# Can Skill Diversification Improve Welfare in Rural Areas? Evidence from the Rural Skills Development Project in Bhutan

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Asian Development Bank

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**ABSTRACT**

Income growth in rural areas is a considerable challenge to further poverty reduction and economic development. Using a survey of rural Bhutanese households, we investigate the impacts of a vocational skills training program that was intended to diversify incomes outside of agriculture. We find that the program had limited positive impacts along various economic and psychosocial dimensions, but that it diversified household incomes into these skill areas. Notably, the program did raise incomes for trainees in non-competitive labor markets where trainees accounted for only a small percentage of the overall population. The results and anecdotal evidence suggests that: (i) a greater emphasis on creating a mechanism to connect the training program to income generating opportunities via job placement services, entrepreneurship, or mentoring services is needed—especially in competitive labor markets where there are too many trainees in relation to the population; (ii) refining the curriculum and extending the training time to allow trainees to develop their skills may be important; (iii) encouraging greater equality in the skill development process may require providing more female-friendly training that has flexibility in training time and venues and focuses on other skill areas.

## I.  INTRODUCTION

Seventy-five percent of the world's most vulnerable and impoverished populations reside in rural areas and have income sources that are primarily derived from agriculture activities (World Bank, 2008). How to effectively raise the income of this population and reduce poverty and vulnerability remains a contentious development issue. On one side are those who argue for greater investment in activities aimed at raising agriculture productivity, while on the other is a set that suggests non-agricultural activities have an important role in increasing the welfare of the rural poor.

Recognizing that rural populations are constrained by low skills and agricultural activities are highly risky, training programs for non-agricultural activities have been promoted based on the premise that skill diversification can create additional opportunities for income generation that will improve the circumstances of the rural poor. The extensive evaluation of training programs in developed and developing countries, however, have revealed that program design is crucial to ensuring the effectiveness and success of such programs (Betcherman, Olivas, and Dar 2004).

This paper examines the impact of the three-month training component of the Rural Skills Development Project (RSDP) in Bhutan. This project was designed to diversify income sources of rural households beyond agriculture and to reduce expenses spent on housing repairs by training villagers in carpentry, masonry, plumbing, and electrical wiring. The absence of a valid baseline survey and the non-randomized nature of beneficiaries to the project imposed significant challenges in assessing the program impacts. Time and cost considerations also resulted in an endline survey of Bhutanese households that had a control group that was drawn from the same villages as the trainees. To resolve these limitations, we used a match estimator that includes a proxy for motivation to create the match between the treatment and control group to estimate the causal impacts of the training. Potential pitfalls to our estimates that could arise from unobserved selection bias and spillover effects were examined in detail and supplemented with qualitative analysis to support the validity of our results.

We make several important contributions to the literature. First, as most evaluations have been based primarily in urban environments, we provide important insights into the effects of training in a rural environment. Secondly, we focus on a wider array of outcome variables which we believe is important as the justification for implementing the project was not solely to raise income, but also to reduce housing costs and diversify incomes outside of agriculture. Finally, we examine distributional issues related to the project impact and specifically the impact that the training has on competitive and non-competitive labor

markets. Taking into account competition within a labor market and how it may influence the effectiveness of a training program appears to be absent in previous studies.

The analysis shows limited evidence of income increases, decrease in housing repair costs, and increases in psychosocial outlook due to the training program. However, there is evidence that the training program allowed for diversification of income sources into skills outside of agriculture by increasing the amount of income received from the skill areas covered by the training program. Income diversification mainly occurred for females, those who are less educated, and those who were trained in carpentry or masonry. Most notably, in geographic areas that were non-competitive, meaning trainees accounted for a smaller percent of the population, the training did lead to significantly higher household per capita incomes. These findings are significant as it suggests that having too many people in the labor market within a small geographic area may be counterproductive without an explicit mechanism to connect trainees to the labor market or provide work opportunities. Finally, as women are much less likely to participate in the training, encouraging greater equality in the skill development process may require providing more female-friendly training that has flexibility in training time and venues as well as training in other skill areas.

The remainder of the paper is organized as follows. Section II provides background on RSDP. Section III describes the related literature. Section IV describes the research design including the choice of the control group and the data gathered for the evaluation process. Section V describes the empirical approach and results for a variety of specifications and sub-populations. Section VI aims to validate the results by eliminating unobserved selection and spillover effects as possible factors. Section VII discusses the findings and provides qualitative evidence for our findings. Finally, Section VIII concludes.

## II. RURAL POVERTY IN BHUTAN AND THE RURAL SKILLS DEVELOPMENT PROJECT

In Bhutan, where 80% of the population relies on subsistence farming, and rural poverty at 30.9% stands in contrast to an urban poverty rate of only 1.7%, as of 2007. Trying to implement effective policies and programs to reduce rural poverty and have more inclusive growth remains a critical development challenge (Asian Development Bank, 2009). The majority of the rural population relies on largely unproductive subsistence agriculture for their income and daily consumption needs. Moreover, many rural Bhutanese are exposed to considerably difficult climate and terrain where generating income from agriculture is nearly impossible for a good part of the year. Due to limited income generating opportunities and the high incidence of poverty in rural areas, there has been an increasing rate of urban migration especially among the youth. This has contributed to an urban unemployment rate of 33% among youth (ages 15–24 years) as the demand for labor has not kept pace with the supply (Ministry of Labour and Human Resources, 2008). The high rate of urban youth

unemployment and the disparities in poverty incidence between urban and rural areas are increasingly seen as a threat to social stability and economic development in Bhutan. Thus, raising the incomes of the rural poor is potentially crucial to ensuring continued stability in the development process.

RSDP was a $1.99 million project implemented by the Royal Government of Bhutan in collaboration with the Asian Development Bank (ADB) running from February 2007 to December 2010. RSDP was designed to mitigate the degree of poverty among rural Bhutanese by diversifying income opportunities outside of agriculture during the off season and providing cost savings on expenditures required for minor repairs.[1] As a result, this project was seen as an important step in increasing the economic and social welfare of the rural population.

RSDP provided training in four basic construction skills of carpentry, masonry, plumbing, and electrical wiring to poor villagers to increase their net income. Training was provided in three stages. The first two stages took place over 3 months. Stage 1 comprised theory lessons where participants reviewed basic concepts related to the skills. Stage 2 involved practical demonstration where participants were introduced to building and took part in construction of a toilet structure. Stage 3 provided on-the-job (OJT) training where participants were involved in school hostel construction over a period of approximately 4–6 months. For all stages, training was held for seven hours each day, excluding one hour for lunch. The average attendance rate was high at nearly 98%. Stipend compensation for the first two stages was approximately 4,100 ngultrum (Nu) per month or about Nu180 per day ($3.6), while Stage 3 participants received Nu6,000 per month or about Nu270 per day ($5.4).[2]

The training was provided in all 25 *gewogs*[3] across the three districts of Bumthang, Haa, and Trashigang from February 2007 to November 2010. Potential candidates for training cited their interest in a particular skill program while trainers and the project team subsequently selected candidates based on demand. There were a pre-allocated number of seats for each skill area. To ensure sufficient participation to build a toilet in Stage 2 and a hostel in Stage 3, some participants who volunteered for a specific trade that were over the quota were offered to enter training for their second priority skill area. For the training program to occur in a *gewog,* the project management required recruitment of a minimum of 35 trainees for participation in Stages 1 and 2. Participants for Stage 3 were selected from those who completed the first two stages and were interested in further developing their skills. In most *gewogs* participation targets were not met under a voluntary basis and pressure was placed on villagers to participate in the program so that the 35-trainee threshold for the program to occur in a given *gewog* could be reached. However, interviews with villagers indicated that many were not fully aware of the existence of the training program at inception and sometimes was the main driving factor for lack of participation.

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1    The off season for farming depends on the particular district.
2    All conversions are based on an exchange rate of 50 ngultrum (Nu) to $1.
3    Each district is broken into smaller areas known as *gewog,* similar to the concept of sub-districts. Each *gewog* comprises 4–10 *chiwogs,* which is equivalent to a cluster of villages. Each *chiwog* consists of approximately 200–400 households.

By end of project, 831 trainees completed Stages 1 and 2 of the training. Of these, 280 completed Stage 3 across the three target districts. In addition, 81 trainees, of which 71.6% were female, received training in hairdressing. Table 1 shows the completion of training activities for the different stages for the three different districts. It shows that most stage 1 and 2 activities had been completed in Haa and Trashigang districts by November 2009, but that many of the stage 3 activities in Trashigang, where the majority of trainees occurred, did not complete until July of 2010.

**Table 1: Dates of Training Activities by District**

| | Dates | | | |
| | Stage 1 and 2 | | Stage 3 | |
| Districts | Start | End | Start | End |
|---|---|---|---|---|
| Haa (Dawakha LSS) | September 2008 | December 2008 | February 2009 | June 2009 |
| Haa (Sombeykha CPS) | January 2009 | February 2009 | February 2009 | April 2009 |
| Haa (Gakiling) | July 2010 | August 2010 | September 2010 | December 2010 |
| Trashigang (Thungkhar LSS) | September 2009 | November 2009 | April 2010 | July 2010 |
| Trashigang (Bikhar LSS) | September 2009 | November 2009 | February 2010 | April 2010 |
| Trashigang (Joenkhar CPS) | September 2009 | November 2009 | January 2010 | March 2010 |
| Bumthang | March 2010 | June 2010 | July 2010 | November 2010 |

Source:  Lower secondary school (LSS), central primary school (CPS).

Given plans to scale up the approach in other *gewogs* after project completion and assessing whether the project achieved its intended goals is important. By identifying which populations can benefit the most from these training programs and where there are constraints to participation, it is possible to help guide how to effectively expand future training programs so as to maximize the impacts and to have greater equality in the development process.

## III. RELATED LITERATURE

The methods and means to effectively raise the income and overall welfare of the poor and vulnerable in rural areas are debatable. While the primary focus on raising rural incomes has been through increases in agriculture productivity, there is a growing body of evidence that diversification into non-farm activities can have large benefits for poor rural households and can serve as an engine of growth for the rural economy. The origins of advocating investment in agriculture activities to reduce poverty has arisen primarily from cross-country studies such as Loayza and Raddatz (2010); de Janvry and Sadoulet (2009); and Christiaensen, Demery, and Kuhl (2010) who have found that greater increases in agriculture gross domestic product (GDP) generally tends to have a greater effect on reducing poverty than other sectors. However, these findings based on aggregate data may arise from the fact that agriculture is dominated by poorer households and therefore has less insight into what are truly effective options to alleviate poverty in rural areas.

In comparison, findings on the effects of sectoral investment at a microeconomic and country-specific level are more disparate. Manufacturing and tertiary sector in the People's Republic of China are found to contribute less to poverty reduction than agriculture by Ravallion and Chen (2007) and Montalvo and Ravallion (2010). However, while agriculture growth is cited as playing a major role in contributing to poverty reduction in Indonesia, growth in rural services is found as a significant contributor to reduced poverty using regional aggregate data by Suryahadi, Suryadarma, and Sumarto (2009). Evidence on India provided by Lanjouw and Murgai (2009), finds only small growth in the self-employed non-farm sector in rural India. Moreover, Foster and Rosenzweig (2004) find that rural industrialization is a substitute for added productivity in the farm sector in India which suggests that trying to promote both farm and non-farm sectors in rural areas may not be an optimal strategy.

Yet, while there is support for the idea that investment in agriculture is important for helping the rural poor, there are a number of studies that advocate non-farm activities as a pathway out of poverty. Kinda and Loening (2010) and Babatunde and Qaim (2009) suggest that while agriculture holds a large potential for growth, it cannot solely meet the challenge of rural employment. A large number of studies have found significant evidence to support policies and programs focused on non-agricultural activities in rural areas. Deininger, Jin, and Sur (2007) find that the value added in the non-farm formal sector in Sri Lanka amounts to almost 80% of agricultural GDP. Adams (2002) finds that increases in aggregate non-farm income has a greater impact on reducing inequality and poverty in Egypt than increases in agriculture income. However, this is likely driven by Egypt's institutional characteristics where highly productive agricultural land has limited access mandating that the poor enter non-farm activities to increase their incomes. Barrett et al. (2005) finds that diversification in Africa especially those associated with work not related to unskilled labor results in higher income and greater income mobility. Barrett (2005) examines diversification strategies in Africa and arrives at the conclusion that those who devote themselves solely to agricultural production must rely on a low-return strategy of complete dependence on the agricultural sector and often find themselves in a poverty trap. Escobal (2001) finds that 51% of net income of rural households comes from off-farm activities in Peru. Access to public assets such as roads, and private assets such as education and credit, are important factors in diversification. The finding that increasing access to these assets will help rural households to increase their employment in non-farm sector. These findings support the claim that the non-farm sector holds considerable potential for growth and poverty reduction.[4]

Besides sectoral factors that contribute to increased welfare, identifying the types of programs and policies that are effective in raising incomes of the poor is important. In particular, training programs are increasingly used by development organizations to improve the welfare of the poor. However, the findings are typically mixed especially when comparing between the results of experimental and non-experimental evaluations. The findings from experimental evaluations typically provide less support for training programs as an effective strategy to raise employment and incomes. Card et al. (2011) evaluated a training program

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[4]   A report reviewing some anecdotal evidence on rural non-farm economy is Davis (2004).

based on random assignment that was targeted at young adults between the ages of 18–29 in the Dominican Republic. They find no positive impact on employment with only minor evidence of an impact on hourly wages and the probability of having health insurance coverage. Attanasio, Kugler, and Meghir (2011) evaluates a relatively short term training program requiring 3 months of in-classroom training and 3 months of OJT to young adults between the ages of 18–25 in urban areas based on randomized assignment of program offering. In contrast, they find the program did raise earnings and employment for both men and women in Colombia with lower bound estimates for internal rates of return of 13.5% for women and 4.5% for men. Betcherma, Olivas, and Dar (2004) provides an extensive review of studies that have examined the impacts of training programs. With the exception of training programs that explicitly facilitated labor market linkages, they found that most training programs had small or no effects. Ibarraran and Rosas-Shady (2009) reviewed seven different rigorous evaluations of training programs intended to increase employment opportunities for youth in Latin America that primarily provided demand driven training courses creating a more direct linkage with labor market opportunities. Five of the seven countries showed significant improvements in employment prospects due to the training. This suggests that training, and even short-term training, can potentially help increase employment outcomes and incomes of disadvantaged populations.

Yet, very little exists in the literature on the effectiveness of training programs in rural environments. As education and incomes are much lower in rural areas, and the local labor markets are smaller, one could expect very different findings than those in urban environments and it is not necessarily straightforward that training rural populations in skills outside of agriculture will improve their circumstances. This study which examines the impacts of a short-term training program for rural households in Bhutan will greatly complement past studies in understanding not just whether training programs work, but whether rural workers have the capacity and capability to take advantage of such a training program without an explicit component that also helps provide job opportunities.

## IV. RSDP SURVEY

### A. Research Design

The challenge in conducting this study was an absence of a valid baseline study, the non-randomized nature of the participation in the training program, and time and cost constraints. This ultimately determined our design of survey, sampling selection, and subsequent empirical methodology. We developed an extensive endline household survey to capture detailed characteristics of the respondents and households and measure the project impacts over a variety of economic, social, and psychosocial outcomes.[5] This survey was run from late September to mid-November of 2010.

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[5] The survey can be obtained from the authors upon request. Household per capita income measures were constructed from detailed responses to income generating activities in the main and secondary occupations of each household member over the course of the past year, as well as income obtained from other sources such as rentals, remittances, and sale of assets. For agriculture income, specific details were asked on cropping patterns and the amount sold of each crop in the last year. The main part of our survey was modeled after the Bhutan Living Standard Survey.

Our approach was to gather a census survey of all trainees between the ages of 18–40 years of age. This age range covers 90% of the trainee population. We also surveyed a set of non-trainees that were characteristically similar in age and gender profiles under the assumption that this control group could serve as an appropriate counterfactual to the set of trainees. We required that this control group was between 18-40 years of age and were roughly 33% female. To remain within the time and cost constraints, we derived a control group from within the *gewogs* covered by the project.[6] Surveying was largely done by calling trainee and control group members into the *gewog* offices in return for a compensation of Nu100 to take a survey that took on average 75 minutes to complete. For trainees that were out of their *gewog* during the survey period, an effort was made to conduct phone interviews. Further details on the sampling and survey methodology are provided in Appendix A and Appendix B.

## B.    Sample and Descriptives

To focus on moderately longer-term impacts, we imposed the restriction that trainees had completed the training program at least 12 months prior to the survey and focused on trainees that had only completed stages 1 and 2. We dropped stage 3 trainees as very few had completed training 12 months prior to the survey.[7] Table 2 shows survey rates for trainees in our sample. We were able to survey 74% of all trainees. Survey rates differ considerably dependent on trainee characteristics with higher survey rates for females than males, for those less than 25 years of age, and over the skills of masonry and plumbing compared to carpentry and electrical.

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[6]    The terrain in Bhutan is very challenging making it difficult and costly to send enumerators to comparable villages. Had time and cost considerations not been a factor, we would have surveyed a control group of individuals who were outside of the pilot *gewogs* and have similar characteristics to trainees. This set of trainees should be drawn from similar sized villages and should be a set we can claim have otherwise been interested in participating in Rural Skills Development Project (RSDP) had RSDP been offered in the village.

[7]    The attempt to survey stage 3 trainees to obtain information on their current status reached only 27%. It was indicated to be due to stage 3 trainees having contract employment on hydropower projects and other construction projects outside of the districts. Marriage and migration to urban centers to pursue other occupations was also cited in some cases.

**Table 2: Trainee Counts**

| | Counts | | |
| Variable | Actual | Surveyed | Percent Surveyed |
|---|---|---|---|
| Total | 432 | 320 | 0.74 |
| District: Haa | 128 | 101 | 0.79 |
| District: Trashigang | 304 | 219 | 0.72 |
| Skill: plumbing | 70 | 66 | 0.94 |
| Skill: masonry | 163 | 121 | 0.74 |
| Skill: carpentry | 127 | 77 | 0.61 |
| Skill: electrical | 72 | 56 | 0.78 |
| Male | 283 | 203 | 0.72 |
| Female | 149 | 117 | 0.79 |
| Age < 25 | 129 | 105 | 0.81 |
| Age >= 25 | 303 | 215 | 0.71 |

Notes:: Trainees are ages 18-40 years who have completed training at least 12 months prior to the survey, and were only in stages 1 and 2 of the training program.
Source: Authors' estimates.

Our final sample consists of 451 non-trainees and 320 trainees. Table 3 summarizes the characteristics of trainees and non-trainees over key variables. This table shows there are differences between control group population and the trainee population. The control group has an average age that is three years older, a larger percentage of females and married individuals, and lower levels of education suggesting that there are selection effects at work. Some of the outcome measures of interest show that non-trainees appear somewhat worse off than trainees with lower household per capita incomes and higher costs of household repairs. In general, the surveyed population is poor with yearly average household per capita income of Nu9,076 ($181) for non-trainees and Nu11,376 ($226) for trainees. Moreover, the cost of household repairs at around Nu5,000 per year account for almost 10% of a household's budget.

**Table 3: Summary Statistics of Key Variables**

| Variable | Non-trainee | | | |
| --- | --- | --- | --- | --- |
| | Mean | SD | Min. | Max. |
| HH per capita income (ngultrum) | 9,076 | 12,881 | 0 | 88,333 |
| HH per capita expenditures (ngultrum) | 3,266 | 7,634 | 0 | 129,043 |
| Total cost of HH repairs (ngultrum) | 5,701 | 25,747 | 0 | 505,069 |
| Loan amount–present (ngultrum) | 16,485 | 59,197 | 0 | 900,000 |
| Change loan amount 3 years prior (ngultrum) | 9,327 | 68,030 | −500,000 | 900,000 |
| % HH per capita income from skill area | 0.06 | 0.23 | 0 | 1.87 |
| Change status from 3 years ago | 0.91 | 1.41 | −5 | 9 |
| Prospects now compared to 3 years ago | 4.77 | 1.56 | 1 | 10 |
| Number of assets livestock | 0.23 | 0.46 | 0 | 2 |
| Change # assets livestock from 3 years ago | 0.09 | 0.39 | −2 | 2 |
| Number of HH assets | 7.64 | 2.48 | 0 | 15 |
| Change # HH assets from 3 years ago | 0 | 0.16 | −1 | 1 |
| Received income from skill area | 0.09 | 0.28 | 0 | 1 |
| HH size | 4.8 | 2.38 | 1 | 31 |
| Number of of HH > 15 years of age | 0.71 | 0.25 | 0 | 1 |
| Respondent: female | 0.49 | 0.5 | 0 | 1 |
| Respondent: age | 32 | 6 | 18 | 40 |
| Years of education | 1.58 | 3.36 | 0 | 16 |
| Respondent: married | 0.71 | 0.45 | 0 | 1 |
| Approximate observations | 451 | | | |

| Variable | Trainees | | | |
| --- | --- | --- | --- | --- |
| | Mean | SD | Min. | Max. |
| HH per capita income (ngultrum) | 11,376 | 13,756 | 0 | 95,000 |
| HH per capita expenditures (ngultrum) | 2,844 | 4,634 | 0 | 41,433 |
| Total cost of HH repairs (ngultrum) | 4,946 | 11,333 | 0 | 88,216 |
| Loan amount–present (ngultrum) | 12,561 | 37,348 | 0 | 500,000 |
| Change loan amount 3 years prior (ngultrum) | 4,503 | 43,559 | −300,000 | 500,000 |
| % HH per capita income from skill area | 0.17 | 0.29 | 0 | 1 |
| Change status from 3 years ago | 0.95 | 1.29 | −5 | 6 |
| Prospects now compared to 3 years ago | 4.74 | 1.51 | 1 | 9 |
| # assets livestock | 0.16 | 0.37 | 0 | 2 |
| Change # assets livestock from 3 years ago | 0.03 | 0.4 | −2 | 2 |
| # HH assets | 7.87 | 2.38 | 0 | 14 |
| Change # HH assets from 3 years ago | 0.02 | 0.16 | −1 | 1 |
| Received income from skill area | 0.32 | 0.47 | 0 | 1 |
| HH size | 4.94 | 1.94 | 1 | 14 |
| # of HH > 15 years of age | 0.73 | 0.23 | 0 | 1 |
| Respondent: female | 0.37 | 0.48 | 0 | 1 |
| Respondent: age | 28 | 6 | 18 | 40 |
| Years of education | 2.01 | 3.18 | 0 | 13 |
| Respondent: married | 0.62 | 0.49 | 0 | 1 |
| Approximate observations | 320 | | | |

HH = household, Min = minimum, Max = maximum, SD = standard deviation.
Notes: Trainees and non-trainees are ages 18–40 years. Trainees completed training at least 12 months prior to the survey, and were only in stages 1 and 2 of the training program.

## V. EMPIRICAL METHODS AND RESULTS

Our objective is to identify the impacts of participation of individual, *i*, in stages 1 and 2 of the skills training program, $T_i$, on a series of social and economic outcomes, $y_i$. This is done by creating a counterfactual comparison group, $y_i(0)$, for those who participated in the program, $y_i(1)$. As the non-randomized nature of the program makes selection bias a relevant concern, we investigate the effects of the training program primarily relying on a non-parametric matching estimator to estimate the average treatment effect (ATE), $\tau^{ATE}=E[y_i(1)- y_i(0)]$, and average treatment effect on the treated (ATT), $\tau^{ATT}=E[y_i(1)- y_i(0)| T_i=1]$.

### A.  Matching Estimator

Matching estimators attempt to resolve the endogenous nature of participation in the training program. However, in the context of an endline survey, it relies heavily on the assumption that (i) all observables adequately capture participation into the training program such that there is no unobserved effect that both drives the participation decision and the resulting outcomes, and (ii) observables capture characteristics of the individuals prior to the treatment. Given these assumptions hold true, then it is possible to obtain an unbiased estimate of the ATE and ATT. The matching estimator used is based on a bias-adjusted nearest neighbor match estimator proposed by Abadie and Imbens (2006, 2011). This relaxes restrictions imposed by the original parametric propensity score match estimator of Rosenbaum and Rubin (1983). This estimator is computed via non-parametric methods with each trainee matched to the five closest non-trainees based on the relevant variables.

Variations in the variables to construct matches were investigated. Household size, sex, age, education, and civil status were ultimately chosen for inclusion in our model based on their significance in determining the outcomes of interest and the high likelihood that they did not change due to selection into the program. As our control group is drawn from the same villages as the treated group, unobserved motivation and innate skills related to work performance is a concern as they are often correlated with higher participation rates and better outcomes.[8] This implies that the estimates may be biased away from zero and cause us to falsely find that the program had an effect. Our survey asked several questions on willingness to participate in a hypothetical training program for six weeks at a stipend compensation of Nu160 per day and willingness to travel for work that could potentially proxy for motivation. Answers displayed in Table 4 show that trainees are more motivated to participate in a training program and are more willing to travel further for work opportunities. We therefore included the indicators on stated willingness to travel for work to proxy for motivation and limit the selection bias. These variables are interacted with the sex indicator as the decision

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[8]  Having panel data (that is baseline and endline data) would have helped in estimation as we would have been able to control for unobserved characteristics via individual fixed effects. This bias is believed to be reduced in instances where *gewog* heads placed pressure on villagers to participate in the program suggesting that some of the trainees may not be as different from the set of control group drawn from the same villages lessening selection bias on the basis of motivation.

to enter training, earnings potential, and outlook are believed to differ substantially between male and female. Kernel density estimates of a propensity score estimated from a probit model of the probability to participate in the training based on the set of variables chosen for estimating the match estimator are shown in Figure 1. It shows a high degree of overlap between the set of trainees and the set of non-trainees indicating that it may be possible to construct a relevant counterfactual control group.

**Table 4: Behavioral Factors Affecting Participation in Training**

| Categories | Nontrainee | Trainee |
|---|---|---|
| Observations | 451 | 320 |
| Willing to participate in training | 0.70 | 0.76 |
| **Distance willing to travel for work** | | |
| Outside district | 0.27 | 0.38 |
| Within *gewog* | 0.08 | 0.09 |
| Within district | 0.18 | 0.19 |
| Within village | 0.17 | 0.10 |

Notes: See Table 3 notes.
Source: Authors' estimates.

**Figure 1: Propensity Scores for Trainees and Non-trainees**



Predicted Probability

——— Trainee    ----- Non-Trainee
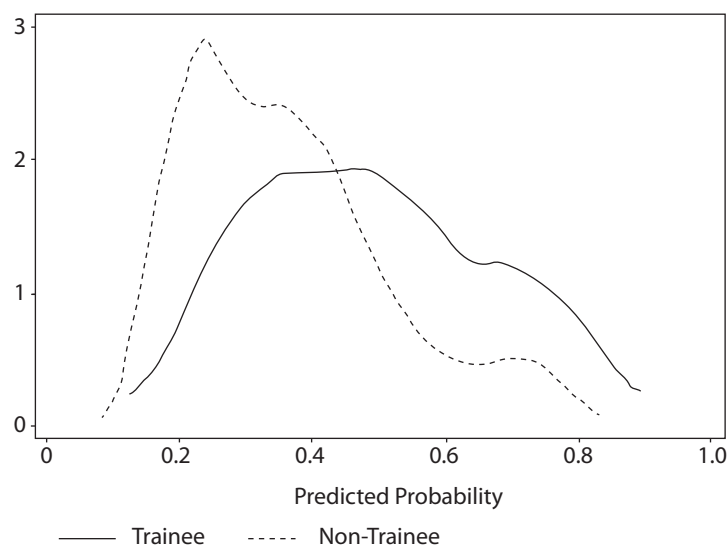
Source: Authors' estimates.

### 1. Impacts on the Average Trainee

Table 5 shows results of the ATE and ATT using the matching estimator. Column 1 estimates the ATE for the basic match estimator with biased-adjusted standard errors and exact matching on sex and years of education variable, but excludes the willingness to travel for

work variables. Column 2 includes the willingness to travel for work variables as criteria for matching. Columns 3 and 4 are like columns 1 and 2, respectively, except it estimates the ATT. Column 5 imposes on top of the criteria of column 4 that the matches are exact over the civil status variable. Column 6 additionally imposes over the estimate of column 5 that all matches only occur within the area of common support defined by a regression based propensity score and trims 0.05 from the minimum and maximum values. Both columns 1 and 3 are for comparison purposes, to show what the exclusion of the proxy for motivation from the match may have on the estimated impacts. It shows that the exclusion of this proxy results in a finding that household per capita income significantly increased due to the training program. However, columns 2, 4, 5, and 6 are likely more valid estimates as it tries to account for selection bias in the estimates by including the proxy for motivation in the match.

In all cases, there is limited evidence that the training program had any effect on incomes or other outcomes such as housing repair costs, loan amounts, and self-perceived status of trainees more than one year after completion of the training program. The only outcome that is robustly significant to our variations in specification is the percentage share of household income coming from construction related employment.

### Table 5: Estimated Impact of Training on Different Outcomes

| Outcome Variable | Matching Estimator | | | | | |
|---|---|---|---|---|---|---|
| | ATE | ATE | ATT | ATT | ATT | ATT |
| | (1)a | (2)b | (3)a | (4)b | (5)c | (6)d |
| HH per capita income (ngultrum) | 1838.40* | 1616.77 | 2529.65** | 1858.67* | 1538.65 | 1701.15 |
| | [1063.52] | [1035.52] | [1077.94] | [1124.08] | [1158.96] | [1146.32] |
| HH per capita expenditures (ngultrum) | −365.90 | −545.50 | −686.57 | −570.13 | −653.96 | −987.27 |
| | [548.95] | [507.53] | [601.73] | [563.87] | [473.57] | [600.15] |
| Total cost of HH repairs (ngultrum) | −771.87 | −945.62 | 312.71 | 492.33 | −196.26 | −962.25 |
| | [1429.72] | [1398.90] | [1173.11] | [1153.67] | [1435.84] | [1499.91] |
| Loan amount−present (ngultrum) | −3283.66 | −3463.82 | −3394.57 | −2965.99 | −2366.02 | −3865.98 |
| | [3657.61] | [3526.45] | [3354.87] | [3103.64] | [3106.24] | [3193.25] |
| Change loan amount 3 years prior (ngultrum) | −6969.52 | −6078.94 | −6914.65* | −5560.17 | −5470.93 | −6774.89* |
| | [4237.31] | [4156.06] | [3719.27] | [3651.31] | [3613.45] | [3778.38] |
| % HH per capita income from skill area | 0.085*** | 0.087*** | 0.070*** | 0.062** | 0.058** | 0.063** |
| | [0.022] | [0.023] | [0.025] | [0.026] | [0.026] | [0.025] |
| Change status from 3 years ago | 0.023 | 0.013 | 0.054 | 0.031 | 0.082 | 0.069 |
| | [0.111] | [0.105] | [0.117] | [0.109] | [0.114] | [0.117] |
| Prospects now compared to 3 years ago | −0.099 | −0.142 | −0.113 | −0.185 | −0.146 | −0.172 |
| | [0.126] | [0.121] | [0.131] | [0.122] | [0.126] | [0.128] |

ATE = average treatment effect, ATT = average treatment effect on the treated, HH = household.
Notes: Robust standard errors in brackets. * p<0.1, ** p<0.05, *** p<0.01. See table 3 note.
 All estimates are computed via STATA nnmatch function with bias-adjusted standard errors, where each trainee match to closest 5 non-trainees in sample based on match variables.
 a Columns 1 and 3: Match variables are household size, sex, age, years of education, civil status, and sex interacted with all other variables. Exact matching on sex and years of education.
 b Columns 2 and 4: Match variables as in column 1 with willingness to travel for work indicators.
 c Column 5: Match variables as in column 2. Exact matching also on civil status.
 d Column 6: Match variables as in column 5 with observations trimmed with propensity score that are 0.05 from the minimum and maximum values of common support defined by the propensity score.

## 2. Impacts on Different Subgroups of Trainees

Even if there are relatively few impacts overall, it is possible that different subgroups may have benefitted from the training program. We examine the impact on different subgroups to see whether the training program should use more targeted strategies to maximize its impact. Table 6 provides the ATT of the distributional impacts of the training among different sub-populations as well as the different skill areas for the outcomes of household per capita income, housing repair costs, and percent of household per capita income obtained from the different skill areas. These estimates are based on the basic match estimator from Column 4 of Table 5. Similar to the overall results, the findings show no evidence of any significant impacts of the training program on household per capita income or in the cost of housing repairs for the different sub-groups. However, the training had an impact on the diversification of incomes primarily for females, low-educated individuals, and those trained in carpentry and masonry.

### Table 6: Estimated Impact of Training on Log Household Per Capita Income for Different Sub-Groups

| Outcome Variable | HH per Capita Income (ngultrum) | Cost of Housing Repairs (ngultrum) | % HH Per Capita Income Skill Area |
|---|---|---|---|
| Female | 2,051.72 | 75.24 | 0.100*** |
| | [1,543.31] | [1,142.47] | [0.031] |
| Male | 1,743.36 | 1,476.24 | 0.044 |
| | [1,566.68] | [1,188.46] | [0.037] |
| Age < 25 | 2,422.02 | −2,711.98 | 0.044 |
| | [1,935.16] | [2,460.59] | [0.048] |
| Age >= 25 | 1,415.89 | 614.73 | 0.054* |
| | [1,359.11] | [1,467.64] | [0.030] |
| Highest Education: Jr High or Above | 2,395.88 | −3,964.89 | 0.026 |
| | [2,716.56] | [6,719.49] | [0.054] |
| Highest Education: Elementary | 1,265.16 | 931.24 | 0.070** |
| | [1,237.60] | [950.93] | [0.029] |
| Skill: Carpentry | 2,012.18 | 1,215.45 | 0.084*** |
| | [1,307.15] | [1,078.38] | [0.032] |
| Skill: Masonry | 897.9 | 2,124.59 | 0.090** |
| | [1,459.15] | [1,340.25] | [0.037] |
| Skill: Electrical | 2,615.52 | 320.77 | 0.058 |
| | [2,407.63] | [1,859.70] | [0.047] |
| Skill: Plumbing | 683.47 | −2,329.8 | 0.027 |
| | [1,903.94] | [3,149.85] | [0.035] |

Notes: Robust standard errors in brackets. * p<0.1, ** p<0.05, *** p<0.01.
See Table 5 notes. Average treatment effect on treated (ATT) estimated.
Match variables are household size, sex, age, years of education, civil status, and sex interacted with all other variables. Exact matching on sex and years of education (where applicable).

### 3. Impacts on Trainees in Non-Competitive and Competitive Labor Markets

The impact of the training may also have a differential effect depending on the percentage of trainees within a geographic area. This may arise if a labor market becomes too competitive or saturated with too many workers of a certain skill set. This would lower the expected income that can be obtained from that skill area unless demand for that skill rises. We combine our data with 2010 voter eligibility data from the Election Commission of Bhutan at the *chiwog* level (sub-area of a *gewog*) and construct an indicator for the percent of trainees within a *chiwog*. We reestimate ATT effects for the nearest neighbor matches on a sample where individuals resided in non-competitive labor markets (*chiwogs* where trainees comprise less than 2% of the population) and on a sample where individuals resided in competitive labor markets (*chiwogs* that had trainees comprising greater than 2% of the population). We added several indicators for average incomes in a *gewog* to better control *gewog* resources that can affect outcomes. This roughly splits our sample into equal halves with approximately 350 observations per sample.

Table 7 display results from this analysis. It shows that there is a significant increase in household per capita income for trainees in non-competitive labor markets (column 1), but not competitive labor markets (column 3). These results are robust to trimming the top 0.05 and bottom 0.05 of the sample using an estimated propensity score as seen in columns 2 and 4. In non-competitive labor markets, the training is also shown to have significantly led to a diversification of incomes, by increasing the percent of household per capita income that comes from one of the skill areas served by the training.[9] In comparison, the training appears to have no impact on percent of household per capita incomes that come from the four skill areas for trainees residing in competitive labor markets. However, trainees in competitive labor markets are found to have lower household per capita expenditure and reduced amounts spent on loans. Given that the current average per capita income of a trainee is Nu11,376 and the estimated rise in income for trainees in non-competitive labor markets is approximately Nu3,000 this accounts for over a 33% increase in household per capita income.

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[9] Analysis of sub-groups were not considered for competitive and non-competitive labor markets as the reduced sample size makes it difficult to obtain a sufficient number of matches between treatment and control groups.

**Table 7: Estimated Impact of Training on Different Outcomes**

| Outcome Variable | Matching Estimator ATT Percent of Trainees in *Chiwog* | | | |
|---|---|---|---|---|
| | < 0.02 | < 0.02 | > 0.02 | > 0.02 |
| | (1)a | (2)b | (3)a | (4)b |
| HH per capita income (ngultrum) | 3191.96** | 3041.30** | 2230.14 | 2197.24 |
| | [1255.87] | [1278.84] | [1419.93] | [1427.70] |
| HH per capita expenditures (ngultrum) | −1492.48 | −1898.91 | −1046.30* | −1109.07* |
| | [1195.96] | [1238.66] | [552.91] | [567.27] |
| Total cost of HH repairs (ngultrum) | 387.52 | 30.96 | −530.85 | −1138.84 |
| | [1014.47] | [1031.93] | [2561.85] | [2795.74] |
| Loan amount–present (ngultrum) | 386.81 | 691.72 | -4685.67 | −6201.19 |
| | [5090.65] | [5248.27] | [4124.03] | [4248.38] |
| Change loan amount 3 years prior (ngultrum) | 1753.78 | 2911.53 | −11967.64** | −14393.53*** |
| | [5384.00] | [5590.30] | [5074.38] | [5068.46] |
| % HH per capita income from skill area | 0.134*** | 0.123*** | 0.032 | 0.036 |
| | [0.032] | [0.032] | [0.036] | [0.037] |
| Change status from 3 years ago | −0.171 | −0.218 | −0.057 | 0.04 |
| | [0.168] | [0.171] | [0.140] | [0.143] |
| Prospects now compared to 3 years ago | −0.237 | −0.302* | −0.229 | −0.181 |
| | [0.177] | [0.180] | [0.153] | [0.157] |

ATT = average treatment effect on the treated, HH = household.
Notes:  Robust standard errors in brackets. * p<0.1,  ** p<0.05, *** p<0.01.
See Table 5 notes. Average treatment effect on treated (ATT) estimated.
   a   Columns 1 and 3:  Match variables are household size, sex, age, years of education, civil status, and sex interacted with all other variables. Exact matching on sex and years of education.
   b   Columns 2 and 4:  Match variables as in column 1 and 3 with observations  trimmed with propensity score that are 0.05 from the minimum and maximum values of common support defined by the propensity score.

# VI. ROBUSTNESS CHECKS

The validity of our results relies heavily on the assumption that unobserved selection bias and spillover effects are not contaminating our control sample, that is used as our counterfactual for our trainee sample. This section discusses whether these are likely factors driving our results.

## A.   Unobserved Selection Bias

There is a lack of evidence that the training program had any effect on the average trainee who participated in the training program except on the basis of raising the percentage of household per capita income obtained from construction-related skills as opposed to other sources of income. While the estimates consistently showed no effect over most outcome measures and various modeling assumptions, it is plausible that trainees are not randomly selected given our variables chosen for matching or that the proxy for motivation, the willingness to travel for work, was changed by entry into the training program. While we believe that such a motivation indicator should remain the same, we re-examine the impact

of the training program within the context of maximum likelihood estimation of treatment-effects (MLE-TE) and two-stage least squares (2SLS) to estimate the local-average treatment effect (LATE).[10] In this analysis, we attempt to identify the training effects using a person's stated belief on the importance of the construction skill area as an instrument for opting into the training program.

We impose exclusion restrictions in the treatment equation when estimating using maximum MLE-TE and 2SLS. We use the respondent's perceived belief in the value of construction-related activities offered by the training program as indicators that would affect participation in the training program, but does not affect actual outcomes of the household. It is hypothesized that this variable should be excluded from the main regression equations because after including motivation factors, which we proxy by the stated willingness to travel for work, this should not explicitly enter the main outcome equations.[11] This instrument is shown to be significantly correlated with entry into the training program at the 10 per cent significance level. This estimation provides the local average treatment effect (LATE) which models the effect of treatment of the marginal individual who decides to enter treatment due to an increased belief that construction-related activities are useful.[12] It therefore has a useful policy interpretation in that, if this effect is significant, it suggests that greater outreach and encouragement on the value of having construction related skills would lead to significant changes in outcomes for the marginal participant who decides to enter the training program.

Table 8 shows that for the marginal trainee who decides to participate in the training program, because of perceived belief that the training is useful, there is no significant increase in household per capita income, total cost of household repairs, or even a rise in percent of household per capita income from this skill areas.

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[10] The MLE-TE regression model is a maximum likelihood estimator that assumes joint normality of the error terms for the selection, modeled via probit and outcome equation. It explicitly addresses the bias caused by correlation of the regression variables with omitted variables by adding a term to the regression that represents the non-zero expectation of the error term. Given the specification equations are correctly modeled and the errors are joint normal, then this approach will essentially resolve the bias stemming from unobservables that may arise in the propensity score matching methods. However, joint normality is a fairly restrictive assumption especially in smaller sample sizes in the context of this model. On the other hand, the 2SLS regression model is the first stage via linear probability model. And the error from both the first stage and the second stage are assumed to be independent.

[11] The validity of this instrument is debatable within the context of our analysis if the training program affected the perceived belief on the importance of construction skills.

[12] Imbens and Angrist (1994) show that only under very restrictive assumptions will the LATE actually coincide with the ATE.

**Table 8: Estimated Impact of Training on Different Outcomes**

| Outcome Variable | TE-MLE | 2SLS |
|---|---|---|
| | LATE | LATE |
| | (1) | (2) |
| HH per capita income (ngultrum) | 9635.31 | 15386.16 |
| | [20963.53] | [18219.8] |
| HH per capita expenditures (ngultrum) | −6067.35 | 9543.12 |
| | [9843.43] | [7891.51] |
| Total cost of HH repairs (ngultrum) | 36122.88 | −16900.7 |
| | [37128.6] | [35111.26] |
| Loan amount–present (ngultrum) | 4319.23 | −11269.9 |
| | [75481.16] | [56732.68] |
| Change loan amount 3 years prior (ngultrum) | −21855.7 | −968886.8 |
| | [88202.91] | [81347.42] |
| % HH per capita income from skill area | −0.88 | 0.23 |
| | [0.72] | [0.34] |
| Change status from 3 years ago | 0.22 | −3.43 |
| | [2.01] | [2.84] |
| Prospects now compared to 3 years ago | −3.38 | −8.18 |
| | [2.66] | [5.48] |

Notes:    Robust standard errors in brackets. * p<0.1, ** p<0.05, *** p<0.01.
See Table 5 notes. Treatment-effect maximum likelihood estimate (TE-MLE) via STATA treatreg and two-stage least squares (2SLS).
Variables included in outcome equation are household size, sex, age, years of education, civil status, indicators for willingness to travel, and sex interacted with all variables and average income in *gewog*.
Variables included in selection equation are as in c, but with instrument on stated belief in value of one of the skills provided by training program.
Standard errors clustered by *chiwog*.

## B.    Spillover Effects

Our models have attempted to address the selection bias that arises when unobserved motivational factors determine selection into the training and subsequent outcomes. However, the absence of any effect on household incomes, cost of housing repairs, or other outcomes could be due to spillover effects that arise from using a control group that are obtained from the same village as the treatment. Spillovers could also explain why we observe significant increases in household per capita incomes in *chiwogs* with non-competitive labor markets compared to *chiwogs* with competitive labor markets.

For example, spillovers would invalidate our use of a control group if trainees were sharing knowledge obtained from the training with non-trainees. This would allow non-trainees to use these skills to also earn income and lower the cost of house repairs and would be a positive indirect benefit of the program. Spillovers could also lead to reduced differences in total cost of household repairs if trainees were providing non-trainee households with free or bartered labor or higher costs of household repairs if more trainees make it easier to access construction-related services. Thus, in the presence of spillovers we would expect to underestimate the true impacts of the program.

To test for the possibility of spillover effects, where $i$ indexes the household, $c$ the *chiwog*, and $g$ the *gewog*, we run a probit model on the probability of earning income from one of the skill areas, $P(y_{icg}^{skill} > 0)$, on the percent of trainees within a *chiwog*, $p_{cg}^{tr}$, individual characteristics, $X_{icg}$, and indicators for average income within a *gewog*, $\mu_g$ given that an individual is a non-trainee, $T_{icg} = 0$. Namely,

$$P(y_{icg}^{skill} > 0) = \Phi(\alpha + \gamma p_{cg}^{tr} + \rho X_{icg} + \sigma \mu_g + \varepsilon_{icg} > 0 | T_{icg} = 0) \tag{1}$$

If there are spillover effects then we should find that the non-trainee sample is more likely to have obtained income from one of the skill areas if they are living in a *chiwog* with a higher density of trainees. That is $\gamma \Phi(.) > 0.$ We find no evidence that there is any relationship between the density of trainees in a *chiwog* and obtaining income from the skill area provided by the training program as seen in Table 9.

**Table 9: Tests of Spillover Effects**

|  | Marginal Effects Estimates |
| --- | --- |
|  | Equation (1)a: % Trainees in *Chiwog* |
| P(Income from skill > 0) | −0.222 |
|  | [0.531] |
| Observations | 342 |
|  | Equation (2)b: Training*% Trainees in *Chiwog* |
| Outcome Variable | (1) |
| HH per capita income | −12,516 |
|  | [56,929] |
| HH per Capita Expenditures | 2,391 |
|  | [19,569] |
| Total Cost of HH Repairs | −45,664 |
|  | [52,545] |
| Loan Amount - Present | 36,780 |
|  | [105,427] |
| Change Loan Amount | −46,326 |
|  | [99,873] |
| % HH per capita income from skill area | −2.386** |
|  | [0.977] |
| Change Status from 3 Yrs Ago | −1.28 |
|  | [3.778] |
| Prospects Now Compared to 3 Yrs Ago | 5.913 |
|  | [3.979] |
| Approximate Observations | 661 |

HH = households.
Notes: Robust standard errors in brackets. * p<0.1, ** p<0.05, *** p<0.01.
See table notes:
Equation (1)a. Non-trainee sample only. Probit estimate of obtaining any income from skill area regressed on % of trainees plus household size, sex, age, years of education, civil status. Indicators for willingness to travel and sex interacted with all variables and average income wealth of *gewog* indicators.
Equation (2)b. Full sample. Standard regression estimate of outcomes regressed on trainee indicator, % of trainees in *chiwog*, and trainee interacted with % of trainees in *chiwog*, plus household size, sex, age, years of education, civil status, indicators for willingness to travel and sex interacted with all variables, and average income wealth of *gewog* indicators.

We also perform a second test for the possibility of spillover effects where each outcome measure of interest was regressed on an indicator for trainee, $T_{icg}$, the percentage of trainees within a *chiwog*, $p_{cg}^{tr}$, plus the standard control variables, $X_{icg}$, included in the match estimates. *Gewog* fixed effects, $\mu_g$, are included to account for average outcomes values within a *gewog*. Standard errors were clustered by *chiwog*. Specifically we ran a regression as follows:

$$y_{icg} = \alpha + \beta T_{icg} + \gamma p_{cg}^{tr} + \delta T_{icg}p_{icg}^{tr} + \rho X_{icg} + \mu_g + \varepsilon_{icg} \qquad (2)$$

If there are spillover effects, then we would expect that $\delta < (>)0$, if $\beta > (<)0$. This implies that in the presence of spillovers, and after controlling for other factors that drive the outcomes of interest, we should expect to observe that trainees in *chiwogs,* where there are a higher percentage of trainees there is less of an impact on outcomes than in *chiwogs* where there is a smaller percentage of. Table 9 shows coefficient estimates for $\delta$, the interaction between trainee indicator and density of trainees in *chiwog*. It shows that there is no evidence that trainees are differentially affected by the percentage of trainees in *chiwogs* for all outcomes with the exception of percentage of household per capita income from the skill area. Given that the results from equation (1) showed that non-trainees in more competitive markets were no more likely to have made income from construction-related skills suggests that the findings are more consistent with a story of competitive effects rather than a story of spillovers.

In addition, spillovers are unlikely to exist for several other reasons. First, the typical villages covered by the project are significantly spread out over a sizable land area, making information flow difficult and raises the cost of training other villagers. Second, for 86 *chiwogs*, trainees accounted for an average of 2.2% of the voting age population with the highest percentage *chiwogs* accounting for less than 11% of the population. Third, previous studies such as Godtland et al. (2004), have found that diffusion of knowledge and transference of skills are low in the short-term. As trainees in our sample had completed the training on average 1.25 years before the survey, this is still considered a relatively short-term examination of the training effects. Thus, we believe that spillover effects are not driving our results.

## VII. DISCUSSION

The overall results on the impact of the training showed limited economic, social, and psychosocial impacts. These conclusions appear robust to varying specifications with no evidence that these weak results are driven by potential spillover effects. However, significant impacts of the training program on household per capita income were found for trainees that reside in non-competitive labor markets. Thus, the training is potentially useful in raising incomes given that the labor market is not saturated with too many people holding the same skills. In labor markets where there is an oversupply of people that have been trained in a given skill area, equilibrium wages may become depressed and job opportunities more limited and thus may negate much of the monetary benefits of the program. As most individuals

remained in their village, it reflects the lack of opportunities that can be generated based purely on local market demand for this type of work within a given *gewog* or district.

The average trainee was able to diversify their incomes, but not increase their overall income, coincides with anecdotal reports that a large number of rural villagers merely substituted agriculture activities for construction-related activities. This is in partly because the agriculture season and peak construction season occurs during similar months. More generally, it highlights the difficulties that rural workers may face in trying to obtain decent work opportunities. Without having an explicit mechanism that allows trainees to access employment opportunities in these skills the current Stages 1 and 2 training program will have little benefit for the average trainee in a competitive market.

Yet, providing an explicit mechanism for trainees to access a larger labor market for their skills may not be sufficient. Intensifying the learning materials and providing more training to build the skills and confidence of trainees to be competitive in a larger labor market may be necessary. In our attempt to survey trainees who also went through Stage 3 training, we were only able to track down 27%. This is potentially indicative of the benefits of the longer term training program as the trainees who were not reached were mostly indicated as employed in contract work by relatives and friends in their home village.

Table 10 provides qualitative support for these claims. Over half of all trainees suggested that the training program could be improved through provision of better work opportunities and 52% of trainees from Stage 1 and 2 training cited that the training program could be longer. This suggests that increasing the length of the training program and providing job placement services and entrepreneurship support to facilitate the trainees to start up their own business may also greatly facilitate rural households in reaping more immediate benefits from the training program.

**Table 10: Evaluation of Training Program**

|  | Stage 1–2: Theory and Toilet | Stage 3: Hostel Construction |
|---|---|---|
| Observations | 429 | 71 |
| Rating (scale 1–10) | 6.6 | 7.0 |
| Better Work Opportunities | 0.56 | 0.72 |
| Length of Training Longer | 0.52 | 0.56 |
| Better Training Materials | 0.19 | 0.14 |
| Length of Training Shorter | 0.17 | 0.18 |
| Timeliness in Delivery of Tools | 0.13 | 0.13 |
| Timeliness of Stipend Payments | 0.13 | 0.10 |
| Occupational Health and Safety | 0.10 | 0.06 |
| Better Demonstration Tools | 0.09 | 0.06 |
| Quality of Trainer | 0.09 | 0.10 |
| Training Environment | 0.07 | 0.06 |
| Geographical Location | 0.06 | 0.07 |
| Curriculum Content | 0.05 | 0.10 |

Note:    Contains all trainees surveyed independent of age or months since training.

## VIII. CONCLUSION

There exists a variety of programs to raise the incomes of the rural poor. The potential for non-agriculture activities to have greater value added while serving as an alternative source of income has garnered support as a way to alleviate poverty and vulnerability to poverty of the rural poor. The rural skills development program in Bhutan was a short-term training program that was designed with this purpose in mind. It trained the rural poor in practical construction skills with the intention of raising overall incomes, diversifying incomes outside of agriculture activities, and reducing the costs of household repairs.

This study relied purely on an endline survey and was faced with limitations on the control population. Thus, to examine the effects of the training program on different social and psychosocial outcomes relies on heavy assumptions to identify the causal impact of the training program on the average trainee. This underscores the need for the process of serious program evaluation to begin at the start of the project. Had a sufficient baseline existed and a randomization component been included, it would have been possible to rely less on the modeling assumptions and more on simple specifications to arrive at more definitive estimates on the magnitude of the effects.

Our conclusions are inevitably constrained by the process of evaluation and we recognize that there are potential limits to our method of evaluation that are impossible to resolve within the constraints imposed by the available data. A number of researchers such as LaLonde (1986) and McKenzie et al. (2010) have shown that propensity score matching tends to overestimate the effects that are found using experimental methods. Moreover, Heckman et al (1997) has found that bias due to omitted variables is significant in typical parametric methods of matching, but non-parametric methods can potentially resolve this bias given baseline and endline data. In the absence of spillovers, our data may represent a possible upper bound on the benefits that can be achieved from such a short-term training program.

Nevertheless, by addressing selection and spillover effects and supplementing it with some qualitative analysis, we believe our results are valid. Our analysis revealed a number of potential challenges and constraints to achieving highly positive economic, social, and psychosocial impacts for the general rural villager who decides to enter the short-term training program. Our finding that the training was beneficial in raising incomes for trainees who resided in non-competitive labor markets, but not in competitive labor markets, highlights the need for program design to better assess the labor market and ensure that it does not become over saturated. If the labor market situation is not taken into account then the training program may have little effect on the average rural trainee.

Still, if diversification of incomes into non-agriculture activities is believed to help in reducing vulnerability to poverty, then the program has partially achieved one of its objectives. However, refining the curriculum and extending the training time to provide more time for trainees to really develop their skills may help improve the success of the training. Having

an explicit mechanism for trainees to enter the larger labor market through job placement services and providing entrepreneurship support can further help to increase employment opportunities that lead to greater rise in income for trainees and can in turn lead to greater improvements in psychosocial outlook.

Finally, there are significant distributional implications based on the population set that decided to voluntarily participate in the program. As females are significantly less likely to participate even after controlling for education, household size, and village characteristics, having greater inclusiveness across genders may entail providing more flexible training times and venues that accommodate the needs of women and a selection of courses that are more appealing to the female population. By refining both the training program and the targeting of the program it may help to better accelerate the speed at which it is possible to reduce poverty and vulnerability to poverty of the rural poor and increase equality in the development process.

## APPENDIX A

**Survey Design, Sampling, and Implementation**

**A.    Survey Design**

The evaluation was intended to focus on the project impacts for trainees in Haa and Trashigang districts as the trainees in Bumthang had only completed Stages 1 and 2 of the training program 6 months prior to the evaluation. The evaluation used two survey instruments consisting of a household survey and a village head survey. The household survey asked a set of questions designed to measure outcomes including household and community welfare, individual level behavior and attitudes, and perspectives on the program itself. A parallel survey of village heads focused on community-level outcomes including material wellbeing and collective action.

A number of compromises were made in the survey design and sampling strategy due to time constraints and difficulty in correctly surveying rural households. These households, in general, do not keep precise records of their income or expenses and their generally low-levels of education made it improbable that we could accurately measure either. The ability for rural household to accurately recall their expenditures versus their income was suspect. The pre-testing phase of the survey revealed that their recall of expenditures was generally much higher than their recall of income causing us to suspect the accuracy of the expenditure measures. The initial pre-test of the survey in a village in the Haa district, the time to conduct the survey ranged from 2–3.5 hours. In the second test pre-test in Kabisa, a peri-urban setting, survey times were significantly shorter ranging from about 1.15Aria1.5 hours. The times improved substantially as enumerators further familiarized themselves with the survey allowing some to complete the survey in approximately 25 minutes.

Many of the questions were substantially revised from the initial survey during the pre-test phase as we realized that many of them did not pertain to the rural context or were inevitably difficult to ask. We also cut some questions to minimize the time burden on the respondents with the goal of still having the survey capture the majority of issues related to the project that could be relatively accurately measured by the population group of interest.

The final survey was designed to explore the following key hypotheses:

**1.    Economic Welfare**

1.1   Villagers who received training will have higher increases in income and greater welfare improvements than villagers who didn't receive the training.

1.2   Villagers who received the training will spend less on household repairs as a proportion of their income than villagers who did more receive the training.

1.3   In particular, male trainees below age 25 will have higher income increase and greater socio-economic welfare improvements than female trainees.

1.4   In particular, trainees trained in house wiring and carpentry will have higher income increase than trainees trained in masonry and plumbing.

1.5   Trainees who underwent Stages 1–3 training will have higher income increase than trainees who only underwent Stages 1 & 2 training.

1.6    Villages with high number of trainees will have higher socio-economic welfare and welfare improvements than villages with less number of trainees.

1.7    Villagers with higher education who received the training were able to increase their incomes more than villagers with lower levels of education.

1.8    Villagers who received the training are more likely to migrate to urban centers.

### 2.    Social Benefits

2.1    Villagers who received training will have a more positive outlook in life and stronger standing within the community and within the household as a result of increased employment and income.

2.2    Villagers who received training will participate more proactively in community activities and take on leadership roles than villagers who didn't.

Some of these hypotheses ultimately ended up being difficult to examine due to sample size and selection effects.

### B.    Sample Size and Sampling Strategy

The objective was to test with a 95% probability a difference of 10% increase in wages/income at a power of .90 for each of the two treatment groups: (i) the group of 467 individuals that have completed stage 1 and stage 2 only and (ii) a group of 214 individuals that have completed Stage 1, Stage 2, and Stage 3. Assuming standard deviations in incomes or other variables interest of approximately 25%, this suggested a minimum sample size of 132 observations to test differences between each treatment group based on rough sample size calculations in STATA. This would also imply a minimum control sample size of at least 132 observations.

The intent was to improve the power of our predictions on the impact of the program and to do a more in-depth analysis of how the program may have benefitted different types of households who opted to participate in the program. We therefore attempted to obtain a census of all trainees who entered the program and a control group sample that was similar in size.

Ideally, individuals in the control groups would have been randomly selected from a sample of other villages that are characteristically similar to RSDP-offered villages (e.g., based on distance to market, access to roads, and average income levels), but were not offered any opportunities to join RSDP. The individuals in the control group would then be chosen given their likelihood to join RSDP. This approach is usually taken when only endline data is available due to the possibility that there are spillover effects within villages where individuals may share their training with others who may not have had time to participate in the actual programs and the concern that one cannot accurately control for selection bias into the program given that participation in the program was on voluntary basis.

However, the logistical challenges of surveying in the rural areas of Bhutan and cost constraints required us to make several simplifying adjustments. First, we focused on simply ensuring that control group is between the ages of 18–37 years old and contain females that account for roughly 33% of the control group. The ratio of 33% is roughly the gender proportion observed in the treatment groups and the 18-37 population is the age range of the treated trainees that captures 90% of all of the trainees. It is expected that sampling of the control group population across the different *gewogs* and districts will be done in proportion to the population shares of this age group in each *gewog* for

a given district. This should adequately capture a similar population group to the treated trainees. Second, instead of gathering a random sample outside of the village areas, our control group was obtained from within the already designated village areas for RSDP through a selection process of the *gewog* officials.

The time burden and costs on enumerators of having to travel in difficult conditions and go from village to village, drove the decision to ask respondents to come for personal interviews at the closest *gewog* centers. These respondents were compensated at a rate of Nu150 Nu ($3). This compensation is roughly an average pay for a semi-skilled worker. We also aimed to gather infrastructure and population information on all villages by asking the *gup* or the *gup*-assistant, who are government administrators in a given *gewog*, basic information, but were ultimately only able to obtain information for less than half of the villages. The surveying strategy was run through the *gewog* centers with each *gewog* asked to select respondents for the control group.

In general, while there are trade-offs between using this sampling strategy including possible spillovers or selection on certain types of people who may be more willing to show up at the *gewog* centers we hoped the characteristics that were gathered in the survey would allow us to ultimately control for the crucial external factors that may affect income so as to adequately measure the project's impacts. The other advantage of having the respondents come to the *gewog* centers was that it allowed for better control over the quality of the surveys as in most cases each *gewog* center was manned by several supervisors associated with LMID or a survey firm. These supervisors were tasked to audit 10% of the surveys using a subset of questions from the household survey to check for consistency and further insure the accuracy of the collection process with two versions of this one for trainees and the other for non-trainees. For this enumeration process 18 surveyors were hired at a rate of Nu300 per day with an additional Nu200 per day compensation upon adequate and quality completion of each survey round with about 3 individuals taking part in the supervisory role.

In addition to the survey instruments, a database entry program in CSPro was created that allowed us to replicate the look of the survey and introduce logic into the input of answers to further insure the accuracy. Double encoding of about 10% of the surveys was done to make sure that they are accurately input into the databases. Approximately 6–7 encoders were hired for this process.

## APPENDIX B

**Surveying Notes from the Survey Roll-Out**

It was noticed there were high sex female-to-male ratios on order of 30% or greater. Discussion with a female village head claimed that it was because males have multiple wives and bring these women in from other places. However, discussions with people familiar with cultural and demographic aspects of this claimed that monkhood and males joining the army could be one of the reasons for these skewed ratios toward females. Other discussions with the household head specialist indicated that there is bias in favor of women to receive the inheritance of the parents and could explain why effects of the RSDP may be significantly less for females than males. The claim was that the inheritance and the fact that in Bhutan males tend to move in with the females' family rather than the other way around demotivates women to work.

Surveying in Katsho *Gewog* of Haa was extremely difficult. Given the proximity to the largest urban center in Haa (Haa Town) meant that many people had a wider variety of income sources making the cost of participating in the survey higher than other areas. In the Somar *Gewog* there were a number of trainees (at least 6) that had gone out of the district for employment to build the hydropower dam as well as other government projects. Several districts in Trashigang (in particular Merak) are nomadic and migrate South before October. This meant that it was difficult to survey any of this population during the time of the survey roll-out.

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**About the Paper**

This paper investigates the short-term effects and the determinants of participation in a vocational skills training program that was intended to diversify incomes outside of agriculture. They find limited positive impacts of the program along various economic, social, and psychosocial dimensions with the exception of diversifying household incomes into the skill areas. Females are significantly less likely to participate. The results and anecdotal evidence suggest the need to place a greater emphasis on creating a mechanism to connect the training program to income-generating opportunities and possibly refine the curriculum and extend the training time to allow trainees to develop their skills. Greater equality in the skill development process may require providing more female-friendly training that has flexibility in training time and venues as well as training in other skill areas.

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