NERAP Final Report

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Executive Summary

NERAP's main objective was to provide year-round access to basic services and facilities in the rural areas of Afghanistan. The impact evaluation was designed to match those roads that had benefitted from a NERAP rehabilitation project with those that had not in order to estimate the effect of the program.

When considering the gains among the treatment group alone, NERAP achieved its primary objective of increasing access to all-access roads in rural areas and met other Key Performance Indicators, such as increased access to economic centers, increased accessibility of services, increased uptake of services, and diversification of income, along with increased consumption.

However, the impact of NERAP relative to not having a roads rehabilitation project is unclear. There were multiple problems with the data, due to poor survey design and security concerns. The greatest issue with the data stemmed from problems involving contamination; NERAP's impact is obscured by ongoing and concurrent projects in both treatment and control villages. Due to these concerns, the estimates provided in this paper, which are almost exclusively statistically insignificant, should be considered as lower bounds.

Introduction

Project Overview

The National Emergency Rural Access Project (NERAP) aimed to help improve accessibility, integrate village economies with regional and national markets, and stimulate a more efficient allocation of resources and an increased level of productivity and economic output.

NERAP was a component of the Government of Afghanistan's National Rural Access Program (NRAP), a multi-donor effort executed jointly by the Ministry of Rural Rehabilitation and Development (MRRD) and the Ministry of Public Works (MPW). The main objective of the program was to create rural access network that connects communities across all of Afghanistan's 34 provinces to essential services and markets.

The program was not focused solely on the rehabilitation of roads, but also on the improvement of rural accessibility and the integration of Afghan communities into regional and national markets. Further, it was expected that such improvements in accessibility would trigger second-order economic benefits, such as an improvement in the efficiency of resource allocation, higher productivity and outputs, and the diversification of household income sources to reduce vulnerability to external shocks. It was hoped that improved accessibility would also lead to more use of basic services, such as education and healthcare.

The Project Development Objective (PDO) of NERAP was to "provide year-round access to basic services and facilities in the rural areas of Afghanistan covered by the project." The program's Key Performance Indicators (KPIs) reflect the expectation that successful road rehabilitation projects provoke significant improvements in accessibility and the functioning of local markets. The KPIs for monitoring the achievement of the PDO included:

- After completion of a road, the number of trips taken by beneficiaries living along the improved road to district centers would increase by 30 percent
- After completion of a road, travel time of beneficiaries living along the improved road to the first available schools, health care facilities and administrative services would be reduced by 30 percent

Villages were selected for the project based on five main criteria. First, many villages chosen were part of former priorities still not financed from other projects, such as the Provincial Planning Process and the National Emergency Employment Program (NEEP). NEEP priorities involved 12 provinces affected by drought. Second, community requests were taken into consideration. These requests could be direct requests from communities or come through other administrative channels. Third, villages were chosen in order to consolidate works to sustain previous rural road investments. Fourth, under the theory that highly rural areas are more poppy dependent, villages that were highly isolated were also prioritized for the project. Finally, villages were chosen due to complementarities with other social and rural development projects.

There were 413 planned constructions or rehabilitations under the NERAP program, consisting of 1,364 kilometers of road and 1.6 kilometers of bridges. This broke down into 365 road projects and 44

bridge projects.[1] Of the 413 planned projects, 64 percent were completed, based on monitoring data, by the start of endline surveying in September 2012. Six projects were completed before the baseline, while the last one was completed in October of 2013.[2] The average construction length was 3.7 kilometers for roads and 0.04 kilometers for bridges. Road construction length ranged from 30.1 kilometers to 0.1 km. The longest bridge construction was 0.12 kilometers and was in the Parwan Province.[3]

The impact evaluation was carefully designed prior to the implementation of the project. It was planned to match those roads that had benefitted from a NERAP project with those that did not in order to estimate the effect of the project. Unfortunately, as we will see, this approach was complicated by other road projects being implemented at the same time. The net effect of this is that those villages that were not near a NERAP project were almost as likely to receive a road project as those that were. This issue will be discussed in more detail in a later section, but it means that we cannot truly judge the effect of the program relative to not having received any road project, only the effect of the program relative to the other projects underway at the time.

Given this constraint, overall we find only suggestive evidence that NERAP improved access and increased the number of trips being made. The evidence for effects on second-order outcome variables such as the uptake of health or education services is still weaker, with only some slight evidence of gains in consumption for several key food items.

[1] Some projects were done in conjunction with each other, such that a bridge and road project would complement each other. As such, the individual bridge and road projects do not add up to 413.
[2] These were not included in the experimental sample.
[3] Information comes from the Awarded Contracts of NERAP internal document.

Evaluation Approach

Impact evaluations rely on identifying a counterfactual: what would have happened in the absence of the project. Ideally, selection bias would be eliminated through a randomized control trial, but this was impossible for NERAP. Instead, the evaluation tried to use information collected about the areas in which road projects were undertaken and about a large collection of roads not affected by NERAP to create a synthetic control group of roads that did not receive treatment but are otherwise similar to those that did.

To find a suitable control group, a matching procedure was used to identify unselected roads that were similar along important characteristics to selected roads. The procedure, known as propensity score matching, quantifies the selection process by identifying characteristics that increase the likelihood of a road to be selected to receive the program. A control group was then chosen from unselected units which had "propensity scores" similar to the treatment units.

Evaluations of road projects present a unique set of challenges. Roads are designed to connect clusters of villages. This introduces two new challenges for the assumptions necessary for matching. First, the selection of road projects will depend upon the characteristics of villages around the road, not just one village. Second, any village's response to road rehabilitation will depend upon the response of surrounding villages. Hence, if matching is done at the village level without considering the characteristics of villages around the road then the estimate of the causal effect will be biased.

This interaction between units would violate the standard assumptions usually used to justify matching to identify causal effects (Rosenbaum and Rubin, 1983). Therefore, matching is instead based upon the aggregate characteristics of the villages that lie along roads. The level of assignment - clusters of villages that lie near a treated or control road - is, as a consequence, not the same as the level of analysis - the effect of the program on villages or households.

The evaluation split the sample between a treatment group of villages that lay along 73 roads to be rehabilitated under NERAP and a control group of villages that lay along 128 roads with similar characteristics to NERAP roads but which were not scheduled for rehabilitation through NERAP. Surveyed control group villages were initially selected through propensity score matching based on crude geographical and demographic characteristics of roads and villages obtained from the Government of Afghanistan. It was understood from the beginning that the data on which the initial matching was based was of poor quality; therefore, more control groups were identified than treatment villages with the understanding that after the gathering of more information during baseline surveys, the villages would be re-matched for the final analysis. In total, the sample covered approximately 5,660 households, 560 villages, 227 treatment villages and 342 control villages, across 120 districts in 22 provinces of Afghanistan.

Ultimately, we match using nearest-neighbor, 1:1 matching, using a propensity score (or likelihood of being selected into the program) to ensure the treatment and control groups are as similar as possible. We also try two ways of adjusting standard errors to deal with spatial issues: clustering by road and using Conley's spatial clustering correction. A more in-depth discussion of the methodology used in this report can be found in the Appendix.

Data

Surveys

To collect data for the impact evaluation, NERAP relied on four different surveys. The sections and indicators contained in these instruments were selected according to their importance in measuring the effect of the intervention on the expected outcomes, their applicability to rural Afghanistan, and how accurately and cost-effectively they could be measured in the field. GPS coordinates for each participating household were also collected in order to facilitate locating the households within the villages for the follow-up survey, and the geographic encoding of the data sets allowed them to be matched to geographic data about the project. A summary of each of the four surveys follows.

1 ***Household Surveys:*** One household survey with two types of questionnaires was administered in each round of data collection: one for men and one for women. The male household questionnaire was administered to male household heads in 10 randomly selected households in each village at baseline, but only 7 in the follow-up. The female household questionnaire was administered to wives of male household heads in the same randomly selected households. These surveys contained basic household-level information on income; labor; education; household composition; and assets; as well as detailed information on the household's transport costs and needs. Regarding the last of these, the questionnaires asked questions such as whether the household owned any means of transport; average transit times and transport costs between the household and the nearest school, clinic, police, government administrative unit; *etc.*

2 ***Village Focus Groups:*** Focus groups were convened in treatment and control villages. There were two focus groups per village in the baseline survey: one male and one female. At follow-up, only the male focus group survey was implemented. Survey teams, in coordination with the evaluation team, established the criteria for selecting participants. The male focus group questionnaire was administered to 6 – 9 village leaders, such as CDC members, shura members, village headmen, and tribal elders. The female focus group was administered to 6 – 9 prominent women, such as CDC members, wives of shura members, and educated women. These surveys contained information on transport services at the village level: the frequency of services; their reliability; their cost; any unmet demands for transport; *etc.*

3 ***Road Survey:*** This survey targeted owners of minibuses and other commercial means of transport. Sampling was based on information provided by the focus groups on transport providers serving the village or district center. The interviews described the distance from the village center to the nearest road and the condition and type of the road. Data on road quality used for matching came from this survey.

4 ***Spot Market Surveys:*** Enumerators in district markets and villages conducted this survey. It solicited information on the prices of key commodities. This survey was ultimately not used in the analysis, since it had a relatively small sample size that was only further reduced when matched to the household and focus group surveys due to the other factors affecting sample size such as attrition.

The data were collected in two phases: a baseline survey and an endline survey. Sixty enumerators recruited by the Vulnerability Analysis Unit (VAU) of MRRD undertook administration of the baseline survey. Enumerators were selected based on a competitive examination administered following a weeklong training workshop and were carefully monitored by supervisors from VAU as well

as by World Bank staff and consultants. The baseline survey was launched in August 2008, prior to the commencement of program activities along treatment roads, and was concluded in November 2008. The endline survey was conducted by a private firm, the Opinion and Research Center for Afghanistan (ORCA), under strict monitoring of the World Bank, beginning in September 2012 and concluding in December 2012.

Issues with Data

A few problems came up during the implementation of the impact evaluation which affected the data that were collected and consequently what one can say with those data. These problems can roughly be categorized as problems with survey design; problems in survey administration; and problems relating to non-response.

First, the baseline and endline surveys had some design limitations. Questions often did not appear or were modified in both the baseline and the endline survey, limiting their usefulness. Further, some questions moved between surveys. For example, a question regarding child's health might be asked in the male household survey in the baseline but appear in the female household survey in the endline. Since the men and women might answer the question differently, this complicates the interpretation of the values given.

Worse, no adequate price data was collected during the baseline surveys. Initially, the goal was to be able to measure price dispersion between the local villages and the nearest markets, with the hypothesis that the price dispersion would decrease as roads were rehabilitated. The male focus groups were asked the price of different goods, but since they were not asked where they bought the goods to which they were referring, we could not use these data to construct measures of price dispersion as initially planned. The questionnaire did not provide information on which prices referred to goods bought locally versus in other villages.

Another survey was conducted to specifically gather data on market prices, but this had the limitation of a relatively small sample size. Due to other factors also diminishing the sample size, such as attrition and lack of follow-up, we do not end up using these data, as only 269 observations remain after these factors are taken into consideration and after matching.

In addition to these design flaws, the project encountered some administration issues. During the validation process of the data, the Quality Control Teams in Afghanistan found the fieldwork of Badakhshan Province to be of low quality. Therefore, the entire work of Badakhshan was discarded and a newly trained team of interviewers and engineers travelled to the field together with a staff member from World Bank headquarters. Thus, the data from this province were ultimately collected at a later date than the rest of the surveys.

Security remained a major challenge in conducting the endline survey. The security situation had deteriorated drastically by 2012 as compared to 2008, when the baseline survey had been completed. An agreement was reached between the ORCA and the World Bank that, if a village was reported by ORCA team to be insecure, that village would not be surveyed only after the World Bank team independently verified the security conditions in that village. The real security threats in some of the provinces made a total of 68 out of 567 villages too risky to be surveyed. These were dropped from the sample villages. Therefore, 88 percent of the villages surveyed for the baseline were surveyed again for the endline.

Within these villages, not all of the households surveyed at baseline could be located at follow-up. Further, many of the "unique household identifiers" at endline were not, in fact, unique. In order to

ensure that we were looking at the same people at baseline as in endline, these also had to be dropped. Finally, propensity score matching only preserved those roads which had the most in common. Of the 490 villages along a treatment or potential control road, 118 were dropped in the course of the matching due to their road not having a good match; the matched sample ultimately included 98 roads. In the end, of the respectively 5660 and 5640 baseline male households and female households who were surveyed, results from 2661 and 2645 remained after propensity score matching. Of the villages, out of the 566 initial baseline villages, 372 remained after propensity score matching. The Table 1a (below) summarizes, and characteristics of the matched and unmatched roads are presented in the Methodology section.

Table 1a: Survey attrition

Male household survey					
Baseline: households	Baseline: unique households	Endline: households	Endline: unique households	Successful baseline/ endline matched households	Propensity score matched households
5660	5660	4066	3508	3501	2661

Female household survey					
Baseline: households	Baseline: unique households	Endline: households	Endline: unique households	Successful baseline/ endline matched households	Propensity score matched households
5640	5640	4056	3508	3485	2645

Focus group survey					
Baseline: villages	Baseline: unique villages	Endline: villages	Endline: unique villages	Successful baseline/ endline matched villages	Propensity score matched villages
566	566	506	506	490	372

Finally, even when similar questions were asked both in the baseline and in the endline surveys, to the same people, respondents did not always answer the questions fully. Few people, for example, admitted to having anything to do with the opium trade, which is understandable and was to be expected but does limit the questions we can answer using the data.

Initial Matching and Checks

An initial analysis of baseline characteristics was conducted in 2011 and compared all of the treatment villages with all of the control villages.[4] The report looked at road access, travel to district center, market travel information, prices, and income and farming diversity. These characteristics paint a

[4] Callen and Kuhn 2011.

portrait of the villages, and show the need for re-matching, as we did in this analysis. Overall, the analysis found that the treatment sample consisted of locations that were somewhat more connected to district center and somewhat worse off economically than their control counterparts. Therefore, the re-matching in this paper paid particular attention to these issues.

Overall, the baseline access to a road was not different in treatment and control villages. The average distance from a village to a road was 1.5 km, with 80 percent of villages lying within two kilometers of a road. However, only 40 percent of the nearest roads to villages were open year-round. This left only about 35 percent of villages within 2 kilometers of an all-season road.

The initial baseline analysis did find differences in connection to the district center. Using a mix of household surveys and focus group surveys, they found that road quality is poor, 41 percent of people walk to the DC, the cost of a trip to the DC is about a day's wage, and 63 percent of the sample reports taking non-motorized transport to the DC, presumably because roads and paths do not easily support motorized transit. However, the travel time to the DC was roughly 1.1 hour less in treatment villages as compared to unmatched control (1.3 hours in treatment versus 2.4 in unmatched control). This was significant at the 1 percent level. In addition, more treatment villages were less than 1 hour to DC, fewer treatment villages were more than 3 hours to DC, and fewer respondents in treatment villages claimed that vehicles never traveled to the DC.

Further, the initial analysis found suggestive evidence that farmers in the treatment sample are somewhat less diverse and flexible than their control counterparts. They cultivate significantly fewer summer crops and appear to depend on rain more frequently. Therefore, a well-matched sample would better match, among other things, mean time to district center, mean number of trips to district center, and the mean income of individuals.

Contamination Evident in Endline Data

A key issue that affects the interpretation of all results is whether the control roads were subject to rehabilitation projects, whether the treatment roads were also subject to rehabilitation projects outside of NERAP, and whether the treatment roads were actually upgraded before the endline survey.

Two parts of the focus group endline survey asked about road projects. For robustness, we will look at both.

The first question asked whether a road project had been started or completed in the last 5 years. 59 percent of control focus groups at endline said that a road project had been started or completed in the last 5 years, compared to 70 percent of treatment focus groups. While the gap between the treatment and control group is disappointingly small, the treatment groups were clearly more likely to have received a road project under this measure (with a p-value of <0.001). Matching somewhat reduced the extent of the problem, suggesting that there was selection bias both in the control roads that received projects and in the treatment roads. In the sample most frequently used throughout this paper (detailed in the section on the matching fit), 58 percent of the control focus groups responded that a road project had been started or completed in the last 5 years, compared to 75 percent of the treatment focus groups.

The second question asked when the most recent road project was started and completed and who implemented it.[5] Respondents were also asked how far away the project was. The four tables below show the start and end years of the most recent project started in 2008 or later, on both the full sample and, separately, the main matched set used in this paper.[6] The two questions were asked in different sections of the survey and the responses are not always consistent across questions.

Table 1b: Projects started 2008 or later, unmatched set

Start year	Percent of treatment villages starting a road project that year	Percent of control villages starting a road project that year	Percent of treatment villages having started a road since 2008 (cumulative)	Percent of control villages having started a road since 2008 (cumulative)
2008	10.1	6.0	10.1	6.0
2009	16.4	12.6	26.5	18.6
2010	11.6	15.0	38.1	33.6
2011	13.8	19.6	51.9	53.2
2012	7.4	6.3	59.3	59.5

Table 1c: Projects finished 2008 or later, unmatched set

End year	Percent of treatment villages finishing a road project that year	Percent of control villages finishing a road project that year	Percent of treatment villages having finished a road since 2008 (cumulative)	Percent of control villages having finished a road since 2008 (cumulative)
2008	5.3	0.3	5.3	0.3
2009	7.9	9.3	13.2	9.6
2010	7.9	12.3	21.2	21.9
2011	14.3	17.3	35.4	39.2
2012	7.4	7.6	42.9	46.8

Table 1d: Projects started 2008 or later, matched set

Start year	Percent of treatment villages starting a road project that year	Percent of control villages starting a road project that year	Percent of treatment villages having started a road since 2008 (cumulative)	Percent of control villages having started a road since 2008 (cumulative)

[5] Specifically, respondents were asked "Are there any development projects in the village that are currently in progress or which have been in progress in the past 5 years." If respondents answered yes, they were given an option to check of the kind of project and then were asked "Who is paying for these development projects?"

[6] It should be noted that the year 2008 and all subsequent years are considered to have begun in March, in accordance with the Solar Hijri calendar.

2008	10.7	5.2	10.7	5.2
2009	16.9	11.9	27.5	17.0
2010	12.4	16.0	39.9	33.0
2011	12.9	21.1	52.8	54.1
2012	6.7	7.7	59.6	61.9

Table 1e: Projects finished 2008 or later, matched set

End year	Percent of treatment villages finishing a road project that year	Percent of control villages finishing a road project that year	Percent of treatment villages having finished a road since 2008 (cumulative)	Percent of control villages having finished a road since 2008 (cumulative)
2008	5.6	0.5	5.6	0.5
2009	8.4	8.8	14.0	9.3
2010	8.4	12.9	22.5	22.2
2011	15.2	20.1	37.6	42.3
2012	6.2	7.7	43.8	50.0

There is even less difference between the treatment and control villages according to the responses to this question - if anything, more control villages received road projects than treatment villages.

We can also look at the distribution of project implementers. For both treatment and control groups, the most recent road project was usually an NSP project (Figure 1a and Figure 1b), pointing to the vast number of on-going projects in the areas under consideration. Even though we ensured that the control villages were no less than 5 kilometers away from a treatment road, to avoid contamination, some control villages reported being near a NERAP road project. This would make sense if the focus group responses were based on considering roads that were farther away; villagers were unsure who built the road; villagers built their own connecting roads to a farther away NERAP road and then considered that when responding; or villagers were influenced by the enumerators and reported what they thought the enumerators wanted to hear. Further, the categories referred to in the survey do not clearly indicate NERAP status. For example, the Ministry of Public Works (MOPW) projects are counted separately from NERAP projects in the survey, though many NERAP roads were MOPW roads; the NSP was implemented by the MRRD. It could be difficult for respondents to accurately identify whether their road project was a NERAP project, particularly if they were to see the same agencies implementing several different projects.

Figure 1a: Implementing agency of most recent project, treatment villages, matched set

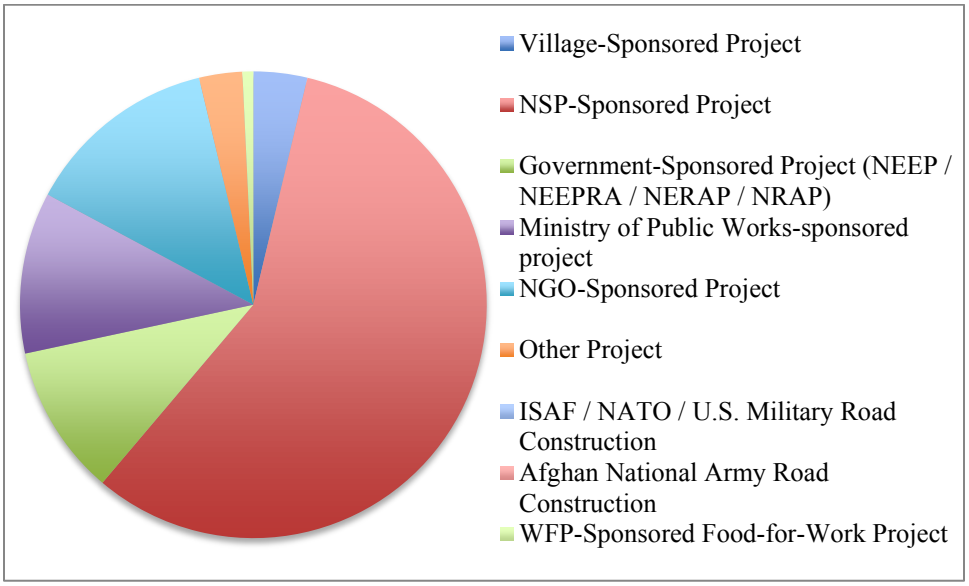
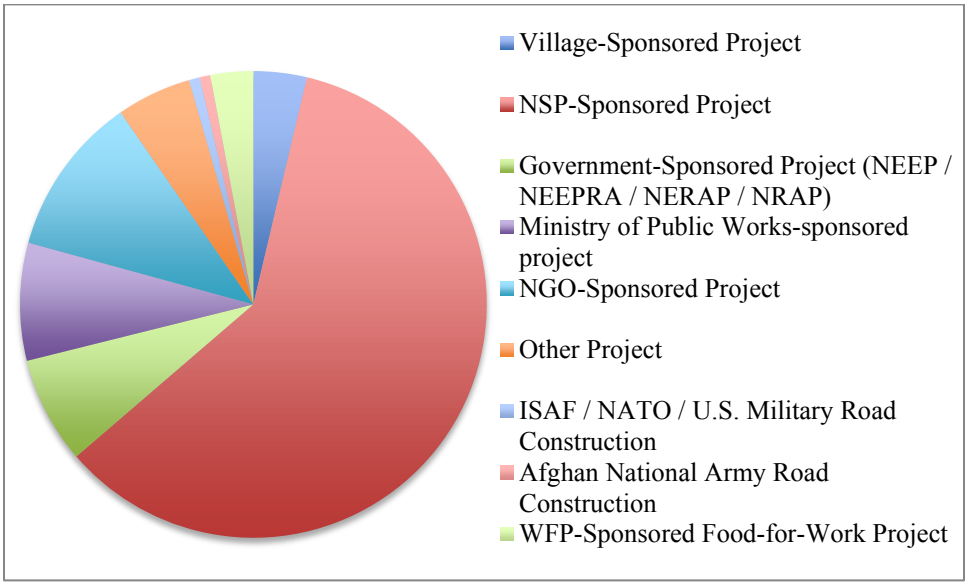


Figure 1b: Implementing agency of most recent project, control villages, matched set



The average control village focus group that had reported a NERAP road, excluding those that could not say how far away the road was, reported it to be 2.8 kilometers away, compared to 2.2 kilometers for the treatment village focus groups. While farther away, it cannot explain all of the discrepancy between what we would expect and what is reported in response to this question.

The distances used in this paper are as-the-crow-flies distances rather than the "true" distances perceived on the ground, which may differ depending on geographical artifacts, but they can only understate the true distances. Thus, it seems more likely that the responses to this question reflect respondent error, illustrating the kinds of data quality issues endemic throughout these surveys.

It should be noted that it was initially suggested that control villages be considered "contaminated" if they were within 2 kilometers of a NERAP road, as it was thought that this was the distance beyond which we may not expect to see many spillover effects. Taking 5 kilometers as the cut-off threshold is more conservative and appropriate given all the data issues, although the results presented in this paper are comparable when using the alternative cut-off threshold of 2 kilometers.

While these survey questions suggest that roads were also built in control areas, they do not say anything about the magnitude of the road quality improvements, which may also have differed between treatment and control areas. The control groups that did receive a road project reported more satisfaction with their projects than the treatment groups which received a road project: 89 percent compared to 78 percent (p-value of <0.001). If we take these reports at face value and use the more optimistic first focus group measure, at endline 52.5 percent of control areas received a road project with which they were satisfied in the past 5 years and 54.6 percent of treatment areas received a road project with which they were satisfied in the past 5 years; while, overall, treatment areas still fare better, the gap between them has narrowed substantially. The treatment and control areas reported comparable rates of road construction (11 percent and 11 percent, respectively) and road rehabilitation (53 percent and 55 percent, respectively).

As noted earlier, the large number reporting a roads project in the control group implies that the results of this study should be regarded as measuring the difference between the NERAP roads projects and the other roads projects that were completed during this time. They are thus a lower bound for what we might expect the effects of NERAP to be compared to not having received any roads project. That we might expect this effect to be smaller than the effect relative to not having received any roads project implies that the statistical power of the impact evaluation is also lower, making it less likely for any effect to be found to be statistically significant. Compounded with the security issues, attrition, non-unique household identifiers, non-response rates to individual questions, and the necessity of having common support, we are not likely to observe any significant effects.

Methodology

This section discusses the similarities and differences between the treatment and control group after matching. A more detailed discussion of the issues associated with measuring the impact of NERAP and how they were addressed in the impact evaluation is included in the Appendix.

Matching Fit

As mentioned, the initial matching done in 2011 found that the treatment villages were somewhat more connected to economic centers and somewhat worse-off economically than their control counterparts. Therefore, for re-matching, we sought to mitigate these issues and built upon the initial matching variables (found in Appendix). To address the issue of treatment villages being more connected, we added matches for the mean time to nearest hospital, mean time to nearest school, and mean time to the district center. We also matched based on the mean number of trips to the district center (which was imbalanced in the initial check) and maximum speed of the road, which could affect motorized transportation to economic centers. Further, in this set of matching variables, called Matching Set 1, we added mean income of individuals. As a robustness check, and due to issues with self-reported income, we also matched based on mean expenditure of individuals instead of income. This set of matching variables we called Matching Set 2, and results based on it are included throughout the Appendix. The full set of matching variables can be found in the Methodology Appendix under Table 1g.

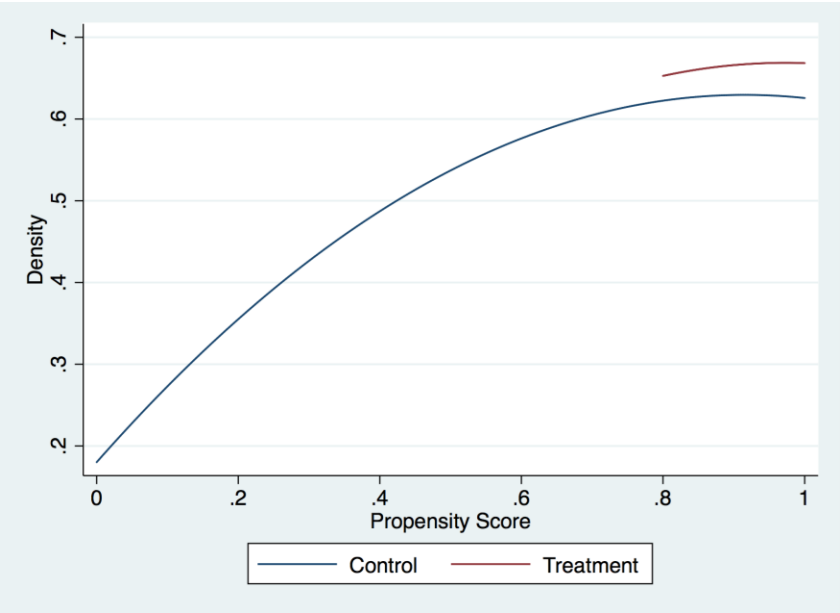
The key summary statistics describing the fit of the matching set that was ultimately chosen are included in the table below, with results for MOPW and MRRD roads presented separately as the roads that MOPW and MRRD covered were qualitatively different, MOPW road projects focusing on secondary roads, while MRRD projects focused on tertiary roads. The full summary statistics can be found in the Methodology Appendix under Table 1h and 1i. As we can see, the three variables we were most worried about in the earlier matching-- mean time to district center, mean number of trips to district center, and the mean income of individuals—are better matched now than they were before matching and in the previous-matched set. The lower the p-value, the less likely the differences in the mean are due to chance.

Table 1f: Key matching variables fit after matching

		Pre-Matching			Post-Matching		
	Variable	**Treat**	**Control**	**P-value**	**Treat**	**Control**	**P-value**
MOPW Projects	Mean time to district center	3.664	5.474	0.355	3.664	5.499	0.475
	Mean number of trips to district center	7.311	6.897	0.257	7.311	7.129	0.653
	Mean household income (AFS)	87183.000	91432.000	0.535	87183.000	89851.000	0.755
MRRD Projects	Mean time to district center	2.312	3.231	0.106	2.312	2.939	0.277
	Mean number of trips to district center	7.264	6.346	**0.060**	7.264	6.549	0.116
	Mean household income (AFS)	95682.000	82457.000	0.129	95682.000	85160.000	0.362

The fit can also be summarized by the distribution of the propensity scores for the treatment and control group, illustrated in Figure 1c. The shorter line in the diagram represents the distribution of the propensity score for those in the treatment group, given their covariates; the longer line represents the distribution for those in the control group. Clearly, by restricting attention to only the area with common support, we can gain closer matches.

Figure 1c: Distribution of propensity scores



Descriptive Statistics of Baseline Survey

This section describes key responses to the Baseline Survey. The sociodemographic characteristics only relate to those who were surveyed, but unlike in the Baseline Report, these characteristics come from both the treatment and control villages. Due to the fact that the sample design does not allow the analysis to be representative of Afghanistan, this section allows us to understand the sociodemographics of the subset of the Afghan population located close to the treated roads or in areas that could be surveyed. It should not, however, be considered to provide a picture of the population living near all NERAP projects (as it is representative only of the "secure" areas), nor of Afghanistan as a whole.

Overall, the matched sample of villages and households show poorly connected farmers with low education and income diversity. This aligns with the initial targeting of NERAP projects by providing roads to people most disconnected from services and economic centers. The average village is situated within 1.5 kilometers of a road, and 80 percent are within two kilometers. However, only around 40 percent of the nearest roads to villages are open year round. This leaves only about 35 percent of villages within 2 kilometers of an all-season road.[7] This is important as over one-third of matched respondents used motorized transport, which could be greatly affected by increased road quality. These respondents also reported travelling travelled long distances to access economic centers. Due to this poor connection, building roads in these highly rural areas should lead to larger gains than one would expect in cities. We may expect to see some convergence between treatment and control areas, given the discrepancies remaining after matching.

Not only were villagers poorly connected, but they also had to travel long distances to access basic services. As such, their uptake of services was low. On average, women at baseline had to travel 1.3 hours to the hospital. Therefore only 21 percent even when to a hospital in the last year and 42 percent went to a clinic. Further, just one in five of those who claimed to have given birth in the past year did so at a medical facility.

Villagers were mostly small subsistence farmers and very few had received any education. Sixty-three percent of male respondents were farmers, and twelve percent were professionals. A third of farmers travelled to the district to sell their crops. Fifty-nine percent of households cultivate their own land, which average size is 2.12 hectares. Over half of male respondents have access to irrigation in the form of rivers, canals, dams, or a deep well. The most important crops harvested during the last summer are wheat and barley. Only 21 percent of the households sold their production in the last year, from which 55 percent sold it in the market. Thirty-seven percent sold it to a middleman. Male respondents were also asked their highest level of schooling. Over 60 percent of male respondents claimed they had no schooling.

One of the goals of NERAP was to build roads that would decrease the impact of shocks by helping to diversify income sources. 46 percent of the men surveyed in the household survey did some work off their farm at baseline, as did 30 percent of the women. One-third of the women surveyed also reported working outside of the home in the past year. The average household income, which includes both primary and secondary sources of income, was estimated to be 80923 AFG per year.

[7] This analysis was originally performed in Callen and Kuhn 2011

Results

In order to determine the impact of the NERAP project, we looked at five categories of outcomes: access, accessibility of services, uptake of services, diversification of income, and consumption. These categories were chosen in order to account for and expand the initial key indicators for success.

The indicators for access include hours to district center, number of trips to district center, access to an all season road, number of people using motorized transport, and costs of travel to various important locations. These were closely tied to the Key Performance Indicators (KPIs), which considered travel time to the district center and number of trips as important measures of access. Access to an all-season road was similarly selected as an indicator in the *Baseline Report on the Evaluation of NERAP (Baseline Report)*[8] and subsequent *Analysis of NERAP Baseline Data (Baseline Analysis).*[9] It was also listed as the main objective of NERAP in the PDO. Further, we believed that transport costs to a variety of important centers, such as the province center, district center, closest big city and market, could also reflect accessibility.

The outcome variables relating to accessibility of services were derived directly from the KPIs, which stated in part that "travel time of beneficiaries living along the improved road to the first available schools, health care facilities and administrative services would be reduced." To examine whether NERAP had an effect on access to these services, we first considered the hours it took to travel to a hospital and whether respondents used a local clinic or hospital. We do not focus on educational services as people only rarely used motorized transit to go to school, so we may not expect NERAP to have any effect on reducing travel time or increasing access, particularly given the existing concerns with low statistical power inhibiting our ability to discern any effects. Finally, administrative services were assumed to be in the district or Provincial Centers and therefore part of the earlier set of outcome variables.

We also decided to examine uptake of services, as plausible second-order effects of any increased accessibility of services an due to strong client demand. Thus, we looked at use of hospital facilities for giving birth, the percent of children vaccinated, and the number of boys and percent of boys in school. Subsequently, uptake of services was derived as an indicator in order to make the indicator for accessibility of services more robust. Here we would be able to include information relating to schooling, such as the number of boys in school and the percent of boys in school. Unfortunately, data on girls' school attendance was not collected in a comparable fashion in the baseline survey, precluding its inclusion here.

Our fourth set of results focus on diversification of income. This topic was initially specified in both the *Baseline Report* and *Baseline Analysis.* In the *Baseline Report,* the authors explain that road construction is meant to trigger second-order economic benefits, such as diversification of income due to increased economic linkages. In the *Baseline Analysis,* diversification of off-farm activity becomes a key indicator. We therefore examined several outcome variables relating to this topic, namely the percent of men and women engaging in off-farm economic activities, as well as whether women worked outside the house.

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[8] Minaya 2010
[9] Callen and Kuhn 2011

Finally, we looked at consumption as a measure of welfare. We looked at whether a respondent claimed they had issues satisfying the food needs of their household. We also looked at the reported consumption of several key consumption goods: flour, rice, beef, lamb and eggs.

The NERAP impact evaluation was initially meant to also consider prices, under the theory that increased road connectivity would smooth the price differentials between villages. However, the baseline survey did not gather adequate price data. The male focus groups were asked the price of different goods, but since they were not asked where they bought the goods to which they were referring, we could not use these data to construct measures of price dispersion as initially planned. The questionnaire did not provide information on which prices referred to goods bought locally versus in other villages. As mentioned earlier, another survey was conducted to specifically gather data on market prices, but this had the limitation of a relatively small sample size, prohibiting analysis.

As noted in the discussion in the Methodology section, the main results focus on the set of treatment and control roads that come out of the nearest neighbor, 1:1 matching based on the covariate balancing propensity score. The disadvantage of focusing on this set of roads and consequently nearby villages and households is that it is a fairly limited sample, and power is a concern. On the other hand, the values that we see in this restricted set are much more plausible than when we do not restrict attention to the 1:1 matched set. It appears that the more distant matches are indeed much worse matches. We will have to accept the possibility that some results may appear insignificant due to the relatively small number of clusters. Therefore, we may fail to see some impacts of the project.

All outcome variables that were not limited to a set range (as in variables reporting percentages, which run from 0 to 100 percent) were winsorized at 1 percent, due to noise in the data. We restricted our attention to those households and villages who responded to both the baseline survey and the endline survey. As a further robustness check, we specifically focus on those reporting using cars. These results are in the Appendix but we cannot expect to see any significant results there due to lack of power. Similarly, we check whether results are different in those villages that are farther away or have smaller populations, but find no significant differences.

We also limit attention to those reporting using a motorized means of transport[10] for all questions involving travel time or number of trips. This is because we might expect that those who report using a motorized means of transportation are the most likely to experience decreases in travel time and increases in the number of trips taken. There are two ways in which access could improve: (1) it is possible that with newly rehabilitated roads, more people start using motorized transport; (2) people using motorized transport could experience reductions in travel time due to the improved quality of roads. We first explore (1) and find no difference in the proportion of people using motorized transport. This then allows us to look at (2) by focusing on just people using motorized transport. If we found differences in (1) we would not be able to use this approach in (2) since we would likely be underestimating the impact since we would miss the effect of increased use of motorized transport. However, since we find no effect on (1), this is not a problem and we can get a plausible estimate of (2) by restricting to the subset of households using motorized transport.

With these points in mind, we turn to discussing each set of results in turn.

Access

[10] Bus, minibus, private car, rental car with driver, truck, taxi, or motorcycle.

We first looked at whether roads increased access to the district center (DC). To measure this, we used data from the male household survey on the number of hours it took to travel to the district center and the frequency of trips to the district center. We also used a focus group survey question that asked about whether the nearest road was accessible year-round. The results are limited to those respondents who reported using motorized transport, as we may believe that those who do not use motorized transport are less likely to be affected by the project. Motorized transport is considered any form of transport that is not walking, bicycling, or by animal, and includes bus, minibus, and private car, rental car with driver, truck, taxi, or motorcycle.[11] Again, the share of those using motorized transport does not differ between the treatment and control group, allaying concerns that by increasing the percent using motorized transit, a focus on those using motorized transit would bias the results.

Table 3 depicts the associated regression tables and robustness checks are in the Appendix under Table 3a - Table 3l.

It should be noted that even after matching, treatment villages appeared to be closer to the district center within than control villages, and people who lived there made more trips to the district center than those in the control group. However, the treatment and control groups were still more similar using matching than either without any matching or under the matching conducted in 2011.

With this caveat, results suggest that the project resulted in a shorter travel time to the district center (around 2.2 hours) and a greater number of trips (roughly 25 trips/year). However, the majority of the results are not significant. Still, NERAP seems to have significantly improved access to an all-season road by 18 percent. This result was significant at the 10 percent level. Overall, these results are quite large in magnitude, highlighting how power may have been an issue.

The number of trips taken to the district center is the only outcome variable that exhibits a lot of heterogeneity in outcomes. Those in farther-away villages were significantly less likely to have benefitted from the project than those in closer villages, though both farther and closer villages still increased their number of trips to the district center by a few dozen times per year (see Appendix). Those villages with greater populations were also more likely to have benefitted.[12]

Finally, we looked at whether the rehabilitation of roads led to decreases in costs to reach a variety of important destinations: the district center (DC), the Provincial Center (PC), the closest big city, the closest market for buying produce, and the closest market for selling produce according to the male household survey, as well as the closest market for selling produce according to the focus group survey. We found no significant effects on any of these costs. Costs generally fall, with the exception of rising for those reporting selling their produce at a market. This anomaly could be a function of sellers deciding to sell their produce at farther-away markets.

Unfortunately, we cannot standardize the time taken or costs required to get to any location by distance (e.g. minutes per km or cost per km) since the distances were not captured.

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[11] Results restricted to respondents who used cars, the most frequently used motorized transport, show similar results to overall motorized transport. We were unable to restrict to other forms of motorized transport, as few respondents indicated their use so we did not have enough power for any meaningful regression.

[12] Similar results were obtained when matching using the alternative set of matching variables ("Matching Set 2" in Table 1); there was significance in the simple OLS regression only at the 10 percent level.

Table 3: Access indicators summary

Indicator	Baseline: control	Baseline: treatment	Change at endline: control	Change at endline: treatment	Difference-in-Difference result
Hours to district center	2.31	1.19	1.84	-0.40	-2.24
Number of trips to district center	25.49	42.14	-0.59	24.56	25.16
Percent with access to an all-season road	0.29	0.38	0.17	0.36	**0.18**
Percent of people using motorized transport (DC)	0.38	0.39	0.28	0.29	0.01
Percent of people using motorized transport (market)	0.39	0.54	0.19	0.20	0.01
Cost to DC	153.60	109.38	150.48	133.14	-17.34
Cost to PC	440.39	259.04	251.98	185.51	-66.47
Cost to closest biggest city	1238.75	682.70	709.53	682.84	-26.69
Cost to market (selling produce)	2282.25	1222.68	-699.10	482.87	1181.96
Cost to market, focus group (selling produce)	169.23	150.27	388.74	1026.00	637.25

Accessibility of Services

A desired outcome of the road rehabilitation program was to decrease travel time to the nearest health facility and increase use of health services. Table 4a (Appendix) reports travel time to the nearest hospital, a question asked in the female household survey. The sample is again limited to those who report using motorized forms of transport. Respondents were also asked where, during their most recent illness or injury, they sought treatment.

Results are mixed. On the one hand, as seen in Table 4, travel time to the nearest hospital seems to have increased in treatment villages, although this increase in time is insignificant. This remains insignificant despite different specifications.

On the other hand, respondents reporting travel to the nearest hospital or clinic increased in treatment villages; these results are also not significant but are slightly stronger than the results for travel time. The point estimates are also relatively large; approximately 20 percent more of the population went to the hospital or clinic the last time they were ill. Therefore, people may be accessing nearest hospitals or clinics despite not reporting a decrease in travel time.[13] There may be ongoing factors that influence this decision that are unrelated to the NERAP project.

Table 4: Accessibility to services indicators summary

Indicator	Baseline: control	Baseline: treatment	Change at endline: control	Change at endline: treatment	Difference-in-Difference result
Hours to hospital	2.48	1.89	-0.15	0.07	0.21
Went to hospital	0.41	0.23	-0.04	0.11	0.15
Went to clinic	0.13	0.08	0.02	0.08	0.06

Uptake of Services

With regards to the uptake of services, we look at the use of healthcare services, focusing first on medically assisted deliveries as the primary reason people might go to the hospital. The female head of household was asked the location of her most recent delivery. The results, shown in Table 5 in the main text and Tables 5.1a and 5.1b in the Appendix, tell an interesting story. Although we previously saw an increase in reported hospital visits, this did not seem to translate into more women reporting giving birth in the hospital. If anything, roughly 8 percent fewer reported delivering a baby in the treatment group relative to the control.

Further, the increase in hospital visits did not lead to more children being vaccinated as reported by their mothers. This makes sense if children are not being vaccinated in hospitals, but it also points to potential problems with data quality given the extremely steep decline.

The second component of uptake in services in which we are interested is that of educational services. Table 5 shows the results of these indicators, while the significance can be found using various checks in the Appendix under Tables 5.2a and 5.2b. We see no statistically significant change in either the total number of boys in school or the percentage of boys in school in treatment villages. Unfortunately, data were not gathered in a comparable format for girls in the baseline.

Overall, the results in this section highlight the fact that other factors beyond access alone are affecting decisions to use medical and educational services. This is in line with other studies that have found that many factors influence a person's decision to attend a clinic or hospital in developing countries, such as exposure to mass media and birth order of child (for those who accessed natal care)[14],

[13] Travel time is reported conditional on going to a hospital.
[14] Titaley et al. 2010a.

behavioral risk factors,[15] health worker-client interactions,[16] perceived quality of facilities and availability of drugs,[17] and financial difficulty.[18] For example, one study on factors influencing decisions of women to attend a clinic found that level of education, income, knowledge, attitude, distance, availability of public transportation and cost of service were all more predictive of a person using a clinic than the cost of transportation.[19]

Table 5: Uptake of services indicators summary

Indicator	Baseline: control	Baseline: treatment	Change at endline: control	Change at endline: treatment	Difference-in-Difference result
Medically assisted delivery	0.15	0.27	0.15	0.07	-0.08
Number of children vaccinated	3.92	3.94	-3.48	-3.50	-0.02
Number of children of school-going age currently enrolled in school	0.92	1.02	0.07	0.05	-0.02
Percent in school	0.85	0.83	-0.10	-0.07	0.02

Diversification of Income

One of the goals of the road rehabilitation program was the triggering of second-order economic benefits, such as diversification of income. We looked at three indicators of income diversification. First, the male head of the household was asked which activities had earned him the most income in the past year. The female household head was asked the same question. Those answers that did not involve crop or livestock production were considered "off-farm."[20] Second, the female household head was asked if she worked outside of the house.

[15] Riebiero et al. 2009.
[16] Pell et al. 2011.
[17] Kiwanuku et al. 2008.
[18] Titaley et al. 2010b.
[19] Ye et al. 2010.
[20] Specifically, these included Sales of Prepared Foods, Shepherding, Milling, Other wage labour, Skilled worker, Small business, Shopkeeper, Cross Border Trade, Sale of Firewood or Charcoal, Handicraft, Carpet weaving, Mining, Taxi/transportation, Job with Non-Government Organization, Job with NGO or UN, Job with Company or Private Sector, Rental Income in the baseline survey. In the endline survey, the above were options, along with the additional options of Butcher, Producing and selling leather, wool and skin, Producing and selling diary products, Smuggling, Carpenter, Blacksmith, Brick baking, Mason, Construction wage laborer, Metal worker, Tinsmith, Barber, Baker, Tailor, Lender/ money trader, Physician, Health worker/nurse/midwife, Manager/principle/teacher, Malik/Arbab/Qaryadar, Village leader, Selling home appliances.

As seen in Table 6, the average impact of the program on any of these measures is small and insignificant.

Table 6: Diversification of income indicators summary

Indicator	Baseline: control	Baseline: treatment	Change at endline: control	Change at endline: treatment	Difference-in-Difference result
Percent off-farm activity (male)	0.44	0.47	0.09	0.11	0.01
Percent off-farm activity (female)	0.10	0.11	0.05	0.03	-0.03
Percent works outside the house (female)	0.39	0.27	-0.15	-0.10	0.06

Consumption

The road rehabilitation program was also hoped to increase consumption. Multiple different measures of consumption were considered for robustness.

First, we consider food consumption as a proxy of welfare. The male head of the household was asked how often, in the last 12 months, he had problems satisfying the food needs of the household. A higher number indicates a greater frequency of problems feeding their family. The female head of household was also asked questions about the purchases of the household in the past seven days. We focus here on the amount beef and lamb consumed, as these were the two main meat products consumed, as well as the amount of eggs, flour and rice.

Counter-intuitively, hunger seems to have been a greater problem in treatment areas compared to control areas by the time of the endline survey, yet treatment groups fared better than control groups on consumption of proteins. Lamb consumption also increased. As all food items' consumption increased, the hunger measure is an anomaly. As it fell in both the treatment and control groups, simply falling more in the control groups that were worse off to begin with, it is possible it could reflect convergence or targeted efforts by other organizations such as the World Food Programme. Those in the bottom 50% of consumption of rice or flour did not report any significant changes and there were too few reporting eating meat or eggs to do this analysis for those outcome variables.

We then looked at expenditures more generally. People were asked to report their expenditures on different sets of goods in the last 30 days and over the last 12 months, separately. Those goods in the 12-month set were goods that one did not buy very frequently. Since expenditure patterns could move in different directions over these two sets of goods, we considered them separately ("Expenditure 1" and "Expenditure 2", respectively) as well as together ("Total Expenditure"). The results can be seen in Table

5.4a in the Appendix. The only significant changes were negative; when we break expenditures down and pay specific attention to those in the bottom 30 percent, we still see few effects.

Table 7: Consumption indicators summary

Indicator	Baseline: control	Baseline: treatment	Change at endline: control	Change at endline: treatment	Difference-in-Difference result
Hunger scale[21]	2.99	2.75	-1.04	-0.63	**0.41**
Beef (kg per week)	0.36	0.40	0.06	0.14	0.08
Lamb (kg per week)	0.84	0.51	-0.38	-0.08	**0.30**
Eggs (number per week)	2.23	3.32	0.21	0.41	0.20
Flour (kg per week)	27.19	26.53	-9.02	-5.05	3.97
Rice (kg per week)	3.08	4.03	4.22	4.55	1.14
Flour (bottom 50%, kg per week)	17.59	16.97	-7.98	-6.32	**1.66**
Rice (bottom 50%, kg per week)	0.73	0.99	-0.23	-0.33	-0.11

[21] Hunger scale runs from 1-5 with 1 being hungry "never" and 5 being hungry "mostly", over the last 12 months.

Conclusion

The National Emergency Rural Access Project (NERAP) sought to rehabilitate much-needed rural roads and improve access to services. In this regard, it has been a mixed success. There is suggestive evidence that access to an all-season road and the number of trips taken to the district center increased as a result of the project; however, these changes largely did not result in increased uptake of services, particularly health services. The intuitive explanation is that other factors aside from the presence of roads affect use of services. Other key findings include a slight increase in household food consumption.

The project was complicated by numerous stumbling blocks, including a poor security environment, the possibility of contamination, and at times questionable data. In particular, there were major issues with contamination both of the treatment and control group, given the numerous concurrent projects in Afghanistan. In light of these issues, the results presented here can most charitably be taken as the effect of NERAP relative to other projects, rather than the effect of NERAP relative to no roads rehabilitation project. It is recommended that in the future, more attention be paid to ensure that all the desired outcome variables are included in the baseline survey and to take extra precautions in planning an impact evaluation in a volatile context so that adequate uncontaminated treatment and control groups can be used for analysis.

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Appendix

Data Appendix

Outcome variable	Survey source	Survey question in baseline	Survey question in endline
Access			
Hours to district center	MHH	How long did it take to make the trip from the village to [LOCATION] the last time you made it? (one-way)	How long did it take to make the trip from the village to [LOCATION] the last time you made it? (one-way)
Number of trips to district center	MHH	In the 12 months, how many times have you been to [LOCATION]?	In the 12 months, how many times have you been to [LOCATION]?
Cost of going to the district center	MHH	How much does the trip to [LOCATION] cost, both there and back?	How much does the trip to [LOCATION] cost, both there and back?
Cost of going to the provincial center	MHH	How much does the trip to [LOCATION] cost, both there and back?	How much does the trip to [LOCATION] cost, both there and back?
Cost of going to the closest big city	MHH	How much does the trip to [LOCATION] cost, both there and back?	How much does the trip to [LOCATION] cost, both there and back?
Cost of going to the market to buy produce	MHH	How much did the trip cost the last time you brought produce [answer 3.12] back to the village from the market using [answer to 3.11]? (one way)	How much did the trip cost the last time you brought produce from [answer T4] back to the village from the market using [answer toT5]? (one way)
Cost of going to the market to sell produce (MHH)	MHH	During your last trip to the market at [ANSWER TO 9.15], using [ANSWER TO 9.16] how much did the trip cost, both there and back? [the amount spent for transportation of the person and the produce]	How much was the cost of round trip the last time when you carried your crops to this market using [Type of Transportation]? (cost of carrying goods and the person)

Cost of going to the market to sell produce (MFG)	MFG	Usually, how much would it cost for people in this village to transport 50 kg. of produce from this village to the permanent food market by [ANSWER TO 6.03]?	Usually, how much would it cost for people in this village to transport 50 kg. of produce from this village to the permanent food market by [ANSWER TO 6.03]?
Number of people using motorized transit to get to the district center	MHH	How would you usually travel to [LOCATION]?	How would you usually travel to [LOCATION]?
Access to an all-season road	MFG	During the past 12 months, was the closest road to your village usable by vehicles during all months?	During the past 12 months, was the closest road to your village usable by vehicles during all months?
Access to services			
Hours to hospital	FHH	How long did it take to travel from the village to [location of treatment in 2.17]? (one-way)	How long did it take to travel from the village to [location of treatment in H11]? (one-way)
Number of trips to hospital	FHH	Who provided the treatment for the [illness or injury in 2.11]?	Who provided the treatment for the [illness or injury in H5]?
Number of trips to clinic	FHH	Who provided the treatment for the [illness or injury in 2.11]?	Who provided the treatment for the [illness or injury in H5]?
Uptake of services			
Medically assisted delivery	FHH	Where did the delivery of this baby take place?	Where did the delivery of this baby take place?
Percent of vaccinated children	FHH	Does [NAME] have an immunization card? [IF YES] May I see it? COPY ALL INFORMATION	Does [NAME] have an immunization card? [IF YES] May I see it? COPY ALL INFORMATION
Percent of boys in school	MHH/FHH	How many of the boys in this household that are above 6 -14 years and living in this household and how many of them are going to school?	Is [NAME OF CHILD] enrolled in school?

Diversification of income			
Male works off-farm	MHH	In the past 12 months, what activity or source brought in the most income to your household?	In the past 12 months, what activity or source brought in the most income to your household?
Female works off-farm	FHH	What type of work is this? IF RESPONDENT CURRENTLY PERFORMS MORE THAN ONE ACTIVITY, SELECT ACTIVITY WHICH TAKES UP MOST OF HER TIME	What type of work is this? IF RESPONDENT CURRENTLY PERFORMS MORE THAN ONE ACTIVITY, SELECT ACTIVITY WHICH TAKES UP MOST OF HER TIME
Female works outside the house	FHH	Currently, do you perform any work which generates income in money or products that you, your family, or other villagers consume?	Currently, do you perform any work which generates income in money or products that you, your family, or other villagers consume?
Consumption			
Hunger	MHH	How often in the last 12 months did you have problems satisfying the food needs of the household?	How often in the last 12 months did you have difficulties satisfying the food needs of the household?
Amount beef	FHH	What was the total amount consumed in the last 7 days in kilograms?	What was the total amount consumed in the last 7 days in kilograms?
Amount lamb	FHH	What was the total amount consumed in the last 7 days in kilograms?	What was the total amount consumed in the last 7 days in kilograms?
Amount eggs	FHH	What was the total amount consumed in the last 7 days [in eggs]?	What was the total amount consumed in the last 7 days [in eggs]?
Amount flour	FHH	What was the total amount consumed in the last 7 days in kilograms?	What was the total amount consumed in the last 7 days in kilograms?
Amount rice	FHH	What was the total amount consumed in the last 7 days in	What was the total amount consumed in the last 7 days in

		kilograms?	kilograms?
Expenditures (past month)	MHH	In the last 30 days, what was the total household expenditure on the following items:	In the last 30 days, what was the total household expenditure on the following items:
Expenditures (past year)	MHH	In the last 12 months, what was the total household expenditure on the following items:	In the last 12 months, what was the total household expenditure on the following items:
Income	MHH	In the past 12 months, how much income did you receive from this source of activity?	In the past 12 months, how much income did you receive from this source of activity?

Methodological Appendix

This section will walk through the issues associated with measuring the impact of NERAP and how the issues were addressed in the impact evaluation. These issues can roughly be divided into two families: those surrounding selection bias and those surrounding spatial correlation. We discuss each in turn.

Selection Bias

Impact evaluation relies on identifying the counterfactual – what would have happened in the absence of the intervention. In general, many factors can affect the outcome variables, so in order to isolate the effects of the intervention we need to compare the endline responses given in those areas that received the intervention with those given in areas that are ideally completely identical except for having not benefitted from the intervention.

Randomized controlled trials, in which some areas are randomly assigned to receive treatment and others not to, are the gold standard in impact evaluation. However, it was not possible to do a randomized controlled trial of NERAP. Roads were instead targeted if their nearby villages met five main criteria.

First, villages chosen were part of former priorities still not financed from other projects, such as the Provincial Planning Process and NEEP. NEEP priorities involved 12 provinces affected by drought. Secondly, villages were chosen due to communities' requests. These requests could be direct requests from communities or channeled through other administrative instances, often with peace and reconciliation objectives in strategic areas. Thirdly, villages were chosen in order to consolidate works to sustain previous rural road investments. Fourthly, under the theory that highly rural areas are more poppy-dependent, villages that were highly isolated were chosen for the project. Finally, villages were chosen due to complementarities with other social and rural development projects.

Given that the roads were non-randomly selected, we must produce a set of synthetic control roads – roads that we believe are similar in every relevant aspect to the roads that were selected for NERAP except for having not been selected. The methods we will discuss to address the potential bias introduced by non-random selection are ordinary least squares (OLS) with controls, matching, and propensity score weighting.

OLS, Matching and Weighting

OLS is a standard first approach to any problem and we will also use it in this evaluation. Adding some controls can reduce selection bias, but this method suffers from some well-known problems such as potential omitted variable bias and reverse causality. A different technique one can use to account for bias is matching or propensity score weighting. However, whether matching or weighting is better than OLS is actually a matter of some debate.

To set the scene, let us review the assumptions required by OLS as opposed to matching and weighting methods.

A standard regression that ignores potential endogeneity simply assumes that treatment is exogenous conditional on covariates. For us, this would mean that where roads were upgraded is

not correlated with any increase in the number of trips to the district centre, decrease in travel time, or convergence of prices except through the upgrading of the roads; in other words, there is no selection that biases results. This is known as the conditional independence assumption.

Matching makes the same assumption. As a consequence, it is only unbiased when the assumption that the covariates that are matched on are the only relevant covariates is correct. If there remain any important characteristics that are not matched on (the analog of being controlled for), results will be biased. Weighting also requires the same assumption. The real difference between the three is in how they weight individual observations. In an OLS regression, a few extreme data points can have a lot of influence. When using propensity score matching or weighting, those data points are typically either excluded, as they may not be in the area of common support, or down-weighted.

An intermediate option is checking the balance of covariates and running an OLS regression on only those observations that would be assigned a positive weight through propensity score matching or weighting. Linear regressions also have an advantage: when matching, if one has a particularly small sample, the closest match might still be quite far away and the results highly dependent on the individual, quirky matches, whereas a regression is often more stable. Abadie and Imbens (2008) recently highlighted the fact that matching estimators are typically biased due to imperfect matches (and the smaller the sample size, the more likely the matches to be imperfect). Given the relatively small set of roads that received treatment and that the balance of econometricians are skeptical of matching compared to OLS (*e.g.* Angrist and Pischke, 2009), we would not have enough observations for one to one matching to be credible. However, we should still check the similarity of the treatment group and the control group, and we can estimate the propensity score and use it for weighting in a regression. It will behoove us to use this method as a robustness check.

Covariate Balancing

Once we have decided to estimate the propensity score, we need to select which method to use to estimate it.

A number of new methods have been developed to ensure balance of covariates, as opposed to the traditional methods in which one had to repeatedly "guess and check" for covariate balance. In particular, two similar methods are the Covariate Balancing Propensity Score (CBPS) (Imai and Ratkovic, 2012) and entropy balancing (Hainmueller, 2012). Practically speaking, if results depend on whether CBPS or entropy balancing is used, they would seem too sensitive to be credible. In either case, if matching is used, the standard errors must be adjusted according to Abadie and Imbens (2008).

The propensity score should be estimated at the road level. An assumption required for unbiased estimates, SUTVA, would be violated if individual villages were matched, as the effects of a road on one village are not independent from its effects on other nearby villages.

Spatial Clustering

A second set of methodological issues surround the spatial correlation inherent in the data. As mentioned, the effects of a road can spillover to nearby areas; further, even without a road the characteristics and development of nearby villages may be correlated.

There are several ways of addressing the spatial correlation.

One can aggregate the villages into groups which are assumed to be independent from one another; one can eliminate the intra-group correlation by selecting only one village's outcomes within each group; or one can estimate the spatial correlation and account for it in one's regressions.

The last of these options is the most attractive but also the trickiest. Anselin (2001) provides a great overview of ways that spatial correlation can be approached. We frequently look at outcomes at a level below the level at which the treatment was administered; see, for example, Miguel and Kremer's paper on deworming drugs that were randomly assigned to different schools as there was thought to be spillovers within schools (2004). The key is to cluster the standard errors at the level of the intervention. With spatial data, we may believe that observations have an even more complex relationship: villages do not uniformly depend on each other within a road cluster but are dependent in ways that depend on distance, for example. We will try different ways of addressing this correlation for robustness.

Out of the many ways of modeling spatially dependent data, most (such as SUR) are out of reach because they require a sufficient number of time periods, T, and we only have two time periods. Instead, we can adjust the standard errors to take spatial correlation into account in the same way that Newey-West is used to adjust standard errors to cluster for time periods in longer panel data. Timothy Conley's spatial clustering code essentially takes clusters of observations, models the dependence of the observations within the cluster and how that dependence varies with distance between the observations, and then adjusts standard errors to account for that dependence.

To use this method, we must define a boundary beyond which we assume that two observations are not affected by each other. Bank staff with knowledge of the project suggested using 2 kilometers as the cut-off threshold.

Methods Summary

In light of the numerous technical issues discussed above, we use several methods for robustness.

First, we use CBPS weighting on two lists of variables that we believe had theoretical reason to affect the outcome variables. The variables can be found in Table 1h. Care was taken to include key outcome variables in the set of matching variables. The propensity score was estimated at the cluster (road) level.

Table 1g: Variables for CBPS

Variable	Initial Matching	Matching Set 1	Matching Set 2
Number of villages along road	▯	▯	▯
Mean density of villages along road	▯	▯	▯
Length of project	▯	▯	▯

Sum of the total population	▯	▯	▯
Minimum distance to the district center (DC)	▯	▯	▯
Mean average house size	▯	▯	▯
Mean population speaking Dari	▯	▯	▯
Mean population speaking Pashto	▯	▯	▯
Mean population near flat terrain	▯	▯	▯
Mean population near mountainous terrain	▯	▯	▯
Mean population with car access all season	▯	▯	▯
Mean population with no roads	▯	▯	▯
Mean population with radio access	▯	▯	▯
Mean population with TV access	▯	▯	▯
Mean population with lit center w/in 5km	▯	▯	▯
Mean population with health center w/in 5km	▯	▯	▯
Mean population with school w/in 5km	▯	▯	▯
Mean population with river access	▯	▯	▯
Mean number of NEEPRA projects along road	▯	▯	▯
Standard deviation of village's altitude	▯	▯	▯
Mean population growing potatoes	▯	▯	▯
Mean population growing rice	▯	▯	▯
Mean population with other (non-farming) industry	▯	▯	▯
Mean time to hospital		▯	▯
Mean time to school		▯	▯
Mean time to DC		▯	▯
Mean number of trips to DC		▯	▯
Maximum speed of road		▯	▯
Road quality index		▯	▯
Mean household income		▯	
Mean household expenditure			▯

The main results use nearest-neighbor, 1:1 matching to create a set of treatment and control roads that are more comparable to each other, then running OLS regressions within these restricted sets, adjusting standard errors in two alternative ways: clustering by road and using Conley's spatial clustering correction. An alternative set of matching variables is tried for robustness.

All hypotheses are tested by regressing the measures relevant for each hypothesis on a treatment indicator variable using the following diff-in-diff model:

$$\text{Outcome}_{v,t} = \alpha + \beta(R_v) + \delta(T_t) + \sigma(R_v{*}T_t) + \pi_v + \varepsilon$$

Where the Outcome is the outcome indicator (ie access, uptake of services) in given village *v* in the baseline (0) or endline (1) survey *t* time period, R_v is the village treatment dummy

(ie whether this is a NERAP village or not), T_t is the dummy for *t,* σ is the interaction term, π_v is the village-pair fixed effect, and ε is the error term.

One could also use propensity score weighting. Results using propensity score weighting were sometimes comparable but sometimes inexplicable, and so are not among either of our top two specifications (OLS on different matched sets). For example, under weighting, it appears that NERAP insignificantly increased travel time to the district by 0.4 hours - or 0.7 hours including controls. It also significantly increased travel time to the nearest hospital by 1.8 to 2.3 hours without affecting hospital visits or use of health services.

Table 1f. MOPW covariate balance after matching

	Pre-Matching			Post-Matching		
Variable	**Treat**	**Control**	**P-value**	**Treat**	**Control**	**P-value**
Number of villages along road	16.136	16.824	0.788	16.136	16.386	0.923
Mean density of villages along road	422.680	464.290	0.776	422.680	427.800	0.975
Length of project	15.870	14.251	0.557	15.870	13.039	0.193
Sum of the total population	7460.000	7662.300	0.865	7460.000	7662.100	0.871
Minimum distance to the district center	48.820	53.463	0.509	48.820	56.371	0.334
Mean average house size	4.896	4.936	**0.083**	4.896	4.938	**0.067**
Mean population speaking Dari	0.655	0.564	0.239	0.655	0.548	0.208
Mean population speaking Pashto	0.173	0.214	0.493	0.173	0.246	0.285
Mean population near flat terrain	0.252	0.330	0.227	0.252	0.349	0.213
Mean population near mountainous terrain	0.529	0.374	**0.020**	0.529	0.407	**0.081**
Mean population with car access all season	0.504	0.513	0.890	0.504	0.522	0.791
Mean population with no roads	0.230	0.191	0.454	0.230	0.198	0.597
Mean population with radio access	0.965	0.971	0.692	0.965	0.973	0.627
Mean population with TV access	0.137	0.164	0.488	0.137	0.209	0.130
Mean population with lit center w/in 5 km	0.086	0.094	0.752	0.086	0.089	0.934
Mean population with school w/in 5 km	0.380	0.357	0.581	0.380	0.366	0.739
Mean population with health center w/in 5 km	0.268	0.279	0.757	0.268	0.297	0.464
Mean population with river access	0.360	0.451	0.105	0.360	0.453	0.130
Mean number of NEEPRA projects along road	0.678	0.693	0.919	0.678	0.622	0.746

Standard deviation of villages' altitude	884.490	561.750	**0.001**	884.490	617.390	**0.033**
Mean population growing potatoes	275.820	149.060	0.252	275.820	179.680	0.479
Mean population growing rice	111.520	158.080	0.552	111.520	204.980	0.376
Mean population with other (non-farming industry)	737.630	775.770	0.769	737.630	785.240	0.709
Mean distance to nearest village	4.888	4.560	0.664	4.888	4.527	0.642
Mean time to hospital	4.515	2.885	0.161	4.515	3.041	0.282
Mean time to school	33.073	35.919	0.463	33.073	35.921	0.481
Mean time to district center	3.664	5.474	0.355	3.664	5.499	0.475
Mean number of trips to district center	7.311	6.897	0.257	7.311	7.129	0.653
Mean income of individuals	87183.000	91432.000	0.535	87183.000	89851.000	0.755
Maximum speed of road	27.623	24.555	0.333	27.623	24.499	0.362
Quality index of road	148.040	162.560	**0.086**	148.040	159.800	0.248

Table 1g. MRRD covariate balance after matching

	Pre-Matching			Post-Matching		
Variable	**Treat**	**Control**	**P-value**	**Treat**	**Control**	**P-value**
Number of villages along road	16.136	16.824	0.788	16.136	16.386	0.923
Mean density of villages along road	422.680	464.290	0.776	422.680	427.800	0.975
Length of project	15.870	14.251	0.557	15.870	13.039	0.193
Sum of the total population	7460.000	7662.300	0.865	7460.000	7662.100	0.871
Minimum distance to the district center	48.820	53.463	0.509	48.820	56.371	0.334
Mean average house size	4.896	4.936	**0.083**	4.896	4.938	**0.067**
Mean population speaking Dari	0.655	0.564	0.239	0.655	0.548	0.208
Mean population speaking Pashto	0.173	0.214	0.493	0.173	0.246	0.285
Mean population near flat terrain	0.252	0.330	0.227	0.252	0.349	0.213
Mean population near mountainous terrain	0.529	0.374	**0.020**	0.529	0.407	**0.081**
Mean population with car access all season	0.504	0.513	0.890	0.504	0.522	0.791
Mean population with no roads	0.230	0.191	0.454	0.230	0.198	0.597
Mean population with radio access	0.965	0.971	0.692	0.965	0.973	0.627
Mean population with TV access	0.137	0.164	0.488	0.137	0.209	0.130
Mean population with lit center w/in 5 km	0.086	0.094	0.752	0.086	0.089	0.934
Mean population with school w/in 5 km	0.380	0.357	0.581	0.380	0.366	0.739
Mean population with health center w/in 5 km	0.268	0.279	0.757	0.268	0.297	0.464
Mean population with river access	0.360	0.451	0.105	0.360	0.453	0.130
Mean number of NEEPRA	0.678	0.693	0.919	0.678	0.622	0.746

projects along road						
Standard deviation of villages' altitude	884.490	561.750	**0.001**	884.490	617.390	**0.033**
Mean population growing potatoes	275.820	149.060	0.252	275.820	179.680	0.479
Mean population growing rice	111.520	158.080	0.552	111.520	204.980	0.376
Mean population with other (non-farming industry)	737.630	775.770	0.769	737.630	785.240	0.709
Mean distance to nearest village	4.888	4.560	0.664	4.888	4.527	0.642
Mean time to hospital	4.515	2.885	0.161	4.515	3.041	0.282
Mean time to school	33.073	35.919	0.463	33.073	35.921	0.481
Mean time to district center	3.664	5.474	0.355	3.664	5.499	0.475
Mean number of trips to district center	7.311	6.897	0.257	7.311	7.129	0.653
Mean income of individuals	87183.000	91432.000	0.535	87183.000	89851.000	0.755
Maximum speed of road	27.623	24.555	0.333	27.623	24.499	0.362
Quality index of road	148.040	162.560	**0.086**	148.040	159.800	0.248

Results Appendix

A note on how to read the tables:

The tables below show robustness checks for each outcome variable. With impact evaluations, we often worry that the way we define the formulas could influence whether or not we see an impact. For example, if we forget to include proper controls, such as distance or population, we might see an impact where there is none. Therefore in this analysis we included many robustness checks to ensure the reliability of our initial specification.

In the following section, we look at the outcomes discussed in the report. Tables 3a-3l look at outcomes related to access. Tables 4a-4g examine the outcomes related to access to health services. Tables 5.1a-5.1f deal with health services uptake and 5.2a-5.2b with educational services uptake. Tables 6a-6f are the results from diversification of income. Tables 7.1a-7.1f are the outcomes for food purchases and 7.2a-7.2c are expenditures.

The robustness checks are as follows. We look at our first matching set and run the regressions with limited additional controls. These are reflected in the actual document. As a first robustness check, we add additional controls of population and distance to DC. This first regression and the robustness check with additional controls are represented in the same tables. Second, we look at outcomes based on matching of Set 2, both with and without additional controls. This informs us that our results are not based solely on our matching specification. Third, we interact the base variables (endline, treatment and interaction) with distance and population, seperately. This allows us to relax assumptions in case the relationship between these variables and the outcome is not linear. Our fourth and final robustness check used OLS results corrected for spatial dependence. Here we adjusted the standard error dependent on the distance. This robustness check was used for access, health services, health uptake, diversification of incomes and food purchases. For more information on the methodology, see the Methodological Appendix.

In general, the charts reveal regression outcomes. The “endline” variable shows the time difference, or how the outcome variable was impacted by the passage of time between the baseline survey and endline survey. It therefore captures changes that occurred over time among all villages and households. The “treatment” variable represents the differences between treatment and control villages. For example, if a treatment village already experienced lower travel time, in hours, to the district center, this is reflected in the treatment variable and is represented by a negative number. The “interaction” term is the one that shows the impact of the program. It is an interaction between the treatment and endline variable, meaning it shows the effect of both time and treatment. It is the effect using a difference-in-difference approach. The “constant” variable shows an underlying starting point of the outcome. For example, if we are looking at the number of hours to the district center in the control group at baseline with no controls, it would be represented by the “constant” variable. The N is the number of observations used in the analysis, and it effects our ability to see statistically significant results. In general, we can have more confidence in higher N than in lower N in the below specifications.

Table 3a: Access to the district center and an all-season road, matched on Set 1

	(1) Hours to district center	(2) Hours to district center	(3) Number of trips	(4) Number of trips	(5) Accessible all year round
	b/se	b/se	b/se	b/se	b/se
Endline	1.840	1.738	-0.594	-0.137	0.174**
	(1.62)	(1.58)	(6.17)	(6.93)	(0.08)
Treatment	-1.123*	-0.788	16.649*	12.195	0.097
	(0.59)	(0.64)	(8.97)	(10.07)	(0.08)
Interaction	-2.237	-2.263	25.155	29.775*	0.184*
	(1.66)	(1.61)	(15.35)	(15.96)	(0.11)
Population		-0.000		0.006*	
		(0.00)		(0.00)	
Distance to DC		0.011		-0.240	
		(0.02)		(0.16)	
Constant	2.313***	2.074**	25.494***	26.636***	0.285***
	(0.48)	(0.88)	(6.94)	(8.63)	(0.06)
N	674	577	677	577	10058

Table 3b: Access to the district center and an all-season road, matched on Set 2

	(1) Hours to district center	(2) Hours to district center	(3) Number of trips	(4) Number of trips	(5) Accessible all year round
	b/se	b/se	b/se	b/se	
Endline	1.927	1.140	5.387	5.125	0.174**
	(1.61)	(1.28)	(8.19)	(10.13)	(0.08)
Treatment	-1.073*	-1.175	16.994*	12.924	0.143*
	(0.62)	(0.71)	(8.57)	(11.20)	(0.08)

Interaction	-2.324	-1.612	19.175	24.476	-0.033
	(1.65)	(1.34)	(16.26)	(18.47)	(0.11)
Population		-0.000		0.009**	
		(0.00)		(0.00)	
Distance to DC		-0.002		-0.006	
		(0.01)		(0.22)	
Constant	2.264***	2.680***	25.148***	18.736	0.308***
	(0.51)	(0.87)	(6.41)	(11.87)	(0.06)
N	669	560	670	558	9790

Table 3c: Access to the district center and an all-season road, distance interactions

	(1) Hours to district center	**(2) Hours to district center**	**(3) Number of trips**	**(4) Number of trips**	**(5) Accessible all year round**
	b/se	b/se	b/se	b/se	b/se
Endline	1.855	2.017	7.729	3.265	1.855
	(1.88)	(1.94)	(12.06)	(10.99)	(1.88)
Treatment	-1.062	-0.883	15.730	10.851	-1.062
	(1.10)	(1.04)	(16.50)	(16.57)	(1.10)
Interaction	-2.394	-2.699	36.975	45.211*	-2.394
	(1.97)	(2.04)	(23.32)	(23.04)	(1.97)
Distance	0.004	0.003	-0.110	-0.064	0.004
	(0.01)	(0.01)	(0.16)	(0.16)	(0.01)
Endline * Distance	0.010	0.008	-0.204	-0.148	0.010
	(0.03)	(0.03)	(0.17)	(0.16)	(0.03)
Treatment * Distance	-0.005	-0.007	-0.056	0.009	-0.005
	(0.02)	(0.02)	(0.46)	(0.45)	(0.02)
Interaction * Distance	-0.006	-0.000	-0.802	-0.943*	-0.006
	(0.03)	(0.03)	(0.51)	(0.51)	(0.03)
Population		-0.000		0.008**	
		(0.00)		(0.00)	
Constant	2.329**	2.598**	28.993**	21.586*	2.329**
	(0.94)	(1.00)	(12.44)	(12.28)	(0.94)

N	570	570	569	569	570

Table 3d: Access to the district center and an all-season road, population interactions

	(1) Hours to district center	**(2) Hours to district center**	**(3) Number of trips**	**(4) Number of trips**	**(5) Accessible all year round**
	b/se	b/se	b/se	b/se	b/se
Endline	2.539	3.087	-0.890	0.225	0.107
	(2.25)	(2.64)	(6.29)	(7.61)	(0.11)
Treatment	-2.201*	-2.323*	11.799	4.618	0.047
	(1.16)	(1.34)	(12.02)	(12.30)	(0.10)
Interaction	-2.009	-2.608	32.189*	31.793*	0.154
	(2.39)	(2.73)	(17.25)	(18.71)	(0.13)
Population	-0.000	-0.000	0.012**	0.007	0.000
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Endline * Population	-0.000	-0.001	-0.004	-0.002	-0.000
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Treatment * Population	0.001	0.001	-0.001	0.003	-0.000
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Interaction * Population	-0.000	0.000	-0.000	-0.002	0.000
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Distance to DC		0.003		-0.252	0.289***
		(0.02)		(0.15)	(0.07)
Constant	2.496***	2.558**	16.638**	28.030**	10080
	(0.83)	(1.21)	(6.83)	(11.27)	0.107
N	665	570	668	569	(0.11)

Table 3e: Number of people using motorized transport, matched on Set 1

	Number of people using motorized transit to get to DC	**Number of people using motorized transit to get to the market**
	b/se	b/se
Endline	0.284***	0.190***
	(0.03)	(0.04)

Treatment	0.011	0.159**
	(0.08)	(0.07)
Interaction	0.006	0.012
	(0.06)	(0.06)
Population	-0.000	0.000
	(0.00)	(0.00)
Distance to DC	0.000	-0.002
	(0.00)	(0.00)
Constant	0.381***	0.385***
	(0.06)	(0.07)
N	3508	3702

Table 3f: Number of people using motorized transport, matched on Set 2

	Number of people using motorized transit to get to DC	**Number of people using motorized transit to get to the market**
	b/se	b/se
Endline	0.280***	0.163***
	(0.03)	(0.05)
Treatment	0.036	0.178**
	(0.07)	(0.07)
Interaction	0.003	0.044
	(0.06)	(0.06)
Population	-0.000	0.000
	(0.00)	(0.00)
Distance to DC	0.001	-0.001
	(0.00)	(0.00)
Constant	0.344***	0.346***
	(0.06)	(0.06)
N	3434	3533

Table 3g: Costs to the nearest location, matched on Set 1

	Cost to DC	**Cost to PC**	**Cost to closest big city**	**Cost to Market (selling produce)**

	b/se	b/se	b/se	b/se
Endline	150.479***	251.975***	709.531***	-699.095
	(37.69)	(45.28)	(219.56)	(1164.32)
Treatment	-44.215	-181.356*	-556.056**	-1059.573
	(36.41)	(99.94)	(217.54)	(1223.06)
Interaction	-17.340	-66.467	-26.691	1181.961
	(52.86)	(58.63)	(262.39)	(1310.31)
Population	-0.009	-0.039**	-0.067	-0.152
	(0.01)	(0.02)	(0.05)	(0.13)
Distance to DC	0.662*	4.055***	0.871	-7.826
	(0.39)	(1.38)	(3.64)	(6.05)
Constant	153.596***	440.393***	1238.754***	2282.251*
	(36.95)	(103.78)	(196.69)	(1217.51)
N	2248	3400	2369	698

Table 3h: Costs to the nearest location, matched on Set 2

	Cost to DC	**Cost to PC**	**Cost to closest big city**	**Cost to Market (selling produce)**
	b/se	b/se	b/se	b/se
Endline	143.674***	245.622***	869.715***	-245.316
	(31.48)	(49.78)	(273.32)	(928.39)
Treatment	-48.984	-216.241**	-528.667**	-483.954
	(30.81)	(103.51)	(235.94)	(977.78)
Interaction	-8.361	-67.090	-232.194	798.021
	(50.57)	(62.48)	(302.51)	(1108.05)
Population	-0.014	-0.044**	-0.137**	-0.138
	(0.01)	(0.02)	(0.06)	(0.14)
Distance to DC	0.305	2.806*	1.882	-7.477
	(0.44)	(1.64)	(4.24)	(6.90)
Constant	170.600***	511.899***	1300.040***	1669.132*
	(32.13)	(109.55)	(206.20)	(969.25)
N	2153	3225	2230	670

Table 3i: Cost to market, focus group

Variable	(1) Cost to market, focus group	(2) Cost to market, focus group
	b/se	b/se
Endline	388.743***	343.321***
	-105.43	-82.67
Treatment	-18.952	-19.19
	-32.28	-41.86
Interaction	637.252**	613.383**
	-288.1	-250.5
Constant		-0.046
		-0.02
N		-1.358

Table 3j: OLS results corrected for spatial dependence: hours to DC

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	2.3134***	.4614	2.0737**	.8559
Endline	1.8404	1.2583	1.7378	1.3137
Treatment	-1.1227**	.5233	-.7875	.6032
Interaction	-2.2374*	1.2814	-2.2630*	1.3424
Population			-.0002	.00028
Distance to DC			.0108	.01476
N	674		577	

Table 3k: OLS results corrected for spatial dependence: number of trips

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	25.4937***	4.8904	26.6361***	7.9092
Endline	-.5937	5.2779	-.1374	5.4952
Treatment	16.6487***	6.3687	12.1949*	7.2142
Interaction	25.1552***	9.5884	29.7745***	10.2195
Population			.0065*	.0035
Distance to DC			-.2397	.1564
N	677		577	

Table 3l: OLS results corrected for spatial dependence: people using motorized transit

Variable	(1) OLS Estimates (to DC)	(1) SE corrected for Spatial Dependence (to DC)	(2) OLS Estimates (to market)	(2) SE corrected for Spatial Dependence (to market)
Constant	.38135***	.0406	.3849***	.0409
Endline	.2839***	.0304	.1903***	.0301

Treatment	.0113	.0503	.1594***	.0477
Interaction	.0060	.0502	.0117	.0413
Population	.0000	.0000	.0000	.0000
Distance to DC	.0002	.0007	-.0016**	.0007

Table 4a: Access to health services, matched on Set 1

	(1) Hours to hospital	**(2) Hours to hospital**	**(3) Went to hospital**	**(4) Went to hospital**	**(5) Went to clinic**	**(6) Went to clinic**
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-0.145	-0.138	-0.040	-0.055	0.024	0.048
	(0.20)	(0.19)	(0.08)	(0.08)	(0.08)	(0.09)
Treatment	-0.590	-0.526	-0.185**	-0.237**	-0.050	-0.036
	(0.38)	(0.38)	(0.09)	(0.09)	(0.06)	(0.06)
Interaction	0.212	0.343	0.147	0.146	0.059	0.036
	(0.38)	(0.38)	(0.10)	(0.10)	(0.10)	(0.11)
Population		-0.000		-0.000***		-0.000**
		(0.00)		(0.00)		(0.00)
Distance to DC		0.001		-0.005***		0.003***
		(0.01)		(0.00)		(0.00)
Constant	2.478***	2.449***	0.411***	0.622***	0.133**	0.067
	(0.27)	(0.49)	(0.08)	(0.09)	(0.05)	(0.06)
N	341	291	347	295	347	295

Table 4b: Access to health services, matched on Set 2

	(1) Hours to hospital	**(2) Hours to hospital**	**(3) Went to hospital**	**(4) Went to hospital**	**(5) Went to clinic**	**(6) Went to clinic**
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-0.337	-0.232	-0.043	-0.062	0.037	0.063
	(0.27)	(0.30)	(0.08)	(0.08)	(0.08)	(0.08)
Treatment	-0.728*	-0.451	-0.146	-0.192*	-0.068	-0.037
	(0.42)	(0.41)	(0.09)	(0.10)	(0.06)	(0.07)
Interaction	0.404	0.317	0.150	0.156	0.046	0.005
	(0.42)	(0.45)	(0.10)	(0.10)	(0.10)	(0.10)

Population		-0.000		-0.000**		-0.000***
		(0.00)		(0.00)		(0.00)
Distance to DC		0.017**		-0.002		0.002
		(0.01)		(0.00)		(0.00)
Constant	2.616***	2.166***	0.372***	0.488***	0.151***	0.132**
	(0.32)	(0.48)	(0.08)	(0.11)	(0.05)	(0.06)
N	333	295	339	300	339	300

Table 4c: Access to health services, distance interactions

	(1) Hours to hospital	(2) Hours to hospital	(3) Went to hospital	(4) Went to hospital	(5) Went to clinic	(6) Went to clinic
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-0.012	0.075	-0.033	-0.010	0.052	0.062
	(0.32)	(0.33)	(0.11)	(0.11)	(0.07)	(0.07)
Treatment	-0.805	-0.746	-0.279*	-0.263*	0.073	0.080
	(0.53)	(0.50)	(0.15)	(0.15)	(0.05)	(0.05)
Interaction	0.569	0.441	0.184	0.150	-0.036	-0.051
	(0.52)	(0.51)	(0.12)	(0.13)	(0.10)	(0.10)
Baseline Distance	0.010	0.010	-0.002	-0.002	0.004*	0.004*
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Endline * Distance	-0.001	-0.002	-0.001	-0.001	-0.000	-0.000
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Treatment * Distance	0.025*	0.023	0.001	0.001	-0.004	-0.004*
	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Interaction * Distance	-0.024	-0.021	-0.002	-0.002	0.005	0.005
	(0.02)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
Population		-0.000		-0.000		-0.000**
		(0.00)		(0.00)		(0.00)
Constant	2.054***	2.201***	0.490***	0.530***	0.001	0.017
	(0.43)	(0.46)	(0.14)	(0.15)	(0.04)	(0.04)
N	294	294	297	297	297	297

Table 4d: Access to health services, population interactions

	(1) Hours to hospital	(2) Hours to hospital	(3) Went to hospital	(4) Went to hospital	(5) Went to clinic	(6) Went to clinic
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-0.233	-0.084	0.002	-0.057	0.030	0.055
	(0.35)	(0.40)	(0.09)	(0.09)	(0.10)	(0.10)
Treatment	-1.088**	-0.890*	-0.148	-0.244*	-0.041	-0.004
	(0.48)	(0.53)	(0.13)	(0.14)	(0.08)	(0.08)
Interaction	0.443	0.440	-0.022	0.008	0.070	0.065
	(0.56)	(0.59)	(0.13)	(0.13)	(0.14)	(0.13)
Population	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Endline * Population	0.000	0.000	-0.000	0.000	0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treatment * Population	0.000	0.000	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Interaction * Population	-0.000	-0.000	0.000	0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Distance		0.012		-0.003**		0.003***
		(0.01)		(0.00)		(0.00)
Constant	2.859***	2.360***	0.434***	0.572***	0.158**	0.028
	(0.40)	(0.55)	(0.11)	(0.12)	(0.07)	(0.06)
N	340	294	345	297	345	297

Table 4e: OLS results corrected for spatial dependence: hours to hospital

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	2.4782***	.2975	2.4488***	.4763
Endline	-.1445	.3658	-.1378	.4053
Treatment	-.5903	.3667	-.5256	.3982
Interaction	.2120	.4631	.3432	.5034
Population			-.0001	.0001
Distance to DC			.0014	.0076
N	341		291	

Table 4f: OLS results corrected for spatial dependence: went to hospital

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	.4111***	.0643	.6219***	.0808
Endline	-.0403	.0740	-.0548	.0749
Treatment	-.1849**	.0795	-.2370***	.0856
Interaction	.1475	.0931	.1462	.0959
Population			.0000	.0000
Distance to DC			-.0048***	.0013
N	347		295	

Table 4g: OLS results corrected for spatial dependence: went to clinic

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	.1333***	.0420	.0668	.0484
Endline	.0240	.0542	.0481	.0626
Treatment	-.05	.0512	-.0364	.0553
Interaction	.0594	.0749	.0360	.0854
Population			.0000	.0000
Distance to DC			.0029***	.0009
N	347		295	

Table 5.1a: Health services uptake, matched on Set 1

	(1) Medical delivery	(2) Medical delivery	(3) Percent vaccinated	(4) Percent vaccinated
	b/se	b/se	b/se	b/se
Endline	0.151***	0.141***	-3.476***	-3.482***
	(0.04)	(0.04)	(0.04)	(0.05)
Treatment	0.126***	0.094*	0.023	0.016
	(0.04)	(0.05)	(0.03)	(0.04)
Interaction	-0.084*	-0.092	-0.025	-0.014
	(0.05)	(0.06)	(0.06)	(0.07)
Population		0.000**		0.000***
		(0.00)		(0.00)
Distance to DC		-0.002***		0.000
		(0.00)		(0.00)
Constant	0.148***	0.219***	3.919***	3.883***
	(0.02)	(0.03)	(0.02)	(0.03)

N	2775	2152	3658	2839

Table 5.1b: Health services uptake, matched on Set 2

	(1) Medical Delivery	**(2) Medical Delivery**	**(3) Percent vaccinated**	**(4) Percent vaccinated**
	b/se	b/se	b/se	b/se
Endline	0.151***	0.157***	-3.489***	-3.508***
	(0.04)	(0.04)	(0.04)	(0.04)
Treatment	0.120***	0.095*	0.005	-0.010
	(0.04)	(0.05)	(0.03)	(0.03)
Interaction	-0.080	-0.098*	-0.013	0.009
	(0.05)	(0.05)	(0.06)	(0.07)
Population		0.000***		0.000***
		(0.00)		(0.00)
Distance to DC		-0.002***		0.000
		(0.00)		(0.00)
Constant	0.146***	0.200***	3.940***	3.917***
	(0.02)	(0.03)	(0.02)	(0.02)
N	2698	2113	3566	2776

Table 5.1c: Health services uptake, distance interactions

	(1) Medical delivery	**(2) Medical delivery**	**(3) Percent vaccinated**	**(4) Percent vaccinated**
	b/se	b/se	b/se	b/se
Endline	0.161***	0.141***	-0.540***	-0.552***
	(0.05)	(0.04)	(0.05)	(0.05)
Treatment	0.102	0.082	0.010	-0.003
	(0.07)	(0.07)	(0.01)	(0.01)
Interaction	-0.143**	-0.133**	-0.041	-0.036
	(0.07)	(0.07)	(0.08)	(0.08)
Distance	-0.002***	-0.002***	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Endline * Distance	-0.000	-0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)

Treatment * Distance	0.000	0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Interaction * Distance	0.001	0.002	0.002	0.002
	(0.00)	(0.00)	(0.00)	(0.00)
Population		0.000**		0.000***
		(0.00)		(0.00)
Constant	0.247***	0.230***	0.975***	0.958***
	(0.04)	(0.04)	(0.01)	(0.01)
N	2128	2120	2797	2787

Table 5.1d: Health services uptake, population interactions

	(1) Medical delivery	**(2) Medical delivery**	**(3) Percent vaccinated**	**(4) Percent vaccinated**
	b/se	b/se	b/se	b/se
Endline	0.136***	0.140***	-0.588***	-0.591***
	(0.04)	(0.04)	(0.04)	(0.05)
Treatment	0.130**	0.142**	0.007	0.013
	(0.05)	(0.06)	(0.01)	(0.01)
Interaction	-0.104*	-0.146**	0.024	0.030
	(0.06)	(0.06)	(0.06)	(0.07)
Population	0.000	0.000	0.000	0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Endline * Population	0.000	-0.000	0.000***	0.000***
	(0.00)	(0.00)	(0.00)	(0.00)
Treatment * Population	-0.000	-0.000	-0.000	-0.000
	(0.00)	(0.00)	(0.00)	(0.00)
Interaction * Population	0.000	0.000	-0.000*	-0.000*
	(0.00)	(0.00)	(0.00)	(0.00)
Distance		-0.002***		0.001*
		(0.00)		(0.00)
Constant	0.120***	0.203***	0.976***	0.957***
	(0.03)	(0.04)	(0.01)	(0.01)
N	2717	2120	3575	2787

Table 5.1e: OLS results corrected for spatial dependence: medical delivery

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	.1479***	.0187	.2186***	.0301
Endline	.1513***	.0252	.1409***	.0300
Treatment	.1260***	.0313	.0936**	.0387
Interaction	-.0837**	.0385	-.0922**	.0448
Population			.0000	.0000
Distance to DC			-.0021***	.0004
N	2775		2152	

Table 5.1f: OLS results corrected for spatial dependence: percent vaccinated

Variable	(1) OLS Estimates	(1) SE corrected for Spatial Dependence	(2) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	3.919***	.0165	3.8833***	.0246
Endline	-3.4762***	.0260	-3.4824***	.0297
Treatment	.0226	.0217	.0156	.0260
Interaction	-.0248	.0386	-.0139	.0432
Population			.0000	.0000
Distance to DC			.0004	.0003
N	3658		2839	

Table 5.2a: Educational services uptake, matched on Set 1

	(1) Number of boys in school	(2) Number of boys in school	(3) Percent boys in school	(4) Percent boys in school
	b/se	b/se	b/se	b/se
Endline	0.073	0.112*	-0.097***	-0.077***
	(0.05)	(0.06)	(0.02)	(0.03)
Treatment	0.099	0.076	-0.015	-0.017
	(0.07)	(0.08)	(0.03)	(0.03)
Interaction	-0.019	-0.110	0.023	-0.017
	(0.08)	(0.09)	(0.03)	(0.04)
Population		0.000***		0.000**
		(0.00)		(0.00)

Distance to DC		0.000		-0.000
		(0.00)		(0.00)
Constant	0.923***	0.906***	0.848***	0.842***
	(0.05)	(0.07)	(0.02)	(0.03)
N	5296	4123	3473	2732

Table 5.2b: Educational services uptake, matched on Set 2

	(1) Number of boys in school	**(2) Number of boys in school**	**(3) Percent boys in school**	**(4) Percent boys in school**
	b/se	b/se	b/se	b/se
Endline	0.071	0.084	-0.104***	-0.122***
	(0.05)	(0.06)	(0.02)	(0.03)
Treatment	0.093	0.034	-0.021	-0.054
	(0.06)	(0.07)	(0.03)	(0.03)
Interaction	-0.015	-0.025	0.027	0.057
	(0.08)	(0.10)	(0.03)	(0.04)
Population		0.000***		0.000***
		(0.00)		(0.00)
Distance to DC		-0.001		-0.001*
		(0.00)		(0.00)
Constant	0.907***	0.894***	0.851***	0.862***
	(0.05)	(0.07)	(0.02)	(0.03)
N	5153	3912	3338	2524

Table 6a: Diversification of income, matched on Set 1

	(1) Off-farm activity (male)	**(2) Off-farm activity (female)**	**(3) Female works outside household**
	b/se	b/se	b/se
Endline	0.092***	0.381***	-0.154**
	(0.03)	(0.08)	(0.06)
Treatment	0.028	0.158	-0.120*

	(0.03)	(0.11)	(0.07)
Interaction	0.013	-0.008	0.057
	(0.04)	(0.12)	(0.07)
Constant	0.442***	0.268***	0.389***
	(0.02)	(0.07)	(0.06)
N	5261	1326	5266

Table 6b: Diversification of income, matched on Set 2

	(1) Off-farm activity (male)	**(2) Off-farm activity (female)**	**(3) Female works outside household**
	b/se	b/se	b/se
Endline	0.101***	0.453***	-0.161***
	(0.03)	(0.08)	(0.06)
Treatment	0.023	0.179	-0.123*
	(0.03)	(0.11)	(0.07)
Interaction	0.001	-0.090	0.079
	(0.04)	(0.11)	(0.07)
Constant	0.444***	0.256***	0.380***
	(0.02)	(0.07)	(0.06)
N	5124	1250	5125

Table 6c: Diversification of income, distance interactions

	(1) Off-farm activity (male)	**(2) Off-farm activity (female)**	**(3) Female works outside household**
	b/se	b/se	b/se
Endline	0.095**	0.333***	-0.143**
	(0.05)	(0.10)	(0.07)
Treatment	0.002	0.105	-0.111
	(0.05)	(0.12)	(0.09)
Interaction	0.031	0.133	-0.022
	(0.06)	(0.13)	(0.09)
Distance	-0.001***	0.001	0.000
	(0.00)	(0.00)	(0.00)
Endline * Distance	0.001	0.001	-0.000
	(0.00)	(0.00)	(0.00)
Treatment * Distance	0.001	0.003	-0.001
	(0.00)	(0.00)	(0.00)
Interaction * Distance	-0.002	-0.004**	0.003

	(0.00)	(0.00)	(0.00)
Constant	0.475***	0.231**	0.391***
	(0.03)	(0.09)	(0.07)
N	4105	1056	4108

Table 6d: Diversity of income, population interactions

	(1) Off-farm activity (male)	**(2) Off-farm activity (female)**	**(3) Female works outside household**
	b/se	b/se	b/se
Endline	0.086**	0.424***	-0.228***
	(0.04)	(0.11)	(0.08)
Treatment	0.048	0.232**	-0.193**
	(0.03)	(0.10)	(0.09)
Interaction	0.014	0.002	0.138
	(0.05)	(0.15)	(0.09)
Population	0.000	0.000***	-0.000*
	(0.00)	(0.00)	(0.00)
Endline * Population	-0.000	-0.000**	0.000**
	(0.00)	(0.00)	(0.00)
Treatment * Population	-0.000	-0.000**	0.000*
	(0.00)	(0.00)	(0.00)
Interaction * Population	0.000	0.000	-0.000**
	(0.00)	(0.00)	(0.00)
Constant	0.421***	0.102*	0.452***
	(0.03)	(0.05)	(0.08)
N	5153	1314	5156

Table 6e: OLS results corrected for spatial dependence: off farm and outside employment

Variable	**(1) OLS Estimates (Off farm activity)**	**(1) SE corrected for Spatial Dependence (Off farm activity)**	**(2) OLS Estimates (Off farm activity)**	**(2) SE corrected for Spatial Dependence (Off farm activity)**	**(3) OLS Estimates (FHH Works outside household)**	**(3) SE corrected for Spatial Dependence (FHH Works outside household)**
Constant	.4424***	.0177	.2675***	.0348	.3891***	.0253

Endline	.0923***	.0275	.3809***	.0499	-.1543***	.0283
Treatment	.0275	.0250	.1576***	.0608	-.1198***	.0355
Interaction	.0133	.0351	-.0077	.0724	.0567	.0399
N	5261		1326		5266	

Table 7.1a: Food purchases by households in the past week, matched on Set 1

	(1) Hunger	**(2) Beef**	**(3) Lamb**	**(4) Eggs**	**(5) Flour**	**(6) Flour (among bottom 50 percent)**
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-1.039***	0.055	-0.376***	-0.209	-9.019***	-7.981***
	(0.18)	(0.13)	(0.12)	(0.59)	(1.61)	(0.56)
Treatment	-0.241	0.045	-0.329***	1.092*	-0.655	-0.619
	(0.20)	(0.10)	(0.11)	(0.56)	(1.49)	(0.63)
Interaction	0.409*	0.081	0.298**	0.198	3.967	1.657**
	(0.24)	(0.15)	(0.14)	(0.85)	(2.56)	(0.79)
Constant	2.986***	0.358***	0.842***	2.226***	27.188***	17.592***
	(0.15)	(0.09)	(0.09)	(0.30)	(1.10)	(0.46)
N	5151	5258	5272	5224	5276	2801

Table 7.1b: Rice purchases by household in past week, matched on Set 1

Variable	**(1) Rice**	**(2) Rice (among bottom 50%)**
	b/se	b/se
Endline	1.136**	-0.225**
	(0.45)	(0.10)
Treatment	0.949*	0.261**
	(0.54)	(0.12)
Interaction	-0.616	-0.106
	(0.57)	(0.14)
Constant	3.080***	0.731***
	(0.44)	(0.08)
N	5280	1760

Table 7.1c: Food purchases by households in past week, matched on Set 2

	(1)Hunger	(2) Beef	(3) Lamb	(4) Eggs	(5) Flour	(6) Flour (among bottom 50 percent)
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	-1.016***	0.015	-0.388***	-0.696	-8.498***	-8.066***
	(0.18)	(0.13)	(0.12)	(0.55)	(1.50)	(0.58)
Treatment	-0.217	-0.017	-0.335***	0.756	-0.401	-0.678
	(0.21)	(0.11)	(0.11)	(0.57)	(1.46)	(0.62)
Interaction	0.377	0.139	0.309**	0.738	3.427	1.665**
	(0.24)	(0.15)	(0.15)	(0.83)	(2.52)	(0.80)
Constant	2.977***	0.413***	0.858***	2.518***	26.927***	17.694***
	(0.15)	(0.09)	(0.09)	(0.32)	(1.03)	(0.45)
N	5013	5128	5135	5095	5131	2746

Table 71d: Rice purchases by household in past week, matched on Set 2

Variable	(1) Rice	(2) Rice (50%)
	b/se	b/se
Endline	1.094**	-0.202*
	(0.46)	(0.10)
Treatment	0.849	0.282**
	(0.56)	(0.12)
Interaction	-0.575	-0.124
	(0.59)	(0.14)
Constant	3.214***	0.701***
	(0.47)	(0.08)
N	5135	1693

Table 7.1e: OLS results corrected for spatial dependence: hunger

Variable	(1) OLS Estimates	(2) SE corrected for Spatial Dependence
Constant	2.9863***	.0748
Endline	-1.0388***	.0927
Treatment	-.2411**	.1109
Interaction	.4093***	.1316
N	5151	

Table 7.1f: OLS results corrected for spatial dependence: specific expenditures

Variable	(1) OLS Estimates (Beef)	(1) SE corrected for Spatial Depende	(2) OLS Estimates (Lamb	(2) SE corrected for Spatial Dependen	(3) OLS Estimates (Eggs)	(3) SE corrected for Spatial Depende	(4) OLS Estimates (Flour)	(4) SE corrected for Spatial Depende

		nce (Beef)	)	ce (Lamb)		nce (Eggs)		nce (Flour)
Constant	.3581***	.0425	.8420***	.0759	2.2260***	.2095	27.1880***	.6291
Endline	.0551	.0575	-.3760***	.0890	-.2095	.3668	-9.0191***	.8982
Treatment	.0454	.0580	-.3286***	.0883	1.092***	.3689	-.6551	.8666
Interaction	.0812	.0772	.2978***	.1109	.1978	.5724	3.9671***	1.3889
N	5258		5272		5224		5276	

Table 7.2a: Expenditures by households in the past month, matched on Set 1

	(1) Expenditure	**(2) Expenditure 1**	**(3) Expenditure 2**	**(4) Expenditure**	**(5) Expenditure 1**	**(6) Expenditure 2**
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	8832.463***	3113.805***	68623.885***	-3821.203***	-3149.051***	-8065.828***
	(1791.45)	(1039.92)	(16842.41)	(117.05)	(118.48)	(494.77)
Treatment	-175.500	310.109	-5827.299	8.178	94.090	-1030.941
	(758.22)	(524.16)	(4542.95)	(172.59)	(175.09)	(647.53)
Interaction	604693.977	607268.894	-30898.845*	-8.178	-94.090	1030.941
	(606547.82)	(606183.88)	(18516.27)	(172.59)	(175.09)	(647.53)
Constant	10111.886***	7252.506***	34312.558***	3821.203***	3149.051***	8065.828***
	(592.12)	(377.77)	(3979.17)	(117.05)	(118.48)	(494.77)
N	5322	5322	5322	828	828	828

Table 7.2b: Expenditures by households in the past month, matched on Set 2

	(1) Expenditure	(2) Expenditure 1	(3) Expenditure 2	(4) Expenditure	(5) Expenditure 1	(6) Expenditure 2
	b/se	b/se	b/se	b/se	b/se	b/se
Endline	8063.300***	2915.361**	61775.264***	-3785.181***	-3105.284***	-8158.767***
	(1773.49)	(1135.78)	(16789.02)	(118.67)	(117.36)	(471.44)
Treatment	-329.528	260.238	-7077.193	37.603	132.520	-1139.005*
	(776.63)	(529.29)	(4826.63)	(174.74)	(175.42)	(629.88)
Interaction	616923.085	618856.722	-23203.480	-37.603	-132.520	1139.005*
	(617728.52)	(617359.39)	(18492.14)	(174.74)	(175.42)	(629.88)
_cons	10269.147***	7309.883***	35511.170***	3785.181***	3105.284***	8158.767***
	(609.99)	(378.62)	(4290.85)	(118.67)	(117.36)	(471.44)
N	5178	5178	5178	828	828	828

Table 7.2c: OLS results corrected for spatial dependence: expenditures

Variable	(1) OLS Estimates (Expenditure)	(1) SE corrected for Spatial Dependence (Expenditure)	(2) OLS Estimates (Expenditure1)	(2) SE corrected for Spatial Dependence (Expenditure1)	(3) OLS Estimates (Expenditure2)	(3) SE corrected for Spatial Dependence (Expenditure2)
Constant	10111.89***	412.82	7252.51***	257.01	34312.56***	2946.87
Endline	8832.46***	1394.03	3113.80***	825.76	68623.89***	11389.77
Treatment	-175.50	548.32	310.11	381.24	-5827.30*	3481.87
Interaction	604693.98	607361.13	607268.90	607302.69	-30898.85**	13242.59
N	5322		5322		5322	