second stage as a very time consuming effort will be required to generate a consistent panel of the three rounds (2000, 2004 and 2006).

[12] The 2004 round of the survey was applied by the firm *Cuánto* while the 2006 round was applied by GRADE. Both surveys were done in coordination with the Rural Roads Program as part of the program's impact evaluation strategy.

services offered by public health facilities, judiciary and police crime records, use of associated roads, among many other variables.

Recall treated and control roads are associated to villages by defining the origin and the end of the road. In the case of small roads or tracks (less than 20 kms.), 6 households were randomly selected within each the initial and end villages. In the case of large roads, an extra intermediate village is included in the sample. The sample of 2004 cohort of interventions involved 92 treated road segments in 13 of the poorest departments in the country. At baseline, we interviewed a total of 2,457 households in 387 villages associated to treatment and control road segments[14]. In 2006, we were able to re-interview 2,167 of them, that is, we had an attrition rate of 11.8%[15].

3.3 Methodology

The study uses the double difference estimate to determine the impact of the rural roads program on a wide variety of indicators at the level of the household and the localities involved. A basic regression-based DD estimate can be obtained from the following regression:

$$Y_{ijt} = \beta_0 + \beta_1 \cdot D_t^{AD} + \beta_2 \cdot D_j^{TC} + \beta_3 \cdot D_t^{AD} \cdot D_j^{TC} + \varepsilon_{ijt} \qquad (1)$$

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[13] See Table 1 and Table 2 below for a list of the main indicators available in all survey rounds.

[14] In 2000, a baseline was established for a sample of 2,000 household associated to the road segments that were treated during 2000-2001. That sample was also followed in the 2004 and 2006 round of surveys.

[15] When a household was not initially identified, the field procedures included asking the neighbors, relatives and community leaders. Although there were a few rejections, most of the missing households corresponded to cases in which the nuclear family had moved outside the province.

where Y_{ijt} denotes the value of an indicator of interest for household i that resides in village j at period t (t=0 is the baseline; t=1 is the follow-up survey). D^{TC} is a categorical variable that takes value one if the household resides in a treated village and zero if it resides in a control village. D^{AD} is a categorical variable that takes value one if the observation is from the follow-up survey and zero if it comes from the baseline. Finally, ε_{ijt} denotes the error term which is assumed to be independent across villages but not necessarily within them[16]. In that setting, β_3 is the DD estimator of the impact of the program on variable Y, and is often referred as an average effect as it refers to all beneficiaries without distinction.

If we identify systematic differences between the treatment and control groups in observable variables, we would need to include some controls in expression (1) to check the robustness of our DD estimate. Furthermore, we cannot assure that there are non-observables that can establish systematic differences between treatment and control groups, but the double-difference (DD) estimate can help control for any time-invariant systematic non-observable difference by including household fixed effects[17]. Thus, a full version of the average DD estimate can be obtained from the following expression:

$$Y_{ijt} = \beta_0 + \beta_3 \cdot D_t^{AD} \cdot D_j^{TC} + \lambda_t + \upsilon_{ij} + \varepsilon_{ijt} \qquad (2)$$

[16] Thus, we use the Huber-White covariance matrix estimator to obtain the standard error of our coefficients of interest.

[17] Still, since we do not have a randomized control trial, we cannot discard that some time-variant unobservable effects may bias the DD estimate. One way we could argue against such bias is to show that trends prior to the intervention are similar in treatment and control groups (see Galiani, Gertler and Schargrodsky, 2005). That could be done for schooling variables as we have a series of school censuses, but not for most of the variables analyzed here.

where λ_t and υ_{ij} denote the year and household fixed effects, respectively. As we plan to analyze the heterogeneity of the effects depending on the characteristics of the roads and the beneficiary villages, the associated econometric analysis will use the following formulation:

$$Y_{ijt} = \beta_0 + \beta_3 \cdot D_t^{AD} \cdot D_j^{TC} + \gamma_3 \cdot D_t^{AD} \cdot D_j^{TC} \cdot X_{ij} + \lambda_t + \upsilon_{ij} + \varepsilon_{ijt} \qquad (3)$$

where X is another dichotomic variable that takes value 1 if the household or village has the characteristic of interest or concern. In that case, β_3 comes to be the DD estimator of the program's impact for those households or villages that does not have the characteristic of interest X, and $\beta_3 + \gamma_3$ is the one for those that do have it. The impact evaluation proposed here will pay special attention to differentiated impacts by gender, education, ethnicity and village's size.

An important issue is the implications of the attrition rate on the estimates we report here. Random attrition may restrict the statistical power of a study but is mostly harmless. However, treatment effects can be biased if missing observations are correlated with treatment. In general, if the missing observations correspond to those that would have benefited less from the RPP, then our estimates would be overestimating the treatment effects. The opposite would occur if the missing observations correspond to those that would have benefited most. An interesting point established by Angrist, Bettinger and Kremer (2006) is that if we assume that treatment is never harmful, analyzing observed households will give us a lower bound for the effect of a program. In that regard, understanding the improvement of roads can only be positive, then the estimates we report

here would need to be considered as a lower bound for the impacts of the PROVIAS intervention, although our attrition rates are relatively low.

4. Results

Table 4-Table 10 present the results obtained for road transitability, income, expenditures, poverty, employment, school attendance and access to health services. In each table, we first report the number of households or individuals involved in the estimation. Next, the first two numbered columns present the averages for each outcome for the treatment and control groups at baseline, while column (3) reports the difference[18]. Columns (4)-(6) do the same for the follow-up survey. Column (7) reports the DD estimate that result from estimating expression (1) in the previous section, while column (8) reports the DD estimate when controlling for household fixed effects as specified in expression (2). Results are always shown separately for motorized roads and non-motorized tracks as they may play different roles in connecting rural households to basic services.

4.1 Impacts on transitability of roads

The first important verification is that the presence of the program generated a significant reduction in the average time required to travel from the start point to the endpoint of the road of reference (Table 4). In the case of the motorized roads, the reduction is of 28

[18] Notice that for almost all analyzed outcomes, differences between treatment and control groups are not significant, as it is also the case for the variables reported in Table 3.

minutes from an initial travel time of 100 minutes. In the case of the non-motorized tracks, the reduction is of 37 minutes from an initial travel time of 173 minutes.

As it was suggested from the discussion in the previous section, it would have been very interesting to measure the effect on the number of months a year the road stays closed as a result of climatic shocks. Unfortunately, such information was only collected for treated roads in the 2004 round so we cannot estimate the DD estimator for such variable. The only thing we can verify is that such blockages were even more problematic in 2006 than in 2004. However, Table 5 also shows that community leaders and household heads tend to report an increased level of satisfaction with rehabilitation work in treated localities. In the case of rehabilitation of motorized roads, community leaders in treated villages reported an adequate work in 81% of the cases by the follow up survey, up from 62% at baseline. A similar increased satisfaction is observed among households as they increased the reports of positive benefits out of the rehabilitated road. This increased satisfaction among household heads is also found in the case of non-motorized tracks. Nevertheless, an initially puzzling result was to find that community leaders reported deterioration in the quality of the rehabilitation and maintenance work in non-motorized tracks with the program. However, discussions with PROVIAS officers suggest that such reports may be more connected to dissatisfaction with the unfulfilled expectation for the track to be upgraded to a motorized road, rather than an evaluation of the quality of the rehabilitation performed by the contracted MEMV[19].

[19] Also, recall that monthly payments to local MEMVs are contingent on a satisfactory report from program supervisors, and consecutive negative reports lead to a cancellation of the contract.

These results are non-trivial for the program. First, the positive results on travel time and in the perception of the quality of the program intervention provide evidence against corruption of the program's mechanism in the sense that, for instance, payments to MEMVs would continue even with reductions in the quality of the rehabilitation and maintenance. Second, positive impacts are still present despite the fact that the program has been operating since 1998 and many local governments have seen their revenues increase during this decade as a result of economic growth and progress in the decentralization process. Indeed, the mechanism could have been reproduced by other public agencies, as it is suggested by the number of other roads associated to treatment and control villages that were treated during the period of observation (Table 4). In the case of motorized roads, for instance, villages associated to treated (control) roads had 0.92 (0.73) roads rehabilitated during the previous two years at baseline, and that number increased to 1.72 (1.57) by the follow up survey in 2006[20]. Thus, the program positive impacts indicate that the difference with the program would likely go beyond the extra money spent in road rehabilitation and maintenance, emphasizing on the clear incentives provided by the contracts with MEMVs.

In the following sub-sections, we analyze the implications of the improved transitability upon income and expenditure patterns, employment decisions and household human capital investments by type of road.

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[20] Although, road rehabilitation also increased in control roads, it is clear there is no observed bias in the quantity of rehabilitated roads between treatment and control groups, so that no strong evidence is found for a contamination bias. There could be a difference in the quality of rehabilitation work in favor of treated villages, but such difference would be rightly assigned to the impact of the program if associated to the transmission of the program's methodology to the local governments involved.

4.2 Impacts on labor income, employment, expenditures and poverty

Table 6 reports the effects of the RRP on household labor income (farm and non-farm), expenditures and poverty. We do not find significant effects in any of these variables. These results are not that surprising considering previous studies that have argued that investments in rural roads are not enough to improve the way rural household connect to key markets, For this study, we also need to keep in mind that two years may not be enough time for changes to materialize[21]. Although markets could become closer as a result of improved roads, key agents may need time to adjust to new conditions. For instance, for farm income, farmers may take more time to recognize that it has become less convenient for them to sell their crops by the field or adjacent road than in local fairs or regional markets. Even if they already noticed it, it may not be that easy for farmers to break the long-term relationship with local merchants[22].

Before going further trying to explain the absence of these income effects, we may want to further explore the existence of evidence that economic conditions may have changed with the RRP as a result of improved transitability, by looking at sub-groups of the beneficiary population, or by looking at other related variables such as individual employment decisions. Table 7 starts by exploring whether there are sub-groups that could

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[21] Recall that the interventions in the round of treated roads we are analyzing started in 2004 and the follow up survey was done in 2006 (see discussion in section 2).

[22] See Escobal (2005) for a discussion of the complexity of the decision process associated to selection of markets by Peruvian rural farmers in Huancavelica. The author argues that local merchants establish more personal relationships with local farmers as they tend to be their first alternative to sell. Such sales provide the farmer with the cash necessary to afford taking chances at more profitable but also further and riskier markets such as local fairs or regional markets. Huancavelica is part of the area targeted by the Peruvian RRP.

present some positive and significant effects, despite that none are found on average. Interventions that enhance productivity of farmers tend to benefit most, or first, those that were better off before the program, as they tend to have all the other conditions required to benefit from improved roads. However, it is also feasible to find a pro-poor bias if the less poor are less constrained by bad roads because they may have other assets to compensate. Indeed, recent studies evaluating rural roads programs in developing countries have reported encouraging pro-poor biases in their impacts. Khandker et. al. (2006), for instance, find that some of the effects of a rural roads program in Bangladesh on household expenditures accrue among the poorest households. Mu and van de Valle (2007) also finds that the impacts of the Vietnamese rural roads project concentrate in the poorer communes of Vietnam. Still, it would be important to check whether this bias works also in the case of the Peruvian RRP, considering the results we are reporting here ought to be considered as early impacts, and also because of the peculiarities of the Peruvian program with respect to the other cases discussed here, namely that the Peruvian RRP does not include pavement upgrades or building new roads. On the other hand, it does include financing of permanent maintenance of the treated rural roads[23].

In particular, we explore these hypotheses by checking for heterogeneous impacts by schooling, ethnicity of the household head, village size and altitude. The analysis by village size may be particularly important in the case of the Peruvian RRP. Fieldwork for the 2006 survey showed that in many cases treated roads were connecting a relatively large village

[23] Although, the Peruvian RRP already focus in some of the poorest provinces, they likely still hide large inequalities across households and villages.

with a very small one, with many other small villages along the road. If one thinks that some of the relatively large villages already have key markets and public services available, then we could expect that larger impacts would concentrate on the smaller villages as they would be the ones for whom transaction costs would reduce most.

Table 7 shows that the RRP did have effects on household labor income for households residing in villages above 3,400 meters above sea level for which a motorized road was treated. Households with more educated heads and residing in larger villages (more than 850 inhabitants) also present positive impacts but they fall short of being statistically significant. On the other hand, treated non-motorized tracks show even weaker average effects and more variability, with no specific group presenting any positive significant income effects.

These results suggest that the RRP might have generated some positive changes in economic conditions, at least for motorized roads, but they need more time or complementary investments to lead towards strong effects on household labor income. Such hypothesis is further supported when we observe effects on employments decisions by individuals. Indeed, Table 7 shows that individuals residing in villages associated to treated motorized roads increase in 10 days a year their dedication to waged employment, both agricultural and non-agricultural, and reduce their participation in the family farm as unpaid family worker. These effects are small with respect to total days worked a year by an individual (171), which could explain why it does not lead towards significant increases in household income, but they are indeed important with respect to the time dedicated to waged employment at baseline. Moreover, non-motorized tracks increase in 16 days a year

their dedication to the farm as a non-remunerated family worker. Those 16 days a year represent a 30% increase from the number of working days they dedicated to this kind of labor at baseline.

In Table 8, we analyze the changes in employment patterns generated by the RRP intervention by age, gender and mother tongue. The switch observed in motorized roads towards wage employment seems to be led by adults in the peak of their productive years; that is, between 25 and 50 years old. At the same time, though, females seem to increase their participation in agricultural jobs while males focus on non-agricultural jobs. Notice that in the case of females, they seem to be abandoning work at the farm where they were participating as a non-remunerated family worker. In the case of non-motorized tracks, the opposite switch towards farm work is also concentrated among females. Another important result is that employment effects seem to accrue among individuals reporting *quechua* or *aymara* as their mother tongue.

The important gender effects on employment are very relevant, especially in the case of non-motorized tracks, as they were specifically included in the program for their relevance to women. We would need to explore further these changes to determine which of these opposite switches in employment patterns imply some empowering of women.

Thus, employment effects support the hypothesis that economic opportunities may have indeed changed with the RRP, but they are not large enough to imply income effects, except when focusing on households residing in villages above 3,400 meters above sea level for which a motorized road was treated. Next, we analyze effects on household investments in the education and health of their members.

4.3 Impacts on household investments in human capital

With respect to household investments in human capital, we find a strong school attendance effect for children in villages associated to treated motorized tracks, and in morbidity and use of local health facilities for both types of roads. School attendance effects are clearly differentiated by gender and age (Table 9)[24]. Attendance increased about 7 percentage points among older boys (12-18 years old), an important effect considering that attendance by such group at baseline was only 84 percent. Considering the age group, it is possible that this effect may imply that boys are better able to attend secondary school while continuing living within the nuclear family, rather than permanently migrating to a larger city[25]. For the younger girls (6-11 years old), school attendance increased 6 percentage points from an initial 93% attendance rate for this group at baseline. That is, these young girls are reaching perfect attendance to primary school, eliminating a previously negative gender bias.

Lack of effects on younger boys may not be of concern, considering that the level of attendance of this group was already very high (95%) at baseline. On the other hand, the lack of effects among older girls is worrisome as this group had the lower level of attendance at baseline, and indicates that gender inequalities are still affecting girls in the higher levels of primary school or at the entrance to high school. If that is the case, it would be useful to identify whether the reason is associated to a lower value parents give to higher

[24] The attendance reported here refers to the period prior to the survey, and not the current one. Luckily, ENAHO has both variables. Current attendance is much lower at around 50% for high school level students, but the reports on attendance in the previous period in ENAHO are similar to those reported here.

[25] This hypothesis will be further evaluated with the individual migration data that has not been included in this version of the study.

education of girls, or if it is explained instead by the higher vulnerability girls face with respect to the level of insecurity when traveling longer distances.

Finally, Table 10 reports the impact of the Peruvian RRP on morbidity and use of health services by adults and children under five. We find a reduction in the incidence of illnesses and accidents in the four weeks prior to the date of the survey, especially for children under five and in the villages with treated motorized roads. In this case, the morbidity rate falls almost 4 percentage points among all members, but the reduction is almost 9 percentage points when looking only at children under five. The effect on the use of health services (consultations) is also negative which is somewhat puzzling. An explanation could be that improved rural roads may be helping the health system reach better the population from remote areas not by attending them when sick but rather by providing them with useful health information that helps them prevent the illness events and the need for consultations at the health center. Such hypothesis is indeed consistent with the finding that households with children under five in these localities report having benefited more (6-8 percentage points) from early childhood development programs, and considering that most of the work of the corresponding nutritional and health programs is made off the health post or center (see Table 11).

On the other hand, a somewhat puzzling result is found among those that were treated through non-motorized tracks, as the use of consultations at health facilities for children under five drops by 12 percentage points, even though the reduction in morbidity is not found statistically significant. However, we should be careful with these results as the

sample size for children under five in these localities is rather small, something that is also true for the educational outcomes reported in Table 9 (see Table 12)

5. Summary and discussion

We studied here the early impacts of a rural roads program that is based on an institutional innovation characterized by the contracting of private local firms for the rehabilitation and maintenance of rural roads with local supervision by community leaders setting incentives that favour prevention activities and a sustainable and timely maintenance of rural roads. We find that this institutional innovation, promoted by the PROVIAS DESCENTRALIZADO of the Ministry of Transport and Communication, quickly improved road transitability which in turn led to significant changes in employment patterns and increased investments in education and health, but not on household labor income. These results vary significantly by the type of road under consideration, though, consistent with the idea that motorized roads and non-motorized tracks play different roles in the road network connecting individuals to key places such as farm fields, markets, schools, etc.

Lack of income effects may be due to the need of complementary investments or because it is still too early for these effects to materialize. Unfortunately, we are not able to disentangle these forces, although it is true that the follow up survey we analyze here was done only two years after the beginning of the intervention. When looking at income effects by sub-group, though, we do find positive and significant effects for households residing in high altitude villages where a motorized road was treated. Households with more educated

heads and in larger villages that were treated with motorized roads also show positive effects, but they fell short of becoming statistically significant. Also, employment changes show that more waged employment opportunities, both agricultural and non-agricultural, become available for those treated with motorized roads. On the other hand, for treated non-motorized tracks, individuals tend to increase their participation in the family farm. Thus, economic opportunities seem to be changing as a result of the program although they have not yet led to increased labor income.

Also, the differences in employment opportunities by type of road indicate they play different roles in connecting rural people to key markets. Motorized roads seem to be playing the more recognized role of connecting rural households to larger cities where product and job markets are more developed. Non-motorized tracks, on the other hand, play a more important role in mobilizing individuals from their houses to the farm fields, especially for adult women.

Changes in employment patterns also vary substantially by gender. In treated motorized roads, females tend to reduce their participation as unpaid family worker at the family farm to work more outside, though still in agricultural activities. Males on the other hand seem to have better access to waged non-agricultural jobs as a result of the RRP. In turn, in treated non-motorized tracks, changes are only found on females, who also work more, but at the family farm. Thus, they can work more at the family farm without having to reduce time dedicated to domestic work.

The Peruvian RRP also had early effects on school attendance and morbidity in the case of treated motorized roads underscoring the importance of this type of interventions

for household investments in human capital. The school attendance effects, however, are not found significant for older girls (12-18 years old), for which the attendance problem was more worrisome to begin with, and suggest the need for further interventions to promote gender equity in schooling investments by rural households. The morbidity effects in turn are especially significant for children under five. These results would indicate the need to consider availability of rural roads when analyzing the capacity of the Juntos program to enforce the conditionalities on school attendance and health checkups by mothers and children.

In sum, the results of this early evaluation are positive and hopeful about the impacts of the Peruvian RRP, and more generally, for the contracting of local private firms for the rehabilitation and maintenance of rural roads in developing countries. Although we do not find robust income effects, we have strong evidence that transitability of rural roads improves substantially with the program, and that led to meaningful changes in economic opportunities and increases in household investments in education and health of their children. Also, the analysis by road type confirms that the inclusion of non-motorized tracks allows women to increase their participation in labor activities at the family farm. Considering that this study focuses on the cohort of interventions that started in 2004, six years after the beginning of the program, the positive effects also indicate that the intervention has been able to control corruption threats. It may still be the case that some money be deviated and or that some special families benefit more from employment by local firms, but the output-based contracts have allowed for improvements in road transitability to still be observed.

Finally, considering that the analysis presented here is based on a follow up survey applied after only 1-2 years from the beginning of the intervention, it would be important to extend the analysis to earlier interventions by the RRP so that we could elucidate whether, for instance, the lack of income effects is because they need more time to show up, or whether complementary interventions are required. More generally, following older interventions over time would allow us to explore deeply into the dynamics of the effects of the RRP, that is, which impacts need more time to mature, and also, whether earlier impacts are sustained in time.

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Table 1: Key indicators available in household and community-level surveys

Indicators	Description of variables	Source [a/]
Transport		
Travel time	Time in minutes needed to go from the initial to the final point of the road	CLS
Traffic intensity	Average number of public and private transportation units using the road, and frequency of public units	CLS
Cost of public transportation	Ticket prices for transporting people and cargo	CLS
Usability of the road	Number of months the road was closed over the past 12 months	CLS
Access to health and education		
Schooling	Maximum level of schooling attained by each individual	HLS
School attendance	Proportion of children currently attending school	HLS
School accessibility	Means of transport used to go to school and travel time	HLS
School availability in the locality	Number of schools available in the locality, by level	CLS
Illness	Number of days individuals were sick/disabled, incidence of diarrhea among children	HLS
Use of health care	Number of individuals that consulted with doctors	HLS
	Pregnancies with birth control consultancies, institutional births over the last two years	HLS
Accessibility to health care	Means of transport used to go to the nearest health facility and travel time	HLS
Availability of health facilities	Number of health facilities available in the locality, by level	CLS
Access to other services		
Public telephone	Availability of public phone in the locality	CLS
Internet	Availability of internet in the locality	CLS
Radio	Availability of radio in the locality	CLS
TV signal	Availability of public TV signal in the locality	CLS
Income and employment		
Income	Total monthly labor income, by individual and household	HLS
Diversification	Proportion of income coming from agricultural, livestock and non-agricultural activities	HLS
Wages	Average agricultural and non-agricultural wages for unskilled labor in the locality	HLS
Time use	Time dedicated to domestic activities, by age and gender	HLS

(continue …)

Table 2: Key indicators available in household and community-level surveys (… continue)

Indicators	Description of variables	Source
Productive activities		
Agricultural land	Size of the plots owned and managed by household members	HLS
Land use intensity	Land cultivated by household members	HLS
Productivity	Yields of main products and value added per hectare	HLS
Livestock	Number of heads by type of animal	HLS
Productive assets	Number and value of key equipment and machinery	HLS
Trade	Proportion of production destined to the local and regional markets	HLS
Market accessibility	Means of transport used to go to the main market (local fair) and travel time	HLS
Access to agricultural services	Number of households with access to credit and technical assistance	HLS
Expenditures and poverty		
Household expenditures	Total per capita monthly expenditures	HLS
Poverty rate	Proportion of households with expenditures under the poverty and extreme poverty lines	HLS
Unmet basic needs	Proportion of households without at least one of the basic needs unmet (treated water, sewage, type of roof, children in school age not attending school, large dependency ratio)	HLS
Social capital		
Migration	Number of permanent and temporary migrants and immigrants	HLS
Social organizations	Number of social organizations in the locality	CLS
Presence of public programs	number of public programs that operated in the locality over the past two years, and number of beneficiaries in the locality	HLS
Participation	Number of households with individuals that are active members of local social organizations	HLS
Opinion of the program		
Performance of the program	Perception of the quality of rehabilitation and maintenance of roads	CLS
Impact	Perception of the types of benefits brought by the rehabilitation and maintenance of the road	HLS / CLS
Distribution of benefits	Proportion of households that report having benefited with the rehabilitation and maintenance of road	HLS

CLS - community-level survey; HLS – household level survey

Table 3: Pre-treatment differences for 2004 cohort

Variables	Control	Treatment	Difference	T-stat	
Household variables					
Age groups					
[0-8]	26.8	28.1	-1.4	-1.52	*
[9-18]	25.1	24.5	0.6	0.70	
[19-35]	23.5	23.5	0.0	-0.05	
older than 36	24.7	23.9	0.8	0.92	
School attainment (3 years or older)					
None	15.6	15.4	0.2	0.20	
Pre - school	8.6	9.5	-0.9	-1.56	*
Primary	48.5	48.5	0.0	0.04	
Secondary	24.1	23.7	0.4	0.46	
Superior	3.2	2.8	0.3	0.93	
Access to water (%)	52.7	52.2	0.5	0.20	
Female head (%)	11.1	10.9	0.2	0.15	
Head with indigenous mother tongue (%)	62.5	60.0	2.5	1.15	
Per Capita Expenditure (monthly soles)	87.0	91.8	-4.8	-1.21	
Per Capita Income (monthly soles)	91.1	92.3	-1.3	-0.24	
Poverty					
Extreme (%)	51.8	50.5	1.3	0.57	
No extreme (%)	30.4	30.4	0.0	0.02	
Village level variables					
Population size (# individuals)	874.7	1023.4	-148.7	-0.76	
Altitude (meters above sea level)	2722.6	2799.0	-76.4	-0.55	

Source: 2004 Household and Community-level Baseline Surveys

Table 4: Baseline - Follow-up Statistics and Impact of Rural Roads on Transportation

Dependent variable [a/]	Nº of villages	Baseline Treated (1)	Baseline Control (2)	Baseline Diff (3)		Follow-up Treated (4)	Follow-up Control (5)	Follow-up Diff (6)	DD (7)		DD (FE) (8)	
Motorized tracks												
Roads rehabilitated in past 2 years	235	0.92	0.73	0.182		1.72	1.57	0.15	-0.03		-0.03	
				(0.141)				(0.15)	(0.20)		(0.22)	
Road of reference												
Travel time	235	101.45	99.55	1.90		69.55	84.71	-15.16	-17.06		-28.07	*
				(11.26)				(13.01)	(14.70)		(16.51)	
# months road remained blocked		1.97	n.a.	n.a.		2.60	n.a.	n.a.	0.63	***	0.59	***
									(0.16)		-0.178	
Non-Motorized tracks												
Roads rehabilitated in past 2 years	74	1.14	0.39	0.750	***	1.31	1.03	0.28	-0.47		-0.44	
				(0.232)				(0.25)	(0.34)		(0.38)	
Road of reference												
Travel time	74	172.87	180.34	-7.46		137.27	162.97	-25.7	-18.24	*	-37.04	***
				(5.45)				(37.35)	(12.69)		(9.55)	
# months road remained blocked		2.13	n.a.	n.a.		2.85	n.a.	n.a.	0.73	*	0.64	
									(0.40)		(0.46)	

Notes: Each row in the table is from a separate regression. Double difference (DD) estimates are reported as measures of impact. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in column 8 control for household-level fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Baseline - Follow-up Statistics and Impact of Rural Roads on Transportation

	Motorized roads			Non-motorized tracks		
Variables	2004	2006		2004	2006	
Perception of quality of intervention by community leaders						
Rehabilitation (=1 if considered adequate)	62.1	80.6	***	84.1	60.0	**
Maintenance (=1 if considered adequate)	67.9	75.0		70.5	47.1	**
Perception of quality of intervention by households						
Both (=1 if hh benefited from road intervention´)	60.1	73.9	***	47.4	65.0	***
Reasons						
Improved access to:						
Health care	48.9	64.2	***	33.3	63.2	***
Schools	38.1	57.3	***	21.8	54.2	***
Markets	85.1	69.3	***	74.4	65.3	
Job opportunities	40.6	57.3	***	14.1	56.3	***
Reduced prices of processed goods	21.1	30.0	***	5.1	11.1	
Other	15.8	20.4		20.5	17.4	

*** significant at 0.01, ** significant at 0.05, * significant at 0.1.

Table 6: Baseline - Follow-up Statistics and Impact of Rural Roads on Household Labor Income, Expenditures and Poverty

Dependent variable [a/]	Baseline				Follow-up				DD	DD (FE)
	Treated	Control	Diff		Treated	Control	Diff			
	(1)	(2)	(3)		(4)	(5)	(6)		(7)	(8)
Motorized roads										
Household monthly income	358.31	365.17	-6.87		339.62	340.07	-0.45		6.42	6.18
			(18.51)				18.55		(19.36)	(19.63)
Household monthly expenditure	407.01	388.55	18.47		443.82	427.98	15.84		-2.63	-2.63
			(24.49)				24.49		(29.51)	(29.51)
Poverty rate										
Extreme	61.25	59.55	1.69		62.80	65.62	-2.82		-4.51	-4.91
			(2.52)				2.54		(3.04)	(3.07)
No extreme	20.04	23.41	-3.38	*	16.18	16.20	-0.02		3.36	3.68
			(2.04)				2.05		(2.81)	(2.85)
Non-poor	18.70	17.06	1.65		20.99	18.26	2.73		1.08	1.24
			(2.04)				2.05		(2.39)	(2.44)
Non-Motorized tracks										
Household monthly income	322.06	330.81	-8.76		291.14	262.46	28.67		37.43	31.86
			(27.96)				28.09		(32.59)	(32.97)
Household monthly expenditure	372.62	371.45	1.18		393.15	349.08	44.07		42.90	42.90
			(37.19)				37.19		(47.29)	(47.29)
Poverty rate										
Extreme	62.58	58.71	3.87		69.79	69.03	0.76		-3.11	-2.26
			(4.42)				4.43		(5.82)	(5.88)
No extreme	20.43	24.03	-3.60		12.99	19.57	-6.58	*	-2.98	-2.63
			(3.65)				3.66		(5.17)	(5.29)
Non-poor	16.97	17.20	-0.23		17.26	11.51	5.76	*	5.99	4.89
			(3.38)				3.39		(4.35)	(4.41)

Notes: Each row in the table is from a separate regression. Double difference (DD) estimates are reported as measures of impact. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in column (8) control for household-level fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Heterogeneities in the Impact of the RRP: Household Labor Income

	Motorized roads			Non-motorized tracks	
	Treated at baseline	DD (FE)		Treated at baseline	DD (FE)
Base Model	358.31	6.18		322.06	31.86
		(19.63)			(32.97)
Household head´s schooling					
Lower	308.17	-8.25		309.62	40.90
		(24.45)			(41.13)
Higher	432.30	32.01		359.90	15.60
		(32.93)			(55.43)
Village size					
Small	356.46	35.82		312.13	6.37
		(34.42)			(52.71)
Medium	406.95	-14.13		382.24	62.09
		(35.57)			(66.02)
Large	459.65	47.72		390.15	-79.08
		(41.45)			(71.45)
Altitude					
Low	354.90	-34.26		265.88	65.52
		(38.16)			(78.36)
Medium	380.30	21.97		229.09	-8.77
		(37.65)			(68.86)
High	378.48	78.40	**	199.84	2.99
		(35.69)			(57.53)

Notes: Each group of rows of the same category is from a separate regression. Coefficient and standard deviations control for household-level fixed effects. Double difference estimates are reported as measures of impact. * significant at 10%; ** significant at 5%; *** significant at 1%. Heterogeneous categories are defined as follows. Schooling: Lower if for household head with primary or no education; Higher, with secondary or higher education. Village size: Small is for villages with less than 300 inhabitants; Medium, with more than 300 to 850 inhabitants; Large, with more than 850 inhabitants. Altitude: Low is for villages which are 2500 meters above sea level, Medium, between 2500 and 3400 meters above sea level, High, from more than 3400 to 5500 meters above sea level.

Table 7: Baseline - Follow-up Statistics and Impact of Rural Roads on Employment (%)

Dependent variable [a/]	Nº of individuals	Baseline Treated (1)	Baseline Control (2)	Baseline Diff (3)		Follow-up Treated (4)	Follow-up Control (5)	Follow-up Diff (6)		DD (7)		DD (FE) (8)	
Motorized tracks													
Working days per year	4141	170.929	170.206	0.723		168.105	172.134	-4.029		-4.752		-4.553	
				(4.412)				(4.417)		(5.367)		(5.420)	
Wage - Agriculture	4141	7.501	6.313	1.188		10.506	5.712	4.793	**	3.606	**	3.808	**
				(1.574)				(1.579)		(1.678)		(1.702)	
Wage - No agriculture	4141	10.044	10.883	-0.839		16.108	11.397	4.710	**	5.549	**	5.683	**
				(2.011)				(2.017)		(2.213)		(2.244)	
Non wage - Agriculture	4141	74.869	77.989	-3.120		75.625	78.078	-2.453		0.667		-0.539	
				(3.777)				(3.781)		(5.342)		(5.436)	
Non wage - No Agriculture	4141	23.776	20.495	3.281		22.810	21.542	1.268		-2.014		-2.036	
				(3.062)				(3.072)		(3.275)		(3.320)	
Unpaid family worker - Agriculture	4141	50.991	50.942	0.049		37.773	47.895	-10.122	***	-10.171	**	-8.986	**
				(2.944)				(2.950)		(4.024)		(4.026)	
Unpaid family worker - No Agriculture	4141	2.754	2.368	0.385		4.630	6.524	-1.894		-2.280		-2.527	
				(1.189)				(1.193)		(1.523)		(1.539)	
Non-Motorized tracks													
Working days per year	1322	177.461	176.311	1.149		179.608	167.201	12.408	*	11.258		10.141	
				(7.556)				(7.536)		(9.680)		(9.781)	
Wage - Agriculture	1322	1.996	4.377	-2.380		4.118	5.425	-1.307		1.074		1.383	
				(1.802)				(1.795)		(2.431)		(2.457)	
Wage - No agriculture	1322	8.976	7.978	0.998		9.858	13.874	-4.016		-5.014		-4.994	
				(3.191)				(3.190)		(3.567)		(3.633)	
Non wage - Agriculture	1322	92.564	92.491	0.072		89.580	89.679	-0.099		-0.171		1.584	
				(7.275)				(7.234)		(10.260)		(10.544)	
Non wage - No Agriculture	1322	16.713	9.898	6.815	*	17.582	10.571	7.011	*	0.197		-1.373	
				(3.854)				(3.848)		(4.705)		(4.747)	
Unpaid family worker - Agriculture	1322	53.783	58.624	-4.841		53.795	41.515	12.279	**	17.120	**	16.393	**
				(5.472)				(5.448)		(7.500)		(7.511)	
Unpaid family worker - No Agriculture	1322	2.470	1.526	0.944		3.752	4.742	-0.990		-1.935		-2.701	
				(1.772)				(1.762)		(2.499)		(2.530)	

Notes: Each row in the table is from a separate regression. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in column (8) control for household-level fixed effects. Double difference estimates are reported as measures of impact. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Heterogeneities in the Impact of the RRP – Employment

Dependent variable [a/]	Base model	Age			Gender		Mother Tongue	
		Younger	Middle	Older	Female	Male	Quechua/Aymara	Spanish
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Motorized tracks								
Working days per year	-4.55	-15.55	5.79	-12.43	-9.61	3.33	-7.56	0.83
	(5.42)	(9.78)	(7.24)	(11.81)	-(7.43)	(7.33)	(6.86)	(8.85)
Wage - Agriculture	3.81 **	0.69	5.53 **	3.85	4.06 *	3.53	2.59	5.79 **
	(1.70)	(3.25)	(2.40)	(3.92)	-(2.41)	(2.38)	(2.15)	(2.78)
Wage - No agriculture	5.68 **	4.14	7.07 **	4.47	3.56	7.98 **	6.23 **	4.73
	(2.24)	(4.27)	(3.14)	(5.14)	-(3.16)	(3.12)	(2.84)	(3.66)
Non wage - Agriculture	-0.54	3.78	-3.55	0.60	2.90	-3.44	-1.31	1.19
	(5.44)	(9.82)	(7.24)	(11.83)	-(6.92)	(6.84)	(6.88)	(8.87)
Non wage - No Agriculture	-2.04	-8.08	1.34	-3.14	-2.91	-0.73	-5.09	3.12
	(3.32)	(6.30)	(4.64)	(7.58)	-(4.72)	(4.66)	(4.20)	(5.42)
Unpaid family worker - Agriculture	-8.99 **	-14.53 *	-1.16	-17.01 *	-14.45 ***	-1.86	-7.53	-11.46 *
	(4.03)	(7.60)	(5.60)	(9.15)	(5.45)	(5.38)	(5.09)	(6.57)
Unpaid family worker - No Agriculture	-2.53	-1.58	-3.38	-1.75	-3.14	-2.00	-2.57	-2.47
	(1.54)	(2.94)	(2.17)	(3.54)	-(2.18)	(2.16)	(1.95)	(2.51)
Non-Motorized tracks								
Working days per year	10.14	26.03	7.56	-0.96	29.92 **	-8.73	5.81	13.92
	(9.78)	(17.22)	(13.34)	(21.49)	(13.29)	(13.22)	(13.21)	(14.62)
Wage - Agriculture	1.38	2.09	0.06	4.70	1.09	1.87	4.63	-2.44
	(2.46)	(4.53)	(3.51)	(5.65)	-(3.47)	(3.46)	(3.32)	(3.67)
Wage - No agriculture	-4.99	-4.38	-4.97	-3.54	-4.23	-5.66	-3.94	-5.11
	(3.63)	(6.69)	(5.18)	(8.34)	(5.10)	(5.07)	(4.90)	(5.43)
Non wage - Agriculture	1.58	7.34	-0.14	2.20	16.37	-12.77	3.25	-1.32
	(10.54)	(18.26)	(14.14)	(22.77)	-(13.48)	(13.41)	(14.24)	(15.77)
Non wage - No Agriculture	-1.37	0.55	-0.31	-4.93	1.03	-3.79	1.48	-3.84
	(4.75)	(8.72)	(6.75)	(10.87)	-(6.72)	(6.69)	(6.41)	(7.09)
Unpaid family worker - Agriculture	16.39 **	15.83	16.02	13.73	20.46 **	12.59	4.11	29.29 ***
	(7.51)	(13.60)	(10.53)	(16.96)	(10.24)	(10.19)	(10.13)	(11.21)
Unpaid family worker - No Agriculture	-2.70	4.77	-3.10	-13.07 **	-4.65	-0.95	-3.72	-2.33
	(2.53)	(4.66)	(3.61)	(5.81)	-(3.58)	(3.56)	(3.41)	(3.78)

Notes: Each group of rows of the same category is from a separate regression. Coefficient and standard deviations control for household-level fixed effects. Double difference estimates are reported as measures of impact. * significant at 10%; ** significant at 5%; *** significant at 1%. Heterogeneous categories are defined as follows. Age: Cohort 1 is for indivuduals younger than 25 years old; Cohort 2, from 26 to 50 years old; Cohort 3, older than 50. Gender: if the individual is female or male. Village size: Small is for villages with less than 300 inhabitants; Medium, with more than 300 to 850 inhabitants; Large, with more than 850 inhabitants. Altitude: Low is for villages which are 2500 meters above sea level, Medium, between 2500 and 3400 meters above sea level, High, from more than 3400 to 5500 meters above sea level. Mother tongue: if the mother tongue of the household head is quechua/aymara or spanish.

Table 9: Baseline - Follow-up Statistics and Impact of Rural Roads on School Attendance (%)

Dependent variable [a/]	Nº of individuals	Baseline			Follow-up			DD		DD (FE)	
		Treated	Control	Diff	Treated	Control	Diff				
		(1)	(2)	(3)	(4)	(5)	(6)	(7)		(8)	
Motorized tracks											
Males 6-11 years	711	95.435	92.597	2.837	91.938	90.872	1.066	-1.772		-3.752	
				(2.009)			(2.027)	(2.614)		(3.082)	
Males 12-18 years	612	84.153	89.268	-5.115 *	84.104	79.561	4.543	9.659	**	7.291	*
				(3.044)			(2.921)	(3.808)		(4.276)	
Females 6-11 years	734	93.196	95.524	-2.328	95.180	88.719	6.462 ***	8.789	***	6.898	**
				(1.913)			(1.953)	(2.459)		(2.862)	
Females 12-18 years	521	80.628	84.530	-3.902	84.022	85.976	-1.954	1.948		-0.222	
				(3.319)			(3.139)	(3.696)		(4.156)	
Non-Motorized tracks											
Males 6-11 years	232	92.936	95.637	-2.702	83.689	92.352	-8.662 **	-5.960		-2.751	
				(3.704)			(3.866)	(5.275)		(6.276)	
Males 12-18 years	212	83.541	91.777	-8.236	72.452	85.730	-13.277 **	-5.041		-6.706	
				(5.328)			(4.965)	(6.509)		(7.372)	
Females 6-11 years	222	89.290	91.215	-1.925	85.740	89.567	-3.827	-1.903		-1.789	
				(4.331)			(4.301)	(5.678)		(6.592)	
Females 12-18 years	172	79.579	78.880	0.699	76.656	83.504	-6.847	-7.546		-8.741	
				(6.410)			(5.965)	(7.247)		(8.286)	

Notes: Each row in the table is from a separate regression. Double difference (DD) estimates are reported as measures of impact. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviation in column (8) control for household fixed effects. * significant at 10%; ** significant at 5%; ***significant at 1%.

Table 10: Baseline - Follow-up Statistics and Impact of Rural Roads on Morbidity and Use of Health Services (%)

Dependent variable [a/]	Nª of individuals	Baseline			Follow-up			DD		DD (FE)	
		Treated (1)	Control (2)	Diff (3)	Treated (4)	Control (5)	Diff (6)	(7)		(8)	
Motorized tracks											
Sickness and accidents in last 4 weeks											
All household members	7574	38.569	35.573	2.996 **	30.661	31.471	-0.811	-3.807	**	-3.724	**
				(1.401)			(1.413)	(1.426)		(1.434)	
Children 0-5 years	1396	47.757	42.929	4.828 *	35.830	38.233	-2.403	-7.231	**	-8.789	**
				(2.780)			(3.100)	(3.685)		(3.998)	
Attention from a heath professional											
All household members	7574	21.441	19.804	1.637	20.718	21.661	-0.943	-2.581	**	-2.591	**
				(1.154)			(1.165)	(1.246)		(1.255)	
Children 0-5 years	1396	38.368	38.442	-0.074	30.063	35.271	-5.208 *	-5.134		-8.078	**
				(2.673)			(2.990)	(3.659)		(3.972)	
Non-Motorized tracks											
Sickness and accidents in last 4 weeks											
All household members	2348	32.806	34.790	-1.984	33.783	33.219	0.565	2.549		2.141	
				(2.483)			(2.478)	(2.552)		(2.567)	
Children 0-5 years	369	46.487	36.114	10.374 **	33.724	35.066	-1.342	-11.715	*	-7.385	
				(5.284)			(5.508)	(6.824)		(7.526)	
Attention from a heath professional											
All household members	2348	17.254	18.677	-1.422	19.933	18.406	1.526	2.949		2.598	
				(1.989)			(1.985)	(2.109)		(2.120)	
Children 0-5 years	369	35.768	31.436	4.332	22.120	30.876	-8.756 *	-13.087	**	-12.498	*
				(4.999)			(5.207)	(6.375)		(7.008)	

Notes: Each row in the table is from a separate regression. Double difference (DD) estimates are reported as measures of impact. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in column (8) control for household-level fixed effects. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Baseline - Follow-up Statistics and Impact of Rural Roads on Access to Social Programs (%)

Dependent variable [a/]	Nº of households	Baseline			Follow-up				DD		DD (FE)	
		Treated (1)	Control (2)	Diff (3)	Treated (4)	Control (5)	Diff (6)		(7)		(8)	
Motorized tracks												
Social Programs												
Food	1525	73.208	72.055	1.153	62.390	56.849	5.541	**	4.388		4.388	
				(2.402)			(2.402)		(2.695)		(2.695)	
Education	1525	63.396	64.658	-1.261	65.409	62.603	2.806		4.067		4.067	
				(2.461)			(2.461)		(2.595)		(2.595)	
Health	1525	54.843	53.288	1.555	75.849	73.425	2.424		0.869		0.869	
				(2.398)			(2.398)		(2.852)		(2.852)	
ECD1												
All households	1525	57.484	54.658	2.827	76.226	73.699	2.528		-0.299		-0.299	
				(2.388)			(2.388)		(2.834)		(2.834)	
Households with children under 6 years	958	79.923	81.136	-1.214	88.996	82.273	6.723	***	7.937	**	7.937	**
				(2.415)			(2.415)		(3.353)		(3.353)	
ECD2												
All households	1525	76.730	76.027	0.702	81.761	80.548	1.213		0.511		0.511	
				(2.093)			(2.093)		(2.442)		(2.442)	
Households with children under 6 years	958	95.560	97.727	-2.167	93.243	89.318	3.925	***	6.092	***	6.092	***
				(1.529)			(1.529)		(2.093)		(2.093)	
Non-Motorized tracks												
Social Programs												
Food	478	68.619	67.364	1.255	60.251	61.088	-0.837		-2.092		-2.092	
				(4.378)			(4.378)		(4.788)		(4.788)	
Education	478	69.874	64.854	5.021	67.782	70.293	-2.510		-7.531		-7.531	
				(4.264)			(4.264)		(4.875)		(4.875)	
Health	478	44.351	51.046	-6.695	73.222	75.314	-2.092		4.603		4.603	
				(4.297)			(4.297)		(5.118)		(5.118)	
ECD1												
All households	478	46.025	51.046	-5.021	74.059	76.151	-2.092		2.929		2.929	
				(4.280)			(4.280)		(5.086)		(5.086)	
Households with children under 6 years	261	73.134	79.528	-6.393	82.836	92.126	-9.290	**	-2.897		-2.897	
				(4.721)			(4.721)		(6.143)		(6.143)	
ECD2												
All households	478	71.967	74.477	-2.510	80.753	80.753	0.000		2.510		2.510	
				(3.842)			(3.842)		(4.422)		(4.422)	
Households with children under 6 years	261	94.776	94.488	0.288	91.045	96.850	-5.806	**	-6.094		-6.094	
				(2.882)			(2.882)		(3.904)		(3.904)	

Notes: Each row in the table is from a separate regression. Standard deviations in columns (3), (6) and (7) are adjusted by clustering at household level. Coefficient and standard deviations in column 8 control for household-level fixed effects. Double difference estimates are reported as measures of impact. * significant at 10%; ** significant at 5%; *** significant at 1%. Dependent variables are defined as follows. Food programs include the following: a) Glass of Milk Program, Nurturing Basket Program (PANFAR), b) Food for Children Program (PACFO), c) Food for the sick and the elder and d) Cheap eateries. Education Programs include the following: a) Breakfast or food for school students, b) School uniforms and school footwear, c) School books and schooling material, d) Student insurance at school level, e) Juvenile job training and d) Job training for women. Health Programs include de following: a) Control of children´s growth and development (CRED) , b) Family planning, c) Control of Tuberculosis and d) Vaccination Program. ECD1 denotes Early Childhood Development Programs, includes the following: PANFAR, PACFO, CRED, Vaccination Program. ECD2 adds to ECD1 the Glass of Milk Program.

Table 12: Number of observations per group of analysis

		Education		Village Size			Altitude			Gender		Age			Mother Tongue	
	Total	Lower	Higher	Small	Medium	Large	Low	Medium	High	Female	Male	Younger	Middle	Older	Quechua/ Aymara	Spanish
Motorized tracks																
Households	1521	965	556	510	480	344	407	402	443	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
All individuals	7574	4849	2725	2431	2471	1769	2082	1970	2193	n.a.	n.a.	n.a.	n.a.	n.a.	4936	2638
All members > 15 years old	3642	n.a.	n.a.	1360	1321	961	1127	1090	1193	2064	2077	1270	2091	780	2594	1547
Children 0-5 years	1396	881	515	424	465	334	389	351	399	n.a.	n.a.	n.a.	n.a.	n.a.	964	432
Boys																
Males 6-11 years	711	459	252	215	263	157	210	188	198	n.a.	n.a.	n.a.	n.a.	n.a.	483	228
Males 12-18 years	612	403	209	194	198	144	168	149	183	n.a.	n.a.	n.a.	n.a.	n.a.	401	211
Girls																
Females 6-11 years	734	466	268	239	233	180	195	197	218	n.a.	n.a.	n.a.	n.a.	n.a.	513	221
Females 12-18 years	521	331	190	165	168	124	144	130	153	n.a.	n.a.	n.a.	n.a.	n.a.	332	189
Non-Motorized tracks																
Households	474	303	171	185	126	111	89	124	168	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
All individuals	2348	1501	847	890	633	555	431	603	856	n.a.	n.a.	n.a.	n.a.	n.a.	1319	1029
All members > 15 years old	1181	n.a.	n.a.	488	350	343	230	362	478	662	660	421	661	240	732	590
Children 0-5 years	369	228	141	153	103	78	78	80	146	n.a.	n.a.	n.a.	n.a.	n.a.	223	146
Boys																
Males 6-11 years	232	156	76	83	62	48	37	58	79	n.a.	n.a.	n.a.	n.a.	n.a.	130	102
Males 12-18 years	212	143	69	83	51	53	42	52	80	n.a.	n.a.	n.a.	n.a.	n.a.	114	98
Girls																
Females 6-11 years	222	143	79	95	67	31	42	49	85	n.a.	n.a.	n.a.	n.a.	n.a.	129	93
Females 12-18 years	172	98	74	55	50	52	32	57	58	n.a.	n.a.	n.a.	n.a.	n.a.	89	83

Notes: Heterogeneous categories are defined as follows. Education: Lower is for household head with primary or no education; Higher, with secondary or higher education. Village size: Small is for villages with less than 300 inhabitants; Medium, with more than 300 to 850 inhabitants; Large, with more than 850 inhabitants. Altitude: Low is for villages which are 2500 meters above sea level, Medium, between 2500 and 3400 meters above sea level, High, from more than 3400 to 5500 meters above sea level. Gender: if the individual is female or male. Age: Cohort 1 is for indivuduals younger than 25 years old; Cohort 2, from 26 to 50 years old; Cohort 3, older than 50. Mother tongue: if the mother tongue of the household head is quechua/aymara or spanish.