



Does Community-Based Development Program Boost Access of Rural Households to Utilities and Services? Evidence from A Controlled Randomized Trial in Afghanistan

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ABSTRACT

This paper explores the impact of National Solidarity Program (NSP), which operates under umbrella of Community-Based Development approach, on access of rural households to utilities (i.e. clean drinking water and electricity) and services (i.e. education and health services). Using field experimental data collected by the NSP evaluation team to mitigate selection bias and Ordinary Least Square (OLS) method, the paper found that the program has, in general, increased access of households to utilities and services in the medium-term due to spanning of deep wells and implementing power generation projects in the treatment villages. Particularly, the evidence indicates that the program has decreased water collecting time and increased electricity usage by 88 and 102 percent, respectively. Likewise, the intervention has increased girls' school attendance too, while the program has relatively decreased water quality. In the short-term, the intervention happens to have negative impact on some outcome indicators in the treatment villages. While in the short-term, the program increases water quality and probability of illnesses being treated by a medical professional, it does significantly increase water collecting time and decrease electricity usage of households by 84 and 70 percent, respectively. These negative impacts might have happened due to poor projects design, or perhaps enough budget was not allocated to infrastructure projects. In the medium term, the program does not impact school attendance of boys and infant mortality. It is also documented that in the short-term, the intervention has no effect on infant mortality. Therefore, a follow up study is required to find out the reasons that caused the program to not impact the treatment villages.

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INTRODUCTION

After the establishment of the President Karzai's administration in 2001, the world community has been considerably involved in Afghanistan. International development agencies have been providing financial and technical assistance to the Government of Afghanistan. The effectiveness of aid programs has, however, been a subject of controversy. The unequal distribution of aids among regions, especially with respect to conflict-affected areas, has been accused of further fueling the conflict dynamics in the country. As one consequence, the international development agencies, in particular the World Bank, proposed the so-called Community-Based Development (CBD) program to promote equal geographical distribution of development aid programs in the rural areas of Afghanistan.

The community-based and -driven development programs¹ are regarded as a popular approach for transferring development assistance to aid poor people in developing countries. Essentially, in this modal the community members possess and exercise all key decision-making rights, including prioritizing their needs, selecting, implementing, overseeing and maintaining the projects. These programs increase social interaction and dialogue among community members and between communities and local government, which can contribute to conflict suppression (Schuler et al., 1991).

This approach can be of importance for sustainable and balanced development in countries with elevated levels of urban-rural disparity. By channeling development assistance to rural areas and involving rural households in key decisions of the implementing of assistance, typically through projects, institutions of governance ensure their legitimacy and promote sustainable development. However, the potential gain and loss of these programs depend on a range of other factors that are the object of controversy among researchers. Some researchers argue that these programs involve community members in all decision-making process of the projects, provide an advocacy platform for poorer people, and distribute power (Labonne and Chase, 2011), decrease conflict (Mansuri and Rao, 2004), and build capacity in the rural areas (Beath et al., 2013). Furthermore, some researchers argue that while these models are conflict sensitive, they decrease efficiency by involving villagers in technical decisions (Khwaja, 2009) and perhaps pave the way for elites and/or ethnic groups to override the rights of others (Rao and Ibáñez, 2005).

The past four decades of war in Afghanistan, at its worst featuring civil and ideological identity groups, exacerbated income inequality all over the country, and increased the disparity between urban and rural populations. Therefore, the newly established Government of Afghanistan initiated the CBD program in 2003 to inhibit conflict dynamics in the country, and to decrease the disparity between urban and rural areas (Anthony et al., 2014). For this purpose, the National Solidarity Program (NSP), utilizing a standard CBD approach, was launched.

NSP was the first CBD program to be implemented in Afghanistan. The program was originally established to raise living conditions, tackle poverty, and decrease conflict in rural areas. NSP provides a broad range of facilities, including infrastructure development such as roads and irrigation systems, services including schooling and basic health care, and utilities provision including electricity and clean drinking water. Furthermore, the program works to strengthen the relations between government and public, to increase local governance capacity to perform and deliver, and provide equal opportunity for both men and women to represent their communities through democratic process (King and Samii, 2014).

Though international and national development agencies and independent academics have been evaluating the impact of the CBD programs on rural households through experimental, quasi-experimental, and qualitative research, remain few academic papers on the impact of CBD programs which utilize household micro experimental data. Thus, using field experimental data from the NSP to reveal the causal effect of a CBD program on the access of poor people to utilities and services in rural areas of Afghanistan can be considered complementary to existing studies.

Research Objectives

This paper examines the causal effect of the CBD program, using NSP as a case study, on access of rural households to utilities and services. For this purpose, several outcome variables that are affected by CBD programs in rural areas have been selected, notably access to: clean drinking water, electricity, basic education, and basic health care services. Since NSP is the first CBD program in Afghanistan, evaluating its impact on utilities and services is of particular importance for policy makers.

¹ Community driven development is the recent version of CBD program which consists of projects with a broader range of objectives such as empower local governance, downward authority and accountability, and enriched local capacity (Schuler et al., 1991).

Endogeneity is the main possible challenge for the analysis. International development agencies or implementers of the program, for instance, may choose to implement projects in communities with high potential return, or in those communities that have low potential returns. Either way, the estimation of Ordinary Least Square (OLS) will be biased. To mitigate this intrinsic bias, the research utilizes randomized village-level field experimental data collected by the NSP. The data were collected by the NSP impact evaluation team in three phases – baseline, mid-line, and end-line surveys in 2007, 2009, and 2011, respectively.

LITERATURE REVIEW

The impact of CBD programs has been extensively evaluated to determine whether they positively affect communities or have detrimental effect. Not only independent researchers, but also national public agencies and international organizations have been interested in revealing the causal effect of CBD modal on communities.

Rocha and Soares (2009) and Bjorkman and Svensson (2009) found that CBD intervention reduced child mortality in Brazil and Uganda, respectively. Furthermore, Labonne and Chase (2011) and Rao and Ibáñez (2005) showed that CBD programs increased equality in the Philippines and Jamaica, respectively. Desta and Jun (2015) found that the program reduced poverty and increased income from agriculture in Indonesia. Similarly, Park and Wang (2010) found that CBD programs increased village-financed investment in China. Likewise, Sarwary and Jinnai (2017) present evidence that CBD program improved economic welfare in Afghanistan.

Some examples of quantitative and qualitative research contest the positive impact of community-based and -driven development programs. Labonne and Chase (2011) and Park and Wang (2010) found that CBD program decreased social capital² in the Philippines and China, respectively.

Although there is some research on the impact of the CBD program in Afghanistan, there are few research papers that examine the impact of NSP on improving access of rural households to utilities and services. In response, this paper reveals the causal effect of NSP on the access of rural households to utilities and services, as a contribution to the existing literature.

DATA AND METHODOLOGY

Data

This paper utilizes village level experimental data gathered by the NSP evaluation team in three phases – baseline, midline, and end-line in 2007, 2009, and 2011, respectively. The baseline data were collected to measure the pre-treatment balance of treatment and control villages in 2007. In 2009, during program implementation, the survey collected data in the midline to assess the short-term or immediate impact of NSP in treatment villages. However, the end line-data were collected at the completion point of the program – prior to NSP intervention in control villages – to evaluate the medium-term effect of the program in treatment villages. The sample of three consecutive surveys consisted of 25,000 households, 500 villages, 10 districts, and 6 provinces of Afghanistan (Beath et al., 2013).

Experimental Design

In the experimental method employed, the treatment and control groups have been chosen randomly and treated equally to reduce the risk of advantage of one over the other. In the randomly assigned method, observed and unobserved factors that influence the results are equally likely to be involved in both treatment and control groups (Conquest, 2000). The NSP evaluation team has used field experimental method to test the causal effect of the NSP on rural households.

Sample Selection

The sample consists of 500 villages, located in 10 districts, and 6 provinces, which are distributed evenly across the country to provide for geographical inclusivity. The selection of sample was executed in two stages: firstly, 10 districts were selected out of Afghanistan's 398 districts; and thereafter, 50 villages were selected in each of the sample districts. Three main criteria for selecting the districts are noted below:

² The ease with which the community people act collectively is called social capital.

1. *New District*: since its outset in 2003 to spring 2007, NSP operations were expanded to 279 out of 398 districts in Afghanistan. Hence, the NSP evaluation team was to choose the districts that had not yet received NSP. As a result, 74 districts detected with no program intervention in 2007.
2. *Security*: the second criterion for selecting sample districts was assessment of the security situation, a necessary consideration in consequence of widespread suffering from insecurity and insurgency in Afghanistan. After complete analysis of the security situation by Vulnerability Analysis Unit and considering the necessity to minimize security risks for data collecting staff, 34 of 74 new districts were removed from consideration.
3. *Minimum of 65 villages*: the final element for choosing sample districts was that districts must contain at least 65 villages. The NSP evaluation team used village-level and district-level data provided by the Central Statistics Organization (CSO), Ministry of Rural Rehabilitation and Development (MRRD), and USAID to make sure that those districts have fulfilled this criterion. In consequence, only 23 districts were selected for the study.

Subsequently, 10 districts were randomly selected from these 23 for inclusion in the study. These 10 districts represent all ethno-linguistics identity groups in Afghanistan, with five predominantly Tajik (Adraskan, Chisht-e Sharif, Gulran, Dualina, and KhostWa Firing), four predominantly Pashtun (Balkh, Fersi, Hisarak, and Shirzad), and one mostly Hazara (Sang Takht) by ethnicity.

Treatment and Control Groups Assignment

Out of 65 sample villages, 50 villages in each district were selected by random sampling to be included in the study. Of these, 25 villages were selected by random sampling as the treatment group (in receipt of NSP), while the remaining 25 villages comprised the control group (not in receipt of NSP). The evaluation team used Global Positioning System (GPS) data and consulted with community members to ensure that the correct villages were included in the research.

A major consideration was to ensure that the estimated impact of the intervention is not contaminated by selection bias. For this purpose, a mechanism was adopted for sample villages to have equal probability to be assigned to the treatment group. Another main challenge in ensuring randomization is the possibility of contamination by attrition, i.e. villages voluntarily or involuntarily withdrawing from the study, and to keep statistical balance between both treatment and control groups. The NSP evaluation team used a special randomization technique to match pairs of sample villages in each district. In result, 25 matched pairs of sample villages were formed in each district, where one village of each pair was randomly assigned in treatment group and one in control group (Beath et al., 2013). The definition of matched-pair randomization is provided by (King et al., 2007).

In matched pair randomization, we first select pairs of [sample units] that are matched, or at least as similar as possible, on a large set of available background characteristics. Then we randomly choose one of the two [sample units] within each pair, by flipping a coin, to receive treatment and the other to be the control. The result of this process is exact balance between the entire treated and control groups of [sample units] on all variables included in the matching and for which exact matches among the [sample units] are available, or near matches otherwise. Variables not matched on are balanced by randomization and therefore only match on average (14-15).

In addition, another critical concern regarding assignment to the treatment group was the possibility of spillover between villages. In case of positive spillover (of benefit) from a village in the treatment group to a village in the control group, the actual impact of the intervention will be underestimated. Hence, one will not be able to accurately identify the effect of the program even if such exists. Likewise, a negative spillover from treatment village to control village will overestimate the impact of the treatment, resulting in a false identification of the program effect even if it does not exist. Either way the result will be biased. To mitigate this problem, the NSP evaluation team placed a condition that villages within one-kilometer distance should be assigned to the same treatment status. However, this rule was not applicable in two districts due to close distance of villages.

An optimal greedy matching algorithm³ based on Mahalanobis distance⁴ between the observations was used to construct matched pairs. Firstly, optimal greedy matching procedure was used to calculate the Mahalanobis between

³The optimal greedy matching algorithm is a method used for matching purposes. This approach generates optimal constructions for several statistical matchings including the formation of matched samples with multiple controls, with a variable number of controls, and the creation of balanced matched samples that combine features of pair matching and frequency matching (Rosenbaum, 1989).

⁴Mahalanobis distance is described as $d = \sqrt{(\bar{x}_1 - \bar{x}_2)' V^{-1} (\bar{x}_1 - \bar{x}_2)}$, where \bar{x}_1 is a vector of specification of community i , and V is the covariance matrix of matching characteristics. Intuitively, we first compute the difference in each of the principal characteristics separately and then

each viable pair of villages in the district. Then, pairs of villages were selected with least Mahalanobis distance as a matched-pair, with a condition that pairs should not belong to the same cluster⁵. The chosen pair was then excluded from the collection of feasible pairs. This procedure was repeated until all the villages were matched in pairs.

The random assignment of treatment and control groups were executed in four steps as below:

1. *Clusters*: To minimize the risk of spillover between treatment and control groups, villages located within one kilometer of each other were grouped in clusters. 107 out of 500 villages were assigned to 41 clusters averaging 2 to 6 villages in each cluster. All districts had at least one cluster, with maximum 10 clusters in Khost Wa Fring district of Baghlan province, and 6 clusters in Shirzad district of Ningarhar province.
2. *Matched Pairs*: Using an optimal greedy matching algorithm, the 50 sampled villages were paired into 25 groups with each pair having 2 villages. This practice was done to ensure the background similarities of villages, and that the villages were not placed in the same cluster. The matching criteria include number of households, distance to nearest river, main language, distance to district center, topography, and availability of primary school in the community.
3. *Assignment of Treatment*: Using a random number generator, in each matched pair, one village was selected in the treatment group and the other in the control group. In order to limit the risk of spillover, clusters of villages were either all assigned to the treatment group or all assigned to the control group⁶.
4. *Clustering Violations*: In some villages, due to substantial number of clustered villages, the co-assignment of clustered villages to the same treatment and control groups was not applicable. To minimize the possibility of violations for such cases, the evaluation team used a simulation approach⁷.

Dependent Variables

NSP intervention has been divided into five families, one of which is “utilities and services”, therefore the dependent variables are selected based on the NSP intervention classification. The main dependent variables are access to clean drinking water, electricity usage, education, and health services. Furthermore, each of these dependent variables is measured by multiple indicators, which are discussed below:

- Access to clean drinking water is identified by the number of seasons in which water had inferior quality in the past year, and hours spent collecting water in the past week (log).
- Electricity usage is measured by hours of electricity used in the past month (log).
- Access to education is measured by school attendance of boys and girls in the past week.
- Access to health care is measured by child mortality, and the number of illness treated by medical doctor in the past year.

Independent Variables

The treatment effect of the intervention is the main independent variable. However, to check the robustness of the treatment effects, the independent control variables (which are used to check balance of treatment and control groups in the baseline), are also used. The independent variable takes the value of “1” for the treatment group, and the value of “0” for the control group. The control variables are household-level and village-level characteristics collected in the baseline survey before the treatment intervention and consist of:

merge these differences, which gives more weight to those characteristics that have the lowest variance and covariance with other characteristics. Considering variance makes this computation independent of the units of measurement, whereas taking into account covariance reduces individual weights for the characteristics that normally go hand in hand. Due to the significant heterogeneity among districts, a covariance matrix was computed for each district independently, using the data for all communities in the district for which the matching data was available, not limited to those that were included in the evaluation study (Beath, et al., 2008).

⁵This method differs from optimal matching, in which pairs are chosen to reduce the total Mahalanobis distance between each of the pairs. The disadvantage of optimal matching is that any drop in the sample results in a loss of optimality. In contrast, matched pairs produced with a greedy optimal algorithm are optimal, given the constraints, and maintain their optimality even if matched-pairs are lost. As was expected, some of the matched-pairs were missing during the course of the study, therefore an optimal greedy matching procedure was used.

⁶This was done by executing an algorithm: after a village has been assigned to a treatment status, all the other villages in the same cluster were assigned to the same treatment status. The other villages in the respective matched-pairs were then assigned the complimentary treatment status (Beath et al. 2013).

⁷ To understand why this might happen, imagine a situation in which there are three clusters with two villages in each cluster. Suppose a village from cluster 1 is matched with a village from cluster 2, the other village from cluster 2 is matched to a village from cluster 3, and the other village from cluster 3 is matched to the remaining village from cluster 1. If both villages in cluster 1 are assigned to the treatment group, then their matches in the clusters 2 and 3 will be assigned to the control group. Whichever way we assign treatment status to the remaining pair of villages (one in cluster 2 and one in cluster 3), one of them will have to be assigned to the treatment group even though the other village in that cluster is already assigned to the control group (Beath et al., 2013).

- Water source: what is your main source of drinking water (hand pump, deep well, spring, pipe, Kariz, canal, lake)?
- Water shortage: has your household faced shortage of water in the past week?
- Last month electricity shortage: how many days has your household had electricity last month?
- School accessibility: how many schools are in your village, are schools accessible to both girls and boys?
- Study place (girl): where do the girls in your village study (school, home, mosque, other)?
- Study place (boy): where do the boys in your village study (school, home, mosque, other)?
- Doctor availability: is there a doctor available to treat a patient at any time?
- Clinic availability: when someone is ill, where do u usually take him/her (hospital, clinic, home/traditional, other)?

Methodology

This paper uses Ordinary Least Square (OLS) method to test the impact of the NSP on access of rural households to utilities and services. Particularly, the paper estimates the impact of NSP intervention by comparing the average changes in outcomes of interest between a treatment group (those villages that received NSP intervention) and a control group (those villages that did not receive NSP intervention). For all individual indicators, the treatment impact of the program was estimated both for the midline (short-term effect of the program during its implementation) and for the end line (medium-term impact of the program after its implementation). By estimating the effects of the program during its implementation and after its completion, one might compare the program effects average changes over time⁸.

Model:

$$Y_{tvi} = \beta_0 + \beta_1 T_v + \varepsilon_{iv} \quad (1)$$

where Y_{tvi} is the outcome variable of interest for household i in village v at time t , T_v is village dummy for treatment village, and ε_{iv} is the error term. To check the robustness, the baseline characteristics of households are added as independent control variables

$$Y_{tvi} = \beta_0 + \beta_1 T_v + \beta_2 x_{iv} + \varepsilon_{iv} \quad (2)$$

$\beta_2 x_{iv}$ shows the socio-economic characteristics of the households and functions here as a control variable.

With estimating Equation (1) and (2), the self-selection or intentional intervention placement problem may happen. Self-selection may occur because of observed and unobserved factors. In the case of unobserved characteristics, there will be a correlation between the error term and explanatory variable (i.e. treatment village dummy T_v), which would result in selection bias. To paraphrase, the covariance of independent variable (treatment village dummy) and error term will not be equal to zero [i.e. $\text{cov}(T_v, \varepsilon) \neq E(T_v, \varepsilon) \neq 0$]. In this case, an important assumption of ordinary least square method (estimating unbiased parameter) will be violated, which will further bias other estimates in the Equation (Wooldridge, 2016). The dilemma is outlined in a conceptual way by Sarwary and Jinnai (2017) as below:

Suppose one is interested in evaluating the effect of community-based intervention on households [access to electricity]. Let Y_{vi} represent the [electricity] for household i in village v . For beneficiary in treatment village, $T_v = 1$, and the value of Y_{vi} under treatment is represented as $Y_{vi}(1)$. For non-beneficiary, $T_v = 0$, and Y_{vi} can be represented as $Y_{vi}(0)$. If $Y_{vi}(0)$ is used within non-beneficiary households as a comparison outcome for participant outcomes $Y_{vi}(1)$, the average effect of the program might be represented as follows:

$$D = E(Y_{vi}(1) | T_v = 1) - E(Y_{vi}(0) | T_v = 0). \quad (3)$$

There is a concern that the treatment and control groups are likely to be different before intervention, implying that the impact difference between treatment and control groups perhaps are not solely due to program implementation. If, in Equation 3, the expected outcome for non-beneficiaries participated in the program – $E(Y_{vi}(0) / Y_{vi}(1))$, are added and subtracted, the following Equations will be derived.

⁸ This paper follows Sarwary & Jinnai (2017).

$$D = E(Y_{vi}(1) | T_v = 1) - E(Y_{vi}(0) | T_v = 0) + [E(Y_{vi}(0) | T_v = 1) - E(Y_{vi}(0) | T_v = 0)]. \quad (4)$$

$$D = ATE + [E(Y_{vi}(0) | T_v = 1) - E(Y_{vi}(0) | T_v = 0)]. \quad (5)$$

$$D = ATE + B. \quad (6)$$

In these Equations, *ATE* is the average treatment effect [$E(Y_{vi}(1) | T_v = 1) - E(Y_{vi}(0) | T_v = 1)$] (the average benefits in outcome of beneficiaries comparing to non-beneficiaries, pretending that non-beneficiaries are also located in treatment group). *ATE* depicts a situation in which treatment units are randomly chosen from the population (i.e. both treatment and control groups have the same probability to receive the intervention). The term *B*, [$E(Y_{vi}(0) | T_v = 1) - E(Y_{vi}(0) | T_v = 0)$], shows the extent of selection bias that appears in using *D* as an estimate of the *ATE*. The calculation of the size of selection bias is difficult because one cannot exactly realize $E(Y_{vi}(0) | T_v = 1)$. Hence, if one does not know the extent to which selection bias constitute *D*, it is impossible to determine the disparity in outcomes between treatment and control groups. The main purpose of rigorous impact analysis is to eliminate selection bias or reasonably justify it. As suggested by Deaton (2009) and Rosenbaum (2010), one way to solve this problem is to assign the program randomly. Selection bias will also be overcome, if one can assume whether the units of interests (households or individuals) receive treatment independent of the outcome they experienced (Sarwary and Jinnai, 2017).

Since two dependent variables – child mortality and illnesses treated by medical doctor – are binary, the paper uses logistic regression (logit model) to estimate the impact of the intervention on each of them.

$$\text{Ln} \left[\frac{\hat{p}}{1-\hat{p}} \right] = \beta_0 + \beta_1 X \quad (7)$$

The *ln* symbol refers to natural logarithm, \hat{p} is the expected probability of $Y = 1$ for any given value of X and can be computed from regression Equation as below, $\frac{\hat{p}}{1-\hat{p}}$ is the odds ratio and $\beta_0 + \beta_1 X$ refers to regression line.

$$\hat{p} = \frac{\exp(\beta_0 + \beta_1 x)}{1 + \exp(\beta_0 + \beta_1 x)} = \frac{e^{\beta_0 + \beta_1 x}}{1 + e^{\beta_0 + \beta_1 x}} \quad (8)$$

In the Equation (8), *exp* is the exponent function also denoted as *e*.

Internal Validity

Internal validity requires that only independent variables influence the outcome variable, hence anything other than explanatory variables impacting the dependent variable violate internal validity (Neuman, 2011). Internal validity is a crucial factor to overcome selection bias in an experimental study. As discussed earlier, selection bias is the most apparent risk to the experimental approach in the placement of treatment and control groups. One way to eliminate spillover in the assigning of treatment and control groups is for researchers to check for balance between these groups prior to intervention. If the random assignment is statistically independent, then both treatment and control groups should on average have similar mean value (Barrett and Carter, 2010) and (Deaton, 2009). The baseline data collected by the NSP impact evaluation team were used to check for internal validity.

Based on the available data, the baseline typical characteristics of treatment and control groups at both household-level and village-level are presented in Table 1. Panel A shows household-level baseline characteristics of treatment and control groups through statistical balance or mean deviation of both groups. The t-test results without control for other variables indicate that there is no major difference between mean value of treatment and control groups. As shown in the Table 1, mean value of household size, income, expenditure, time to collect water, hours of electricity used, and other characteristics for both treatment and control groups are quite similar. This means that both groups can be considered alike based on these characteristics, and that any difference is not statistically significant.

Table 1 Base-line Characteristics

	Treatment Average (1)	Control Average (2)	Difference (3)	P-Value
Panel A: Household Characteristics				
Household Size	9.761	9.874	-0.112	0.442
Time to Get Drinking Water	1.792	1.783	0.008	0.562
Average Hours Electricity	8.389	9.102	-0.712	0.141
Household Income	4428.583	4634.155	205.572	0.176
Amount of Loan	37977.240	40019.510	-2042.270	0.369
Return from Development Project	1109.971	788.195	321.775	0.253
Household Expenditure	3566.462	3644.121	-77.659	0.178
Irrigation Land	4.006	3.386	0.619	0.286
Panel B: Village Characteristics				
Number of Household in Village	120.148	110.057	10.090	0.262
Migration	9.774	11.399	-1.625	0.518
Hospital in Village	1.926	1.935	0.009	0.373
Development Project in Village	1.027	1.019	0.007	0.452
Number of School	8.022	7.884	0.138	0.212
Labor Wage	168.732	159.020	9.711	0.355
Observations	4929	2441		

Notes: This Table indicates the baseline characteristics of households and villages for treatment and control group using the baseline survey. The first column includes mean value for 4,929 households selected from 250 treatment villages. The second column shows the mean value for 2441 households selected from 250 control villages. All differences are estimated using independent t-test without controls and with standard errors clustered at the village-group level.

Source: Sarwary and Jinnai (2017)

Panel B indicates village-level baseline characteristics of both treatment and control groups. As shown in the Table, there is no major difference between mean values for treatment and control groups based on these indicators. Both groups have very similar mean values for the number of hospitals, number of schools, and number of development projects. The difference in mean value of the village-level characteristics for both treatment and control groups are not statistically significant, which further strengthens the internal validity, and implies that the randomization has created a balanced treatment and control groups.

External Validity

External validity remains a critical concern in an experimental study even though internal validity is assured. Populational generalization, among others (i.e. naturalistic and theoretical generalization), is the main concern for this study. External validity is one of the important assumptions in experimental studies, which implies confidence on whether or not the sample can be considered representative of the population (Cartwright, 2007). If an experiment lacks external validity, the findings perhaps hold true for the sample only. Figure 1 shows that the sample comprises the ethno-linguistics characteristics of Afghanistan that can represent the population of the country. Similarly, ten sample districts are spread evenly across the country, which further suggests that the result of research can be relatively applied to the population in whole.

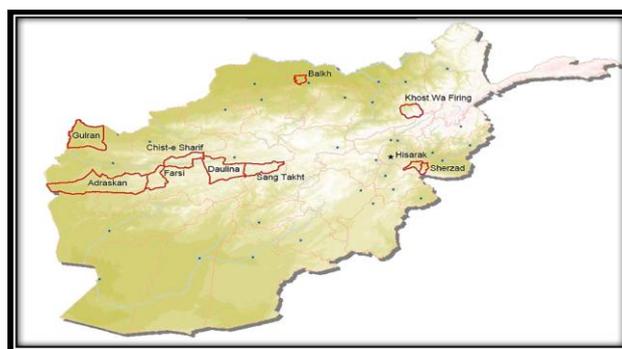


Figure 1 Ten sample districts

Source: Randomized impact evaluation of phase-II of Afghanistan's NSP (World Bank, 2010)

RESULT AND DISCUSSION

Result

NSP has objective, in part, to improve access of rural households to basic services and infrastructures through “block grants” disbursement for village-level projects. OLS regression results on outcome variables (i.e. access to clean drinking water⁹, power generation and electrical connectivity¹⁰, and schooling and health services) are presented in Tables 2, 3, 4, and 5, respectively. Particularly, Tables 2 and 3 indicate short-term and medium-term treatment effects of the intervention without controlling for base-line data. However, Tables 4 and 5 present short-term and medium-term treatment effects of the program with controlling for baseline data.

Table 2 reports regression results for treatment effects of the program on outcome variables in the short-term with no controls. The NSP water funded projects increased water collecting time by around 84 percent, with the coefficient being statistically significant at 1 percent level. However, as shown in column 3, there is a strong evidence that the program improved perceived quality of water, with the coefficient being statistically significant at 1 percent level. Furthermore, the program decreased households’ hours of electricity usage by 69 percent in the short term with the null hypothesis being rejected at 1 percent level. While there is no impact evidence on reducing child mortality, the probability of illness treated by a doctor in treatment villages is two times higher than control villages with the estimated odd ratio being statistically significant at five percent level.

Table 2 Short-Term Effect of NSP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Hours Collecting Water in the Past Week (log)	Water Inferior Quality Seasons	Hours of Electricity Used Last Month (log)	Days Girl Attended School in the Past Week	Days Boy Attended School in the Past Week	Infant Mortality ¹¹	Illness Treated by Medical Professional
Treatment	0.845*** (0.049)	-0.233*** (0.057)	-0.698*** (0.137)	-	-	1.262 (0.289)	2.163** (0.782)
Constant	1.442*** (0.031)	0.972*** (0.038)	2.186*** (0.102)	2.201*** (0.103)	3.065*** (0.108)	27.652*** (2.934)	61.685*** (8.462)
Observations	7,987	7,581	8,932	4,502	4,927	3,534	4,595
R-squared	0.128	0.007	0.016	0.000	0.000	0.001 (Pseudo R2)	0.008 (Pseudo R2)

Note: All regressions are Ordinary Least Square (OLS). Robust standard errors are clustered at village-level and reported in parentheses. Statistical significance at 1, 5, and 10 percent levels is denoted by ***, **, *, respectively.

Medium-term effects of NSP without any control is presented in Table 3. Unlike the short-term, the program substantially reduced the time for collecting water by 88 percent, with the coefficient being statistically significant at 1 percent level. However, there is also evidence that the intervention relatively reduced the quality of water. Interestingly, the program increased electricity usage of treatment households by 102 percent, which shows a huge positive impact of the program. Column 5 and 6 present medium-term estimate of treatment effects on school attendance for both boys and girls in the past week. While NSP does not impact school attendance of boys in the medium term, the program does increase girls’ attendance by 0.33 days per week. It is also evident from the Table that the program does not impact the number of illnesses treated by medical professional and child mortality.

⁹Water supply and sanitation projects (which span deep wells, water supply systems, as well as a few latrines) accounted for 21 percent of NSP-funded projects and 13 percent of block grant expenditure in treatment villages.

¹⁰Electricity projects which includes micro-hydro, solar panel, and power lines are accounted for 10 percent of projects implemented, and 26 percent of “block grants” expenditure in treatment villages.

¹¹ Most recent born was alive after 12 months.

Table 3 Medium-Term Effect of NSP

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Hours Collecting Water in the Past Week (log)	Water Inferior Quality Seasons	Hours of Electricity Used Last Month (log)	Days Girl Attended School in the Past Week	Days Boy Attended School in the Past Week	Infant Mortality	Illness Treated by Medical Professional
Treatment	-0.887*** (0.037)	0.117** (0.055)	1.023*** (0.134)	0.321* (0.190)	0.164 (0.182)	0.692* (0.140)	1.502 (0.501)
Constant	1.873*** (0.033)	0.881*** (0.037)	1.760*** (0.098)	2.046*** (0.140)	2.984*** (0.131)	32.350*** (3.672)	66.134*** (9.240)
Observations	7,987	7,581	8,932	4,502	4,927	3,534	4,595
R-squared	0.129	0.002	0.031	0.003	0.000	0.003	0.002
						(Pseudo R2)	(Pseudo R2)

Note: All regressions are Ordinary Least Square (OLS). Robust standard errors are clustered at village-level and reported in parentheses. Statistical significance at 1, 5, and 10 percent levels is denoted by ***, **, *, respectively.

Finally, the paper uses baseline characteristics of households to check for the robustness of the results. Tables 4 and 5 report the short-term and medium-term impact of the NSP for outcome variables of interest after controlling for baseline data. After controlling for baseline data, the regression results for treatment effects, main independent variable, are consistent with no control estimates. No significant difference is detected after controlling for baseline data, and none of the variables lost any significance level after controls. This similarity is identical with the results presented in Table 1 that indicates balance between treatment and control groups prior to intervention, which further supports the internal validity of research design.

Table 4 Short-Term Effect of NSP (Controlled for Baseline Characteristics)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Hours Collecting Water in the Past Week (log)	Water Inferior Quality Seasons	Hours of Electricity Used Last Month (log)	Days Girl Attended School in the Past Week	Days Boy Attended School in the Past Week	Infant Mortality	Illness Treated by Medical Professional
Treatment	0.843*** (0.049)	-0.223*** (0.055)	-0.680*** (0.120)	-	-	1.290 (0.296)	2.132** (0.771)
Water Source	-0.008 (0.086)	-0.574*** (0.090)					
Water Shortage	0.150** (0.069)	-0.053 (0.080)					
Last Month Electricity Shortage			0.010*** (0.001)				
School Accessibility				0.054 (0.199)	0.007 (0.226)		
Study Place (Girl)				1.106*** (0.205)			
Study Place (Boy)					0.931*** (0.203)		
Doctor Availability						0.314** (0.119)	4.988** (4.747)
Hospital Availability						0.957 (0.208)	1.234 (0.345)
Constant	1.349*** (0.052)	1.115*** (0.056)	1.849*** (0.091)	1.606*** (0.138)	2.577*** (0.147)	31.561*** (5.586)	47.946*** (9.952)
Observations	7,987	7,581	8,932	4,429	4,826	3,469	4,502
R-squared	0.131	0.026	0.164	0.041	0.027	0.009	0.015
						(Pseudo R2)	(Pseudo R2)

Note: All regressions are Ordinary Least Square (OLS). Robust standard errors are clustered at village-level and reported in parentheses. Statistical significance at 1, 5, and 10 percent levels is denoted by ***, **, *, respectively.

Table 5 Medium-Term Effect of NSP (Controlled for Baseline Characteristics)

VARIABLES	(1) Hours Collecting Water in the Past Week (log)	(2) Water Inferior Quality Seasons	(3) Hours of Electricity Used Last Month (log)	(4) Days Girl Attended School in the Past Week	(5) Days Boy Attended School in the Past Week	(6) infant mortality	(7) Illness Treated by Medical Professional
Treatment	-0.887*** (0.037)	0.123** (0.053)	0.998*** (0.128)	0.316* (0.176)	0.161 (0.174)	0.739 (0.151)	1.478 (0.494)
Water Source	0.038 (0.085)	-0.586*** (0.090)					
Water Shortage Last Month	0.167** (0.072)	-0.060 (0.079)					
Electricity School Accessibility Study			0.010*** (0.001)	0.056 (0.198)	0.007 (0.225)		
Place(Girl) Study				1.121*** (0.205)			
Place(Boy) Doctor Availability Hospital Availability					0.938*** (0.203)	0.334** (0.127)	5.005* (4.765)
Constant	1.760*** (0.061)	1.032*** (0.057)	1.435*** (0.084)	1.448*** (0.168)	2.497*** (0.169)	35.716*** (6.410)	51.386*** (10.718)
Observations	7,987	7,581	8,932	4,429	4,826	3,469	4,502
R-squared	0.132	0.021	0.179	0.045	0.028	0.009 (Pseudo R2)	0.009 (Pseudo R2)

Note: All regressions are Ordinary Least Square (OLS). Robust standard errors are clustered at village-level and reported in parentheses. Statistical significance at 1, 5, and 10 percent levels is denoted by ***, **, *, respectively

Discussion

The primary goal of this research was to test whether NSP, a CBD approach, improves access of households to utilities and basic services in rural areas. The results of the study are mixed. In the medium-term, to some extent, the program has been successful in improving treatment households' access to utilities and services. In the short-term, however, NSP seems to reduce welfare of treatment villages comparing to control villages, except for two indicators: water quality and number of illnesses treated by a medical professional.

In the medium-term, the program decreases the time to collect water by 88 percent and increases electricity usage by 102 percent. These results are consistent with the findings of Beath et al. (2013) and most likely arose by completion of spanning deep wells and power generation projects, as spanning deep wells and power generation projects are time-consuming and need time to produce outputs. However, the program decreased the quality of water in the medium-term probably due to poor maintenance of the projects. As community members are primarily responsible for maintaining the projects in the CBD approach, the possibility of neglecting their responsibility that leads to poor maintenance is high. Similarly, the program has increased school attendance of girls by 0.33 days per week in the medium-term, which is not in line with the observation of Humphreys et al. (2012) and Voss (2008)¹² as they found no impact of CBD program on school attendance in Democratic Republic of Congo and Indonesia, respectively. This might be a consequence of constructing road projects that potentially eased mobility for females, and/or valuing of girls' education after the establishment of the new democratic government in Afghanistan that relatively changed perception of families toward girls' education in a positive direction. Furthermore, the program does not impact health outcomes in the medium-term, which is inconsistent with Rocha and Soares (2009), Bjorkman and Svensson (2009) and Voss (2008), as their results report positive impact of CBD programs on health outcomes in Brazil, Uganda, and Indonesia, respectively. This discrepancy might have arisen due to lack of direct investment of NSP on providing health care services except for a small amount, which was not the case in Brazil, Uganda and Indonesia studies.

In the short-term, the quality of water has increased probably due to providing protected deep wells for the treatment villages, as they were previously collecting water from unsafe sources. Likewise, the intervention has increased the probability of illnesses being treated by a medical professional in treatment villages, which is consistent

¹² Humphreys et al. (2012) used difference-in-difference method with *school attendance* being their outcome variable. However, Voss (2008) uses propensity score matching method and *school enrollment* as the outcome variable.

with the findings of Rocha and Soares (2009); Bjorkman and Svensson (2009); and Voss (2008)¹³. This impact might have happened due to improved connectivity between remote rural villages and district centers through the resurfacing of the secondary and tertiary roads, providing better access to district centers where clinics are available. In the short-term, the intervention substantially increases water collecting time and decreases electricity usage in treatment villages by 84 and 70 percent, respectively. These negative impacts likely happened due to involving technically incompetent community members in technical decisions of the projects, as CBD approach suggests, or the projects design might not be as good as it should be, as suggested by (Khawaja, 2009).

CONCLUSION

This paper aims to ascertain whether CBD programs improve access of rural households to utilities and services. Using field experimental data collected by the NSP evaluation team and Ordinary Least Square (OLS) method, the study suggests that the CBD approach, particularly as implemented by the NSP, improved access of rural households to utilities and services in the mid-term. In particular, water collecting time has been drastically decreased, electricity usage increased rapidly, and school attendance of girls also increased in the treatment villages. Despite the decrease of water quality in the medium-term, these effects highlight the success of the program. In the short-term, participating in the NSP has resulted in an increase in water collecting time and a decrease in electricity usage, which undermines the effectiveness of the intervention. However, water quality relatively improved and probability of illnesses being treated by a medical doctor also increased in the short-term. Furthermore, the results of this study show that the program does not affect school attendance of boys and infant mortality outcomes in both short- and mid-term, therefore a follow-up study that investigates the program cavities is suggested.

Policy implications and further research possibilities

Overall, the results suggest that NSP was successful in targeting poor people in rural areas of the country, and that the intervention achieved its goal of mobilizing and involving poor people in development process. However, the program did not improve, in general, rural households access to utilities and services in the short-run except for some indicators. The program was, however, relatively successful in improving access of rural households to utilities and services in the mid-term. These observations imply that the governments and international donors should focus on large-scale projects that can produce long-term benefits to communities rather than small-scale projects targeting short-term improvements. Likewise, the involved parties in implementing a CBD approach may give more attention to the design of the projects, which is a crucial factor for long-term outcome success. Additionally, negative competition among elites, political, and religious groups, involving technically incompetent community members in technical decisions of projects, among others, are the postulated factors that undermine the impact of CBD approach, or at worst, may turn the impact into negative direction. Hence, specific mitigation of these risks is recommended.

The focus of this study has been limited to revealing the impact of the NSP on access of rural households to utilities and services. Further studies are required to investigate security, political, social and cultural, and aid management problems that could have impact on the effectiveness of CBD programs.

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¹³ (Rocha & Soares (2009) and Bjorkman & Svensson (2009)) both used difference-in-difference method and *child mortality* as their outcome variable, whereas Voss (2008) used propensity score matching method with the *outpatient care* as their dependent variable, as is the case in this paper.

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