The Impact of a Rural Road Development Project on Multidimensional Poverty in Nepal

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Abstract

Although the effect of rural road development projects on income poverty has been well studied, little research has been undertaken on the impact on the multiple dimensions of poverty. In this study, we examine the effect of the improvement and construction of rural roads in rural Nepal on household deprivation of health, education, and living standards. We use data from two rounds of the Nepal Demographic and Health Survey (2001, 2011) and a difference-in-differences approach to estimate the average treatment effect on multidimensional poverty. Our study finds evidence of reductions in household deprivation, mainly driven by improvements in asset ownership and dwelling infrastructure. We fail to observe significant effects on the health and education indicators. We test these findings by using propensity-score matching and inverse-probability weighting methods as robustness checks, and generally find similar estimates. In line with the literature in the field, we find heterogeneity in the results across socioeconomic groups and poverty dimensions. Further exploration suggests that household land ownership and economic activity might be driving this heterogeneity. Our work highlights the importance of using multidimensional measures to assess poverty and to empirically evaluate the impact of infrastructure projects on the development of countries, especially their rural regions.

1. INTRODUCTION

The development of a community is strongly linked to the services available to its people. Access to these services is determined, in part, by how difficult, time consuming, and costly it is for individuals to reach health centers, schools, markets, administrative centers, and other sometimes-vital institutions. In 2001, Nepal ranked among the countries with the lowest road density worldwide with 10.8 km/sq. km (MPPW, 2001). By 2007, this figure rose to 12.1 km/sq. km, making it still the lowest in the region (Afram & Salvi Del Pero, 2012). This meant that more than half of the population did not have access to all-season roads. Astonishing figures revealed that some villages were a walk of more than 10 days to the nearest road (Afram & Salvi Del Pero, 2012). Evidence demonstrates that improved access through road the construction affects households in three ways: (a) transportation costs, and input and output prices, (b) labor supply, and agricultural and nonagricultural production; and (c) well-being outcomes (Khandker, Bakht, & Koolwal, 2009).

The purpose of this paper is to examine the impact of a rural road development program in Nepal on household deprivation of health, education, and living standards. This multidimensional focus differs from the traditional practice of employing income to measure poverty. With income, researchers conduct cross-country comparisons; the data are relatively easy to obtain and analyze; and, in the past, it was practically the only way to measure poverty, due to the lack of alternative information. Nevertheless, this measure is incomplete because it does not consider poverty to be a multidimensional problem and because it fails to consider capability deprivation. For instance, it may indicate how much a person earns in a given period, but it says nothing about their needs-satisfaction capabilities. Having money does not imply that the average quality of drinking water will improve, that a hospital will be built nearby, or even that due to cultural characteristics people will increase their intake of animal protein. Moreover, traditional measures of poverty assume perfect substitutability (Lustig, 2011). Under this condition, a dollar spent in consumption goods should be invested as easily in health or in the formation of human capital, something that is seldom true. To address this limitation, this paper uses the 2001 and 2011 rounds of the Nepal Demographic and Health Survey to calculate the Multidimensional Poverty Index, or simply MPI, to assess the effect of road construction on several dimensions of household well-being.

This study contributes in two ways to the body of empirical evidence on the link between road construction and development. First, we expand the analysis of existing studies and propose the implementation of more-accurate ways of measuring poverty. By using 10 indicators to quantify household deprivations, we are able to identify different areas of vulnerability more precisely than commonly used measures. This approach allows us to estimate a composite index and to disaggregate it into individual dimensions, letting us seek answers to questions of congruency across poverty dimensions and interactions among these. Second, unlike previous studies, we quantify poverty with information on actual deprivations rather than on income or per-capita income. This improvement addresses income-data collection problems as well as the assumption of equal income distribution for the per-capita indicators. The results from this analysis identify a positive effect of the development project on poverty alleviation in Nepal but only through improvements in asset ownership and dwelling infrastructure.

The paper is organized as follows. The first two sections comprise a review of the findings from rural road impact evaluations as well as background information on the Nepalese road development project. The third section describes the data and variables used in the analysis. Then, the methodology section describes and explains the techniques and models used in the analysis. Next, we present the results, conduct two robustness checks, and discuss the findings. The final section contains the conclusions and suggestions for further research areas.

2. RURAL ROAD DEVELOPMENT

The evidence on the impact of road investment projects on income poverty, education, and employment in developing countries is mixed. Some studies observe extensive development effects (Khandker et al., 2009; Wondemu & Weiss, 2012), while others note that the magnitude of the impact differs across socio-demographic groups (Bell & van Dilien, 2014; Mu & van de Walle, 2007, 2011). These results, reflecting the heterogeneous nature of the impact of road improvement and construction projects, make generalizations difficult.

The literature finds that rural roads impact development through market outcomes and household well-being. Studies examining labor, commercial, agricultural, and transportation outcomes find large, positive effects as a result of reduced travel times, improved access to markets, and more-integrated road networks (Khandker et al., 2009; Mu & van de Walle, 2011). Studies that explore household-level outcomes also reveal substantial improvements in income, employment, education, and health (Dercon, Gilligan, Hoddinott, & Woldehanna, 2009; Rand, 2011; Warr, 2008, 2010; Wondemu & Weiss, 2012).

Khandker, Bakht, and Koolwal (2009) find that the implementation of a rural road development project in Bangladesh caused income poverty to decline five to seven percent and caused adult labor supply and child schooling to increase. The authors also find evidence of an 11 percent increase in the average household annual per-capita consumption and positive effects on men's agricultural wages. These effects were found to be larger for poorer households than for their wealthier counterparts. Similarly, Wondemu and Weiss (2012) find that improving the quality of rural roads in Ethiopia raises average household income by as much as 63 percent. Mu and van de Walle (2007, 2011), using data from Vietnam, find heterogeneous marginal returns on poverty and markets from the improvement and construction of roads. This evidence indicates that project and beneficiaries' characteristics greatly influence the poverty-alleviation impact of an intervention.

Despite the heterogeneous nature of the impact of road improvement and construction projects on development, most studies find that improving the physical integration of a country has an overall positive effect on income poverty. Nevertheless, with the exception of an article by Bell and van de Walle (2014), few have considered household poverty as an integrated multidimensional problem. The traditional approach in the literature has been to measure poverty through household or per-capita income, washing out any multidimensional effects. Alkire and Santos (2014) warned that income-based measures indicate only whether people have the financial capacity to satisfy a set of needs and do not look into the actual satisfaction of those needs. Deaton and Drèze (2009) have found evidence that despite India's economic growth and a relative decline in food prices, the per capita caloric and nutrient intake has steadily declined across all socioeconomic strata, even the poorest. A researcher who looks at income poverty in India may incorrectly suggest that food consumption has increased based on a rising purchasing power. In the case of per-capita measures, they tend to oversimplify reality as they describe averages but say nothing about distributions or inequality. In addition, the collection of income data in some settings may be incomplete, inaccurate, inconsistent, and/or biased, making it difficult to use in project-impact evaluation frameworks.

By focusing on the multidimensional impact, we can better elucidate the mechanisms through which road development affects poverty and which subgroups of the population are most likely to benefit from these improvements. While the focus of this paper is on road networks and social well-being, the use of a composite multidimensional poverty index to identify the impact of interventions may prove to be useful in other fields. The results from the disaggregation of the index provide a better understanding of the channels through which a rural road development program might affect different dimensions of poverty and how this affects overall deprivation.

3. THE RURAL ACCESS IMPROVEMENT AND DECENTRALIZATION PROJECT

The Rural Access Improvement and Decentralization Project (RAIDP) is a development initiative funded in the amount of $71 million by the World Bank and implemented by the government of Nepal. The execution of RAIDP commenced in 2005,[1] coinciding with the final years of a decade-long civil war. The timing of the project created additional challenges because the re-emergence of local conflicts posed security concerns to the project staff and led to the redistribution of funds to districts that were progressing rapidly. The original aim of RAIDP was to improve rural access in 20 districts in Nepal, benefiting almost two million people with "enhanced access to social services and economic opportunities" through the improvement or construction of more than 1,700 km of all-season roads (WB, 2009, 2014). The specific outcome indicators were: a) a 20 percent increase in motorized and nonmotorized trips by beneficiaries to social and economic centers, and b) a 20 percent reduction in travel time to those centers (WB, 2014). The

[1] For purposes of the present study, 2005 is considered as the year when the intervention-treatment started.

expected direct benefits to the population included “appreciation in land value, enhanced access to motorable roads, reduction in travel time and transport costs, and employment and income generation from the construction works”(WB, 2009). Optimistic project expectations promoted community involvement in the form of labor and $56.3 million in land contributions. Due to the continuation and addition of several subprojects, the total cost of the intervention has increased to more than $90 million. The long-term success of RAIDP is now contingent on road maintenance to ensure the sustainability of the project and its benefits.

4. DATA AND VARIABLES

For this study, we use the 2001 and 2011 rounds of the nationally representative Nepal Demographic and Health Survey (NDHS), with each round providing the pre-treatment and post-treatment information, respectively. The survey is conducted every five years by the Nepalese Ministry of Health and Population with the support of various international organizations. The purpose of the survey is to provide current information on population and health. The survey covers women’s health and reproduction, child health and mortality, nutrition, sexually transmitted infections, the empowerment of women, and asset ownership and general household characteristics. The information in these datasets draws from three questionnaires applied to households as well as to women and men in reproductive age. This study uses 7,027 rural households from the 2001 round and 7,582 rural households from the 2011 round. In addition to the NDHS, the 2001 and 2011 National Population Census are used to obtain district-level information, the Internal Displacement Monitoring Center to collect data on the impact of the Nepalese Civil War in each district, and the World Bank online projects repository (WB, 2015) for RAIDP treatment status.

The focus of this paper is the effect of the improvement and construction of rural roads on the multiple dimensions of poverty.[2] Unfortunately, the data does not contain information regarding income or consumption, so we are unable to assess how the results from the MPI approach differ from a monetary one. Also, we do not have information on the provision of social services, thus we cannot conduct an exploration of potential supply-side effects on multidimensional poverty. The response variables of interest are the MPI score and its three dimensions. We calculate them by following the methodology developed in Alkire and Santos (2010) and in Alkire and Foster (2011).[3] The MPI uses household-level data to estimate the incidence and intensity of deprivation that each household faces. The index ranges between zero and one, where one means total deprivation in every indicator and zero denotes no deprivation in any indicator. The index is calculated with a bundle of equally weighted dimensions –health, education, and living standards– subdivided into 10 indicators (A description of each component can be found in the Appendix.). The MPI is obtained by multiplying the poverty head-count ratio, H (percentage of poor households), by the average intensity of deprivation, A (the proportion of indicators in which the households are deprived). Thus, the MPI is interpreted as the proportion of households that are multidimensionally poor adjusted by the intensity of the deprivation, and the deprivation score for each dimension is interpreted similarly.

The list of explanatory variables, in addition to time and treatment status, includes household and district characteristics and geography covariates. Table (1) presents the descriptive statistics for the household variables by survey round and treatment status. Overall, there are

[2] For a discussion on multidimensional measures of poverty see Tsui (2002), Alkire and Foster (2011), Ravallion (2011), Decancq and Lugo (2012), Batana (2013).
[3] Unlike Alkire and Santos (2010) and Alkire and Foster (2011), this paper does not censor the MPI score to zero for those households with a deprivation of less than 0.3 to preserve data variability.

statistically significant differences in household characteristics across groups, but the economic significance of these differences is not always substantial. In terms of overall poverty, between 2001 and 2011, we see that there was a 20 percentage-point drop driven by a decrease in the three deprivation dimensions.[4] The poverty scores indicate that, on average, households located in treated districts were marginally more deprived than nontreated observations throughout the study period. In terms of the average number of people per household, the results across treatment status groups show a decline in both absolute and relative terms, suggesting a drop in fertility rates. The table also shows differences concerning the caste of the households in treated and nontreated areas. In the pre-intervention round, over half of the treated households self-identified as Janjatis or with similar ethnic groups, while the non-treated observations were evenly distributed across the three main castes. We see that these ethnic disparities within the treated group narrowed in 2011, although they still persisted. These differences between treated and nontreated groups could become problematic when trying to identify the true treatment effect as we would be comparing groups that are not exactly similar. We try to address this issue and to verify the results in the Sensitivity Analysis section.

In terms of the district-level variables, Table (1) shows significant differences between treated and non-treated groups. It is evident that areas that suffered the most violence from the 1995-2006 civil war were largely excluded from RAIDP. Post-treatment figures indicate that a mere two percent of the treated observations were located in districts affected by the conflict. The geographical characteristics of the intervened areas indicate that the development program was not randomly implemented. About three out of every four households in the treated group were located

[4] The figures in the table pertaining to poverty should be interpreted as the proportion of households that are deprived in each dimension.

in the Terai ecological belt, which is mostly grasslands and savannas suitable for agricultural activities. However, if we look at the control group, only about one in every four observations was located in the Terai. The differences in geographical location between treated and non-treated are analogous for the Hill and Mountain ecological belts.

[Table 1 here]

5. CONCEPTUAL FRAMEWORK AND EMPIRICAL STRATEGY

The empirical evaluation of interventions is often conducted retrospectively on non-experimental programs in which the treatment assignment is not random. Such was the case of RAIDP in Nepal since it focused on the development of districts with an underdeveloped network of rural roads. For this reason, we need a method that controls for systematic differences between the households in the treatment districts and those in the control areas. The first empirical modeling strategy we use is the differences-in-differences (DID) method to capture the effect of the rural road development project on poverty. The model that we empirically estimate is,

(1) $y_{hd} = \beta_0 + \beta_1 R_{hd} + \beta_2 t + \beta_3 (R \cdot t)_{hd} + \beta_4 \Gamma_h + \beta_5 Z_d + u_{hd}$,

where y_{hd} is the deprivation outcome for household h in district d, R equals one if the household is located in a district that receives the intervention and zero otherwise, t is a dummy that equals zero if the household was observed in the period before the intervention and one if the observation was done after; and Γ and Z are household and district characteristics, respectively. The term $(R \cdot t)$ is the interaction between treatment status and observation period, thus β_3 is the DID parameter that captures the average treatment effect.

With this DID model, we expect to find similar negative effects of RAIDP on MPI and on its component dimensions as the evidence in previous studies suggests that improved access to employment opportunities and the promotion of socioeconomic inclusion, through the development of road infrastructure, leads to a decline in the intensity and the number of indicators in which households are deprived. Finding evidence of these mechanisms would support the existence of poverty-alleviation effects from the construction and improvement of rural roads. A number of reasons have been proposed to explain the existence of this causal relationship. One is that beneficiaries who join the formal labor market due to the reduced transportation time and costs have better chances of affording more assets and improving the quality of their dwellings. It also becomes safer and less costly, in monetary terms, for these individuals to reach health care services and academic institutions. Even if the marginal utility from education and health remains unchanged, beneficiaries are more likely to seek attention at clinics and attend school if the marginal monetary and time costs of travelling decrease.

Referring back to the methodological requirements, the DID framework assumes that both treatment and control groups have common pre-intervention trends. Given that we only observe one round of data pre- and one round post-treatment, we are unable to test the common trends assumption on the DID approach. Further, in the case of RAIDP, it is reasonable to assume that due to the targeted nature of the intervention, the different groups of districts did not share similar profiles. Even in the presence of the same macro shocks, the differencing procedure does not account for the impact of time-varying household effects. Under these model limitations, there is a risk of obtaining inconsistent estimates by using a DID approach (refer to Appendix for the derivation). In order to address this limitation, we complement our analysis by employing methods that attempt to reproduce the conditions of an experimental setting. For this, we need to create a counterfactual sample for the treated so that we have information on their outcome had they not been treated. We use the propensity score matching (PSM) technique to compare the outcome between households with similar probabilities of being treated (propensity score) given a set of characteristics, X. The observables in X must not perfectly predict the treatment status so that we can ensure that each treated household can be reproduced in the control group. The idea is that if two households had the same probability of benefiting from the construction of rural roads but one did and the other did not, then the treatment assignment can be considered as random. We use a logistic regression to model the probability of receiving treatment given X in the following way:

$$(2)\ \ln\left[\frac{P(R_h=1|X)}{1-P(R_h=1|X)}\right] = \alpha_0 + \alpha_1 x_1 + \alpha_2 x_2 + \cdots + \epsilon,$$

where $P(R_h = 1) = P(X)$ is the propensity score or probability of receiving treatment, and the X terms are district and household observable characteristics. After estimating the propensity scores, we need to match households with similar propensity scores to compare their poverty outcomes. We use four PSM methods to maximize the robustness of the results (see Appendix). We estimate the average treatment effect for the treated (ATT) as noted in Dehejia and Wahba (2002) as follows:

$$(3)\ \hat{\tau}^{ATT} = \frac{1}{|N|}\sum_{h\in N}\left(y_h - \frac{1}{|J_h|}\sum_{j\in J_h} w_j y_j\right),$$

where $|N|$ is the number of treated households, J_h is the set of comparison units matched to treated household h, $|J_h|$ is the number of matched households in J_h, y is the outcome of the treated and matched households; and w_j is the weight assigned to matched household j.

[Table 2 here]

[Table 3 here]

[Table 4 here]

[Table 5 here]

6. EMPIRICAL RESULTS

In the analysis of the data we look at the effect of RAIDP on each of the deprivation indicators – overall, living standards, education, and health. For each dimension, we run the DID model with and without additional controls. In the first DID model specification we only use the treatment and time variables, and the interaction between the two. The second regression controls for district characteristics such as civil war violence, average household size, percentage of rural population, and population density. In the third model specification we incorporate information about ecological regions, household-head age and gender, number of people in the household, and cast self-identification. Finally, the fourth DID regression accounts for the presence of other major development projects being executed in the district where the household is located. Incrementally adding controls at the individual-, district-, and regional-level does not substantially change the estimated treatment effect. This alleviates concerns that unobserved characteristics are confounding our estimates.

Tables (2) to (5) show the DID estimates for the MPI and for each deprivation dimension. Since our interest is the effect of RAIDP on poverty alleviation, we focus on the interaction between time and treatment status, represented in the tables as $[Period^{*}RAIDP]$. For ease of interpretation we refer to the coefficients in terms of percentage points. Table (2) presents the results for the composite MPI estimates obtained with the four model specifications. The statistically significant results suggest that household multidimensional poverty declined between 3.3 and 5.6 percentage points in areas where rural roads were improved or constructed. These results remain consistent even when controlling for household and district characteristics.

As explained above, one of the advantages of using the MPI to measure poverty is that it can be disaggregated into its constituent dimensions. Table (3) presents the results for the DID estimation for the living standards dimension (refer to Appendix for individual indicators). Again, the coefficient on the interaction term denotes that the implementation of RAIDP was associated with a drop of over three percentage points in living standards deprivation. The increase in asset ownership and quality of dwelling infrastructure could have been a direct result of RAIDP through an increase in household and per capita consumption (Dercon et al., 2009; Khandker et al., 2009), and better access to markets and employment (Rand, 2011). We see similar effects with the district-level controls as with the DID model for the composite MPI in terms of sign and significance, but lower in magnitude for the living standards framework.

Table (4) exhibits the estimates from the regression on the education dimension of the MPI. Surprisingly, we now observe that the intervention (RAIDP) is not associated with a statistically significant change in education deprivation. This situation is repeated for the health dimension in Table (5). It seems that the association between the intervention and the degree of health deprivation is also not significant[5].

Overall, the DID results support the existence of poverty-alleviation effects from the implementation of RAIDP. However, this impact appears to be non-congruent across different dimensions of poverty, with the living standards dimension driving the improvements. The impact on the education and health dimensions is not significant, and this might have been caused by a lack of public awareness campaigns and complementary infrastructure such as schools and clinics.

6.1 Heterogenous effects

The studies that have looked at the impact of infrastructure on development have generally identified heterogenous effects on different population groups (Bell & van Dilien, 2014; Mu & van de Walle, 2007, 2011). After all, we cannot assume that households with significant differences will be affected in the same way by road construction and improvement. Consider an individual who owns a car or motorcycle. Will she benefit in the same way as someone who does not own either and who lives in an area with very low public transportation coverage?

To verify the existence of heterogeneity in our results, we run the DID model and include interaction terms for MPI terciles to account for low, moderate, and severe deprivation. Table (6) presents the results from the regressions on the composite MPI and for each deprivation dimension. Also included in the specification, but not shown in the table for ease of comparison, are the household- and district-level controls. The results in the table should be interpreted as the percentage-point treatment effect on deprivation. Negative values indicate a poverty alleviating

[5] To address the concern that our results could vary if we used alternative variables to measure health deprivation, we repeated the analysis with child height-for-age and weight-for-height measures (as proposed in Grantham-McGregor et al. (2007), and Currie and Almond (2011)) and obtained similar results.

effect, while positive coefficients suggest an increase in household deprivation. The results in the first column suggest that the least deprived households experienced an improvement in MPI of about 1.5 percentage points, while moderately and severely deprived households saw their situation deteriorate. This evidence reveals a diminishing marginal effect of the treatment on overall poverty levels. The second column presents the results for the living standards dimension and reveals a U-shaped poverty alleviation effect of 1.4 percentage points for moderately deprived households and at least 3.3 percentage points for the least and severely deprived households.

The U-shaped effect of the living standards dimension on different levels of household deprivation could be explained by the composition of the labor force in each group. If we look at the ratio of unskilled-to-skilled household labor supply per deprivation intensity tercile, we see that among the least deprived observations the number of unskilled households is more than twice the amount of skilled ones (68.26 % unskilled against 31.74 % skilled), but the ratio increases to ten for the most deprived households (90.22 % unskilled against 9.78 % skilled). The ratio for the moderately deprived households is around 6.5 (86.90 % unskilled against 13.10 % skilled). The large differences in labor qualification figures are, however, in sharp contrast with land ownership numbers. We find that three out of every four rural households own land that is suitable for agriculture, regardless of the deprivation tercile. With improved road access, households access agricultural inputs and markets to sell their outputs more easily. Because extremely poor households proportionately supply more unskilled labor and are equally likely to own land than their less deprived counterparts, the construction of roads has the potential to deliver the largest improvements in terms of agricultural activity to the most deprived households. Alternatively, the larger poverty-alleviation effects among the least deprived households might be driven by improved access to economic centers where they can provide their proportionately more skilled labor force. Moderately deprived households benefit the least out of the three groups because they have a lower share of unskilled workers than the poorest group and a lower share of skilled labor than the wealthier households.

The coefficients in column (3) estimate the effect of the treatment on the education dimension. We see that only severely deprived households experienced a 2.2 percentage point improvement in education, while the situation for the remaining households worsened. Finally, the last column indicates negligible effects of the treatment on the health dimension for the least and moderately deprived households, but a detrimental impact on the severely deprived units.

[Table 6 here]

When taken together, the heterogeneous results suggest the existence of a pattern in the poverty-alleviation effect across household deprivation levels. Figure (1) is a graphical representation of these results. The least deprived households experience an improvement in their living standards and education indicators, but a slight decrease in the education outcomes. As household deprivation increases, the alleviation effect on the living standards dimension decreases, the effect on health becomes negligible, and there is a detrimental impact on education. In the case of the most deprived households, the effect on their living standards is about the same as for their least deprived counterparts. We also see an increasing alleviation effect on the education dimension, but the situation for the health outcomes becomes bleak.

[Figure 1 here]

7. SENSITIVITY ANALYSIS

Let the reader be reminded that it would have not been reasonable to assume that the treated and non-treated households were following a pre-intervention common trend, especially when policymakers changed the treatment status of some districts where remnant violence from the civil war resurged. Thus, we run additional analyses to verify the estimates with propensity score matching methods and inverse probability-weighted estimations, which deal with the absence of an appropriate control group derived from the non-random assignment of the treatment.

7.1 Propensity score matching

The reasoning behind using these methods is that there were district-level characteristics that helped the Nepalese authorities to determine which districts to include as part of RAIDP. However, besides rurality, the exact variables that were used by the policymakers to target the districts remain unclear. To overcome this selection issue, we run a logistic regression of treatment status on district-level variables, namely civil war violence, proportion of rural households, and ecological region and retrieve the probability of each household being treated conditional on observed covariates (propensity scores). This model specification satisfies the balancing property of the propensity scores, making the matching process across comparable treated and non-treated households possible. This allows us to estimate the average treatment effect on the treated.

For robustness, the propensity scores from the treated and control observations are matched with three PSM methods: nearest neighbor, radius, and stratification (see Appendix B for a description of each method). We estimate the ATTs with PSM methods for the MPI and each dimension. Although more conservative, the statistical significance and magnitude of the estimates remains fairly consistent and support the DID story. The results from the stratified PSM are the most conservative, indicating that the overall deprivation of households declined 1.2 percentage points. We also note that the living standards dimension, through an increase in asset ownership and dwelling infrastructure, is what is driving the decrease in poverty as the benefitted households saw an improvement in the order of two percentage points. The results for education and health corroborate our story and show that there were mostly no statistically significant effects on these dimensions.

To validate the results obtained with DID and PSM, we carried out two additional procedures. First, we repeated the PSM by using the 2001 data to calculate and impute the propensity scores for the 2011 observations. The decision to pursue this option was grounded on the fact that authorities decided what districts to include in RAIDP based on the information they had prior to the year of the intervention. This analysis estimated even larger ATTs for the overall MPI and the living standards dimension.

7.2 Inverse probability-weighted estimation

The second procedure we performed to verify the DID and PSM results is an inverse probability-weighted (IPW) estimation (refer to Appendix) to calculate the effect of the intervention on the treated households. This method produced similar results to the ones described in the previous section (results available upon request). The results from all three estimation procedures are consistent in finding that the intervention had positive effects on the living standards dimension, which is driving the overall poverty alleviation as there seems to be no significant effect on the health and education dimensions.

8. DISCUSSION

Having obtained significant estimates for the MPI and living standards deprivation, and non-significant results for the health and education dimensions, we would like to comment on a number of potential explanations that could help us understand the heterogeneity in the results. We might have found different poverty-alleviation effects across population segments and deprivation dimensions for a variety of reasons, including additional infrastructure requirements, time considerations; and household risk assessment and decision-making practices.

Unlike health and education, increases in asset ownership and dwelling improvements (captured by the living standards dimension) do not require additional infrastructure other than roads, and the resources required to produce it can be easily substituted. Schools and hospitals offer highly specialized services for which there are not substitutes. Even if families can afford these services, if there are no formal academic centers or health institutions within a reasonable distance, there is nothing rural households can substitute them with. Alternatively, the degree of substitutability for assets and construction materials is considerably higher, making it easier to lessen the deprivation in this area than in health and education. These considerations highlight the possibility of development projects having too little of an impact when additional infrastructure is required to maximize their benefits.

Another potential explanation for the variation in results is how households prioritize investments in one dimension over another. The improvement and construction of roads expands families' choice sets, but deciding among these alternatives may be difficult because the stakes are high, there is a high degree of uncertainty, and individuals might not have previous experience at making such decisions. The alternative that a household chooses determines the outcome of all family members, and sometimes they opt for the one that benefits everyone somewhat equally.

The empirical analysis did not find an average deprivation-alleviating effect from road infrastructure development on the education and health dimensions. This could be a consequence of variation in the perceived costs and benefits from health and education. In addition, the inadequate quality of these services could be outweighing the effect of better access. The results on education, along with the increase in asset ownership and dwelling construction materials improvement, could indicate that the effect on education was washed away by an increase in the opportunity cost of child labor.

With better access to markets and supply of production factors, households could be seeing an increase in their agricultural income. The literature has shown that the delay in rewards from investments in education does motivate individuals to invest in other alternatives (Levitt, List, Neckermann, & Sadoff, 2012). If the perceived long-run benefits from education are low and the expected agricultural profits increase, then it is more likely that parents will pull out their children from school and put them to work in agriculture. Exploring these multidimensional effects under different time horizons would greatly contribute to the development literature.

The most important lesson in terms of empirical implications may derivate from failing to find congruent effects of the road construction across deprivation dimensions. This result highlights the importance of conducting multidimensional analyses when exploring complex social issues. We found no evidence that supports the existence of one-size-fits-all policies to target poverty because there are different types of deprivations and incentives that determine how households will behave in the presence of development projects.

What the evidence on the link between RAIDP and multidimensional deprivation says is that that there are poverty-alleviation effects that are heterogeneous with respect to population deprivation groups and deprivation dimensions. Now, a pressing question arises: how do we amplify and intensity the impact of this program? Policymakers and researchers have generally disregarded the role that information and choice assessment play when households' options expand as a consequence of development interventions. The literature has done a good job at explaining the risks of not investing in assets, education, or health; but has still to address the decision mechanisms that determine where and how households actually invest their resources.

9. CONCLUSION

The calculation of multidimensional poverty measures, such as the one estimated in the present paper, allows for the observation of different types of deprivations that households face. As the quality and coverage of the information that is available for developing countries improves and new data analysis techniques are developed, cutting-edge contributions in the area of poverty alleviation and impact evaluations are possible.

We estimated the MPI for Nepal with data from the 2001 and 2011 NDHS rounds, and we were able to examine the effect of a rural development project on multidimensional household poverty, with a special focus on health, education, and living standards. The DID estimation framework, under certain assumptions, supports the existence of poverty-reduction effects from RAIPD, and identified the living standards dimension as the driver in deprivation reduction. The results were cross-examined with different model specifications and estimation methods, and appear to be consistent in both magnitude and significance.

A limitation of this study is that we did not have information on the supply of social services. The construction of roads does not necessarily mean that residents will have access to new services in their area. For roads to have poverty-alleviation effects, they must reach schools and clinics with properly trained professionals. Having data on the existence of academic and health centers would allow controlling for these variables to minimize potential bias in the estimation of the ATT. Also, it was noted that the impact of RAIDP might have been limited by the lack of long-term entrepreneurial, empowerment, and human capital investment incentives in the project design. Ignoring these amplification components could undermine the potential of development projects.

Future research should conduct cost-effectiveness analyses to identify better options for the allocation of limited resources. We determined that an investment of over US$ 90 million in road development led to an average drop in the deprivation of living standards (asset ownership and dwelling infrastructure) of two to four percentage points among treated households (around two million individuals). The comparison of these costs and benefits with other development interventions is outside the scope of this study but could be advanced in future work.

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TABLES

Table 1: Descriptive statistics for household variables by treatment status and year

	2001			2011		
	Treated	Non-treated	Diff.	Treated	Non-treated	Diff.
Deprivation dimensions†						
Overall deprivation (0,1)	0.63	0.60	***	0.36	0.38	***
Living standards (0,1)	0.29	0.28	***	0.17	0.20	***
Health (0,1)	0.14	0.13	***	0.04	0.04	
Education (0,1)	0.20	0.19	***	0.15	0.14	
Household characteristics						
Age of household head	44.04	44.73	*	46.11	45.71	
Female household head (0/1)	0.14	0.16	***	0.28	0.27	
Number of household members	6.18	5.48	***	4.96	4.66	***
Household in district affected by civil war (0/1)	0.04	0.23	***	0.02	0.29	***
Caste						
Upper caste (0/1)	0.19	0.34	***	0.27	0.37	***
Janjati (0/1)	0.52	0.34	***	0.44	0.32	***
Dalit (0/1)	0.29	0.32	**	0.29	0.30	
Location						
Mountain (0/1)	0.01	0.26	***	0.04	0.30	***
Hill (0/1)	0.25	0.46	***	0.23	0.48	***
Terai (0/1)	0.73	0.28	***	0.73	0.22	***
Observations	2425	4602		2199	5402	

*p<0.1, **p<0.05, ***p<0.01

Note: proportions indicate averages and might not add up to 1 due to rounding.

† The scores for the deprivation dimensions come from the MPI calculation.

Table 2: DID results: Household multidimensional deprivation

	Model 1	Model 2	Model 3	Model 4
Observation period	-0.218***	-0.177***	-0.165***	-0.165***
	(0.0116)	(0.0120)	(0.0112)	(0.0113)
RAIDP	0.036**	0.017	0.011	0.008
	(0.0143)	(0.0153)	(0.0146)	(0.0148)
Period*RAIDP	-0.055***	-0.048**	-0.034*	-0.033*
	(0.0208)	(0.0203)	(0.0182)	(0.0182)
Household characteristics				
Age of household head			-0.002***	-0.002***
			(0.0007)	(0.0007)
Age squared			0.000*	0.000*
			(0.00001)	(0.00001)
Female household head			0.042***	0.042***
			(0.0043)	(0.0043)
Number of household member			0.018***	0.018***
			(0.0012)	(0.0012)
Caste (reference: Upper caste)				
Janjati and others			0.070***	0.069***
			(0.0082)	(0.0082)
Dalit			0.053***	0.053***
			(0.0077)	(0.0077)
District characteristics				
Ecological belt (reference: Mountain)				
Hill			-0.008	-0.015
			(0.0134)	(0.0126)
Terai			-0.049***	-0.058***
			(0.0166)	(0.0163)
Household in civil war district		0.047***	0.043***	0.045***
		(0.0127)	(0.0126)	(0.0127)
Average household size		0.066***	0.067***	0.070****
		(0.0114)	(0.0109)	(0.0112)
Percentage of rural population		0.128**	0.095*	0.106**
		(0.0562)	(0.0529)	(0.0539)
Population density		-0.000***	-0.000***	-0.000***
		(0.00001)	(0.00001)	(0.00001)
Other development programs				-0.021
				(0.0139)
Constant	0.598***	0.124	0.106	0.090
	(0.0084)	(0.0820)	(0.0774)	(0.0798)
R-squared	0.261	0.310	0.376	0.377
AIC	-5628.0	-6626.4	-8091.9	-8106.9
BIC	-5597.6	-6565.6	-7970.4	-7977.9
F	201.7	177.8	140.7	133.6
Observations	14628	14628	14628	14628

* p<0.1, ** p<0.05, *** p<0.01

Table 3: DID results: Household living standards deprivation

	Model 1	Model 2	Model 3	Model 4
Observation period	-0.080***	-0.064***	-0.059***	-0.058***
	(0.0064)	(0.0064)	(0.0062)	(0.0063)
RAIDP	0.014**	0.011	0.012	0.010
	(0.0060)	(0.0068)	(0.0075)	(0.0077)
Period*RAIDP	-0.039***	-0.038***	-0.034***	-0.033***
	(0.0112)	(0.0109)	(0.0103)	(0.0103)
Household characteristics				
Age of household head			-0.001***	-0.001***
			(0.0002)	(0.0003)
Age squared			0.000***	0.000***
			(0.0000)	(0.0000)
Female household head			0.005***	0.005***
			(0.0018)	(0.0018)
Number of household members			-0.0001	-0.0001
			(0.0004)	(0.0004)
Caste (reference: Upper caste)				
Janjati and others			0.030***	0.030***
			(0.0040)	(0.0040)
Dalit			0.036***	0.036***
			(0.0033)	(0.0033)
District characteristics				
Ecological belt (reference: Mountain)				
Hill			0.007	0.004
			(0.0064)	(0.0062)
Terai			-0.024***	-0.030***
			(0.0088)	(0.0089)
Household in civil war district		0.022***	0.019***	0.020*
		(0.0066)	(0.0067)	(0.0066)
Average household size		0.022***	0.035***	0.037***
		(0.0057)	(0.0059)	(0.0062)
Percentage of rural population		0.078**	0.043	0.049
		(0.0313)	(0.0316)	(0.0319)
Population density		-0.000***	-0.000***	-0.000***
		(0.0000)	(0.0000)	(0.0000)
Other development programs				-0.012*
				(0.0067)
Constant	0.278***	0.090**	0.070	0.061
	(0.0033)	(0.0425)	(0.0428)	(0.0441)
R-squared	0.282	0.364	0.408	0.410
AIC	-34238.2	-36020.1	-37043.7	-37084.7
BIC	-342073.8	-35959.4	-36922.3	-36955.7
F	108.9	106.0	68.13	64.80
Observations	14628	14628	14628	14628

* p<0.1, ** p<0.05, *** p<0.01

Table 4: DID results: Household education deprivation

	Model 1	Model 2	Model 3	Model 4
Observation period	-0.047***	-0.036***	-0.034***	-0.034***
	(0.0042)	(0.0048)	(0.0044)	(0.0044)
RAIDP	0.008	0.002	-0.002	-0.003
	(0.0058)	(0.0064)	(0.0060)	(0.0060)
Period*RAIDP	-0.003	-0.002	0.002	0.003
	(0.0079)	(0.0078)	(0.0068)	(0.0068)
Household characteristics				
Age of household head			0.002***	0.002***
			(0.0004)	(0.0004)
Age squared			-0.000***	-0.000***
			(0.0000)	(0.0000)
Female household head			0.036***	0.037***
			(0.0024)	(0.0024)
Number of household members			0.007***	0.007***
			(0.0006)	(0.0006)
Caste (reference: Upper caste)				
Janjati and others			0.032***	0.032***
			(0.0036)	(0.0035)
Dalit			0.037***	0.037***
			(0.0034)	(0.0034)
District characteristics				
Ecological belt (reference: Mountain)				
Hill			-0.010*	-0.012**
			(0.0053)	(0.0052)
Terai			-0.016**	-0.019***
			(0.0061)	(0.0062)
Household in civil war district		0.008*	0.009*	0.010*
		(0.0049)	(0.0046)	(0.0047)
Average household size		0.018***	0.016***	0.017***
		(0.0045)	(0.0043)	(0.0043)
Percentage of rural population		0.021	0.017	0.021
		(0.0216)	(0.0199)	(0.0204)
Population density		-0.000**	-0.000**	-0.000*
		(0.0000)	(0.0000)	(0.0000)
Other development programs				-0.008
				(0.0057)
Constant	0.190***	0.075**	0.000	-0.006
	(0.0034)	(0.0299)	(0.0290)	(0.0297)
R-squared	0.047	0.059	0.117	0.118
AIC	-23032.3	-23206.3	-24129.2	-24134.5
BIC	-23001.9	-23145.5	-24007.8	-24005.4
F	63.67	44.12	71.66	67.89
Observations	14628	14628	14628	14628

* $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 5: DID results: Household health deprivation

	Model 1	Model 2	Model 3	Model 4
Observation period	-0.090***	-0.077***	-0.072***	-0.072***
	(0.0035)	(0.0038)	(0.0035)	(0.0035)
RAIDP	0.014***	0.004	0.002	0.002
	(0.0051)	(0.0050)	(0.0044)	(0.0045)
Period*RAIDP	-0.013**	-0.008	-0.003	-0.003
	(0.0060)	(0.0057)	(0.0054)	(0.0054)
Household characteristics				
Age of household head			-0.003***	-0.003***
			(0.0004)	(0.0004)
Age squared			0.000***	0.000***
			(0.0000)	(0.0000)
Female household head			0.000	0.000
			(0.0022)	(0.0023)
Number of household members			0.011***	0.011***
			(0.0005)	(0.0005)
Caste (reference: Upper caste)				
Janjati and others			0.007**	0.007**
			(0.0030)	(0.0030)
Dalit			-0.020***	-0.020***
			(0.0033)	(0.0033)
District characteristics				
Ecological belt (reference: Mountain)				
Hill			-0.006	-0.006
			(0.0040)	(0.0037)
Terai			-0.009*	-0.009*
			(0.0049)	(0.0046)
Household in civil war district		0.017***	0.015***	0.015*
		(0.0038)	(0.0036)	(0.0037)
Average household size		0.026***	0.016***	0.016***
		(0.0030)	(0.0030)	(0.0030)
Percentage of rural population		0.029**	0.036***	0.036***
		(0.0144)	(0.0140)	(0.0141)
Population density		0.000	-0.000	-0.000
		(0.0000)	(0.0000)	(0.0000)
Other development programs				-0.001
				(0.0043)
Constant	0.130***	-0.041*	0.035*	0.035*
	(0.0033)	(0.0223)	(0.0204)	(0.0205)
R-squared	0.162	0.181	0.274	0.274
AIC	-23514.8	-23831.4	-25596.7	-25594.7
BIC	-23484.5	-23770.7	-25475.2	-25465.7
F	384.4	275.2	194.8	184.2
Observations	14628	14628	14628	14628

* p<0.1, ** p<0.05, *** p<0.01

Table 6: DID model with deprivation tercile interaction effects

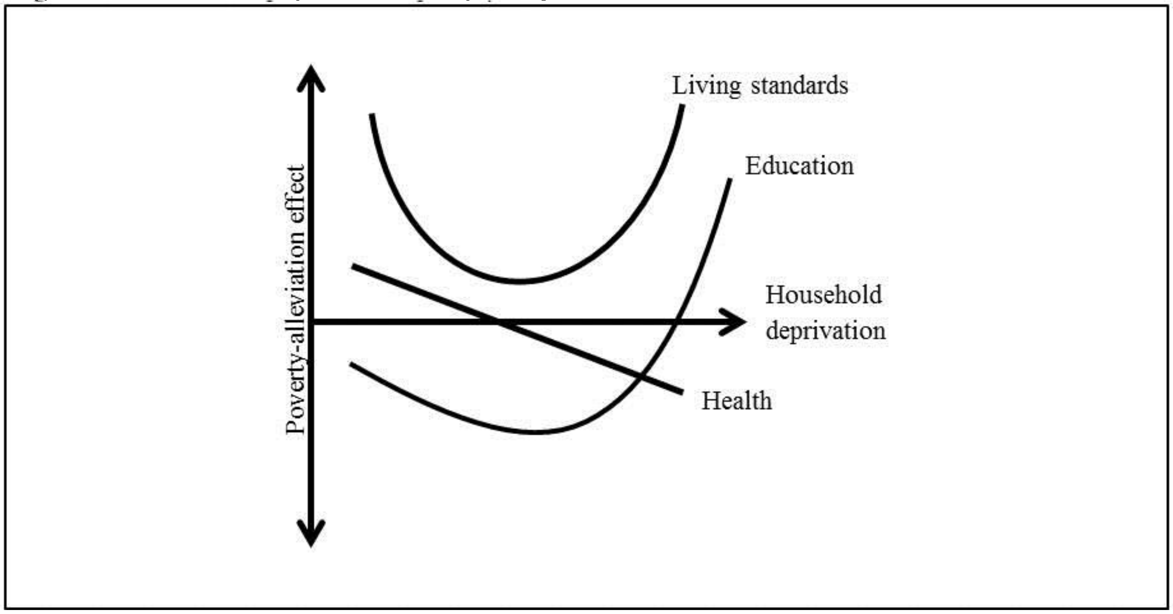
		Deprivation dimension		
DID coefficients	**MPI**	**Living Standards**	**Education**	**Health**
Least deprived households	-0.015*	-0.033***	0.010**	0.007**
	(0.0079)	(0.0086)	(0.0043)	(0.0036)
Moderately deprived households	0.029***	-0.014*	0.037***	0.006
	(0.0061)	(0.0077)	(0.0066)	(0.0064)
Severely deprived households	0.022***	-0.035***	-0.021***	0.078***
	(0.0072)	(0.0085)	(0.0077)	(0.0097)
Observations	14628	14628	14628	14628

*0.1, **0.05, ***0.01 significance levels. Robust standard errors in parenthesis

We use the controls in Model 4 in tables (2) to (5), but do not report them here for ease of presentation and comparison.

FIGURES

Figure 1: Household deprivation and poverty-alleviation effect of RAIDP on MPI dimensions



APPENDIX

APPENDIX A: MULTIDIMENSIONAL POVERTY INDEX

The following table was obtained directly from Alkire and Santos (2010) and shows a breakdown of the dimensions, indicators, and weights of the MPI. The figures for 2001 and 2011 were estimated for the rural sub-samples in the two rounds of the Nepal Demographic and Health Survey (NDHS)

Table A: Dimensions, indicators, cutoffs and weights of the MPI

Dimension	Indicator	Deprived if…	Related to…	Relative Weight	2001	2011
Education	Years of Schooling	No Household member has completed five years of schooling	MDG 2	16.70%	47.54%	27.69%
	Child School Attendance	Any school-aged child is not attending school in years 1 to 8	MDG 2	16.70%	67.64%	58.40%
Health	Mortality	Any child has died in the family	MDG 4	16.70%	18.78%	8.03%
	Nutrition	Any adult or child for whom there is nutritional information is malnourished*	MDG 1	16.70%	63.85%	16.69%
Standard of Living	Electricity	The household has electricity	MDG 7	5.60%	83.90%	30.71%
	Sanitation	The household's sanitation facility is not improved (according to the MDG guidelines), or it is improved but shared with other households	MDG 7	5.60%	81.10%	57.16%
	Water	The household does not have access to clean drinking water (according to MDG guidelines) or clean water is more than 30 minutes walking from home.	MDG 7	5.60%	63.33%	20.63%
	Floor	The household has dirt, sand or dung floor	MDG 7	5.60%	95.19%	80.88%
	Cooking Fuel	The household cooks with dung, wood or charcoal	MDG 7	5.60%	96.43%	87.37%
	Assets	The household does not own more than one of: radio, TV, telephone, bike, motorbike or refrigerator, and does not own a car or truck.	MDG 7	5.60%	85.74%	64.24%

Note 1: MDG1 is Eradicate Extreme Poverty and Hunger, MDG2 is Achieve Universal Primary Education, MDG4 is Reduce Child Mortality, MDG7 is Ensure Environmental Sustainability.

Note 2: The household deprivation percentages for 2001 and 2011 were estimated with the rural sub-sample.

* Adults are considered malnourished if their BMI is below 18.5. Children are considered malnourished if their z-score of weight-for-age is below minus two standard deviations from the median of the reference population.

APPENDIX B: EMPIRICAL ESTIMATION TECHNIQUES

B.1 Difference-in-Differences

The difference-in-differences (DID) method is used to estimate the average effect of an intervention (treatment) occurring at a given time, k. It can be implemented with panel data, by following the same individuals over time, or with repeated cross sections, by drawing samples from a population before ($t_0 < k$) and after ($t_1 > k$) the treatment (Blundell & Costa-Dias, 2008). Let y_{it} be the outcome for individual i at time t, α be the intercept term, β_i be the treatment effect on individual i at time t, A_i be the treatment status equaling 1 if individual i receives the treatment at time t and 0 otherwise; and u_{it} be the unobservable component of y. The model can then be formally expressed as

$$(1)\ y_{it} = \alpha + \beta_i R_{it} + u_{it}.$$

The fundamental requirement of DID is that both treated and non-treated groups experience pre-intervention common trends (macro shocks). The DID approach allows for unobservable time-invariant individual effects, n_i, and the common trend, m, in the error structure,

$$(2)\ E[u_{it}|R_i, t] = E[n_i|R_i] + m_t.$$

It is assumed that the differenced error structure preserves the randomization assumption conditional on observables,

$$(3)\ E\left[u_{it_1} - u_{it_0}|R_i = 1\right] = E\left[u_{it_1} - u_{it_0}|R_i = 0\right] = \left[u_{it_1} - u_{it_0}\right],$$

This assumption rules out selection on time-invariant individual-specific effects but does not eliminate temporary unobservables. Under these assumptions we have:

$$(4)\ E[y_{it}|R_i,t] = \begin{cases} \alpha + E[\beta_i|R_i=1] + E[n_i|R_i] + m_t & \text{if } R_i = 1 \text{ and } t = t_1 \\ \alpha + E[n_i|R_i] + m_t & \text{otherwise.} \end{cases}$$

Applying double differencing across time and treatment status we have,

$$(5)\ \{E[y_{it_1}|R_i=1] - E[y_{it_0}|R_i=1]\} - \{E[y_{it_1}|R_i=0] - E[y_{it_0}|R_i=0]\},$$

which yields the average treatment on the treated (ATT),

$$(6)\ \beta^{ATT} = E[\beta_i|R_i=1].$$

Under repeated cross sections we can still apply this method as long as the treatment and control groups are identifiable before the intervention so that the average fixed effect per group cancels out during the differencing procedure. In this case we would have,

$$(7)\ E[\hat{\beta}^{DID}] = \beta^{ATT} = \{[\bar{y}_{t_1}^1] - [\bar{y}_{t_0}^1]\} - \{[\bar{y}_{t_1}^0] - [\bar{y}_{t_0}^0]\},$$

where $\bar{y}_t^R$ is the average outcome of the group under treatment status R at time t.

Note that in the presence of temporary average fixed effects per group or of different macro trends, the DID approach will not estimate the ATT consistently. To verify this, we next incorporate each component into the model. First, let us assume that there are unobserved temporary group fixed effects, o_t. At the group level, the unobservables structure in equation (2) would be,

$$(8)\ E[u_t|R,t] = E[n|R] + E[o_t|R] + m_t.$$

In this case, the estimation of (7) would yield and inconsistent ATT:

$$(9)\ E\left[\hat{\beta}^{DID}\right] = \beta^{ATT} + E\left[o_{t_1} - o_{t_0} \middle| R = 1\right] - E\left[o_{t_1} - o_{t_0} \middle| R = 0\right]$$

In the same way, if the absence of temporary average fixed effects assumption holds, but we relax the requirement for a common macro trend across the groups, we would still have an inconsistent estimate of the ATT. In this case, the structure of the unobservables would be,

$$(10)\ \ E[u_t|R,t] = E[n|R] + q^R m_t,$$

where q^R is a scalar that allows for different macro trends across the two treatment groups. Again, the estimated ATT would be inconsistent as the DID would be approximating

$$(11)\ \ E\left[\hat{\beta}^{DID}\right] = \beta^{ATT} + E\left[o_{t_1} - o_{t_0} \middle| R = 1\right] - E\left[o_{t_1} - o_{t_0} \middle| R = 0\right].$$

If the treated and untreated groups do not experience a common trend, (7) will not consistently estimate the ATT as it would yield the following:

$$(12)\ \ E\left[\hat{\beta}^{DID}\right] = \beta^{ATT} + (q^1 - q^0)E\left[m_{t_1} - m_{t_0}\right].$$

B.2 Propensity score matching

The PSM method compares the outcome of a treated observation with the outcomes of comparable non-treated observations. To match the treated with the non-treated, we have to choose a matching algorithm. In this study we use the nearest neighbor, radius, and local stratification algorithms; and briefly describe each method below.

With nearest neighbor PSM households in the treated group are matched with the observation in the non-treated group with the closest propensity score. In order to increase reliability and to reduce the variability of the nearest neighbor estimator, we match to the closest ten non-treated households as recommended in Blundell and Costa-Dias (2008). A point to consider when 'oversampling' in the nearest neighbor is that this practice may trade off minimum variance for reduced bias as we use more information from the counterfactual group (Caliendo & Kopeinig, 2005). An issue to be considered when conducting PSM with a nearest neighbor algorithm is whether the matching procedure is done 'with replacement' or 'without replacement'. In the former case, non-treated observations can be matched to more than one treated observation, whereas in the latter case the non-treated are only matched once. If we allow replacement, the quality of the matching will increase and the bias will decrease because it minimizes the distance between the treated unit and the matched comparison units (Dehejia & Wahba, 2002). In contrast, if the matching occurs without replacement, we run the risk of comparing the outcome of treated observations with units that may be quite different in terms of propensity score. There is also the possibility that the results are sensitive to the order in which the observations were matched. For these reasons, in this study we allow for replacement of the non-treated units.

The radius method uses a predetermined distance from the propensity score of the treated household and performs the matching on all the control observations that fall within that neighborhood. This method may yield a better estimate than nearest neighbor when the closest neighbors are too far away. However, by imposing a tolerance level on the propensity score distance, we run the risk of having a larger variance if fewer matches are made (Caliendo & Kopeinig, 2005).

Similar to the radius method, we can also match observations through stratification. In this method, the observations are divided into blocks that satisfy the balancing property (similar characteristics across treatment and control observations in each block). The matching is performed with all the observations within the predetermined blocks.

B.3 Inverse probability-weighted estimation

In the attempt to minimize the variance of the estimates by using the common support condition in PSM, we run the risk of omitting a significant amount of observations if they lie outside of the range of the control propensity scores. To mitigate this issue and to test the robustness of the estimated treatment effect, we also conduct an inverse probability-weighted (IPW) estimation.

The first stage of IPW is similar to the PSM inasmuch we fit a logistic regression to obtain the probability of treatment for each household conditional on a set of covariates. Then, we run a linear regression weighted by the inverse of the probability of being treated for the treated households and the inverse of the probability of not being treated for the control households. This allow us to assign higher weights to the households located in the middle of the treatment probability distribution, and lower weights to the observations at the extremes of it (Wooldridge, 2007).