# Glass Walls:
# Experimental Evidence on Access Constraints Faced by Women*

Ali Cheema, Asim I Khwaja, Farooq Naseer, Jacob N Shapiro†

This draft: October 2019

**PRELIMINARY DRAFT**

**Abstract**

Individuals progress when they are better able to access the opportunities offered to them. Yet many, especially women, often face significant barriers in doing so. This paper provides evidence on such access constraints in the context of skill acquisition. Using experimental evidence from rural Pakistan, we show that physical distance poses a significant hurdle. Women whose villages are randomly selected to receive a training center are more than three times as likely to enroll and complete a skills development course than women who have to travel an average distance of just a few kilometers. Over half of this penalty is paid simply upon crossing the (virtual) village boundary and therefore cannot be readily reconciled with time or economic costs associated with travel. Instead, this "boundary effect" is likely due to non-economic/social costs women face when temporarily leaving their village. This constraint is costly to compensate. Using exogenous variation in stipend offered, we estimate that an amount equivalent to half of monthly household expenditure would need to be paid to allow women to cross this boundary. In examining interventions that may ameliorate this barrier, we find that while informational and social interventions have little impact, providing reliable group transportation goes a long way in addressing this access constraint.Our results further suggest the boundary effect is due to social perceptions that constrain women's agency and mobility: It is lower for women who enjoy more influence over domestic affairs,have fewer dependents, higher social status, and are from more ethnically diverse communities. Our work suggests that while non-economic obstacles faced by women are indeed substantial, policy interventions attuned to the local context can offer feasible ways to ameliorate them.

*This project is the result of collaboration among many people. We thank Rosemary Berberian, Ismail Khan, Yanchuan Liu, Kunal Mangal, Guillermo Palacios Diaz, and Landin Smith for outstanding research assistance in Cambridge and Rehan Hassan, Hasan Murtza, Rania Nasir, Sahaab Sheikh, Wafa Masood, Sarah Shaukat, Anam Shoaib, Sameem Siddiqui, Minahil Raza, and Maheen Jahangir Nawabi for outstanding research assistance in Lahore. We are grateful to the seminar participants at the AALIMS - Princeton Conference, Boston College, Cornell University, ITAM, Sussex University, Paris School of Economics, Toulouse School of Economics, Aix-Marseille School of Economics, Trinity College, UC San Diego, and USC for their comments. This paper was funded through support from DfID Pakistan, DfID and IZA's GLM LIC programme, IDRC's GrOW program, and the WAPPP at the Harvard Kennedy School. The views expressed here are those of the authors and do not necessarily reflect those of the many individuals or organizations acknowledged here.

†**Cheema:** Associate Professor of Economics, Lahore University of Management Sciences; cheema@lums.edu.pk. **Khwaja:** Professor of Public Policy, Harvard Kennedy School; khwaja@hks.harvard.edu. **Naseer:** Assistant Professor of Economics, Lahore University of Management Sciences; farooqn@lums.edu.pk. **Shapiro:** Professor of Politics and International Aairs, Princeton University; jns@princeton.edu.

# 1 Introduction

Governments in low-income countries are increasingly setting up welfare systems and providing growth opportunities for their citizens through cash transfer, employment generation, and skills enhancement programs. Many of these policies are directed towards those who have been historically excluded from the largess of the state—the poor, rural inhabitants, and women. Central to this policy is the assumption that these individuals can access the benefits provided to them. While acknowledging that the same factors that led to these vulnerable populations' exclusion from state programs could also generate obstacles in accessing new benefits, policymakers often use simple economic calculi to dismiss the salience of these access constraints. The assumption is that the needy will seek private benefits in any case, and travel costs can be compensated though small financial incentives. Yet in practice, we often see "money left on the table." For instance, we find that Indonesian villagers do not obtain subsidized rice; Indian widows fail to take advantage of monthly stipends; and Kenyan girls do not attend trade-skill classes. [1]

This paper seeks to uncover and understand one such access constraint—travel that requires a woman to move outside her community—a particular concern for women in rural areas where female mobility is a challenge. We examine this constraint in the context of a large and highly subsidized skills development program in rural Pakistan. The implementation of vocational skills training programs is important in its own regard, as emerging economies have begun introducing a plethora of such programs to address substantial skill shortages in their adult populations.[2] Yet little attention has been paid as to whether individuals are readily able to access such programs. The program we study offers a compelling context to examine access constraints since it provides free-of-cost, high-quality, and in-demand skills training. While women are typically required to travel outside their community, attendees receive a generous stipend to compensate for applicable travel costs and foregone wages. In our study, we focus on sewing and tailoring courses, which are by far the most demanded skills (surveys report that 74% of households express a desire for such training)(Cheema et al., 2012a). Yet earlier roll-outs of the program revealed that take-up rates did not come close to matching the expressed demand, with women citing travel as a major barrier despite the financial compensation intended to mitigate travel costs.

Causally identifying such distance-related access barriers poses a key challenge, as the training program location is likely endogenous to various confounding factors. For example, to the extent that such programs locate in impoverished areas, distance-related access constraints may be underestimated if the poor have lower demand (and hence lower program take-up rates) as compared to others (in richer/higher demand areas) that are further away. Conversely, if the poor have higher demand, that would lead to overestimating access constraints. Even if one could accurately identify the presence of access barriers, additional assumptions are needed to capture the economic significance of such barriers and shed light on what factors may underlie them.

Leveraging the training program design, this paper addresses these challenges to identification and examination of underlying channels and makes three contributions. First, by exploiting exogenous variation in the location of the training centers, we are able to provide what is, to our knowledge, the first clean estimate of distance-related access constraints. In doing so, we also novelly isolate a "crossing-the-boundary" effect (hereafter "the boundary effect") that occurs over and above the physical, temporal, and social costs accrued per kilometer of travel. Second, by experimentally varying the stipend amount received by trainees,

[1] Banerjee et al. (2015), Gupta (2017), and Bandiera et al. (2012)

[2] Betcherman et al. (2004) reviews over 80 impact evaluations of labor market programs in developing countries.

we can directly estimate the amount of money that is needed to compensate for these travel-based access constraints. Finally, this study provides a deeper understanding of access constraints through further examination of heterogeneity of the boundary effect as well as examining potential strategies to ameliorate. The latter is achieved by introducing several experimental interventions specifically designed to address possible underlying factors, such as informational failures, community norms, and concerns about travel safety.

Our study builds on a unique multi-year collaboration between our research team and the Punjab Skills Development Fund (PSDF), a public-private multilateral donor and state-funded entity tasked with skills development in the Punjab, the largest state in Pakistan. Preliminary work with PSDF revealed that vocational skills were in high demand: 90% of rural households expressed interest in skills programs and nominated at least one male and one female in the household who would avail themselves of such skills building opportunities. Furthermore, close to 50% of these households nominated at least two male and female members. Yet when the households were actually offered a stipend and the opportunity to take the free training courses, less than 10% of individuals enrolled, and only 3% of women completed the courses (Cheema et al., 2012b). Preliminary quantitative analysis of the initial program rollout complemented with focus groups revealed that individuals—especially women—faced substantial travel-related access constraints. Based on this preliminary work, we designed a range of randomized controlled trial interventions in the subsequent program rollout that could both isolate and estimate the size of any travel–based barriers and address the underlying concerns identified in the focus groups.

The study sample includes 243 villages that collectively offered 3,000+ training slots for women. In order to allow for cleaner comparisons, this paper focuses on the most popular skills, sewing and tailoring, which 74% of baseline female training nominees identify as the skills they would like to acquire (Cheema et al., 2012a). The experimental design randomly allocated 108 training centers in the sample villages (note that while the training centers were located in certain villages, any woman from neighboring villages could also apply for a spot in the program), thereby generating exogenous variation in distance between trainee's home and the training center. In addition, we introduced exogenous variation in the individual's training stipend offered as well as design variations that included enhanced information, trainee and community engagement, and group transport in order to shed light on the underlying factors that may contribute to access constraints.

Our first main result documents the presence of a large access barrier across a range of take-up measures—from the initial desire to apply to course enrollment and completion. Specifically, we find that establishing a training center in a village increases course applications and enrollment by two to three times. While part of this effect is driven by the additional distance a woman must cover to attend a distant, out-of-village training center (measured per kilometer), our empirical design also allows us to isolate any access change generated simply by crossing the village boundary. Strikingly, we find that half of the access difference between in-village and out-of-village is generated simply by crossing this (invisible) boundary. This strong "boundary effect" cannot be reconciled with any standard economic or time opportunity costs; crossing the village boundary does not induce a discontinuous jump in either distance or time (there is no official "village border" one has to wait to cross nor any toll paid at entry or exit), suggesting that non-economic factors must (substantially) be at play.

We next use the exogenous variation in stipend amount within and across villages to quantify these distance penalties (both the boundary effect and per-km traveled costs) in monetary terms. We compare the increase in take-up induced by additional stipend with the distance penalties and provide an equivalence between the penalties and the stipend. Using these estimates, we find that one would need to receive a monthly stipend of Rs 3,000-5,000 (51% of monthly household expenditure and 45% of monthly household

income) to merely overcome the boundary effect—-a non-trivial amount, especially since the course itself is offered free of cost. Moreover, the stipend amount needed to compensate for each "per-km" traveled cost implied by the equivalence is also orders of magnitude higher than what a standard calculation of the opportunity cost of time and economic cost of travel would imply.

Finally, we take our analysis a step further in an attempt to uncover the underlying factors that contribute to the observed access constraints. Before the start of the program, we used focus groups to identify the following key household concerns regarding courses outside their village: limited information, household- and community-level barriers, and perceived safety concerns. We then implemented three additional design variations to address each of these concerns: (i) more intense course dissemination to address informational gaps and trainee/household concerns, (ii) community engagement to discuss societal concerns, and (iii) group transport to address perceived safety and security. We find little impact of the first two interventions but a strong effect of offering group transport. In fact, almost a half to two-thirds of the boundary effect can be compensated for by offering women group transport from their own village to the training center outside of their village. We also find that the boundary effect is mitigated for three types of women: those who enjoy more leverage vis-a-vis men in domestic affairs; those whose families likely have lower socioeconomic status due to their (lower) income, asset and education levels; and those who live in more ethnically diverse communities. Interestingly, there is no heterogeneity in the boundary effect by the (perceived) safety of sending or receiving communities. These results suggest that the access barrier is less likely due to real economic or even (real) safety considerations, but instead arises from women's lack of agency within their households and concerns about perceived "status loss" because of their leaving and potentially being exposed to "outside" influences.

Our findings are especially concerning since our randomized evaluation of the overall program shows that there are substantial and persistent individual benefits arising from the skills training studied (Cheema et al., 2018b,a). Thus, to the extent that these access constraints exist, they are preventing women from accessing skills that can help generate a sustained positive impact on their lives.

Our work speaks to a broad literature that looks at barriers that preclude marginalized communities from utilizing public or private services. Distance, the constraint on which we primarily focus, is not only documented extensively in the trade literature as a barrier to the flow of goods and sevices,[3] but is also cited across a broad range of other fields—including health, finance, and education—as an impediment to service take-up.[4] Transportation is also frequently studied as a constraint to a variety of services, perhaps due to its potential to mitigate distance barriers.[5] Beyond physical constraints, lack of information on the program's potential benefits can also act as a barrier to taking up a service. For example, Jensen (2010) found that providing information to students of the expected returns to education increased the average number of years Dominican students stayed in school. A similar study conducted by Nguyen (2008) in Madagascar found that information increased both school attendance and test scores. Hicks et al. (2011) found that providing women with information on the benefits of their vocational training program in Kenya increased take-up by

[3]For a discussion about the impact of distance on international trade, see Engel and Rogers (1996), Berthelon (2004), Evans and Harrigan (2005) and Gallego and Llano (2014). For its effect on regional integration and knowledge transfer, see Keller (2002)

[4]For a discussion on distance's effect take-up of medical services see Thornton (2005),Müller et al. (1998), Ostermann et al. (2011), Gage and Guirlène Calixte (2006), Jeuland et al. (2010),Kim et al. (2014), Tanser et al. (2006), and Kremer et al. (2011). For it's effect on take-up of financial services see Ashraf et al. (2006). For a discussion on it's effect on educational and training services see Bandiera et al. (2012), Burde and Linden (2013),Jacoby and Mansuri (2011), (Jayachandran, 2015),Maitra and Mani (2017), Mukherjee (2012), and Porter et al. (2011).

[5]For a discussion on the effects of a lack of transportation on take-up of medical services see Ekirapa-Kiracho et al. (2011). For a discussion on the effects of a lack of transportation on employment opportunities see Starkey and Hine (2014), Riverson et al. (2006), Thakuriah et al. (2011), Babinard and Scott (2011), and Uteng (2012).

10%. Within this literature there is a recognition that women often face such access barriers particularly when access requires women to travel.

Moreover, a number of recent studies have shown that a lack of transportation options exacerbates the negative effect of distance on take-up. While men frequently have access to some form of private transportation (most commonly bikes), women must either take public transportation (when available) or walk.[6] Women's lack of transportation is compounded by a multitude of domestic obligations including child rearing, cooking, and cleaning. In order to have the time to fulfill these roles without quick and reliable transportation, women are forced to limit their options for work and educational opportunities to those near (within walking distance) their homes.[7] When women do venture far from their homes, they are frequently plagued by security concerns. Mitra-Sarkar and Partheeban (2011) found that 66% of women reported being sexually harassed on their commutes to work in Chennai, India, and Porter et al. (2011) found that parents were reluctant to send their girls to schools outside their village due to security concerns. There is some evidence that these constraints can be alleviated by bringing opportunities to women. Burde and Linden (2013) found that by opening a primary school within a village in rural Afghanistan, enrollment rates of girls increased by more than 50%. A similar study in rural Pakistan found that (high-caste) girls who have to cross a "settlement" boundary within their village to attend primary school have lower enrollment, while boys face no such boundary constraints (Jacoby and Mansuri, 2011).

Our paper also directly contributes to the understanding of active welfare programs, such as vocational training programs, by focusing on one aspect often neglected in the literature—their take-up rates. Many papers have been written concerning the economic impacts of vocational training programs, and the results are varied in both the developed and developing worlds.[8] However, a consistent theme across these programs, regardless of their impact, is their relatively low take-up rates. Unfortunately, very few studies track enrollment statistics for a general population, making it difficult to measure the "natural" take-up rate of most programs. Those that do track general enrollment indicate that the average take-up rate can range from as low as 5% (Bandiera et al., 2012) to as high as 20% (Hicks et al., 2011); however, studies that track a self-selected population (usually who directly applied for the training program) show course completion rates that range from 55% to 75%.[9] While studies have tried to provide reasons for these low take-up rates post-fact (Maitra and Mani, 2017), our study uniquely identified potential barriers to take-up a priori and built mechanisms into the study design to quantify and address take-up asymmetries. Moreover, as noted previously in related and ongoing work, we are examining whether such take-up constraints lead to systematic compositional differences in successful trainees and if such selection affects the ultimate impact of these programs (Cheema et al., 2018b,a).

The remainder of the paper proceeds as follows: Section 2 describes the context and intervention. Section 3 outlines the experimental and empirical design. Section 4 presents our results, and Section 5 concludes.

[6] See Porter et al. (2011), Babinard and Scott (2011), and Uteng (2012).

[7] See Thakuriah et al. (2011) and Babinard and Scott (2011)

[8] See Schochet et al. (2008) for a thorough review of randomized training programs in the U.S. See Betcherman et al. (2004) for a review of 87 impact evaluations of labor markets in developed and developing countries.

[9] See Attanasio et al. (2008), Attanasio et al. (2009), Maitra and Mani (2017),Card et al. (2011), and Hicks et al. (2011)

# 2 Context & Intervention

## 2.1 Country Context: Gender, Human Capital and Labor Markets

Human capital acquisition offers a pathway for many to improve their economic, social, and health outcomes. Women have historically faced systematic obstacles in accessing human capital enhancement opportunities. However, according to the 2012 World Development Report on Gender Equality, the global gap between male and female education rates has steadily closed over the past two decades. This progress has been made possible by the systematic elimination of barriers, both social and economic, that women face in choosing to pursue education. While policy has very effectively increased female education rates when only a single barrier impedes access, success is much slower to come when multiple barriers must be lifted at once. For example, a women in a wealthy urban family may only face constraints related to her gender, which can be relatively less challenging to address than the constraints faced by a woman in a poor rural family, who must overcome the same gender constraint in addition to a host of others.

Rural Pakistan provides a context in which women face constraints to accessing education on many dimensions. Women must not only overcome social norms against investing in female education, but they also face relatively low availability of education. Unfortunately, this is not an especially unique context. In 2011, the UN Women's Watch published survey results from 42 countries showing that rural girls are more likely to be out of school than rural boys and they are twice as likely to be out of school as urban girls.[10] In our sample, we find that over 70% of women have never been enrolled in any form of formal education. When women are able to acquire human capital in these contexts through schooling and/or other (formal and informal) training opportunities, it is often just as difficult to deploy it in the workforce. Women face similar constraints to accessing labor markets as they do education, with roughly only 10% of rural women engaging in wage labor.

While access constraints could also be present for men, in our initial work (see below) we found that distance was far less of a constraint on men. We therefore focus in this paper on barriers to women's skills acquisition and explore solutions to their unique constraints. Not only do women traditionally face more substantial access issues, but based on our work and the literature, the nature of the underlying factors that contribute to barriers for women and men are likely quite different. We therefore made a conscious choice in our study: rather than compare differences in access across genders, it would be more instructive to compare between women experiencing slightly different constraint alleviation strategies, thereby holding constant any unobservable variables that are unique to all women (but are different for men).[11]

## 2.2 Program Background and Intervention Design

### 2.2.1 The Skills Program

Our study is conducted as part of the Punjab Economic Opportunities Program (PEOP)—a flagship program of the Government of Punjab, which is implemented in partnership with the Department for International Development, Government of UK (DfID). The program aims to create inclusive growth and alleviate poverty

[10] http://www.un.org/womenwatch/feature/ruralwomen/facts-figures.html

[11] It is common in other contexts to study the effects of a constraint on women by using men as a benchmark. For instance, to understand the gender wage gap, one must necessarily include men's wages as a baseline. However, in our case, a more natural benchmark is that women who express a demand for training should (eventually) be able to access it. This benchmark then allows us to consider a ranger of design variations for women skilling programs. Each variation is designed to address an underlying factor/mechanism. By examining the impact on program take-up, we can shed light onto the particular factors that lead to women's access constraints and seek to ameliorate these factors.

in the province's high poverty districts. The focus is on increasing employability and earnings of low income, poor, and vulnerable families by augmenting their skills-base through vocational training. The program is implemented through the Punjab Skills Development Fund (PSDF), a not-for-profit company set up by the government in collaboration with DfID. Our research team, as part of the Center for Economic Research in Pakistan (CERP), has collaborated with PSDF to evaluate their success in meeting this goal and aid in collecting empirical evidence used for program design. This collaboration recognizes that cost-effective impact requires interventions grounded in and informed by solid evidence. It also offers a unique opportunity to conduct preliminary scoping studies on which to base the proposed interventions.

#### 2.2.2 Early Pilot Work: Understanding Access Constraints

The first major undertaking of our collaboration with PSDF was a large-scale baseline survey exercise of over 11,000 households in the program region. This exercise aimed to better understand the demand for skills and the specific constraints faced by potential program participants. To ensure that a holistic understanding of the local skills and labor markets was reached, we included village and household surveys, employer surveys, and trainer surveys in each of the program districts. These surveys revealed a huge latent demand for skills acquisition from both households and employers. Over 92% of households indicated their willingness to nominate at least one male and female member for skills training. Among those nominated, 96% of men and 97% of women reported a desire to acquire skills, and two thirds of households report a (high) willingness to send the nominated household member to a PSDF training in the next year. Furthermore, we found that households selected members for the training course overwhelmingly according to highest earning potential (rather than according to having highest needs, being most liked, or being currently unemployed), suggesting that households took labor market returns seriously and expected high value from the training when nominating members (Cheema et al., 2012a).

Our baseline survey also revealed that household members nominated for training clearly expected financial gains from acquiring skills. The nominees reported a high wage premium for high-skilled jobs compared to low-skills jobs, ranging from Rs. 7,135 to Rs. 17,774 (Cheema et al., 2012a). This expected wage premium was typically largest for those who were unemployed and looking for work (which constitutes nearly half of unemployed women), reflecting a high level of enthusiasm among this population for gaining training. Moreover, individuals also recognized non-economic returns to basic skills as well, such as increased degree of political engagement, the ability to exercise political rights, and health status.

It is therefore reassuring that our ongoing work on the impact of the skills training program indeed confirms such positive returns. Specifically, our initial findings suggest that women who (exogenously) received the training report improved skills, display tailoring activity, and show an increase in individual and household income. Impressively, our preliminary analysis suggests that these gains persist even 2.5 years after the training.[12]. From the perspective of this paper, the evidence shows that women not only expect high value from skills training, but that this belief is confirmed through our ongoing companion studies. This makes understanding and alleviating these access constraints all the more important.

Therefore driven by this high demand for and expectations of high return from skills , PSDF launched the first of its pilot programs, Skills for Employability (SFE), in late 2011. SFE offered a variety of training courses to both (urban and rural) men and women. However, despite the large expressed demand for

[12]Since the access constraints in obtaining skills likely impact the ability of women to also access the market to be able to deploy the skills, in ongoing work, we have consequently also included "market linkage" components.Our results so far suggest that these linkages further enhance the impact of the initial training (Cheema et al., 2018b).

training, CERP's evaluation of the program revealed low take-up from the general population. Take-up was particularly low for females, as only 7% of women who were offered vouchers for training ended up enrolling in courses, and only 3% of women completed the course (Cheema et al., 2012b). Even fewer women who enrolled belonged to poor and vulnerable households and/or lived further from training centers. Through field visits and analytical work, we found that physical distance to the training center arose as one of the main reasons for lack of enrollment in or completion of skill training programs. Moreover, close to half of the targeted trainees that refused to participate in the SFE program stated distance as the primary constraint. These findings raised concerns that the sub-populations of interest (specifically, poor, rural and vulnerable women) were not sufficiently benefiting from the training.

Interestingly, while men's take-up was also low in the SFE, distance was not as strong a constraint for men as for women. While distance was not randomly assigned in SFE, our results still offer strong suggestive evidence. We analyzed the impact of physical distance on the rural sub-population's voucher acceptance, course enrollment and course completion, controlling for a host of individual-level characteristics such as monthly income, education and employment status. Not only does distance not statistically matter for men while it does for women the point estimates of the distance penalty for women are around 10 times larger than that for men.

Using the lessons learned from the first training roll-out (the SFE program), PSDF launched a small sample pilot in 2012-13, Skills for Market - Phase A (SFM-A), specifically targeting rural women in 52 of the villages originally surveyed in the 2011 baseline surveys. In the pilot, we offered training courses in tailoring, rural dairy products, and home decoration. As follow-up interviews with the SFE population revealed distance and social norms as two key constraints, the pilot was designed to specifically address both of them. Distance to the training center was reduced by placing the training center in the village, and social norms were addressed through focus groups that encouraged women to participate by stressing its usefulness. Initial results revealed that these design innovations were promising: women who had training centers located inside their villages had the highest enrollment rates, followed by women who participated in the focus groups, while enrollment rates stayed low for women who were only informed of the program's existence. Furthermore, the highest completion rates were among women who took the tailoring training course, showing a clear preference for tailoring among other vocational skills. This preference matches the baseline survey, which found almost three quarters of all women nominated for the training preferred to acquire skills related to garments and textiles. While the pilot was conducted on a small scale, these findings subsequently informed the design of the current study.

#### 2.2.3 Intervention Design: Addressing Access Constraints

The Skills for Market - Phase B (SFM-B) program was designed and then rolled out in 2013-14 in a larger sample of villages with additional design variations. Rural women remained an important focus since more than 80% of the rural women in the program districts live in acute poverty and have below primary levels of education. Although the main focus of the program was on teaching stitching and embroidery as a vocational skill, the curriculum included several additional subjects related to success in the job market. These included reading and writing Urdu; basic numeracy such as writing numbers, taking measurements, and math skills (addition, subtraction, multiplication, division); and financial literacy, including preparing a budget and using banks to open savings accounts and acquire loans. The four-month training program was held five to six days per week in the morning, typically from 9 am to 1 pm, and required an attendance of 80 percent. Each trainee admitted to the course had a workstation with a desk and a sewing machine to use for the

length of the course, but the sewing machine was not given to trainees after completion of the course so as to maintain the course value in terms of skills gained rather than physical assets gained. The trainings were implemented by high-quality training service providers, which were selected through a rigorous procurement procedure and trained in the intervention's demand creation strategies and implementation protocol. We confirm that trainees recognized the high quality of the training, as after the training, 55% reported that the quality of the course content, training conduct and facilities was high or very high; 69% of trainees said the course met or exceeded their expectations; and 74% reported that the training helped them improve their tailoring skills.

The SFM programs aimed not only to provide skills training, but to also offer that training in such a way that the greatest number of those interested in the program were able to participate. The SFM-A program had already revealed two constraints—distance and social norms—as impediments to taking up training. To identify any further constraints, the CERP Research team carried out a series of field visits to rural households. These visits were structured to elicit qualitative feedback encompassing different limitations that women face in accessing skills training as well as to assess the practicality of different solutions aimed at alleviating these constraints. Interviews were conducted from household members (both males and females) and influential community members to elicit preferences for possible solutions in mitigating these access constraints. These visits helped identify five primary constraints to resolve: distance, information, social norms, reliable transportation, and money.[13]

It is with these five constraints in mind that PSDF developed the SFM-B training course. We began by randomly selecting 243 villages from our original baseline survey to take part in the program and receive the standard training intervention.[14] Within each village, we sampled households to participate in our constraint alleviation activities and to receive enrollment vouchers (through which we measure take-up). Each household in the sample received information about the training through a house visit and additionally received a printed voucher if they declared their intent to avail of the training opportunity. Beyond this standard intervention for all households, we also lifted constraints differently for randomly selected groups of villages, with a village having either none, one, or multiple constraints tackled simultaneously. The following summarizes the activities used to alleviate each constraint:

1. Distance - Given the importance of distance as a constraint, we sought to address this by selecting 108 (of the 243) villages to house a training center in the village itself. As a result, households in these villages are, on average, closer to their training center: the median travel distance for villages with and without a training center is 1.1 kilometers and 9.25 kilometers, respectively. This is the same design that proved to be successful in the SFM-A program, and we refer to the sample villages as Village Based Training (VBT) villages and non-Village Based Training (nVBT) villages.

2. Information - Interviews with the sample population revealed that many people do not fully understand the potential benefits of taking up a skills training program. To address this, we randomly selected 66 villages in which we conducted hour-long all-female information sessions about the training program. Applicable households were then invited to the Trainee Engagement (TE) session and notified of the program's date, time, and location during our team's first house visit. Two to three days later, the training service providers held the TE information sessions, where they disseminated

[13] Interestingly, while our prior had been that child care would be an important issue, our qualitative field visits demonstrated little demand on the part of women for such a service. Women were either fine that their own family members could take care of their children or even when they did not have such help, not comfortable with it being provided by non-family members.

[14] An additional 81 villages were also surveyed as pure controls villages. These will be used to access impact of the training on key outcomes but are not used in this paper.

information regarding course content and quality, female instructor credentials, course timings and duration, training center facility standards, and application submission protocol. Additionally, each of these information sessions shared success stories of three trainees from previous PSDF training in the SFM-A intervention. These testimonies emphasized the lifelong value of the tailoring course by showing how past trainees have used their skills to earn or save money from making higher-quality clothes for themselves, their families and neighbors, and teaching fellow villagers how to stitch (one trainee specifically mentioned how happy her in-laws are that she took the training program). The TE information session included a Q&A, which allowed attendees to ask any logistical or informational questions regarding the course. Finally, during the TE information session, attendees were also given details regarding a three-day long Open Period, during which trainees would visit the training center to see the facilities, meet the training and/or other members of the training service provider's team, and ask any residual questions about the course. Two to three days later, the three-day Open Period occurred. After another one to two days, we conducted a follow-up visit to each household that was invited to the TE information sessions to redistribute written information and answer any remaining questions.

3. Social Norms - Restrictive social norms present an additional barrier to access for rural women. Crucially, men see transgressing restrictive gender norms as impacting their reputation directly (Jamali, 2009) and therefore may be unwilling to allow women of their household to participate, even if they see its value (Naqvi et al., 2002). Our surveys also reveal that such barriers are likely important with household heads often citing social reasons as one of the factors behind a reluctance to have women in their household apply for the skills training. Evidence from earlier pilots showed that removing this constraint could increase enrollment. We sought to address this constraint by conducting lengthy (75- to 90-minute) information sessions separately for males and females in 81 villages. Respected community members were also invited to attend these sessions. In addition to all of the information communicated in the TE information session, the Community Engagement (CE) information session aimed to engage the community by concluding with a discussion about the social challenges women face to accessing and benefiting from the training as well as a discussion on ways in which the community members could facilitate female members to overcome these access barriers. Trained community mobilizers moderated this conversation but were instructed to make no claims regarding the safety or social acceptability of attending the course. As with the TE information intervention, we conducted an Open Period two to three days after the session. For community members invited to the CE information sessions that did not have a training center in their village, we also provided transporation for one day during the Open Period so that everyone (potential trainees and respected community members) could see that the facilities were indeed appropriate. Finally, we conducted a follow-up visit one or two days later to redistribute written information and answer any additional questions.

4. Reliable Transportation - In the context of rural women, a lack of safe and reliable transportation compounds the physical distance constraint. Male household members often cite this concern as a reason to refuse permission for women to attend training centers in other villages unless they are accompanied by others. Group transport, together with the induction of drivers that are familiar and belong to the same community, was cited as the most preferred solution to mitigate access constraints. We therefore sought to address this constraint by offering free group transportation to the training center, taking great care to ensure that the transportation was seen as safe and reliable by the villagers. To

implement this, we first held a meeting with men of sample households in which they were encouraged to nominate drivers and suggest logistical arrangements of the facility. The proposed arrangements were shortlisted and then confirmed with female household members after eliciting their preferences regarding the provision of group transport. A second meeting was then held with the male members in order to finalize these arrangements. Finally, households were provided with printed information about the final arrangements for group transport facility, including the driver's name, mode of transport, pick-up and drop-off locations, and schedules. This service was only offered in nVBT villages, as the distance needed to travel for households in VBT villages was deemed too short for transportation to be a salient constraint.

5. Monetary - For rural women, participation in the training program may imply additional traveling costs or potential income loss due to the opportunity cost of time allocated to the training program. A lack of adequate monetary incentives to compensate for these opportunity costs can also prevent women from participating in the course; in the previous training program roll-outs, this constraint was the second-most cited reason for course dropout (Cheema et al., 2013). Therefore, this assumed risk (i.e. opportunity cost of participating) must be compensated in order to increase enrollment rates. The program sought to lessen this constraint by offering a monthly stipend to all trainees for attending the training. Specifically, every household in our sample was offered a base stipend of Rs. 1,500, paid monthly by the training service provider. In order to causally identify the effect of providing the stipend, we then randomly allocated a stipend top-up to each voucher holder. These additional stipends were as high as 4,500 PKR, resulting in a final variation in monthly stipend amounts from 1,500 to 6,000 PKR; however, we introduced the stipend amount at both the village and the household level such that stipends did not vary by more than 1,000 PKR across households within a village. Before the balloting occurred, we explicitly informed each household about the procedure of allocation of stipend levels, and there were no reported cases of discontent regarding the difference in stipend values. Due to budgetary concerns, we selected a random set of 10 households in every village to receive no additional top-up.[15] Base and top-up stipends were dispersed 5 times and were only dispersed to household members still enrolled in the program with an attendance rate of 80%.[16]

[15] A potential concern is that those who are allocated a smaller stipend may perceive stipend allocation as unfair and this in turn may adversely impact their enrollment behavior. However, field interviews suggested that households were very open to variation in stipend amounts as long as each household received a minimum stipend and any extra amount was determined through an open random ballot. A review of literature also supports this observation. Blount (1995), through ultimatum bargaining games in studying choice behavior, concludes that participants are more likely to accept unequal distributions when they perceive the allocation process to be fair. Bolton et al. (2005) also state that people are as accepting of fair procedures as they are of fair outcomes. In order to ensure our process was viewed as fair, the stipend variation was publicly randomized in stages. We first randomly selected the 10 households to receive only the base stipend. We then randomized the remaining households in each village into one of 8 "stipend buckets." Each bucket allocated one of three stipend amounts (low, medium, or high), and the the difference between high and low amounts within a bucket (i.e. within a village) was always 1,000 PKR so as to avoid significant inequity in allocation amount within a given village.

[16] Stipend top-ups were paid in five monthly installments through EasyPaisa, a mobile phone bank account through Telenor Bank, through which users receive an SMS with two codes necessary to withdraw money free of charge from an EasyPaisa outlet. Only the individual in whose name the EasyPaisa account is registered can withdraw the money, and it must be withdrawn in person at an outlet. However, the individual registered to withdraw the money was often a male member of the household, rather than the trainee herself; 44% of women during the endline survey reported that their spouse/fiance retrieved the top-up stipend, followed by 25% of women reporting their parent retrieved it. While a male family member often picked up the money, 91% of trainees at endline reported having either a large (54%) or moderate (37%) amount of influence over where the money was spent. Our team assisted in setting up the account, made calls to households to ensure their received their stipend top-up, and offered a call center in the case of any issues. If the household did not have a mobile phone, our team hand-delivered the codes so they can withdraw the money. If the household did not have a valid CNIC (government-issued identification number), the additional stipend was hand-delivered in cash.

# 3 Experimental Design, Data and Empirical Strategy

## 3.1 Sample & Experimental Design

Our sampling frame comprised rural areas from the three program districts (Bahawalnagar, Bahawalpur, and Muzaffargarh) in the South of Punjab. These districts are fairly typical of the country, though slightly poorer than the typical district in the Punjab. Our initial sampling unit was the household, who then nominated a female member to take part in the training. Power calculations were performed to determine the sufficient sample size needed to detect differences in take-up between the different treatment branches. These calculations were simulation-based and used estimates of the average take-up and intra-cluster correlation from the data available from earlier pilots. Results indicated that approximately 240 villages and 4,500 households were required to provide at least 80% power at 5% significance level for detecting 0.2-0.3 SD impact on take-up. To account for attrition, we expanded this final sample size to 243 villages and 6,200 households. The villages were randomly selected from all villages in the three districts. Within each village we then randomly selected 25 households to receive a training voucher and participate in survey activities.

We collected basic statistics on both the household and nominated individual, which are summarized in Table 1. We see that the average household in our sample has a monthly income of Rs 11,000 and has between six and seven members. Roughly half of the households are ethnically Punjabi, while the other half are primarily Sariaki (the remaining 3% belong to other minority ethnicities). As for the trainees themselves, we see that 70% are married and only 34% have any formal education. Additionally, 32% are involved in paid work, 31% have had any ability to stitch, and only 5% engaged in any form of stitching in the last month. These basic statistics, along with data collected in our previous baseline, show that our course offered an opportunity with high potential value for our sample. Furthermore, we asked respondents a series of questions regarding women's influence over household and business decisions, as these may impact program take-up. We report summary statistics on these in the form of two influence indices here. We will discuss the distance and take-up measures in further detail below.

We randomly assigned each village to one of eight treatment branches based on the constraint alleviation strategies (referred to here as treatments) described above in section 2.2.3. Refer to Table 2 for a complete breakdown of the number of villages and household in each treatment branch. We completed the random allocation in multiple stages. First, we divided the three districts into 27 grids based on geographical proximity, each containing nine sample villages. Second, we randomly selected four villages in each grid to have a training center (VBT) and five to have no training center (nVBT); we refer to these two primary treatment branches as the standard intervention.[17] All households in the standard intervention (i.e. all households in the study) received basic information about the course through a house visit, during which we offered the households the training, provided a booklet about the PSDF courses offered and training information, communicated information regarding the base stipend, and asked them to identify an eligible female member to participate. If the household accepted the training offer, another visit was conducted during which each household received a printed voucher in the name of the chosen training recipient. Stratifying on this primary randomization, we then further randomly assigned the five nVBT villages within each grid to receive either trainee engagement (TE), community engagement (CE), reliable group transport (GT), a combination of CE and GT, or no additional treatment (standard intervention only). Among the four VBT villages per grid,

[17] Appendix Table A1 shows balance tests for the simpler VBT and nVBT comparisons are as expected. Balance tests across the full eight treatment types shows similar balance. We should also note that in practice there were actually 12 villages per-grid, with an additional 3 randomized into being pure control villages. Vouchers were not distributed in these villages, so they will not be used in the take-up analysis found in this paper.

we randomized three into the CE, TE, or standard-intervention-only branch, and the fourth was randomly assigned to either the TE or standard-intervention-only treatment branch. Note that no VBT villages were randomized into the GT treatment, as we deemed a transportation service less relevant/feasible to provide in VBT villages, given within-village travel distance is much smaller.

Next, we randomly assigned the total stipend amount at both the village and the household level. As noted above, in addition to a base stipend of Rs. 1,500 per month, which was awarded to each voucher holder with minimum course attendance of 80%, a randomly selected subset of households received a stipend top-up which ranged from Rs. 500 to Rs. 4,500. We determined this range through analysis of previous pilot data, which indicated that stipends in this range were most cost-effective at increasing take-up. As noted previously, stipend variation was limited to Rs. 1,000 within a village in order to minimize potential perceived fairness concerns. Additionally, households were explicitly informed about random ballot procedure for stipend top-ups during the first house visit via a sealed envelope to ensure perceptions of fairness. There were no reported cases of discontent regarding the difference in stipend values. Table 3 reports the total number of households which received each level of stipend top-up. Note that while stipend amount was allocated randomly, the probability of being assigned each amount varied throughout the range of possible amounts. In particular, budgetary constraints limited additional top-ups to only 60% of the surveyed households.

Finally, we randomly selected a subset of our original households (from among all eight treatment arms) and additionally offered a voucher to a neighboring household. For each sample household selected to receive the additional neighbor treatment, we visited the sample household's address and identified the closest neighboring household that fulfilled the following criteria: it was not an existing sample household, consented to being interviewed, and contained an eligible female household member. We included this treatment to test whether simultaneously inviting neighboring women would decrease the potential resistance by family members concerned about public perceptions of a woman traveling and training alone. Note that while these additional neighboring households were selected to receive vouchers after the original households, all vouchers were delivered at the same time in order to eliminate any effect of timing or revisits on take-up. We randomly selected neighboring houses stratifying on our primary VBT randomization, thus inviting the neighbors of 550 (20%) of VBT households and 550 (16%) of nVBT households.

## 3.2 Data Collection

Our data comes from three sources—household surveys, administrative data, and a distance exercise. Refer to in Appendix 5 for a timeline of surveys (Figure E1) as well as a brief summary of all data collected in each.

Our earliest household data comes from a baseline survey conducted in November 2013, before treatment activities had begun in order to prevent them from influencing responses. At this time, we also asked households to (informally) nominate a female member for training, and additional survey questions were asked to these nominated individuals. Information collected during this original baseline survey consists mainly of demographic information about the household and nominated female member, which we used to ensure balanced treatment assignment and as controls in our main take-up analysis. We also used the survey to identify the individual within the household to whom we should provide all relevant treatment materials and information in subsequent visits. During these subsequent household visits, we conducted surveys both to verify voucher acceptance and to ensure that households had been informed of all treatment activities within their village. Lastly, we conducted an endline survey in December 2015 (five months after the training concluded) to verify the take-up status recorded through the previous surveys and administrative data, collect

information on the impact of the course on the trainee and her household, and better understand the barriers to course attendance.

Throughout the intervention, our team and the training service providers continued collecting extensive administrative data, including voucher submission lists, initial enrollment status, and regular attendance records, in order to accurately form rosters and disburse stipends. Continuously collecting administrative data also allowed us to track each respondent's take-up status independently of their self-reported status.

Finally, following the conclusion of the intervention in August 2015, we conducted a distance mapping exercise in order to accurately measure the route each respondent would take from an informal cluster of houses where her home is located (i.e. co-locational neighborhoods in this context) to the nearest training center. During this exercise, we recorded not only the distance to the training center, but also the anticipated time and cost of travel for multiple modes of transportation. For a complete description of our method of distance mapping, refer to Appendix E.

The following sections further elaborate on each of these data sources and their relation to our variables of interest.

#### 3.2.1 Measuring Take-up & Distance

Our outcome of interest is take-up of the SFM skills training program. As such, take-up was carefully monitored for all respondents through both village-level administrative data and the follow-up survey. In order to break down the take-up decision-making process, we measure take-up in four stages: voucher acceptance, voucher submission, course enrollment, and course completion.

While voucher acceptance is our preliminary measure, our team had already had several interactions with the households before voucher acceptance was measured. We did a first visit with households during which we notified them of the high-quality training and stipend, provided the households with a training center and course booklet, and implemented treatments according to the household's treatment group. After the completion of these activities, our team visited each of our sample households to deliver a voucher. During this visit, we reminded households of the female member they had nominated for the program, confirmed her eligibility, and offered her a printed voucher, in her name, to attend the training. She was notified that due to a limited number of seats, the voucher does not ensure a spot in the course, but it will increase her chance for successful enrollment if she submits it to the training center. Thus we elicited our first measure of take-up, voucher acceptance, when an eligible female identified the location of the training center which she wanted to attend and accepted the offer of provisional course enrollment. We recorded acceptance rates at the time of delivery and later confirmed them through the follow-up survey. Since accepting the voucher only required an expression of interest in the course, not a formal commitment, we consider voucher acceptance the least demanding measure of take-up.

We then asked those who accepted the vouchers to submit them to the training center in which they wanted to enroll within three days. The voucher submission variable thus measures whether respondents actually submitted their vouchers to the training center for enrollment. Each voucher had a unique ID associated with the household, easily identifying the household and individual who submitted the voucher through training service providers' administrative data. We again confirmed all voucher submission with respondents during the follow-up survey. Since submission required individuals to travel to the training center, we view it as a more demanding measure of take-up.

Our enrollment measure required more careful measurement than the previous take-up measurements, as enrollment changed over time. As the training was open to all women in the village, we also received

applications from self-applicants outside of our sample (i.e. women who opted to register themselves for training in the absence of targeted information). Since the amount of submitted vouchers and applications at times exceeded the training center capacity (20 students per center), we conducted a random ballot to ensure a fair and transparent allocation of slots to applicants without compromising the evaluation. Trainees were therefore given a randomized sort order and categorized as either "admitted" (enrolled in the program) or "waitlisted" (trainees who we kept as a backup in case admitted trainees dropped out). Two and a half weeks after the voucher submission deadline, we announced the enrollment status of applicants for training by posting the list of admitted and wait listed applicants at all training centers on the course start date. To ensure all admitted applicants were aware of their admission status and to record their intention to enroll, we visited the homes of all successful applicants in the enrollment verification phase. During this period, the field staff also visited the training center to independently record trainees' attendance. Based on these sources of information as well as attendance data reported by the training service providers, applicants who did not enroll in classes lost their seats, and admission was offered instead to the waitlisted applicants (who were again informed of their new admission status through house visits). Respondents who remained in class past the closure of this admission process were considered to have enrolled in the class. However, an individual was not considered to be enrolled even if she attended some classes but stopped attending before the admission process closed.[18]

Once the class enrollment lists were finalized at the end of enrollment verification phase, PSDF initiated its independent monitoring process, which sent monitoring staff to each training center once per month until the course concluded. This monthly monitoring was logistically necessary to ensure that stipends were only disbursed to those still attending class, but these visits additionally provided detailed information on how long each respondent remained in the program and final completion rates. Consequently, we can easily identify which trainees had satisfactory attendance (80%) through to the course's completion. We finally confirmed each individual's class completion status through the follow-up survey.

Our main independent variable of interest is distance that a respondent would need to cover to attend the training, and we measure it in three ways. Using GPS coordinates, our first method measures the "straight-line distance" from each nVBT village's centroid to the nearest VBT's village centroid. Since it was not feasible to assign training center randomly *within* a village, we set this measure of distance to be zero for VBT villages. In addition, since we can easily imagine scenarios in which this euclidean measure could underestimate the actual distance a trainee would need to travel, we created a second measure of distance that takes into account the actual routes used to travel between villages. To do this we grouped households into geographic clusters and conducted an elaborate distance mapping exercise, in which we physically measured the distance from each cluster to the training center using a motorcycle and an odometer (for extensive details on this surveying procedure, see Appendix E). This measue is referred to as the "cluster-level travel distance". However, as previously noted, since the training center location within the village is not randomly assigned, an endogeneity problem may arise with this measure (for example, if rich households can choose to live closer to the center, they can select into having a lower distance measure). To account for this issue, we created a third measure of distance, "travel distance", by averaging the cluster-level travel distance measure within each village to find the distance from the village's population centroid to the training center. This strategy removes any parts of distance that could be endogenous within the village while still allowing us to construct a non-zero traveled distance measure, even for villages which received a training center.

[18] Enrollment status for individuals who were never had a chance to get off the waitlist is defined to be missing since we cannot assume what their enrollment status would have been had they been given a choice. Since the (waitlist) order was randomized this does not affect our analysis.

Table 1 reports basic descriptive statistics of the variables outlined above. Note that while voucher acceptance rates are reasonably high at 63%, our final take-up rates are quite low, with only 21% of the population completing the course. However, this average masks substantial variation across villages, a point that we will explore in more detail below. Given the importance of distance, it is interesting to note that while average distances to a training center are not that large (around 3 km), there is still sufficient variation to estimate distance effects on take-up rates. Moreover, not surprisingly, traveled (measured) distance is larger than straight-line distance by almost a factor of 2.

### 3.3 Empirical Strategy

We are interested in measuring the differences in take-up rates between our treatment branches. Since treatment status was assigned randomly, we can interpret any such difference as the causal impact of the treatment. We begin by estimating the effect of our primary treatment, village-based training (VBT), with the equation:

$$Y_i = \alpha + \beta_1 VBT_i + \rho X_i + \epsilon_i \tag{1}$$

where $Y_i$ is an indicator for one of our four measures of take-up for individual $i$; $VBT_i$ is an indicator for individual $i$ living in a village assigned to the VBT treatment branch; $X$ is a matrix of individual controls, both demographic and other characteristics; and $\epsilon_i$ is a random error term. In order to account for any intra-cluster correlation and for the correlation we mechanically create through our stipend treatment design, we cluster this error at the village level. The coefficient $\beta_1$ gives the average treatment effect of placing the training center inside the village—what we call the "boundary effect." Since $VBT_i$ is randomly assigned, we do not require $X_i$ for an unbiased estimate of $\beta_1$, but adding controls can help provide tighter standard errors. We present results from specifications with and without $X_i$.

While the above specification cleanly identifies the effect of locating a training center in the village, we can further decompose this effect into two components—an indicator for leaving the village itself (i.e. crossing the village boundary) and a continuous variable for the actual per-km distance traveled—by estimating the equation:

$$Y_i = \alpha + \beta_1 VBT_i + \beta_2 Dist_i + \beta_3 AveDist_i + \rho X_i + \epsilon_i \tag{2}$$

where $Dist_i$ is a measure of (one of the three measures of) distance to the (closest) training center, and $\beta_2$ is the per-km traveled costs incurred by moving the training center further from a respondent's house. Recall that since the training center location was randomly assigned, the distance to the nearest training center ($Dist_i$) is also exogenous as long as we condition on the average distance between a village and all other villages in our sample ($AveDist_i$).[19] We run several (polynomial) variations of this specification, including

[19] To see why the $AveDist_i$ control is needed, consider an example of three villages being jointly randomized (one to VBT, two to nVBT). Imagine that two are within 1 km of each other, but the third is located 10 km from the others. It is clear that while each has an equal probability of being assigned to the VBT treatment, the respondents in the villages within 1km of each other have a higher probability of having the training center being within 1 km of their home. Moreover, to the extent that the farther away village varies on other characteristics (e.g. income, industry, etc.) that can impact course applications and enrollment, this can introduce a bias into our estimates if not controlled for. This is precisely what the $AveDist_i$ control accomplishes. In our example, it will assign a higher $AveDist_i$ value for the village that is further from the other two so that the distance term of interest ($Dist_i$) will only reflect the random component of the distance variation induced by our assignment. While we can compute $AveDist_i$ for different radii, we consider only the average distance of the village to all sample villages within 15 km (a reasonable radius beyond which travel is likely not feasible). That said, we checked robustness of our results by using average distance to all villages within 5 km, 10 km, 20 km as well as averaging the distance to all sample village within

non–parametric ones, to ensure that we properly account for the role of distance. In these specifications, we always control for $AveDist_i$ using the same functional form as used for $Dist_i$.

After establishing the effect of the VBT treatment and distance on take-up, it is useful to estimate the size of that effect in terms of economic value. Our variation of stipend at the household level allows us to both estimate the impact of money on take-up and compare it to the impact of VBT to determine economic magnitude. We first estimate the equation:

$$Y_i = \alpha + \beta_1 VBT_i + \beta_2 Dist_i + \beta_3 AveDist_i + \beta_4 Stipend + \rho X_i + \epsilon_i \tag{3}$$

We can now determine the stipend amount needed to create the same impact on take-up as the VBT treatment by calculating $\frac{\beta_1}{\beta_4}$ and the "marginal rate of substitution" between distance and stipend with $\frac{\beta_2}{\beta_4}$.

We can look beyond the impact of VBT and extend our analysis to the effects of our other treatment arms by including an additional indicator for each in our main specification. The equation used is:

$$Y_i = \alpha + \beta_1 VBT_i + \beta_2 Info_i + \beta_3 Comm_i + \beta_4 GT_i + \beta_5 Dist_i + \beta_6 Dist_i^2 + \beta_7 AveDist_i + \beta_7 AveDist_i^2 + \rho X_i + \epsilon_i \tag{4}$$

where $VBT_i$, $Dist_i$, and $AveDist_i$ are the same as they appear in equation 2, $Info_i$ is an indicator for the trainee engagement treatment, $Comm_i$ is an indicator for the community engagement treatment branch; and $GT_i$ is an indicator for the group transport treatment. It is worth mentioning that $\alpha$ in this specification now represents the mean take-up in the nVBT baseline intervention villages (refer to Table 2) so that each $\beta$ on a treatment indicator represents the difference in take-up between those villages and the village who received that treatment, controlling for distance.

Finally, we can use heterogeneous effects of the VBT treatment between different sample sub-populations to gain a better understanding of the nature of the effect. To do this, we use the equation:

$$Y_i = \alpha + \beta_1 VBT_i + \beta_2 Hetero_i + \beta_3 (VBT \times Hetero_i) + \beta_4 Dist_i + \beta_5 AveDist_i + \rho X_i + \epsilon_i \tag{5}$$

where $Hetero_i$ is an indicator variable for individual $i$ belonging to the sub-population being considered. Though $Hetero_i$ is imperfect due to non-random assignment, we can interpret the results of this specification if certain demographic characteristics act as a "substitute" for the VBT treatment. Specifically, if $\beta_2$ has the same sign as $\beta_1$ and $\beta_3$ the opposite sign, it is likely that the factor in consideration plays a part in explaining the distance penalty . We will discuss this interpretation further when we present the results below.

# 4 Results

## 4.1 The Importance of Distance & the Boundary Effect

We start by establishing the critical role that distance plays in influencing women's decision to take up skills enhancement opportunities. In doing so, we take advantage of our experimental design, which induces exogenous variation in both the assignment of the in-village training centers and (conditionally) the distance to the nearest training center for villages that did not receive an in-village training center.

the village's randomization grid. None of these alternative controls affected our main results which is not surprising given that these controls themselves are rarely significant.

Table 4, Panel A first examines the impact on take-up rates when a training center is set up in a village. We find large positive effects along all our four measures of take-up, including two intent measures (voucher acceptance and voucher submission), course enrollment, and eventual course completion. The odd number columns present our basic specification, and the even number columns add a host of additional controls. As the measures of take-up grow more demanding, we find increasingly substantial impacts in terms of both the absolute magnitude of the effect and it's relative size increase. For the first stage of take-up, voucher acceptance (i.e. an individual expresses intent to take a course), women in VBT villages show a 22 percentage points higher take-up than nVBT villages (column 1), which reflects a nearly 50 percent increase compared to nVBT reference villages that received the standard intervention including the training voucher and information on the courses. Women in VBT villages have 32 percentage points higher voucher submission rates (more than double the control mean), 34 percentage points higher course enrollment rates , and 27 percentage points higher course completion rates (these effects represent a two to four-fold greater effect relative to the control group). As the mean travel distance of a training center for nVBT women is 9.6 km (6 miles), our results emphasize how severely travel can impact female access to training opportunities, even for relatively short distances.

While take-up differences between VBT and nVBT villages are striking, they do not explain why such severe distance penalties exist. For example, it is possible that substantially large economic costs of travel could explain these magnitudes. While we will return to this possibility in Section 4.2, Table 4, Panels B and C shed further light on this, unpacking the distance penalties by examining it's functional form.[20] Recall from Section 3.3 that since the location of a village training center is randomized, we can include distance controls in the basic specification in Panel A. Accounting for distance (kilometers) traveled allows us to separately identify the continuous per-km traveled costs and any "boundary effect" (a penalty paid simply due to leaving one's village for the training). Interestingly, such boundary effects, unlike per-km costs, cannot be readily explained by the economic costs of travel, since there are no economic "tariffs" charged for crossing village boundaries (unlike in the cross-country trade costs literature). Panels B and C first look at GPS distance (i.e. the straight-line distance) between the closest training center to the nVBT village's geographical centroid (remember that by definition, this distance measure is zero for VBT villages).[21] Panel B introduces a linear control for distance, while Panel C adds a quadratic term to allow for non-linearities in the per-km traveled costs. Both panels demonstrate that the distance penalties increases with travel distance; for example, Panel B shows that class completion rates drop by 1.4 percentage points for each km traveled. However, after accounting for distance, the village boundary effect persists, ranging from 10-22 percentage points for different take-up measures (a slightly smaller effect than Panel A's specification without distance). Therefore, in addition to the economic costs of traveling captured through the per-km measure, we observe a persistent additional effect of (crossing) the village boundary. Figure 1 presents both the intercept shift in take-up resulting from the village boundary as well as the gradual effect of distance on take-up for nVBT women. Note that the non-parametric fit in the graph suggests that the boundary effect is likely to remain robust to functional forms of the distance term (more on this below). Panels B and C thus offer the first striking evidence that the distance penalty observed in Panel A contains other non-economic or non-temporal costs related to travel, as simply crossing a village boundary does not impose any such discrete costs.

While we will explore the nature of these non-economic costs in subsequent sections, we will first address

[20] Note that all regressions which include distance also include our control for remoteness, though they are suppressed in the all tables.

[21] We can also look at distance to closest two or three training centers but doing so does not change our results and shows that it is the closest distance that matters. Therefore, we will stick with that for the remaining analysis.

the possible overestimation of both the intercept term as well as the per-km traveled costs due to using the straight-line measure of distance, which is, by definition, a weak lower bound to true travel distance. Aware of this concern during data collection, we additionally measured real travel distance, a fairly demanding step, as surveyors physically traveled and measured the routes that a villager would most likely take. We have detailed our process in Section 3.2.1 earlier and in Appendix E. Note that unlike the straight-line distance measure, this travel distance measure is not defined to be zero for VBT villages. Since VBT villages can be fairly large (and have multiple distinct clusters of houses to travel between), the travel distance measure will take on positive values in these cases, thus capturing the average distance a typical household had to travel *inside* their village to get to the training center. Controlling for distance with this added precision implies that the VBT coefficient captures more truly the (additional) effect of crossing the village boundary (and not just the zero distance intercept term) on take-up rates. Table 4, Panels D and E present our results using this more refined value of the average distance traveled by households to the nearest training center, either inside or outside their village. We find somewhat smaller per-km costs than in the straight-line distance case (i.e. the coefficient on the linear distance term in Panel D is somewhat smaller than that in Panel B), which we expected since the travel distance measure is on average 1.5 times the straight-line distance measure. However, the boundary effect remains quite large, ranging between 12 to 22 percentage points in Panel D. Interestingly, in contrast to the straight-line distance measure, the travel distance measure captures a slight degree of non-linearity in the take-up-distance relationship (i.e. Panel E shows the quadratic specification fits better than the analogous one in Panel C). Allowing for the quadratic term and actual travel distance does attenuate the boundary effect somewhat, but across all take-up measures, it remains within 11 to 17 percentage points.

Together the results in Table 4 show that the effect of crossing a village boundary is far from negligible. Although this effect is roughly half to one-third (depending on the outcome and specification) of the total VBT effect reported in Panel A, it remains appreciably above a 10 percentage point increase. In Panel E, which we believe is the cleanest measure of the true distance constraint faced by the respondents, we see an average impact 15% across all outcomes and a predicted doubling of course completion relative to control villages.[22]

#### 4.1.1 Robustness

While it is reassuring that both the per-km traveled costs and boundary effect identified hold for the two different distance measures and in both a linear and quadratic specification, we now conduct a series of further robustness tests.

Table 5 first considers the results when we utilize a more demanding functional form specification. Given that the boundary effect was reduced slightly in moving from the linear to quadratic distance controls (Table 4, Panel D compared to Panel E), Panel A of Table 5 allows the preferred traveled distance measure to take polynomial forms up to a 5th order polynomial in distance (controlling for a similar 5th order polynomial in $AveDist_i$). In this exercise, we aim not to interpret the coefficients on each of the distance polynomials, but rather test whether a highly flexible (and perhaps implausibly so) functional form in distance would

[22] Appendix Table B1 reports regressions using an alternative cluster-based, rather than village-based, traveled distance measure. We prefer the latter measure in Table 4 since the training center location was randomized at the village and not cluster level (i.e. we randomly selected which village received a training center but did not specify which exact location in the village received it, as this additional randomization would have raised logistical costs substantially). Therefore, while the *between-village* distance measure is exogenously assigned, the *between-cluster* measure is not guaranteed to be so. Nevertheless, we show that our results (for both linear and quadratic forms) remain essentially unchanged and qualitatively similar even if we use the (potentially endogenous) cluster-based measure.

substantially reduce the boundary effect estimated in Table 4. On the contrary, we find the VBT coefficient largely unchanged, suggesting the high robustness of the boundary effect. Moreover, since the polynomials are not individually significant, we conclude that the underlying relationship between distance and travel is most accurately estimated as a quadratic.

Panel B of Table 5 takes an alternative approach. Rather than forcing a flexible functional form, Panel B relies instead on a non-parametric fit by flexibly controlling for traveled distance bin fixed effects. To do this, we first divide individuals from villages without training centers into decile bins based on their village's average travel distance to the training center. We exclude villages with in-village training when creating the distance thresholds for these bins so that the first bin is not too small. We then use the bin thresholds to categorize all individuals (both those with training centers in their villages and those without in-village training centers) into a given traveled distance bin (we control for analogous $AveDist_i$ bins using the bin cutoffs for the $Dist_i$ measure). This process ensures that an adequate number of individuals from villages both with and without training centers fall into each bin to calculate an impact of the village boundary. As in Table 5 Panel A, this more demanding non-parametric exercise shows similar boundary effects along all four measures of take-up.

Finally, Panel C of Table 5 takes this specification a step further by implementing what is akin to an "Regression Discontinuity" style design. Note that this is not needed for causal inference—distance is exogenously assigned given our intervention design, so we obtain correct causal inference in our basic specification. However, in order to further minimize concerns about the functional forms of distance and its effect on the boundary effect, we compare villages which face similar and relatively small travel distance to the training center, differing only in whether the (equidistant) training center is within the village or not. In order to do this, we restrict the sample in Panel B to only (VBT and nVBT) villages that fall within the first two traveled distance bins (i.e. villages where a training center is located less than 4 km from the population center). We also control for traveled distance within this narrow bin—analogous to an RD design where one also controls parametrically for the running variable and looks for a "jump" at the discontinuity (i.e. the village boundary). Panel C of Table 5 shows that the boundary effects remains robust and is, in fact, even slightly larger. Figure 2 presents the results non-parametrically by plotting the distance means of each village within these bins showing a clear gap in take-up between VBT and nVBT villages with similar travel distance. This final test is perhaps the starkest and most demanding test of the boundary effect and highlights how robust it is.

## 4.2 Are the Boundary Effect and Per-Kilometer Traveled Costs Economically Meaningful?

In the previous section, we established that both distance traveled and crossing the village boundary have sizable impacts on take-up. In addition, our experimental design allows us to quantify these effects in economic terms. Specifically, we can leverage the exogenous variation in the monthly stipend amount to estimate the economic magnitude of the distance and boundary effects. In order to do so, we first estimate how much take-up rates (for our four different measures) are impacted by an increase in stipend amounts. Since the stipend amount is exogenously varied, we can estimate a causal impact of money paid to an individual on her take-up rate. We can then calculate how much extra stipend must be offered to induce a similar take-up rate increase as that implied by the distance and boundary effects.

Panel A of Table 6 first estimates the causal impact of stipend on take-up rates by including (exogenously assigned) monthly stipend amount in our primary empirical specification. These results suggest that a Rs

1,000 (~$10) increase in the monthly stipend raises take-up rates by 3.6, 4.5, 4.2, and 4.3 percentage points respectively for the four increasingly demanding take-up measures.

Panel B then translates the stipend effect into the equivalent monthly stipend amount needed to replicate the full effect of being in a VBT village (i.e. having in-village training). Specifically, women in the average nVBT village would have to be paid an additional Rs 6,000-7,800 per month to achieve the same level of take-up as VBT women (who had training center in their village). This additional monthly stipend totals an additional transfer of Rs 30,000- 39,000 to each individual over the five-month training period. Strikingly, the necessary additional monthly stipend to achieve VBT take-up rates corresponds to 62-84% of monthly household expenditure.

Panel C separates the overall VBT financial equivalent into the boundary effect and the per-km costs (using coefficients from the linear specification in Table 4, Panel D). We find that the additional stipend necessary to induce a woman to simply cross a village boundary is Rs 3,200-4,800 per month. Once past the boundary, she would then require Rs 300-450 per additional km traveled. Since we account for distance in this estimation, the former finding does not represent compensation for economic (travel or time) costs, which are not present simply for crossing a village boundary, but rather our results represent a novel economic measure of the non-economic access barriers faced by women in our context. We can additionally compare the magnitude of the per-km traveled compensation amounts to expected economic costs generated due to travel and time, and we find that the per-km compensation amounts are orders of magnitude larger than estimations based on the economic costs of travel and the opportunity cost of time. Using transport/fuel costs and women's foregone wages (assuming all women work), we estimate an upper bound of Rs 24 per km for these travel and time economic costs, which is one-tenth of the lowest per-km compensation in stipend amounts we find! Therefore, we find substantial non-economic cost factors present in the distance penalties, suggesting that the majority of access costs (at the boundary and per-km traveled) are in fact due to such non-economic factors.

## 4.3 Understanding the Boundary Effect

The previous two sections have demonstrated the effect, size, and economic significance of the distance penalties in terms of both the per-km traveled costs and the boundary effect. Our results also suggest that the boundary effect (and possibly the per-km costs as well) captures a cost other than traditional economic and temporal costs associated with travel. While the following exercise is necessarily more suggestive, we now attempt to elucidate the factors behind the non-economic access costs that women face when traveling beyond their villages.

### 4.3.1 Other Boundaries

While our previous results demonstrate the large, negative effect of crossing a village boundary on take-up rates, the village boundary is potentially just one of several invisible "boundaries" women may have to cross when leaving their household. Our focus on the village boundary is driven both by our prior that this is likely to be significant but also by our ability to cleanly isolate the impact of this boundary through the experimental variation induced in our interventions. While it is more empirically challenging to examine other salient boundaries, settlements within villages present another natural and potentially salient boundary. A village typically has several settlements separated by empty or agricultural land; the median village in our sample has eight settlements. Figure 3, for example, shows a satellite image of a village with settlement

boundaries marked.

Using the same strategy as described in section 4.1, we can estimate the impact of crossing a settlement border (SBT) to reach a training center in addition to the effect of crossing the village border (VBT). Table 7 reports results similar to Table 4 and includes an additional indicator variable for a training center located within the individual's settlement. However, since training centers were not randomly assigned to settlements within villages, these results should be interpreted with some caution.[23] Nevertheless, Panel A shows that there is a SBT effect for all outcomes except voucher acceptance in addition to the VBT effect. Positioning the training center in an average woman's own settlement leads to a 9-12 percentage points higher enrollment rate over and above the 20-30 percentage point increase due to its presence in her village. For example, Column 7 shows that for course completion rates, positioning a training center in a woman's settlement leads to a 33 percentage points higher enrollment (20.5 for the in-village effect and an additional 12.2 for the in-settlement effect) than a woman whose closest training center was outside of both her settlement and village. Panels B and C then include linear and quadratic cluster-based travel distance controls to better isolate the settlement and village boundary effects and the per-km costs. In addition to the village boundary effect, we find suggestive evidence of a settlement boundary effect, which is strongest and most robust for our final measures of take-up—course enrollment and completion.

Using the estimates from Panels A and B, the last panel shows the equivalent economic magnitude of crossing the village and settlement boundaries. For example, Column 8 shows that a household must be paid over 5,400 PKR a month (3,063 for the in-village effect and an additional 2,384 for the in-settlement effect) to simply allow a woman to cross the village and settlement boundary. Therefore, multiple boundaries (other than the village boundary) can greatly impact take-up. If other such boundaries indeed exist as a woman travels from her household, as our results suggest, the overall distance penalty observed could be a function of a series of (non-economic) boundary effects in addition to any economic and temporal costs associated with the travel. We turn to these non-economic factors next.

#### 4.3.2 Addressing Information, Social and Transport Barriers

Our design sought to not only identify the effects of training center location and distance barriers, but also to address other access barriers that may arise due to information, social, and transportation concerns. To this end, in addition to the exogenous variation in distance and stipend, we introduced three distinct interventions: (i) a trainee engagement information session conducted with trainees in each village; (ii) a community engagement exercise whereby community elders and other members were invited to a village level meeting to discuss any course concerns; and (iii) secure and reliable group transportation for women to attend a training outside their villages. While each of these interventions is interesting in its own right, it may also shed light on potential channels at play in generating the per-km traveled costs and boundary effect documented above.

Table 8 presents the impact of each of these treatments on our four take-up measures and allows us to contrast them with the per-km costs and boundary effect observed. Note first that neither the trainee engagement nor the community engagement interventions led to any increase in take-up; on the contrary, the community engagement treatment negatively impacted take-up rates at the first stage only, voucher acceptance. Therefore, it seems unlikely that the distance penalties were generated by factors that can be readily improved by information sharing or engaging with the trainees and their families and community.

[23] For example, if the settlement was chosen based on socioeconomic features then the settlement boundary effect may be partly conflated with such features leading to an under or over estimate of the true boundary effect.

We do not mean to suggest that social norms are not salient; rather, since social norms can be difficult to influence, we conclude that community meetings designed to discuss the appropriateness of taking a course (outside the village) may not have been sufficient to address deeper social concerns. The negative impact of community engagement on voucher acceptance (and not subsequent take-up measures) does, however, suggest that a social norm channel may have been at play: It is likely that the meetings may have lead to discussion about women traveling that highlighted associated social concerns, thus dissuading women (who would have ultimately dropped out) from even accepting the voucher. Therefore, a woman, who would have otherwise accepted the voucher but ultimately drop out when she confronted such norms, would discover the salience of these social concerns at the community meeting itself and anticipating these social concerns, would simply not bother accepting the voucher at all.

Moreover, since both the trainee engagement and community engagement treatments were cross-randomized with village-based training, we can interact them with the VBT dummy. As shown in Table B2, we find no interaction effects for trainee engagement (i.e. it is not effective in either VBT or nVBT villages). However, we do find a significant (positive) interaction effect on the VBT and community engagement interaction, again at the voucher acceptance stage, thus supporting our interpretation that community engagement discourages women from even accepting the voucher. In fact, the negative impact of the community meetings only occurs when the training course was located outside the village, suggesting that the social concerns raised in the meeting related specifically to a woman leaving the village for the training (as opposed to social concerns regarding another aspect of the training). While we find no ultimate impact of community engagement on who enrolls in the course, it does reveal that strong community tensions are present in a woman's training and travel decision very early on in the application process.

In contrast to the sobering results on the trainee engagement and community engagement interventions, we find highly encouraging impact of the group transport (GT) intervention: GT has a large, positive impact on all but the first stage of take-up for women traveling outside of their village for training. Moreover, the GT effect compares quite favorably with the effect of locating the training center inside the village. In the case of course completion, for example, the GT impact is roughly two-thirds the size of the village boundary effect, implying that providing group transport to women can compensate for a sizable fraction of the penalty that women faced when crossing the village boundary. Therefore, whatever (non-economic) factors are at play in women crossing boundaries, they seem to be compensated for by providing safe and reliable group transport.

We next ask whether the group transport effect may be capturing positive peer effects instead of transportation effects (i.e. as women travel to the training together, perhaps such pairing of women encourages them to overcome the [social] access barriers they face). We can test this channel by taking advantage of an additional individual-level randomization in which we also provided a voucher and stipend to the neighbors of a (randomly selected) subset of women. If peer effects are driving the positive GT results, we would expect the neighbor's offer to positively impact an individual's take-up decision. However, we find no such effect (Appendix Table B3), suggesting that peer effects cannot adequately explain the GT effect.In addition to this, we can also take advantage of the fact that while stipend varied at the indivdual level, there was also (random) variation in stipend across villages. Thus we can look at the effect of both the individual and average (village) level stipend. We can see that the individual stipend positively affects takeup. If peer effects were important, one would expect that the average stipend in a village (which effects village level takeup) would have a positive impact on an individual's takeup over and above the effect of the stipend she is paid. However, Appendix Table B4 shows this is not the case, offering further evidence that peer effects

are not salient in affecting takeup.

We can gain a further insights into the GT effect by also examining the heterogeneity in its impact. Appendix Table B2 shows that the interaction between GT and community engagement is positive. Recall that the negative impact of community meetings at the voucher acceptance stage was not present for women whose training center was in their village. Analogously, we see that this negative effect of community engagement on take-up is also mitigated for villages (without a training center) that were offered group transport. Therefore, community engagement only negatively impacted voucher acceptance in villages that received neither a training center (in the village) nor reliable transport, suggesting that these services mitigated whatever objections to training the community members raised. Therefore, providing safe and reliable group transport can partially mitigate the non-economic barriers women face at both the boundary line and as they travel further from their communities, effectively and more cheaply offering many of the benefits of moving a training center into her village.

#### 4.3.3 Heterogeneity of the Boundary Effect

While the previous results—the negative impact of community engagement on voucher acceptance, the ameliorating impact of group transport, and the interactions between the two—present evidence on the importance of a social channel (as opposed to financial or informational), we can shed further light on the underlying social channels by examining how the boundary effect varies by household- and community-level factors. These factors are grouped into the following broad categories that correspond to different aspects that may underlie social concerns around women's mobility: female agency, local safety, household socioeconomic status, village ethnic diversity, and village connectivity as measured by access to infrastructure.[24] Table 9 summarizes the results of this exercise where each row concisely shows the interaction term between the VBT dummy (boundary effect) and the factor of interest. The full regressions are in Appendix C, and their specific locations are mentioned in the last column of Table 9.

**Female Agency & Perceptions** To the extent the boundary effect is not a result of a woman's own concerns(such as concerns about her safety, anxiety around travel, or worry about her own household obligations) but those that are imposed upon her by others, we should find that the boundary effect is smaller for women who have greater agency. This lack of agency likely comes from domestic pressure, which may take a variety of forms including the expectation for women to look after other household members and men's control over women's lives. In general, our findings suggest that the boundary effect is smaller (larger) for women facing less (greater) pressure.

We first turn to measures of the extent to which a woman is "needed" in the household. In rural Pakistan, women are typically the primary caretakers of their families. Consequently, we first examine the level of women's responsibility for looking after their households as a potential channel for heterogeneity, using household size as a proxy. As shown in Row 1 of Panel A, Table 9, the coefficients on the interaction terms between VBT and household size are positive and significant for all four take-up outcomes. That is to say, the larger a woman's household, the larger a boundary effect she faces. [25]

[24] To avoid data mining, we filed a pre-analysis plan and specified all the potential channels through which the boundary effect is likely to vary. Our subsequent analysis closely followed the plan and examined all the relevant channels within each category. Furthermore, for each channel, we code the relevant variables in different ways to test the robustness of our findings.

[25] To check the robustness of our finding, we explore several alternative measures of household responsibility, such as the number of children, the number of elderly, the number of adults aged between 18 and 60, and the presence of sick household members. Based on these checks, it is the number of children and adults that drives the heterogeneity around household responsibility. We do not find any variation of the VBT effect by the number of elderly members or the presence of sick people

In addition to the presence of dependents, needs may also arise if a member is frequently absent. In our baseline surveys, we asked whether a household has at least one (i) male or (ii) female member who was away for three months or more. Rows 2 and 3 in Panel A show that while male absence does not affect the boundary effect, female absence does. Simply having a woman who is absent from home makes it far less likely that a potential female trainee can travel outside her village. We should caution though that such events are unlikely and this result is driven by a very small number of women who have reportedly been gone for an extended period.

Second, we now consider measures that directly capture how much influence a woman has in decision-making within her household. Since we have several questions that get at this in our baseline surveys, for presentational tractabilty we create two separate indices to measure (i) a woman's influence over household matters, such as whether she needs permission to become invovled in new activities,[26] and (ii) over business decisions, such as her confidence in running a business.[27] Rows 4 and 5 in Panel A, Table 9, show that the boundary effect varies by the former and not by the latter. Specifically, the interaction terms between VBT and the domestic influence index are negative and significant for three out of four take-up outcomes, suggesting that women with more leverage vis-à-vis men in the domestic sphere are more able to cross the village boundary. In Appendix Tables C8 to C12, we show that this effect holds for four out of the five sub-components of the index, particularly for course enrollment and completion. In contrast, as Row 5 in Panel A, Table 9 shows, while women's business influence index does increase program take-up directly, an indication that business influence is indeed capturing a real factor that affect women's take-up decisions, the boundary effect does not vary by this index. We find further evidence of our finding in Appendix Tables C14 to C21, where the coefficients on the interaction terms between VBT and the business influence index's components are rarelysignificant while the components mostly contribute directly to program take-up.

Related to the question of influence, we also explore whether the boundary effect varies by women's marital status, which we code as a binary variable. Row 6 of Panel A, Table 9 indicates that for all four take-up outcomes, the coefficients on the interaction terms are positive and significant, which implies that married women face fewer constraints related to crossing boundaries. This may partly reflect the fact that (in our data) married women show significantly higher levels of domestic influence (as measured by the index).

Finally, we turn to a series of belief gender-related questions that we broadly group under a sub-category called "perception". These measures further elucidate how women and men's thinking affects women's ability to travel. We first construct two separate indices based on men's and women's responses to the same set of four gender equality questions.[28] Rows 7 and 8 in Panel A, Table 9 reveal offer some evdience that the more strongly men believe in their superiority to women, the more of a boundary effect is experienced by women. [29] In contrast, women's attitude on gender equality does not seem to matter, which suggests that it is really men's opinion (and likely control) that affects women's mibility in his context. This asymmetry

(see Appendix Tables C1, C2, C3, C4 and C5). This suggests that it is the more regular hosuehold needs that may be restricting womens' travel.

[26]Besides women's ability to take part in new activities without family permission, our domestic influence index is also based on four other variables measuring whether women could influence their husband to begin new activities, keep daughter(s) in school, increase spending on children's clothing, and purchase a new sewing machine despite initial disagreement.

[27]The business influence index is built upon eight variables: Do you think you are able to run your own business? Obain credit? Make sure that your employees get work done properly? Manage financial accounts? Bargain to obtain cheap input prices? Collect debt? If your husband is going to buy land and you think it is not the right time to do it, can you influence him to do it later? If your HH is going to borrow from a source that you think is not the right source, can you influence them to change their decision?

[28]Separately to male and female respondents, we asked how men compare to women intellectually, spiritually, morally and in terms of management of daily affairs.

[29]The result is significant for only one of the four components and marginally significant (p-values of 15 & 13%) for two others (though for these two it is significant in the specifications without controls in Appendix Table C22.

of whose beliefs matter continues in other relevant measures. Surprisingly, a woman's willingness to travel, as proxied by whether the female respondent desires or has attempted to find a job in a different location, has little impact on the boundary effect she may experience (Row 9 of Panel A). However, the interaction terms between the VBT dummy and male openness to traveling for work are positive and significant for all four take-up outcomes (Row 10 of Panel A), implying that crossing the boundary is harder for women whose male household members express an interest in working elsewhere. The latter is line with our hypothesis of domestic pressure, and both results likely imply a household preference for women to stay, thereby raising the costs for women to cross boundaries. Notably, in these regressions male openness to traveling for work is not correlated with women's overall take-up rates (Appendix Table C26), but women's attitude has a signficant, positive relationship with their own take-up (Appendix Table C27). The fact that these measures have such direct impacts on take-up (and the opposite interacted effects with VBT) shows that while our measures of male and female attitude on traveling are valid, men's attitude might take precedence over women's preferences in terms of a woman's ability to travel. This is perhaps even more starkly illustrated when we consider a woman's own desire to enroll.

We next examine whether the boundary effect varies by women's demand for the training. Women's demand is measured as a binary variable, which takes the value of 1 if a female respondent said she was very likely to receive training during our baseline survey. The last row in Panel A of Table 9 reveals that for three out of our four take-up measures, the coefficients on the interaction terms are positive and significant, indicating that women with a stronger demand for training in fact face a larger boundary effect. This result also holds when we use an alternative indicator of a woman's demand which equals 1 if she expects the skills taught by the training to improve her income or social status "a lot." This result is a sobering note and provides additional evidence to support lack of agency (to resist social & household pressures) as a source of access constraints, because women's desire for skills seems to be trumped by other considerations that are rooted in the domestic sphere, such as the expectation to look after family members or male dominance over female activities.

**Perception of Safety** In addition to domestic pressure, another hypothesize is that the boundary effect varies by the perception of safety–in an insecure region, village-based training may go a long way in promoting take-up. In fact such concerns of safety are often given as reasons for potentially paternalistic constraints women often face. However, our analysis does not strongly support the safety hypothesis. For each gender, we construct a dummy indicator of danger that takes the value of 1 if the respondent said he or she did not feel safe in the village during the baseline survey. As Panel B in Table 9 shows, whether we interact the VBT dummy with the male or female measure of danger, all but one coefficients on the interaction terms are insignificant. Moreover, the expanded regression results presented in the Appendix Table C30 show that despite the lack of differential VBT effects, women who felt unsafe are less likely to pursue the training. This result signals that although our measure of safety/danger is valid, safety concerns might not explain the boundary effects we obtain. To verify our finding, we explore several other measures of safety including respondents' trust in the local police, the local court system, the vibrancy of the rule of law; their observation of the trends of local crime rates; their extended households' experience with criminal activities; as well as their perception of safety in the villages hosting the nearest training facilities. In addition, we also examine whether it is men's observation of village safety instead of women's that affects women's program take-up (Appendix Table C29). The effect of VBT does not vary with any of these safety measures, thereby failing to provide strong evidence in favor of the safety hypothesis.

**Socioeconomic Status** We next exmaine whether a household's socioeconomic status may mediate the boundary effect. Conventional wisdom may suggest that poorer people face a larger boundary effect because they may find traveling less affordable. However, our analysis suggests the opposite. In the regressions within Panel C, Table 9, we first use three measures of household economic status: standardized log monthly income and two dummy indicators that take the value of 1 if a respondent's monthly household income and asset index score fall into her village's bottom 30 percent, respectively. The coefficients on the interaction terms suggest that for all four take-up outcomes, women from richer housholds are subject to a larger boundary effect. This is likely because the poor are not only compelled to participate in economic activities under financial pressure but (perhaps as a result) they are often thought not to have as much "social status" to begin with. While the poor actively pursue economic opportunities in violation of some pre-set boundaries, the rich might choose to respect these boundaries to uphold their "social dignity." This is borne out in qualitative work as well as quantitative work. Jacoby and Mansuri (2011) find it is only high-caste girls who face lower primary enrollment when enrollment requires crossing a settlement boundary within their village. Other studies show that not only do rural Pakistani women face tension between employment and familial obligations, but men take the seclusion of women from other men and from the need to enter public space for work or routine chores as a symbol of control and social prestige (Khattak et al. (2010); Mumtaz et al. (2003); Khan (2007); Saigol (2011)). Further analysis leads to similar findings with regards to women's education level. In the last regression in Panel C, Table 9, we interact the VBT treatment dummy with a binary indicator of whether the female respondent has never been to school. For two out of four take-up outcomes, the coefficients on the interaction terms are negative and significant, suggesting that the VBT treatment has a smaller impact on *uneducated* women, perhaps because they, like women from poorer households, have less social status to lose when traveling outside their village. We conduct a number of robustness checks using a variety of measures to determine one's income, asset and education level. The results consistently reveal a larger boundary effect for the rich and the more educated.

**Ethnic Diversity** We next examine whether the boundary effect varies by how ethnically diverse or homogenous one's environment is. First, residents of a diverse village may face a smaller boundary effect because different ethnic groups have been mixed to such an extent that boundaries have become blurred. Second, if the ethnic composition of the sending and receiving villages is similar, then people may feel less constrained in crossing boundaries because the change in social environments between villages is neglegible. Panel D of Table 9 tests these intuitions and examines heterogeneity by village-level ethnic homogeneity. In line with our first intuition, we construct an ethnic fragmentation index, $1 - \sum_{i=1}^{n} p_i^2$, where $p_i$ is the proportion of people who belong to group $i$ and $n$ is the number of groups in a village. Our results (Row 1 in Panel D of Table 9) shows that for three out of the four take-up outcomes, the coefficents on the interaction terms between VBT and the fragmentation measure are positive and significant, which suggests that women living in ethnically diverse villages face a smaller boundary effect.[30] In other words, ethnic diversity makes it easier for a woman to travel outside her village. However, there is a question whether this is just a proxy of ethnic overlap between sending and receving villages. Consequently, we create an inter-village ethnic difference index by calculating the difference in proportion of each ethnic group between the sending and receiving villages, taking the absolute values and adding the numbers up. A larger score therefore implies a larger difference in the ethnic structure between the two villages. The results (Row 2 of Panel D) in fact show that such "ethnic overlap" considerations are not important in generating the boundary

[30] The expanded results of our ethnic diversity analysis is presented in Appendix Tables C35 and C36.

effect (the interactions terms are not significant), suggestting that the etthnic diversity effect is driven more by something that is special about the sending community in terms of its accpetance of women's traveling outside, regardless of their destinations.[31]

**Village Connectivity** Finally, we also exmined whether the boundary effect varies by how connected a village is to the outside world. Services and facilities such as bus lines, railway stops and post offices transfer information in and out. Consequently, residents of more connected villages may have a better understanding of the economic and social consequences associated with crossing boundaries. The results in Panel D of Table 9 test whether the boundary effect varies by the availability of transport and non-transport facilities such as banks, hospitals and post offices.[32] The coefficients on the interaction terms between VBT and the two dummy indicators that measure the presence of these facilities provide little evidence of heterogeneity by village connectivity. This result suggests that economic connectiviy has little bearing on a woman's ability to travel outside her community, further bolstering our explanation that distance effects faced by women are not about economic factors but reflect social constraints.

In summary, our fairly exhaustive examination of the boundary effect lends a lot more support to our hypothesis that distance constraints, especially the boundary effect, are driven primarily by perceived societal concerns that may generate a loss of "social status" for the household when a female household member leaves the village (even temporarily). Our study finds evidence for this argument because women who enjoy more agency, have less social status to begin with (as proxied by their socio-economic status), and come from ethnically diverse communities, seem better able to overcome such societal constraints.

# 5 Conclusion

Our paper highlights the importance of access constraints that women face in emerging economies, especially constraints generated when they travel outside of their communities. While we examine these constraints in the context of a skills acquisition program, the same factors are likely present for women in general whenever travel is involved. We find that these barriers are large and not readily reconcilable with economic factors and the opportunity cost of time associated with travel. On the contrary, we document a stark "boundary effect," whereby training take-up for women falls substantially as they simply cross a (virtual) village boundary. As women continue past the boundary, they experience per-km traveled costs an order of magnitude greater than standard economic costs would imply. Our results suggest that these large non-economic costs are likely generated by conservative norms that women face when leaving their own community. However, we find that these access barriers are less salient for women from more diverse communities, those who have greater agency and face less social constraints, and can also be substantially compensated for by providing safe and reliable group transport for women.

These results have important welfare and distributional consequences on (rural) women and their households. In ongoing work, we show that the skills training studied in this paper have large and persistent economic and non-economic impacts on the trainee and her household, and in fact trained women also help

[31] As a robustness check we also interacted the VBT treatment dummy with a village-level ethnic polarization index, a dummy indicator of whether a village displays any diversity, as well as indicators of a woman's minority and majority status in both her home and destination villages. Our results consistently reveal heterogeneity by village-level ethnic diversity but not by differences in ethnic composition between villages. Therefore our results suggest that, perhaps as a result of intermingling and constant exposure to other groups, residents of ethnically diverse communities are likely to find boundaries less relevant and crossing them less costly.

[32] The expanded results of our examination of village connectivity are presented in Appendix Tables C37 and C38.

others in their community enhance their skills (Cheema et al., 2018b). In addition, in a companion paper, we also find that it is the more marginalized women (those who are less educated, poorer, and more constrained) who get excluded from participating in such programs (Cheema et al., 2018a). Thus addressing the take-up issue not only will help raise individual and collective welfare substantially but will also do so in a progressive way by helping the marginalized even more. Moreover, this suggests that the same access constraints that women face in skill acquisition likely will constrain them in fully deploying their skills. As a result, in an ongoing project, we are examining how linking trained women to outside markets can help leverage their skills further. Initial results show that these linkages are indeed quite effective.

More broadly, our paper also shows that while one may not be able to readily change norms in the short-run, there may be considerable room to work within these norms. While our efforts to work with the community to directly address their concerns regarding female mobility had limited impact, providing a secure way for women to nevertheless travel outside their village while not ostensibly challenging the norms did work. Not surprisingly, there is increasing support in developing countries for providing safe and reliable women-only transport services (Vyborny and Field, 2019). As conservative norms may take time to directly address, our results thus support working within these norms by allaying the concerns behind them may offer a promising and constructive initial direction toward improving women's access to services. What remains to be seen is whether doing so ultimately changes these norms in the longer run and perhaps is even a more effective, albeit slower, way of doing so. We hope to shed further light on this in subsequent work.

## References


Ashraf, N., Karlan, D., and Yin, W. (2006). Deposit collectors. *Advances in Economic Analysis & Policy*, 5(2).

Attanasio, O., Kugler, A., and Meghir, C. (2008). Training disadvantaged youth in latin america: evidence from a randomized trial. Technical report, National Bureau of Economic Research.

Attanasio, O., Kugler, D. D., and Meghir, C. (2009). Subsidizing vocational training for disadvantaged youth in developing countries: evidence from a randomized trial.

Babinard, J. and Scott, K. (2011). What do existing household surveys tell us about gender and transportation in developing countries? *Womens Issues in Transportation*, 2:213–224.

Bandiera, O., Buehren, N., Burgess, R., Goldstein, M., Gulesci, S., Rasul, I., and Sulaiman, M. (2012). Empowering adolescent girls: evidence from a randomized control trial in uganda. *Washington, DC: World Bank*.

Banerjee, A., Hanna, R., Kyle, J., Olken, B. A., and Sumarto, S. (2015). Tangible information and citizen empowerment: Identification cards and food subsidy programs in indonesia. *Massachusetts Institute of Technology*, 39.

Berthelon, Matias Freund, C. (2004). *On the Conservation of Distance in International Trade*. The World Bank.

Betcherman, G., Dar, A., and Olivas, K. (2004). *Impacts of active labor market programs: New evidence from evaluations with particular attention to developing and transition countries*. Social Protection, World Bank.

Burde, D. and Linden, L. L. (2013). Bringing education to afghan girls: A randomized controlled trial of village-based schools. *American Economic Journal: Applied Economics*, 5(3):27–40.

Card, D., Ibarrarán, P., Regalia, F., Rosas-Shady, D., and Soares, Y. (2011). The labor market impacts of youth training in the dominican republic. *Journal of Labor Economics*, 29(2):267–300.

Cheema, A., Khwaja, A., Naseer, M. F., and Shapiro, J. (2012a). Household and community surveys: Baseline household report for skills.

Cheema, A., Khwaja, A., Naseer, M. F., and Shapiro, J. (2012b). Skills intervention report: Results of first round of voucher disbursement and strategies for improving uptake.

Cheema, A., Khwaja, A., Naseer, M. F., and Shapiro, J. (2018a). Addressing selection: Experimental evidence from design variations in a skills training program.

Cheema, A., Khwaja, A., Naseer, M. F., and Shapiro, J. (2018b). Final impact evaluation report: Skills for market (sfm 2013-14) - market linkage (ml 2015-16).

Cheema, A., Khwaja, A., Naseer, M. F., Shapiro, J., Siddiqui, S., and Sheikh, S. (2013). Skills intervention report: Results from sfm 2013-13 phased evaluation.

Ekirapa-Kiracho, E., Waiswa, P., Rahman, M. H., Makumbi, F., Kiwanuka, N., Okui, O., Rutebemberwa, E., Bua, J., Mutebi, A., Nalwadda, G., et al. (2011). Increasing access to institutional deliveries using demand and supply side incentives: early results from a quasi-experimental study. *BMC international health and human rights*, 11(1):S11.

Engel, C. and Rogers, J. (1996). How wide is the border. *The American Economic Review*, 86(5):1112–1125.

Evans, C. and Harrigan, J. (2005). Distance, time, and specialization: Lean retailing in general equilibrium. *The American Economic Review*, 95(1):292–313.

Gage, A. J. and Guirlène Calixte, M. (2006). Effects of the physical accessibility of maternal health services on their use in rural haiti. *Population studies*, 60(3):271–288.

Gallego, N. and Llano, C. (2014). The border effect and the nonlinear relationship between trade and distance. *Review of International Economics*, 22(5):1016–1048.

Gupta, S. (2017). Perils of the paperwork: The impact of information and application assistance on welfare program take-up in india.

Hicks, J. H., Kremer, M., Mbiti, I., and Miguel, E. (2011). Vocational education voucher delivery and labor market returns: A randomized evaluation among kenyan youth. *Report for Spanish Impact Evaluation Fund (SIEF) Phase II. Internet: www. siteresources. worldbank. org/.../VocEd_SIEF_Report_2011-04-07_final*.

Jacoby, H. G. and Mansuri, G. (2011). Crossing boundaries: gender, caste and schooling in rural pakistan.

Jamali, D. (2009). Constraints and opportunities facing women entrepreneurs in developing countries: A relational perspective. *Gender in Management: An International Journal*, 24(4):232–251.

Jayachandran, S. (2015). The roots of gender inequality in developing countries. *economics*, 7(1):63–88.

Jensen, R. (2010). The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics*, 125(2):515–548.

Jeuland, M., Lucas, M., Clemens, J., and Whittington, D. (2010). Estimating the private benefits of vaccination against cholera in beira, mozambique: A travel cost approach. *Journal of Development Economics*, 91(2):310–322.

Keller, W. (2002). Geographic localization of international technology diffusion. *The American Economic Review*, 92(1):120–142.

Khan, A. (2007). Women and paid work in pakistan.

Khattak, S. G., Brohi, N., and Wajiha, A. (2010). Women's land rights: Research findings from pakistan.

Kim, D., Lauria, D. T., and Whittington, D. (2014). Selecting optimal prices and outpost locations for rural vaccination campaigns. *International Regional Science Review*, 37(4):436–458.

Kremer, M., Leino, J., Miguel, E., and Zwane, A. P. (2011). Spring cleaning: Rural water impacts, valuation, and property rights institutions. *The Quarterly Journal of Economics*, 126(1):145–205.

Maitra, P. and Mani, S. (2017). Learning and earning: Evidence from a randomized evaluation in india. *Labour Economics*, 45:116–130.

Mitra-Sarkar, S. and Partheeban, P. (2011). Abandon all hope, ye who enter here: Understanding the problem of eve teasing in chennai, india. In *Transportation Research Board Conference Proceedings*, volume 2.

Mukherjee, M. (2012). Do better roads increase school enrollment? evidence from a unique road policy in india.

Müller, I., Smith, T., Mellor, S., Rare, L., and Genton, B. (1998). The effect of distance from home on attendance at a small rural health centre in papua new guinea. *International journal of epidemiology*, 27(5):878–884.

Mumtaz, Z., Salway, S., Waseem, M., and Umer, N. (2003). Gender-based barriers to primary health care provision in pakistan: the experience of female providers. *Health Policy and Planning*, 18(3):261–269.

Naqvi, Z. F., Shahnaz, L., and Arif, G. M. (2002). How do women decide to work in pakistan?[with comments]. *The Pakistan Development Review*, pages 495–513.

Nguyen, T. (2008). Information, role models and perceived returns to education: Experimental evidence from madagascar. *Unpublished manuscript*, 6.

Ostermann, J., Reddy, E. A., Shorter, M. M., Muiruri, C., Mtalo, A., Itemba, D. K., Njau, B., Bartlett, J. A., Crump, J. A., and Thielman, N. M. (2011). Who tests, who doesn't, and why? uptake of mobile hiv counseling and testing in the kilimanjaro region of tanzania. *PLoS One*, 6(1):e16488.

Porter, G., Hampshire, K., Abane, A., Tanle, A., Munthali, A., Robson, E., Mashiri, M., Maponya, G., and Dube, S. (2011). Young peoples transport and mobility in sub-saharan africa: the gendered journey to school. *Documents danalisi geografica.*, 57(1):61–79.

Riverson, J., Kunieda, M., Roberts, P., Lewi, N., and Walker, W. M. (2006). The challenges in addressing gender dimensions of transport in developing countries: Lessons from world bank's projects. *unpublished, World Bank (June 2006)*.

Saigol, R. (2011). Women's empowerment in pakistan: A scoping study.

Schochet, P. Z., Burghardt, J., and McConnell, S. (2008). Does job corps work? impact findings from the national job corps study. *The American economic review*, 98(5):1864–1886.

Starkey, P. and Hine, J. (2014). Poverty and sustainable transport: How transport affects poor people with policy implications for poverty reduction. a literature review.

Tanser, F., Gijsbertsen, B., and Herbst, K. (2006). Modelling and understanding primary health care accessibility and utilization in rural south africa: an exploration using a geographical information system. *Social science & medicine*, 63(3):691–705.

Thakuriah, P. V., Tang, L., and Menchu, S. (2011). Young womens transportation and labor market experiences. In *Transportation Research Board Conference Proceedings*, volume 2.

Thornton, R. (2005). The demand for and impact of learning hiv status: Evidence from a field experiment. *Informal paper, Harvard University, Cambridge, MA*.

Uteng, T. P. (2012). Gender and mobility in the developing world.

Vyborny, K. and Field, E. (2019). Transport, urban labor market integration, and women's mobility.

## Main Tables and Figures

Figure 1: Effect of Distance on Take

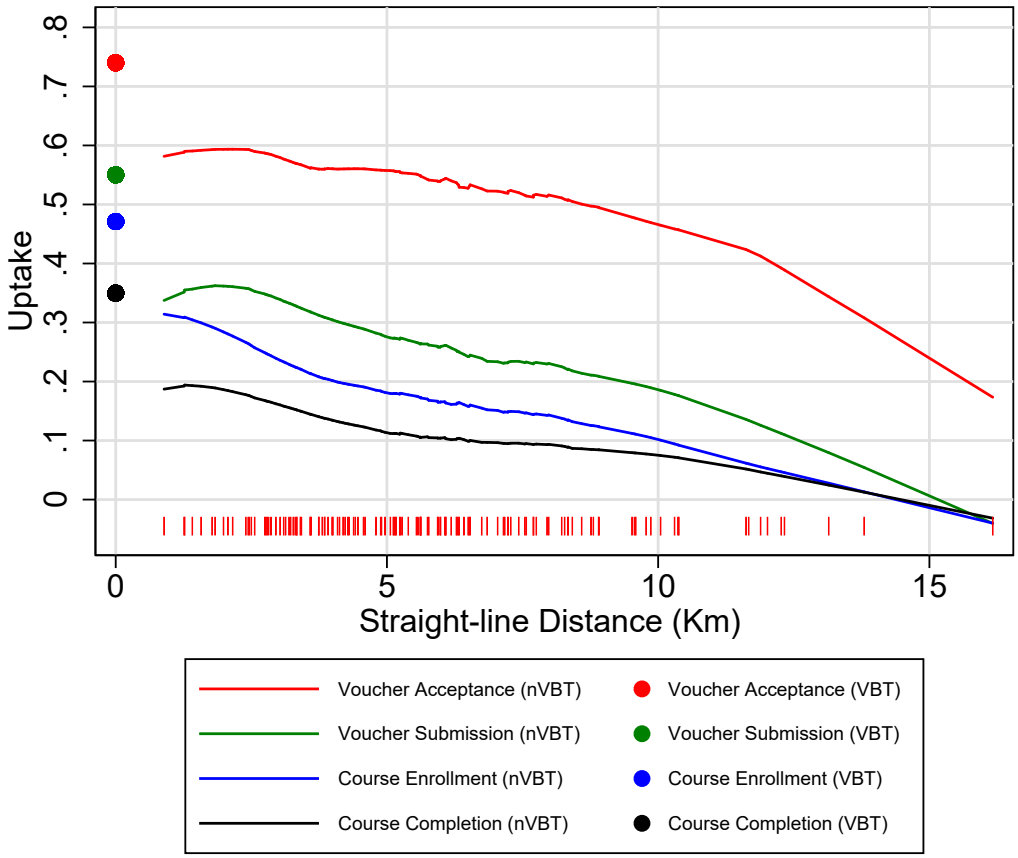


Figure 2: Outcome Mean by (Population Weighted) Distance

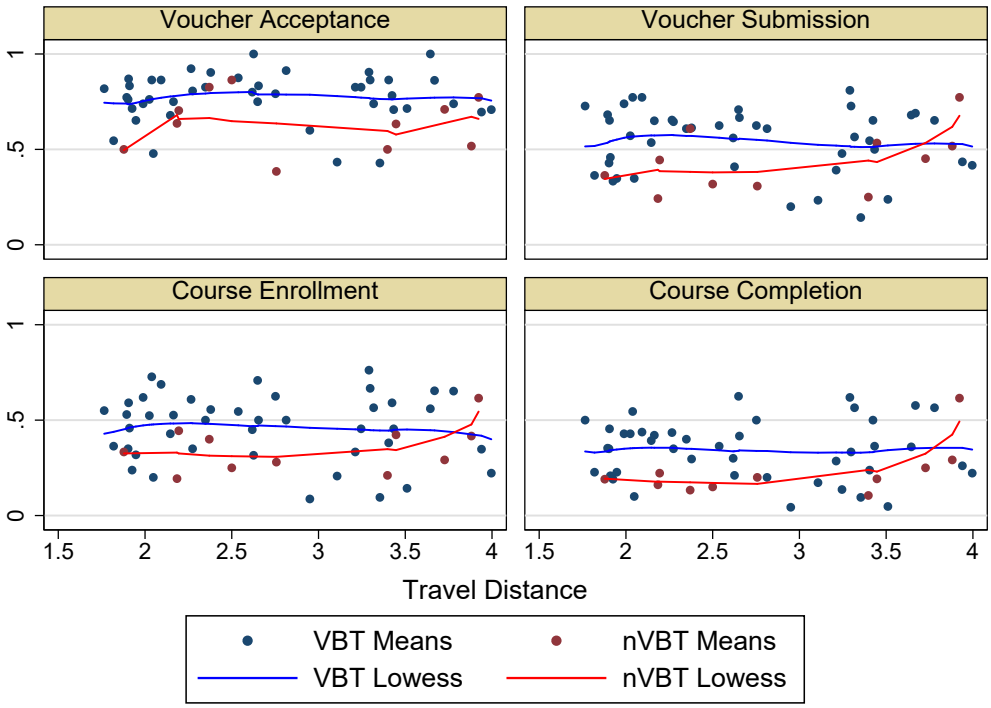



Figure 3: Village with Settlement Boundaries

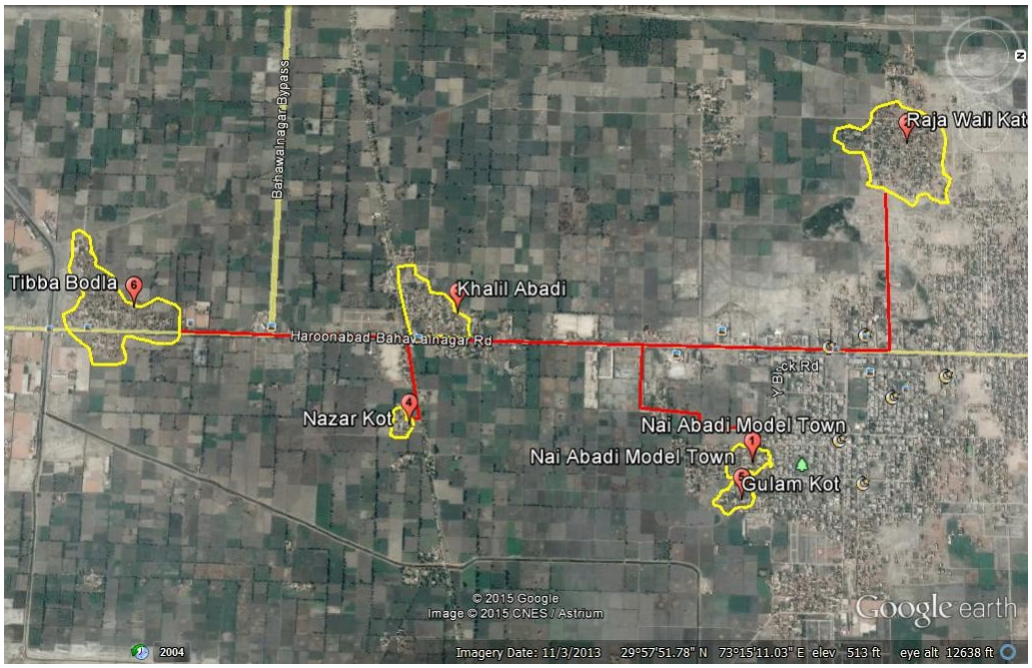


Table 1: Summary Statistics

| | Mean | Std. Dev. | Min | Max |
|---|---|---|---|---|
| **Household Variables:** | | | | |
| Income | 11.561 | 6.992 | 0.000 | 150.000 |
| Size | 6.566 | 2.883 | 1.000 | 31.000 |
| Punjabi | 0.462 | 0.499 | 0.000 | 1.000 |
| Asset Index | -0.004 | 0.955 | -1.128 | 9.563 |
| **Trainee Variables:** | | | | |
| Married | 0.695 | 0.460 | 0.000 | 1.000 |
| Has Formal Education | 0.341 | 0.474 | 0.000 | 1.000 |
| Paid Work | 0.325 | 0.468 | 0.000 | 1.000 |
| Able to Stitch | 0.326 | 0.469 | 0.000 | 1.000 |
| Stitched Last Month | 0.055 | 0.228 | 0.000 | 1.000 |
| Normalized Influence Index: Domestic Decisions | -0.003 | 1.001 | -1.450 | 1.041 |
| Normalized Influence Index: Business Decisions | -0.006 | 1.007 | -1.968 | 1.395 |
| **Village/Distance Variables:** | | | | |
| Straight-Line Distance | 3.220 | 3.636 | 0.000 | 16.168 |
| Travel Distance | 6.100 | 5.283 | 0.168 | 24.209 |
| Cluster-level Travel Distance | 6.137 | 5.584 | 0.045 | 36.200 |
| **Outcome Variables:** | | | | |
| Voucher Acceptance | 0.628 | 0.483 | 0.000 | 1.000 |
| Voucher Submission | 0.394 | 0.489 | 0.000 | 1.000 |
| Class Enrollment | 0.302 | 0.459 | 0.000 | 1.000 |
| Class Completion | 0.214 | 0.410 | 0.000 | 1.000 |

*Notes*: Table reports summary statistics for all variables used in analysis. Married, Formal Education, Able to Stitch, Stitched Last Month, and Engaged in Paid Work are dummy variables repsenting the share of our sample belonging to that category. Straight-line distance is the distance from the voucher holder's house to nearest training center, and it is constrained to be 0 for all VBT voucher holders. Travel distance is the measured distance from the centroid of an informal cluster of houses where a respondent lives to the training center.

Table 2: Village and Household Count by Treatment Branch

| | Village Based Training | non-Village Based Training |
|---|---|---|
| Baseline Intervention | 42 (1052) | 27 (692) |
| Trainee Engagement | 39 (980) | 27 (663) |
| Community Engagement | 27 (687) | 27 (704) |
| Group Transport | | 27 (704) |
| Group Transport + Comm. Engage. | | 27 (672) |

Table 3: Village and Household Count by Stipend Bucket and Amount

| Top-Up Amount (PKR) | Household Count |
|---|---|
| 0 | 2,563 |
| 500 | 280 |
| 1000 | 413 |
| 1500 | 563 |
| 2000 | 544 |
| 2500 | 529 |
| 3000 | 406 |
| 3500 | 419 |
| 4000 | 293 |
| 4500 | 144 |

Table 4: Effect of VBT

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **No Distance Measure** | | | | | | | | |
| **Panel A: Boundary Effect only** | | | | | | | | |
| Village Based Training | 0.221*** | 0.226*** | 0.321*** | 0.333*** | 0.339*** | 0.352*** | 0.272*** | 0.285*** |
| | (0.030) | (0.030) | (0.027) | (0.027) | (0.023) | (0.023) | (0.019) | (0.019) |
| **Distance Measure 1: Straight-Line distance** | | | | | | | | |
| **Panel B: Linear specification** | | | | | | | | |
| Village Based Training | 0.106** | 0.093* | 0.186*** | 0.194*** | 0.214*** | 0.226*** | 0.186*** | 0.195*** |
| | (0.048) | (0.047) | (0.040) | (0.040) | (0.035) | (0.034) | (0.026) | (0.026) |
| Straight-line Distance | -0.019*** | -0.022*** | -0.022*** | -0.023*** | -0.021*** | -0.021*** | -0.015*** | -0.015*** |
| | (0.007) | (0.007) | (0.005) | (0.005) | (0.004) | (0.004) | (0.003) | (0.003) |
| **Panel C: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.213*** | 0.223*** | 0.195*** | 0.242*** | 0.183*** | 0.233*** | 0.149*** | 0.191*** |
| | (0.071) | (0.067) | (0.066) | (0.067) | (0.061) | (0.061) | (0.046) | (0.046) |
| Straight-line Distance | 0.018 | 0.023 | -0.019 | -0.006 | -0.032** | -0.018 | -0.027** | -0.016 |
| | (0.020) | (0.019) | (0.018) | (0.018) | (0.016) | (0.016) | (0.012) | (0.013) |
| (Straight-line Distance)$^2$ | -0.003** | -0.003** | -0.000 | -0.001 | 0.001 | -0.000 | 0.001 | 0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| **Distance Measure 2: Travel distance** | | | | | | | | |
| **Panel D: Linear specification** | | | | | | | | |
| Village Based Training | 0.130*** | 0.130*** | 0.170*** | 0.187*** | 0.205*** | 0.225*** | 0.172*** | 0.193*** |
| | (0.039) | (0.038) | (0.037) | (0.038) | (0.033) | (0.033) | (0.026) | (0.026) |
| Travel Distance | -0.011*** | -0.012*** | -0.019*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| **Panel E: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.146*** | 0.166*** | 0.110** | 0.140*** | 0.150*** | 0.180*** | 0.125*** | 0.156*** |
| | (0.044) | (0.041) | (0.045) | (0.044) | (0.041) | (0.040) | (0.034) | (0.033) |
| Travel Distance | -0.005 | 0.002 | -0.043*** | -0.037*** | -0.038*** | -0.034*** | -0.031*** | -0.026*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.008) | (0.008) |
| (Travel Distance)$^2$ | -0.000 | -0.001 | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Control Mean | 0.536 | 0.625 | 0.266 | 0.254 | 0.170 | 0.129 | 0.112 | 0.081 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy control included in all specifications, and an Average Distance control in included with the same functional form as distance. Straight-Line Distance is the GPS distance from the voucher holder's house to nearest training center and is constrained to be 0 for all VBT voucher holders. Travel Distance is the measured distance from the population centroid of the village to the training center. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table 5: Alternative Distance Controls

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Panel A: Fifth order polynomial of travel distance** | | | | | | | | |
| Village Based Training | 0.141*** | 0.167*** | 0.113** | 0.147*** | 0.158*** | 0.191*** | 0.127*** | 0.164*** |
| | (0.046) | (0.042) | (0.048) | (0.046) | (0.043) | (0.042) | (0.037) | (0.036) |
| Travel Distance | 0.042 | 0.030 | -0.029 | -0.035 | -0.043 | -0.048 | -0.036 | -0.044 |
| | (0.051) | (0.050) | (0.048) | (0.048) | (0.045) | (0.044) | (0.037) | (0.037) |
| (Travel Distance)$^2$ | -0.016 | -0.008 | -0.001 | 0.002 | 0.006 | 0.009 | 0.005 | 0.010 |
| | (0.017) | (0.017) | (0.015) | (0.015) | (0.013) | (0.013) | (0.011) | (0.011) |
| (Travel Distance)$^3$ | 0.002 | 0.001 | 0.000 | -0.000 | -0.001 | -0.001 | -0.001 | -0.001 |
| | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.002) | (0.001) | (0.001) |
| (Travel Distance)$^4$ | -0.000 | -0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| (Travel Distance)$^5$ | 0.000 | 0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| **Panel B: Distance bins** | | | | | | | | |
| Village Based Training | 0.121** | 0.141*** | 0.114** | 0.147*** | 0.148*** | 0.183*** | 0.118*** | 0.156*** |
| | (0.050) | (0.047) | (0.050) | (0.051) | (0.042) | (0.041) | (0.038) | (0.036) |
| Bin 2 | -0.080 | -0.041 | -0.158*** | -0.138*** | -0.123*** | -0.107** | -0.097** | -0.080** |
| | (0.050) | (0.049) | (0.052) | (0.052) | (0.047) | (0.046) | (0.045) | (0.041) |
| Bin 3 | 0.066 | 0.114* | -0.034 | -0.010 | -0.069 | -0.052 | -0.091** | -0.063 |
| | (0.055) | (0.059) | (0.055) | (0.061) | (0.047) | (0.051) | (0.043) | (0.045) |
| Bin 4 | -0.175** | -0.153** | -0.208** | -0.209*** | -0.189*** | -0.188*** | -0.152** | -0.139** |
| | (0.077) | (0.072) | (0.085) | (0.079) | (0.070) | (0.064) | (0.065) | (0.057) |
| Bin 5 | -0.049 | -0.008 | -0.235*** | -0.210*** | -0.213*** | -0.187*** | -0.181*** | -0.152*** |
| | (0.065) | (0.062) | (0.059) | (0.061) | (0.048) | (0.047) | (0.041) | (0.037) |
| Bin 6 | -0.079 | -0.070 | -0.188*** | -0.157** | -0.170*** | -0.133*** | -0.136*** | -0.102** |
| | (0.072) | (0.071) | (0.065) | (0.064) | (0.051) | (0.049) | (0.046) | (0.044) |
| Bin 7 | -0.086 | -0.074 | -0.299*** | -0.249*** | -0.271*** | -0.214*** | -0.204*** | -0.147*** |
| | (0.068) | (0.074) | (0.067) | (0.068) | (0.051) | (0.052) | (0.047) | (0.047) |
| Bin 8 | -0.098 | -0.084 | -0.243*** | -0.241*** | -0.280*** | -0.283*** | -0.230*** | -0.236*** |
| | (0.075) | (0.068) | (0.058) | (0.059) | (0.046) | (0.047) | (0.039) | (0.039) |
| Bin 9 | -0.213** | -0.192** | -0.327*** | -0.303*** | -0.305*** | -0.275*** | -0.229*** | -0.195*** |
| | (0.093) | (0.087) | (0.061) | (0.062) | (0.046) | (0.046) | (0.043) | (0.043) |
| Bin 10 | -0.122 | -0.101 | -0.279*** | -0.257*** | -0.236*** | -0.216*** | -0.191*** | -0.166*** |
| | (0.079) | (0.075) | (0.074) | (0.075) | (0.059) | (0.060) | (0.048) | (0.051) |
| **Panel C: Regression Discontinuity Design** | | | | | | | | |
| Village Based Training | 0.139*** | 0.183*** | 0.007 | 0.041 | 0.061 | 0.105** | 0.054 | 0.120*** |
| | (0.042) | (0.045) | (0.044) | (0.048) | (0.044) | (0.048) | (0.039) | (0.043) |
| Travel Distance | -0.008 | 0.020 | -0.014 | 0.006 | -0.007 | 0.006 | -0.009 | 0.005 |
| | (0.013) | (0.013) | (0.013) | (0.014) | (0.013) | (0.014) | (0.012) | (0.013) |
| Panel A Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Panel B Obs. | 5313 | 4821 | 5313 | 4821 | 4894 | 4432 | 4894 | 4432 |
| Panel C Obs. | 1052 | 955 | 1052 | 955 | 938 | 845 | 938 | 845 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy control included in all specifications, and an Average Distance control in included with the same functional form as distance. Travel Distance is the measured distance from the population centroid of the village to the training center. Distance bins computed using Travel Distance Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table 6: Economic Magnitudes

| | (1) Voucher Acceptance | (2) Voucher Submission | (3) Class Enrollment | (4) Class Completion |
|---|---|---|---|---|
| **Panel A: Regression results** | | | | |
| Stipend (in Thousands) | 0.036*** | 0.045*** | 0.042*** | 0.043*** |
| | (0.006) | (0.005) | (0.005) | (0.005) |
| **Panel B: Economic Magnitudes** | | | | |
| VBT Magnitude | 6035*** | 7010*** | 7840*** | 6247*** |
| | (809.249) | (709.695) | (805.257) | (602.162) |
| **Panel C: Economic Magnitudes with Distance** | | | | |
| VBT Magnitude | 3204*** | 3689*** | 4838*** | 4072*** |
| | (672.598) | (549.707) | (642.473) | (532.212) |
| Distance Magnitude | -367*** | -443*** | -407*** | -298*** |
| | (68.596) | (58.480) | (57.589) | (45.511) |

*Notes*: Panel A reports OLS regressions of uptake variables on stipend level, treatment, and controls. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Panel B reports economic magnitudes derived by dividing the coefficient on VBT by the stipend coefficient. Panel C repeats the regressions in Panel A but includes Travel Distance and Average Distance controls. Economic magnitudes reported in Panel B derived by dividing the relevent coefficient by the stipend coefficient. Standard errors clustered at the village level reported in parentheses.

Table 7: Settlement Boundary Effect

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **No Distance Measure** | | | | | | | | |
| **Panel A: Boundary Effect only** | | | | | | | | |
| Village Based Training | 0.218*** | 0.227*** | 0.272*** | 0.286*** | 0.280*** | 0.293*** | 0.205*** | 0.221*** |
| | (0.035) | (0.034) | (0.034) | (0.034) | (0.030) | (0.031) | (0.026) | (0.026) |
| Settlement Based Training | 0.010 | 0.005 | 0.094*** | 0.089*** | 0.109*** | 0.108*** | 0.122*** | 0.115*** |
| | (0.028) | (0.027) | (0.032) | (0.032) | (0.031) | (0.031) | (0.029) | (0.029) |
| **Distance Measure 3: Cluster-level travel distance** | | | | | | | | |
| **Panel B: Linear specification** | | | | | | | | |
| Village Based Training | 0.136*** | 0.141*** | 0.142*** | 0.156*** | 0.166*** | 0.181*** | 0.116*** | 0.137*** |
| | (0.038) | (0.037) | (0.039) | (0.039) | (0.035) | (0.036) | (0.029) | (0.029) |
| Settlement Based Training | -0.010 | -0.014 | 0.060* | 0.057* | 0.079** | 0.081*** | 0.101*** | 0.097*** |
| | (0.029) | (0.028) | (0.032) | (0.032) | (0.031) | (0.031) | (0.029) | (0.029) |
| Cluster-level Travel Distance | -0.012*** | -0.013*** | -0.019*** | -0.019*** | -0.017*** | -0.017*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| **Panel C: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.129*** | 0.140*** | 0.086** | 0.109*** | 0.110*** | 0.133*** | 0.070** | 0.097*** |
| | (0.039) | (0.038) | (0.040) | (0.040) | (0.037) | (0.037) | (0.030) | (0.030) |
| Settlement Based Training | -0.009 | -0.007 | 0.027 | 0.028 | 0.046 | 0.048 | 0.070** | 0.065** |
| | (0.031) | (0.029) | (0.034) | (0.033) | (0.033) | (0.032) | (0.031) | (0.030) |
| Cluster-level Travel Distance | -0.015** | -0.012* | -0.046*** | -0.042*** | -0.045*** | -0.042*** | -0.036*** | -0.034*** |
| | (0.006) | (0.006) | (0.007) | (0.007) | (0.006) | (0.006) | (0.005) | (0.005) |
| (Cluster-level Travel Distance)$^2$ | 0.000 | -0.000 | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| **Economic Magnitudes** | | | | | | | | |
| **Panel A Magnitudes:** | | | | | | | | |
| VBT | 5247*** | 5445*** | 6529*** | 6882*** | 6724*** | 7038*** | 4915*** | 5321*** |
| | (1026.000) | (1030.880) | (1116.355) | (1135.020) | (1091.395) | (1113.236) | (852.417) | (884.743) |
| SBT | 244 | 123 | 2257*** | 2151*** | 2611*** | 2603*** | 2925*** | 2764*** |
| | (670.680) | (642.080) | (822.569) | (825.420) | (801.478) | (814.269) | (766.920) | (771.855) |
| **Panel B Magnitudes:** | | | | | | | | |
| VBT | 3260*** | 3388*** | 3401*** | 3758*** | 3979*** | 4358*** | 2786*** | 3304*** |
| | (972.240) | (965.609) | (1023.416) | (1031.132) | (989.040) | (1001.785) | (782.279) | (805.783) |
| SBT | -251 | -341 | 1432** | 1368** | 1910*** | 1944*** | 2429*** | 2319*** |
| | (703.897) | (670.365) | (798.884) | (787.278) | (780.124) | (775.330) | (754.570) | (751.870) |
| Obs. | 5797 | 5285 | 5797 | 5285 | 5321 | 4841 | 5321 | 4841 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Settlement-Level Distance is the measured distance from the respondent's settlement boundary to the training center settlement. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Economic magnitudes derived by dividing the relvent coefficient by the stipend coefficient. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table 8: Treatment Breakdown

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.137*** | 0.166*** | 0.110** | 0.140*** | 0.153*** | 0.180*** | 0.125*** | 0.156*** |
| | (0.042) | (0.041) | (0.044) | (0.044) | (0.041) | (0.040) | (0.034) | (0.033) |
| Community Engagement | -0.089*** | -0.098*** | 0.001 | -0.009 | 0.025 | 0.016 | 0.012 | 0.007 |
| | (0.028) | (0.028) | (0.026) | (0.026) | (0.023) | (0.024) | (0.020) | (0.020) |
| Trainee Engagement | -0.032 | -0.043 | 0.019 | 0.013 | 0.028 | 0.027 | 0.027 | 0.024 |
| | (0.031) | (0.030) | (0.028) | (0.028) | (0.025) | (0.025) | (0.022) | (0.022) |
| Group Transport | 0.040 | 0.036 | 0.081** | 0.080** | 0.100*** | 0.105*** | 0.099*** | 0.108*** |
| | (0.037) | (0.038) | (0.032) | (0.032) | (0.026) | (0.027) | (0.022) | (0.023) |
| Travel Distance | -0.005 | 0.002 | -0.042*** | -0.037*** | -0.038*** | -0.034*** | -0.031*** | -0.026*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.008) | (0.008) |
| (Travel Distance)$^2$ | -0.000 | -0.001 | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Control Mean | 0.614 | 0.625 | 0.241 | 0.254 | 0.121 | 0.129 | 0.076 | 0.081 |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Average Distance and Averagde Distance squared terms included as controls in all specifications. Travel Distance is the measured distance from the population centroid of the village to the training center. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table 9: Heterogeneity Effects

| | Voucher Acceptance | Voucher Submission | Class Enrollment | Class Completion | Appendix Table |
|---|---|---|---|---|---|
| **Panel A: Female Agency** | | | | | |
| **Need** | | | | | |
| (1) VBT × STD HH Size | 0.027* | 0.044*** | 0.058*** | 0.060*** | |
| | (0.014) | (0.014) | (0.014) | (0.013) | |
| (2) VBT × At Least 1 Male HH Member Away for 3 Months | 0.011 | 0.037 | 0.023 | 0.015 | |
| | (0.042) | (0.040) | (0.040) | (0.035) | |
| (3) VBT × At Least 1 Female HH Member Away for 3 Months | 0.213** | 0.225*** | 0.274*** | 0.237*** | |
| | (0.094) | (0.084) | (0.078) | (0.083) | |
| **Influence** | | | | | |
| (4) VBT × STD Domestic Influence Index | -0.018 | -0.030** | -0.042*** | -0.046*** | |
| | (0.013) | (0.014) | (0.013) | (0.014) | |
| (5) VBT × STD Business Influence Index | 0.011 | 0.006 | 0.005 | 0.002 | |
| | (0.016) | (0.016) | (0.015) | (0.013) | |
| (6) VBT × Married | -0.054* | -0.069** | -0.099*** | -0.118*** | |
| | (0.030) | (0.028) | (0.028) | (0.028) | |
| **Perception** | | | | | |
| (7) VBT × STD PCA of Male Belief in Men's Superiority | 0.006 | 0.029 | 0.029 | 0.051*** | |
| | (0.021) | (0.020) | (0.019) | (0.018) | |
| (8) VBT × STD PCA of Female Belief in Men's Superiority | 0.002 | 0.003 | 0.005 | 0.024 | |
| | (0.018) | (0.019) | (0.019) | (0.018) | |
| (9) VBT × Woman Open to Traveling for Work | -0.002 | 0.018 | 0.068 | 0.011 | |
| | (0.051) | (0.054) | (0.055) | (0.060) | |
| (10) VBT × Man Open to Traveling for Work | 0.059 | 0.101*** | 0.101*** | 0.071** | |
| | (0.037) | (0.037) | (0.037) | (0.035) | |
| (11) VBT × Very Likely to Enroll | 0.032 | 0.070** | 0.088*** | 0.097*** | |
| | (0.029) | (0.028) | (0.028) | (0.028) | |
| **Panel B: Perception of Safety** | | | | | |
| (1) VBT × Male Doesn't Feel Safe | -0.049 | -0.094 | -0.040 | 0.013 | |
| | (0.050) | (0.058) | (0.051) | (0.051) | |
| (2) VBT × Female Doesn't Feel Safe | 0.111* | 0.069 | 0.069 | 0.099 | |
| | (0.060) | (0.072) | (0.072) | (0.066) | |
| **Panel C: Socioeconomic Status** | | | | | |
| (1) VBT × STD Ln HH Monthly Income | 0.036** | 0.025* | 0.012 | 0.021* | |
| | (0.014) | (0.014) | (0.013) | (0.012) | |
| (2) VBT × HH Income in Village's Bottom 30% | -0.072** | -0.091*** | -0.084*** | -0.077*** | |
| | (0.028) | (0.024) | (0.026) | (0.024) | |
| (3) VBT × HH Asset Index in Village's Bottom 30% | -0.026 | -0.073*** | -0.083*** | -0.087*** | |
| | (0.025) | (0.026) | (0.027) | (0.026) | |
| (4) VBT × Has Never Been to School | -0.091** | -0.042 | -0.041 | -0.046 | |
| | (0.040) | (0.040) | (0.037) | (0.033) | |
| **Panel D: Ethnic Diversity** | | | | | |
| (1) VBT × STD Ethnic Fragmentation Index | -0.007 | -0.038* | -0.039* | -0.030* | |
| | (0.023) | (0.022) | (0.022) | (0.017) | |
| (6) VBT × STD Ethnic Difference Index | 0.007 | -0.007 | -0.016 | -0.021 | |
| | (0.022) | (0.023) | (0.021) | (0.017) | |
| **Panel E: Village Connectivity** | | | | | |
| (1) VBT × Availability of Transport Facilities | -0.037 | -0.043 | -0.040 | -0.037 | |
| | (0.047) | (0.046) | (0.043) | (0.036) | |
| (2) VBT × Availability of Non-transport Facilities | -0.095** | -0.032 | -0.048 | -0.038 | |
| | (0.048) | (0.049) | (0.044) | (0.038) | |

*Notes*: Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Standard errors clustered at the village level reported in parentheses.

# Appendix

## Appendix A: Balance Table

Table A1: Treatment Balance Table

| | Means | | Difference | |
|---|---|---|---|---|
| | (1) VBT | (2) nVBT | (3) (1)-(2) | (4) P-value |
| **Household Variables:** | | | | |
| Income | 11.536 | 11.581 | -0.045 | 0.808 |
| | (0.144) | (0.120) | (0.186) | |
| Size | 6.522 | 6.602 | -0.081 | 0.292 |
| | (0.057) | (0.051) | (0.077) | |
| Asset Index | -0.006 | -0.002 | -0.004 | 0.876 |
| | (0.020) | (0.017) | (0.025) | |
| **Trainee Variables:** | | | | |
| Married | 0.703 | 0.688 | 0.015 | 0.224 |
| | (0.009) | (0.008) | (0.012) | |
| Has Formal Education | 0.331 | 0.348 | -0.017 | 0.178 |
| | (0.009) | (0.009) | (0.013) | |
| Able to Stitch | 0.327 | 0.326 | 0.001 | 0.929 |
| | (0.010) | (0.009) | (0.013) | |
| Stitched Last Month | 0.058 | 0.052 | 0.006 | 0.317 |
| | (0.005) | (0.004) | (0.006) | |
| Age | 30.589 | 29.670 | 0.919 | 0.004 |
| | (0.244) | (0.209) | (0.319) | |

*Notes*: Table shows balance between our two main treatment groups, VBT and nVBT. Note that, while some differences do exist, they are all relatively small.

## Appendix B: Robustness Checks

Table B1: Cluster-Level Distance

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **Cluster-level travel distance** | | | | | | | | |
| **Panel A: Linear specification** | | | | | | | | |
| Village Based Training | 0.130*** | 0.134*** | 0.168*** | 0.182*** | 0.202*** | 0.219*** | 0.162*** | 0.182*** |
| | (0.036) | (0.036) | (0.036) | (0.036) | (0.032) | (0.032) | (0.026) | (0.025) |
| Cluster-level Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.018*** | -0.014*** | -0.013*** |
| | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) | (0.002) |
| **Panel B: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.126*** | 0.138*** | 0.094** | 0.118*** | 0.124*** | 0.148*** | 0.091*** | 0.116*** |
| | (0.038) | (0.037) | (0.038) | (0.038) | (0.035) | (0.035) | (0.029) | (0.029) |
| Cluster-level Travel Distance | -0.014** | -0.012** | -0.048*** | -0.044*** | -0.048*** | -0.045*** | -0.041*** | -0.038*** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.005) | (0.005) |
| (Cluster-level Travel Distance)$^2$ | 0.000 | -0.000 | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Obs. | 5641 | 5135 | 5641 | 5135 | 5172 | 4698 | 5172 | 4698 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Cluster-Level Distance is the measured distance from the respondent's cluster boundary to the training center's cluster. Group Transport dummy and Average Distance control included in all regressions. Panel B regressions also include a squared Average Distance term. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table B2: Full Treatment Breakdown

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.043 | 0.082* | 0.082 | 0.118** | 0.129** | 0.161*** | 0.094** | 0.132*** |
| | (0.052) | (0.050) | (0.055) | (0.052) | (0.052) | (0.051) | (0.046) | (0.045) |
| Community Engagement | -0.191*** | -0.193*** | -0.035 | -0.044 | -0.008 | -0.012 | -0.015 | -0.012 |
| | (0.054) | (0.056) | (0.042) | (0.043) | (0.034) | (0.036) | (0.025) | (0.026) |
| Trainee Engagement | -0.087 | -0.087 | -0.009 | -0.013 | 0.016 | 0.015 | -0.008 | -0.010 |
| | (0.058) | (0.059) | (0.041) | (0.042) | (0.030) | (0.031) | (0.023) | (0.022) |
| Group Transport | -0.022 | -0.024 | 0.046 | 0.042 | 0.075** | 0.081** | 0.073** | 0.085*** |
| | (0.051) | (0.050) | (0.044) | (0.042) | (0.034) | (0.034) | (0.029) | (0.029) |
| VBT × CE | 0.171** | 0.153** | 0.043 | 0.033 | 0.045 | 0.035 | 0.039 | 0.024 |
| | (0.066) | (0.066) | (0.061) | (0.061) | (0.056) | (0.056) | (0.045) | (0.046) |
| VBT × TE | 0.084 | 0.065 | 0.037 | 0.031 | 0.013 | 0.012 | 0.056 | 0.054 |
| | (0.067) | (0.068) | (0.058) | (0.058) | (0.049) | (0.050) | (0.042) | (0.042) |
| GT × CE | 0.120 | 0.122 | 0.063 | 0.071 | 0.052 | 0.049 | 0.038 | 0.030 |
| | (0.079) | (0.079) | (0.067) | (0.067) | (0.055) | (0.056) | (0.045) | (0.046) |
| Travel Distance | -0.009 | -0.003 | -0.043*** | -0.038*** | -0.040*** | -0.035*** | -0.032*** | -0.026*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.008) | (0.008) |
| (Travel Distance)$^2$ | -0.000 | -0.000 | 0.001*** | 0.001** | 0.001*** | 0.001** | 0.001*** | 0.001** |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Control Mean | 0.614 | 0.625 | 0.241 | 0.254 | 0.121 | 0.129 | 0.076 | 0.081 |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Travel Distance is the measured distance from settlement boundary to the training center. Average Distance and Average Distance squared controls included in all regressions. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table B3: Effect by Neighbor Treatment

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **No Distance Measure** | | | | | | | | |
| **Panel A: Boundary Effect only** | | | | | | | | |
| Village Based Training | 0.231*** | 0.235*** | 0.321*** | 0.334*** | 0.345*** | 0.359*** | 0.278*** | 0.291*** |
| | (0.030) | (0.031) | (0.027) | (0.027) | (0.024) | (0.024) | (0.020) | (0.019) |
| Neighbor | 0.017 | 0.008 | 0.009 | -0.001 | 0.018 | 0.012 | 0.018 | 0.014 |
| | (0.024) | (0.025) | (0.023) | (0.022) | (0.018) | (0.018) | (0.017) | (0.017) |
| VBT × Neighbor | -0.052 | -0.046 | -0.002 | -0.006 | -0.032 | -0.038 | -0.035 | -0.033 |
| | (0.032) | (0.034) | (0.033) | (0.033) | (0.032) | (0.033) | (0.030) | (0.031) |
| **Distance Measure 2: Travel distance** | | | | | | | | |
| **Panel B: No Distance Interaction** | | | | | | | | |
| Village Based Training | 0.140*** | 0.139*** | 0.170*** | 0.188*** | 0.210*** | 0.232*** | 0.179*** | 0.199*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.027) |
| Neighbor | 0.017 | 0.008 | 0.009 | 0.000 | 0.016 | 0.012 | 0.018 | 0.014 |
| | (0.024) | (0.025) | (0.023) | (0.022) | (0.018) | (0.018) | (0.017) | (0.017) |
| VBT × Neighbor | -0.052 | -0.046 | -0.001 | -0.005 | -0.030 | -0.037 | -0.033 | -0.032 |
| | (0.032) | (0.034) | (0.033) | (0.033) | (0.032) | (0.033) | (0.030) | (0.031) |
| Travel Distance | -0.011*** | -0.012*** | -0.019*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| **Panel C: Distance Interaction** | | | | | | | | |
| Village Based Training | 0.135*** | 0.134*** | 0.170*** | 0.188*** | 0.218*** | 0.240*** | 0.187*** | 0.207*** |
| | (0.039) | (0.039) | (0.038) | (0.039) | (0.032) | (0.033) | (0.027) | (0.027) |
| Neighbor | -0.097 | -0.128 | -0.166 | -0.197 | -0.178 | -0.225 | -0.188 | -0.230 |
| | (0.131) | (0.144) | (0.145) | (0.144) | (0.140) | (0.146) | (0.139) | (0.146) |
| VBT × Neighbor | -0.024 | -0.015 | 0.004 | -0.000 | -0.074 | -0.080* | -0.079* | -0.076* |
| | (0.047) | (0.050) | (0.048) | (0.048) | (0.047) | (0.047) | (0.043) | (0.044) |
| Travel Distance | -0.012*** | -0.013*** | -0.019*** | -0.018*** | -0.016*** | -0.015*** | -0.012*** | -0.010*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.002) | (0.003) | (0.002) | (0.002) |
| Travel Distance × Neighbor | 0.003 | 0.004 | -0.000 | -0.000 | -0.007* | -0.007* | -0.007** | -0.007** |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.004) |
| Obs. | 5872 | 5348 | 5872 | 5348 | 5392 | 4900 | 5392 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Peer is a dummy variable marking respondents who also had a neighbor invited to enroll in the program. Group Transport dummy and Average Distance control included in all regressions. Regressions in Panel C also include an interaction term between Peer and Average Distance. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from settlement boundary to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table B4: Individual-level Stipend and Village Average Stipend

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **No Distance Measure** | | | | | | | | |
| **Panel A: Boundary Effect only** | | | | | | | | |
| Village Based Training | 0.224*** | 0.226*** | 0.326*** | 0.334*** | 0.343*** | 0.354*** | 0.276*** | 0.287*** |
| | (0.030) | (0.030) | (0.026) | (0.027) | (0.023) | (0.023) | (0.018) | (0.019) |
| Trainee Stipend in 1000s | 0.037*** | 0.037*** | 0.044*** | 0.046*** | 0.040*** | 0.042*** | 0.040*** | 0.041*** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Village Average Stipend in 1000s | -0.004 | -0.007 | 0.009 | 0.005 | 0.015 | 0.013 | 0.015 | 0.013 |
| | (0.019) | (0.019) | (0.016) | (0.017) | (0.015) | (0.015) | (0.013) | (0.013) |
| **Distance Measure 1: Straight-Line distance** | | | | | | | | |
| **Panel B: Linear specification** | | | | | | | | |
| Village Based Training | 0.108** | 0.093* | 0.190*** | 0.196*** | 0.218*** | 0.229*** | 0.190*** | 0.198*** |
| | (0.048) | (0.047) | (0.039) | (0.040) | (0.033) | (0.035) | (0.024) | (0.026) |
| Trainee Stipend in 1000s | 0.037*** | 0.037*** | 0.044*** | 0.046*** | 0.040*** | 0.041*** | 0.040*** | 0.041*** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Village Average Stipend in 1000s | -0.003 | -0.006 | 0.010 | 0.006 | 0.015 | 0.013 | 0.014 | 0.013 |
| | (0.018) | (0.019) | (0.016) | (0.016) | (0.014) | (0.015) | (0.013) | (0.013) |
| Straight-line Distance | -0.019*** | -0.022*** | -0.022*** | -0.023*** | -0.021*** | -0.021*** | -0.015*** | -0.015*** |
| | (0.007) | (0.007) | (0.005) | (0.005) | (0.004) | (0.004) | (0.003) | (0.003) |
| **Panel C: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.234*** | 0.223*** | 0.225*** | 0.241*** | 0.214*** | 0.233*** | 0.180*** | 0.191*** |
| | (0.072) | (0.068) | (0.067) | (0.068) | (0.061) | (0.061) | (0.044) | (0.046) |
| Trainee Stipend in 1000s | 0.037*** | 0.037*** | 0.044*** | 0.046*** | 0.040*** | 0.041*** | 0.040*** | 0.041*** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Village Average Stipend in 1000s | 0.002 | -0.001 | 0.012 | 0.008 | 0.015 | 0.013 | 0.014 | 0.013 |
| | (0.018) | (0.019) | (0.016) | (0.016) | (0.014) | (0.015) | (0.013) | (0.013) |
| Straight-line Distance | 0.025 | 0.023 | -0.010 | -0.007 | -0.022 | -0.019 | -0.018 | -0.017 |
| | (0.020) | (0.019) | (0.018) | (0.018) | (0.016) | (0.016) | (0.012) | (0.013) |
| (Straight-line Distance)$^2$ | -0.003** | -0.003** | -0.001 | -0.001 | 0.000 | -0.000 | 0.000 | 0.000 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| **Distance Measure 2: Travel distance** | | | | | | | | |
| **Panel D: Linear specification** | | | | | | | | |
| Village Based Training | 0.135*** | 0.129*** | 0.178*** | 0.187*** | 0.212*** | 0.227*** | 0.180*** | 0.195*** |
| | (0.039) | (0.039) | (0.037) | (0.038) | (0.032) | (0.033) | (0.025) | (0.026) |
| Trainee Stipend in 1000s | 0.037*** | 0.037*** | 0.044*** | 0.046*** | 0.040*** | 0.041*** | 0.040*** | 0.041*** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Village Average Stipend in 1000s | -0.006 | -0.009 | 0.005 | 0.001 | 0.010 | 0.009 | 0.011 | 0.010 |
| | (0.019) | (0.020) | (0.016) | (0.016) | (0.014) | (0.015) | (0.013) | (0.013) |
| Travel Distance | -0.011*** | -0.012*** | -0.019*** | -0.019*** | -0.016*** | -0.016*** | -0.012*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| **Panel E: Quadratic specification** | | | | | | | | |
| Village Based Training | 0.155*** | 0.165*** | 0.122*** | 0.139*** | 0.163*** | 0.182*** | 0.137*** | 0.158*** |
| | (0.044) | (0.042) | (0.044) | (0.043) | (0.040) | (0.040) | (0.033) | (0.033) |
| Trainee Stipend in 1000s | 0.037*** | 0.037*** | 0.044*** | 0.046*** | 0.040*** | 0.041*** | 0.040*** | 0.041*** |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.004) | (0.005) | (0.004) | (0.005) |
| Village Average Stipend in 1000s | -0.006 | -0.009 | 0.003 | -0.001 | 0.008 | 0.007 | 0.009 | 0.009 |
| | (0.019) | (0.019) | (0.016) | (0.016) | (0.014) | (0.015) | (0.012) | (0.013) |
| Travel Distance | -0.004 | 0.002 | -0.040*** | -0.037*** | -0.036*** | -0.034*** | -0.029*** | -0.027*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.009) | (0.009) | (0.008) | (0.008) |
| (Travel Distance)$^2$ | -0.000 | -0.001 | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** | 0.001** |
| | (0.001) | (0.001) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| Obs. | 5872 | 5348 | 5872 | 5348 | 5392 | 4900 | 5392 | 4900 |
| Control Mean | 0.536 | 0.625 | 0.266 | 0.254 | 0.170 | 0.129 | 0.112 | 0.081 |
| Control | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy control included in all specifications, and an Average Distance control in included with the same functional form as distance. Straight-Line Distance is the GPS distance from the voucher holder's house to nearest training center and is constrained to be 0 for all VBT voucher holders. Travel Distance is the measured distance from the population centroid of the village to the training center. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

# Appendix C: Heterogeneity Analysis

## Heterogeneity by Female Agency

Table C1: Effect by Household Size

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.130*** | 0.128*** | 0.168*** | 0.185*** | 0.204*** | 0.223*** | 0.173*** | 0.192*** |
| | (0.038) | (0.038) | (0.038) | (0.037) | (0.032) | (0.032) | (0.027) | (0.026) |
| STD HH Size | 0.034*** | 0.050*** | 0.010 | 0.019* | 0.008 | 0.016* | 0.003 | 0.006 |
| | (0.010) | (0.011) | (0.009) | (0.010) | (0.007) | (0.008) | (0.006) | (0.007) |
| VBT × STD HH Size | 0.032** | 0.027* | 0.044*** | 0.044*** | 0.056*** | 0.058*** | 0.059*** | 0.060*** |
| | (0.014) | (0.014) | (0.014) | (0.014) | (0.014) | (0.014) | (0.012) | (0.013) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5735 | 5348 | 5735 | 5348 | 5266 | 4900 | 5266 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C2: Effect by Number of Children

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.126*** | 0.127*** | 0.165*** | 0.184*** | 0.201*** | 0.222*** | 0.170*** | 0.190*** |
| | (0.038) | (0.038) | (0.037) | (0.038) | (0.032) | (0.033) | (0.026) | (0.026) |
| STD No. of Dependents Age $\leq$ 9 | 0.019** | 0.035*** | 0.014 | 0.025*** | 0.010 | 0.022*** | 0.002 | 0.016*** |
| | (0.009) | (0.010) | (0.009) | (0.009) | (0.007) | (0.008) | (0.005) | (0.006) |
| VBT × STD No. of Dependents Age $\leq$ 9 | 0.016 | 0.008 | 0.014 | 0.008 | 0.026** | 0.019 | 0.031** | 0.026** |
| | (0.014) | (0.014) | (0.014) | (0.014) | (0.013) | (0.014) | (0.012) | (0.013) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5735 | 5348 | 5735 | 5348 | 5266 | 4900 | 5266 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C3: Effect by Number of Elderly

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.126*** | 0.129*** | 0.166*** | 0.186*** | 0.202*** | 0.224*** | 0.171*** | 0.193*** |
| | (0.039) | (0.038) | (0.037) | (0.038) | (0.033) | (0.033) | (0.027) | (0.026) |
| STD No. of Dependents Aged $\geq$ 60 | -0.023** | -0.018* | -0.014* | -0.013 | -0.011* | -0.011* | -0.011** | -0.015*** |
| | (0.010) | (0.010) | (0.008) | (0.008) | (0.006) | (0.007) | (0.005) | (0.006) |
| VBT $\times$ STD No. of Dependents Aged $\geq$ 60 | 0.002 | 0.000 | -0.012 | -0.004 | -0.018* | -0.012 | -0.004 | 0.005 |
| | (0.013) | (0.014) | (0.012) | (0.013) | (0.011) | (0.012) | (0.010) | (0.011) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5735 | 5348 | 5735 | 5348 | 5266 | 4900 | 5266 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C4: Effect by Number of Adult HH Members

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.132*** | 0.132*** | 0.168*** | 0.187*** | 0.206*** | 0.226*** | 0.174*** | 0.194*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.026) |
| STD No. of HH Members between 18 and 60 | 0.018* | 0.034*** | -0.006 | 0.003 | -0.001 | 0.004 | 0.000 | -0.002 |
| | (0.010) | (0.011) | (0.009) | (0.010) | (0.008) | (0.009) | (0.006) | (0.007) |
| VBT $\times$ STD No. of HH Members between 18 and 60 | 0.032** | 0.030** | 0.045*** | 0.045*** | 0.048*** | 0.054*** | 0.044*** | 0.046*** |
| | (0.013) | (0.014) | (0.013) | (0.013) | (0.013) | (0.014) | (0.012) | (0.012) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5735 | 5348 | 5735 | 5348 | 5266 | 4900 | 5266 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C5: Effect by Presence of Sick HH Members

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.127*** | 0.127*** | 0.168*** | 0.186*** | 0.204*** | 0.225*** | 0.172*** | 0.193*** |
| | (0.039) | (0.039) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.027) |
| HH Has At Least One Sick Member | -0.002 | -0.023 | 0.028 | 0.008 | 0.020 | 0.006 | 0.013 | -0.007 |
| | (0.029) | (0.029) | (0.026) | (0.025) | (0.022) | (0.023) | (0.020) | (0.021) |
| VBT $\times$ HH Has At Least One Sick Member | 0.005 | 0.024 | -0.010 | 0.004 | -0.008 | -0.004 | -0.004 | 0.003 |
| | (0.037) | (0.038) | (0.038) | (0.040) | (0.036) | (0.039) | (0.031) | (0.034) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5735 | 5348 | 5735 | 5348 | 5266 | 4900 | 5266 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C6: Effect by Marital Status

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.169*** | 0.166*** | 0.217*** | 0.234*** | 0.272*** | 0.292*** | 0.255*** | 0.274*** |
| | (0.044) | (0.043) | (0.044) | (0.042) | (0.039) | (0.039) | (0.034) | (0.033) |
| Married | -0.053** | -0.026 | -0.034* | -0.012 | -0.023 | 0.010 | -0.023* | 0.007 |
| | (0.023) | (0.025) | (0.018) | (0.022) | (0.015) | (0.019) | (0.013) | (0.017) |
| VBT × Married | -0.056* | -0.054* | -0.072** | -0.069** | -0.098*** | -0.099*** | -0.116*** | -0.118*** |
| | (0.031) | (0.030) | (0.029) | (0.028) | (0.028) | (0.028) | (0.027) | (0.028) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5348 | 5687 | 5348 | 5220 | 4900 | 5220 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C7: Effect by Domestic Influence Index

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.169*** | 0.166*** | 0.217*** | 0.234*** | 0.272*** | 0.292*** | 0.255*** | 0.274*** |
| | (0.044) | (0.043) | (0.044) | (0.042) | (0.039) | (0.039) | (0.034) | (0.033) |
| Married | -0.053** | -0.026 | -0.034* | -0.012 | -0.023 | 0.010 | -0.023* | 0.007 |
| | (0.023) | (0.025) | (0.018) | (0.022) | (0.015) | (0.019) | (0.013) | (0.017) |
| VBT × Married | -0.056* | -0.054* | -0.072** | -0.069** | -0.098*** | -0.099*** | -0.116*** | -0.118*** |
| | (0.031) | (0.030) | (0.029) | (0.028) | (0.028) | (0.028) | (0.027) | (0.028) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5348 | 5687 | 5348 | 5220 | 4900 | 5220 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C8: Domestic Influence Index Component 1–Need for Household Permission to Begin New Activities

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.136*** | 0.132*** | 0.170*** | 0.182*** | 0.202*** | 0.215*** | 0.170*** | 0.183*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.032) | (0.027) | (0.026) |
| Need HH Permission for New Activities | 0.076** | 0.059 | 0.040 | 0.028 | 0.012 | 0.004 | 0.011 | 0.005 |
| | (0.038) | (0.037) | (0.028) | (0.026) | (0.023) | (0.022) | (0.021) | (0.020) |
| VBT × Need HH Permission for New Activities | -0.097** | -0.099** | -0.055 | -0.057 | -0.011 | -0.014 | -0.003 | -0.006 |
| | (0.049) | (0.048) | (0.044) | (0.041) | (0.042) | (0.040) | (0.038) | (0.037) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C9: Domestic Influence Index Component 2–Influence over Husband's Decision to Begin New Activities

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.132*** | 0.127*** | 0.190*** | 0.201*** | 0.238*** | 0.249*** | 0.213*** | 0.224*** |
| | (0.043) | (0.041) | (0.042) | (0.041) | (0.037) | (0.036) | (0.031) | (0.029) |
| Infl. over Husband's Activities | -0.051** | -0.015 | -0.007 | 0.029 | -0.008 | 0.020 | -0.009 | 0.023 |
| | (0.021) | (0.022) | (0.016) | (0.019) | (0.013) | (0.016) | (0.012) | (0.015) |
| VBT × Infl. over Husband's Activities | -0.012 | -0.011 | -0.048* | -0.047* | -0.069*** | -0.067*** | -0.079*** | -0.077*** |
| | (0.028) | (0.027) | (0.028) | (0.027) | (0.026) | (0.025) | (0.025) | (0.024) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C10: Domestic Influence Index Component 3–Influence over Husband's Decision to Buy A New Sewing Machine

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.151*** | 0.140*** | 0.219*** | 0.221*** | 0.271*** | 0.274*** | 0.244*** | 0.248*** |
| | (0.043) | (0.042) | (0.042) | (0.042) | (0.038) | (0.037) | (0.032) | (0.031) |
| Infl. over Purchase of Sewing Machine | -0.050** | -0.022 | -0.014 | 0.011 | -0.012 | 0.006 | -0.012 | 0.012 |
| | (0.020) | (0.021) | (0.018) | (0.021) | (0.015) | (0.017) | (0.014) | (0.016) |
| VBT × Infl. over Purchase of Sewing Machine | -0.040 | -0.031 | -0.090*** | -0.078*** | -0.115*** | -0.103*** | -0.122*** | -0.111*** |
| | (0.028) | (0.027) | (0.029) | (0.028) | (0.028) | (0.027) | (0.026) | (0.026) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C11: Domestic Influence Index Component 4–Influence over Husband's Decision to Drop Daughter out of School

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.134*** | 0.126*** | 0.192*** | 0.198*** | 0.234*** | 0.241*** | 0.205*** | 0.212*** |
| | (0.042) | (0.040) | (0.040) | (0.040) | (0.037) | (0.036) | (0.030) | (0.029) |
| Infl. over Daugher's Education | -0.027 | 0.014 | 0.001 | 0.038** | -0.007 | 0.022 | -0.011 | 0.020 |
| | (0.020) | (0.020) | (0.017) | (0.018) | (0.013) | (0.015) | (0.012) | (0.013) |
| VBT × Infl. over Daugher's Education | -0.016 | -0.008 | -0.053* | -0.043 | -0.065** | -0.053** | -0.068*** | -0.056** |
| | (0.028) | (0.027) | (0.027) | (0.026) | (0.026) | (0.026) | (0.025) | (0.025) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C12: Domestic Influence Index Component 5–Influence over Husband's Decision to Increase Spending on Children's Clothing

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.139*** | 0.132*** | 0.192*** | 0.201*** | 0.242*** | 0.251*** | 0.215*** | 0.225*** |
| | (0.042) | (0.041) | (0.041) | (0.041) | (0.037) | (0.037) | (0.032) | (0.030) |
| Infl. over Spending on Child's Clothing | -0.039** | 0.010 | -0.012 | 0.035* | -0.013 | 0.024 | -0.010 | 0.032** |
| | (0.019) | (0.020) | (0.017) | (0.020) | (0.014) | (0.017) | (0.013) | (0.015) |
| VBT × Infl. over Spending on Child's Clothing | -0.021 | -0.019 | -0.049* | -0.045* | -0.072*** | -0.068*** | -0.078*** | -0.075*** |
| | (0.028) | (0.027) | (0.027) | (0.026) | (0.026) | (0.025) | (0.025) | (0.025) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C13: Effect by Business Influence Index

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.131*** | 0.129*** | 0.172*** | 0.186*** | 0.206*** | 0.224*** | 0.173*** | 0.193*** |
| | (0.039) | (0.038) | (0.037) | (0.038) | (0.033) | (0.033) | (0.027) | (0.026) |
| STD Business Confidence Index | 0.019 | 0.025** | 0.028*** | 0.029*** | 0.021*** | 0.022*** | 0.009 | 0.009 |
| | (0.012) | (0.012) | (0.010) | (0.010) | (0.008) | (0.008) | (0.007) | (0.007) |
| VBT × STD Business Confidence Index | 0.018 | 0.011 | 0.016 | 0.006 | 0.015 | 0.005 | 0.012 | 0.002 |
| | (0.017) | (0.016) | (0.015) | (0.016) | (0.014) | (0.015) | (0.013) | (0.013) |
| Travel Distance | -0.012*** | -0.012*** | -0.019*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5348 | 5687 | 5348 | 5220 | 4900 | 5220 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C14: Business Influence Index Component 1–Female Ability to Bargain for Cheaper Business Input Prices

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.104** | 0.101** | 0.135*** | 0.153*** | 0.176*** | 0.196*** | 0.145*** | 0.166*** |
| | (0.051) | (0.049) | (0.047) | (0.046) | (0.042) | (0.041) | (0.035) | (0.034) |
| May Be or Definitely Able to Bargain for Cheap Prices | 0.061** | 0.061** | 0.060*** | 0.057*** | 0.050*** | 0.046*** | 0.022 | 0.018 |
| | (0.030) | (0.029) | (0.021) | (0.021) | (0.017) | (0.017) | (0.015) | (0.015) |
| VBT × May Be or Definitely Able to Bargain for Cheap Prices | 0.029 | 0.027 | 0.039 | 0.030 | 0.033 | 0.022 | 0.032 | 0.022 |
| | (0.042) | (0.040) | (0.038) | (0.038) | (0.036) | (0.035) | (0.031) | (0.030) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C15: Business Influence Index Component 2–Female Ability to Collect Business Debt

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.105* | 0.101** | 0.160*** | 0.175*** | 0.198*** | 0.215*** | 0.171*** | 0.189*** |
| | (0.054) | (0.051) | (0.047) | (0.046) | (0.043) | (0.042) | (0.036) | (0.034) |
| May Be or Definitely Able to Collect Debt | 0.054* | 0.058** | 0.073*** | 0.075*** | 0.054*** | 0.053*** | 0.040** | 0.039** |
| | (0.030) | (0.029) | (0.021) | (0.021) | (0.019) | (0.019) | (0.016) | (0.016) |
| VBT × May Be or Definitely Able to Collect Debt | 0.028 | 0.028 | 0.010 | 0.003 | 0.007 | -0.001 | 0.001 | -0.006 |
| | (0.042) | (0.040) | (0.037) | (0.037) | (0.035) | (0.034) | (0.031) | (0.030) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C16: Business Influence Index Component 3–Female Ability to Obtain Credit for Business

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.110** | 0.113** | 0.133*** | 0.153*** | 0.165*** | 0.187*** | 0.144*** | 0.166*** |
| | (0.045) | (0.044) | (0.042) | (0.042) | (0.039) | (0.038) | (0.032) | (0.031) |
| May Be or Definitely Able to Obtain Credit | 0.062** | 0.068*** | 0.048** | 0.053*** | 0.029* | 0.032* | 0.015 | 0.017 |
| | (0.024) | (0.025) | (0.020) | (0.020) | (0.017) | (0.017) | (0.015) | (0.015) |
| VBT × May Be or Definitely Able to Obtain Credit | 0.028 | 0.017 | 0.051 | 0.037 | 0.056* | 0.041 | 0.040 | 0.025 |
| | (0.034) | (0.033) | (0.032) | (0.032) | (0.031) | (0.030) | (0.027) | (0.027) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C17: Business Influence Index Component 4–Female Ability to Supervise Employees

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.102** | 0.099** | 0.129*** | 0.145*** | 0.173*** | 0.192*** | 0.136*** | 0.156*** |
| | (0.047) | (0.047) | (0.043) | (0.042) | (0.039) | (0.038) | (0.031) | (0.030) |
| May Be or Definitely Able to Supervise Employees | 0.062** | 0.067** | 0.046** | 0.047** | 0.029 | 0.028 | 0.009 | 0.008 |
| | (0.026) | (0.026) | (0.021) | (0.021) | (0.018) | (0.019) | (0.015) | (0.015) |
| VBT × May Be or Definitely Able to Supervise Employees | 0.041 | 0.038 | 0.057* | 0.047 | 0.044 | 0.033 | 0.052* | 0.039 |
| | (0.035) | (0.034) | (0.034) | (0.033) | (0.031) | (0.031) | (0.027) | (0.026) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C18: Business Influence Index Component 5–Female Ability to Manage Financial Accounts

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.106** | 0.102** | 0.142*** | 0.156*** | 0.174*** | 0.189*** | 0.147*** | 0.163*** |
| | (0.046) | (0.044) | (0.043) | (0.042) | (0.039) | (0.038) | (0.033) | (0.031) |
| May Be or Definitely Able to Manage Financial Accounts | 0.059** | 0.059** | 0.049** | 0.044** | 0.026 | 0.019 | 0.010 | 0.002 |
| | (0.024) | (0.023) | (0.020) | (0.020) | (0.018) | (0.018) | (0.015) | (0.015) |
| VBT × May Be or Definitely Able to Manage Financial Accounts | 0.038 | 0.037 | 0.041 | 0.034 | 0.046 | 0.039 | 0.038 | 0.030 |
| | (0.034) | (0.032) | (0.033) | (0.032) | (0.031) | (0.030) | (0.027) | (0.027) |
| Travel Distance | -0.011*** | -0.012*** | -0.019*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C19: Business Influence Index Component 6–Female Ability to Run A Business

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.079 | 0.082* | 0.096** | 0.119** | 0.136*** | 0.160*** | 0.121*** | 0.147*** |
| | (0.049) | (0.048) | (0.046) | (0.046) | (0.041) | (0.041) | (0.034) | (0.033) |
| May Be or Definitely Able to Run Business | 0.068** | 0.072** | 0.041* | 0.041* | 0.025 | 0.024 | 0.011 | 0.011 |
| | (0.028) | (0.028) | (0.023) | (0.023) | (0.020) | (0.020) | (0.018) | (0.018) |
| VBT × May Be or Definitely Able to Run Business | 0.063 | 0.052 | 0.090** | 0.074** | 0.086** | 0.069** | 0.065** | 0.047 |
| | (0.038) | (0.037) | (0.037) | (0.037) | (0.034) | (0.034) | (0.031) | (0.031) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | | | | | | | | |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C20: Business Influence Index Component 7–Female Influence over Sources of Household Borrowing

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.147*** | 0.143*** | 0.174*** | 0.182*** | 0.221*** | 0.227*** | 0.193*** | 0.199*** |
| | (0.044) | (0.043) | (0.044) | (0.043) | (0.039) | (0.038) | (0.034) | (0.032) |
| Infl. over Borrowing Choices | -0.036 | -0.017 | -0.019 | -0.006 | -0.012 | -0.003 | -0.017 | -0.006 |
| | (0.023) | (0.022) | (0.020) | (0.019) | (0.016) | (0.016) | (0.012) | (0.012) |
| VBT × Infl. over Borrowing Choices | -0.036 | -0.035 | -0.016 | -0.010 | -0.031 | -0.021 | -0.036 | -0.026 |
| | (0.028) | (0.027) | (0.031) | (0.030) | (0.029) | (0.028) | (0.026) | (0.025) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C21: Business Influence Index Component 8–Female Influence over Timing of Household Land Purchase

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.136*** | 0.131*** | 0.186*** | 0.191*** | 0.229*** | 0.233*** | 0.203*** | 0.208*** |
| | (0.044) | (0.043) | (0.044) | (0.043) | (0.038) | (0.037) | (0.033) | (0.032) |
| Able to Delay Land Purchase | -0.039* | -0.022 | -0.005 | 0.007 | -0.002 | 0.006 | -0.006 | 0.004 |
| | (0.022) | (0.022) | (0.019) | (0.019) | (0.015) | (0.015) | (0.012) | (0.012) |
| VBT × Able to Delay Land Purchase | -0.019 | -0.016 | -0.033 | -0.024 | -0.043 | -0.029 | -0.051* | -0.038 |
| | (0.028) | (0.027) | (0.030) | (0.029) | (0.028) | (0.027) | (0.026) | (0.025) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5687 | 5687 | 5687 | 5220 | 5220 | 5220 | 5220 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Regressions in Panel C also include an interaction term between Average Distance and female influence. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Acceptance to Submission observations change due to attrition in our followup survey. Moving from Submission to Enroll/Complete observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Table C22: Effect by Male Perception of Gender Equality

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.105** | 0.116*** | 0.139*** | 0.159*** | 0.190*** | 0.218*** | 0.170*** | 0.202*** |
| | (0.045) | (0.044) | (0.045) | (0.046) | (0.045) | (0.043) | (0.037) | (0.036) |
| STD PCA of Male Belief in Men's Superiority | 0.006 | -0.001 | -0.018 | -0.016 | -0.013 | -0.011 | -0.026*** | -0.030*** |
| | (0.014) | (0.015) | (0.012) | (0.013) | (0.011) | (0.012) | (0.010) | (0.010) |
| VBT × STD PCA of Male Belief in Men's Superiority | 0.005 | 0.006 | 0.034* | 0.029 | 0.034* | 0.029 | 0.051*** | 0.051*** |
| | (0.021) | (0.021) | (0.020) | (0.020) | (0.019) | (0.019) | (0.018) | (0.018) |
| Travel Distance | -0.012*** | -0.013*** | -0.022*** | -0.021*** | -0.020*** | -0.019*** | -0.016*** | -0.014*** |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) |
| Obs. | 2840 | 2543 | 2840 | 2543 | 2591 | 2311 | 2591 | 2311 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C23: Effect by Female Perception of Gender Equality

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.127*** | 0.131*** | 0.168*** | 0.187*** | 0.227*** | 0.254*** | 0.202*** | 0.232*** |
| | (0.044) | (0.045) | (0.044) | (0.046) | (0.042) | (0.041) | (0.036) | (0.035) |
| STD PCA of Female Belief in Men's Superiority | -0.005 | -0.012 | 0.001 | 0.003 | -0.000 | -0.000 | -0.014* | -0.012 |
| | (0.013) | (0.013) | (0.013) | (0.013) | (0.011) | (0.012) | (0.008) | (0.009) |
| VBT × STD PCA of Female Belief in Men's Superiority | 0.004 | 0.002 | 0.009 | 0.003 | 0.008 | 0.005 | 0.028* | 0.024 |
| | (0.018) | (0.018) | (0.019) | (0.019) | (0.018) | (0.019) | (0.016) | (0.018) |
| Travel Distance | -0.011** | -0.012** | -0.020*** | -0.019*** | -0.019*** | -0.017*** | -0.015*** | -0.013*** |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.003) | (0.004) | (0.003) | (0.003) |
| Obs. | 2947 | 2666 | 2947 | 2666 | 2679 | 2417 | 2679 | 2417 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C24: Effect by Male Migration Pattern

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.129*** | 0.130*** | 0.168*** | 0.187*** | 0.205*** | 0.227*** | 0.172*** | 0.194*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.027) |
| At Least 1 Male HH Member Away for 3 Months | 0.020 | 0.022 | -0.012 | -0.016 | -0.011 | -0.012 | -0.021 | -0.025 |
| | (0.032) | (0.032) | (0.023) | (0.024) | (0.024) | (0.024) | (0.020) | (0.021) |
| VBT × At Least 1 Male HH Member Away for 3 Months | 0.008 | 0.011 | 0.028 | 0.037 | 0.024 | 0.023 | 0.016 | 0.015 |
| | (0.041) | (0.042) | (0.041) | (0.040) | (0.041) | (0.040) | (0.035) | (0.035) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5687 | 5306 | 5687 | 5306 | 5222 | 4862 | 5222 | 4862 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C25: Effect by Female Migration Pattern

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.124*** | 0.125*** | 0.163*** | 0.182*** | 0.199*** | 0.220*** | 0.168*** | 0.189*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.026) |
| At Least 1 Female HH Member Away for 3 Months | -0.078 | -0.116 | -0.059 | -0.064 | -0.101*** | -0.106** | -0.106*** | -0.126*** |
| | (0.070) | (0.076) | (0.053) | (0.060) | (0.033) | (0.043) | (0.015) | (0.025) |
| VBT × At Least 1 Female HH Member Away for 3 Months | 0.141 | 0.213** | 0.176** | 0.225*** | 0.220*** | 0.274*** | 0.196*** | 0.237*** |
| | (0.092) | (0.094) | (0.086) | (0.084) | (0.073) | (0.078) | (0.075) | (0.083) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5715 | 5348 | 5715 | 5348 | 5247 | 4900 | 5247 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C26: Effect by Male Openness to Traveling for Work

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.076* | 0.081* | 0.100** | 0.112** | 0.142*** | 0.163*** | 0.142*** | 0.170*** |
| | (0.045) | (0.043) | (0.045) | (0.046) | (0.043) | (0.043) | (0.036) | (0.037) |
| Man Open to Traveling for Work | 0.057** | 0.055** | 0.004 | 0.004 | 0.014 | 0.017 | 0.016 | 0.018 |
| | (0.027) | (0.027) | (0.023) | (0.024) | (0.019) | (0.021) | (0.017) | (0.018) |
| VBT × Man Open to Traveling for Work | 0.050 | 0.059 | 0.083** | 0.101*** | 0.091** | 0.101*** | 0.068** | 0.071** |
| | (0.038) | (0.037) | (0.037) | (0.037) | (0.036) | (0.037) | (0.034) | (0.035) |
| Travel Distance | -0.014*** | -0.013*** | -0.022*** | -0.021*** | -0.021*** | -0.019*** | -0.016*** | -0.014*** |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| Obs. | 3401 | 3043 | 3401 | 3043 | 3107 | 2770 | 3107 | 2770 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C27: Effect by Female Openness to Traveling for Work

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.101** | 0.110*** | 0.130*** | 0.151*** | 0.182*** | 0.209*** | 0.173*** | 0.203*** |
| | (0.042) | (0.042) | (0.043) | (0.044) | (0.040) | (0.039) | (0.032) | (0.031) |
| Woman Open to Traveling for Work | 0.105*** | 0.130*** | 0.085** | 0.110*** | 0.071** | 0.091** | 0.070** | 0.084*** |
| | (0.039) | (0.042) | (0.041) | (0.041) | (0.034) | (0.036) | (0.030) | (0.032) |
| VBT × Woman Open to Traveling for Work | 0.006 | -0.002 | 0.034 | 0.018 | 0.075 | 0.068 | 0.032 | 0.011 |
| | (0.049) | (0.051) | (0.054) | (0.054) | (0.051) | (0.055) | (0.056) | (0.060) |
| Travel Distance | -0.013*** | -0.013*** | -0.022*** | -0.021*** | -0.020*** | -0.019*** | -0.016*** | -0.014*** |
| | (0.004) | (0.005) | (0.003) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| Obs. | 3548 | 3180 | 3548 | 3180 | 3237 | 2892 | 3237 | 2892 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Table C28: Effect by Female Demand for Skills

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.122*** | 0.119*** | 0.133*** | 0.155*** | 0.164*** | 0.185*** | 0.129*** | 0.149*** |
| | (0.041) | (0.040) | (0.039) | (0.039) | (0.034) | (0.035) | (0.029) | (0.030) |
| Very Likely to Enroll | 0.113*** | 0.089*** | 0.045** | 0.025 | 0.035** | 0.016 | 0.012 | -0.003 |
| | (0.021) | (0.022) | (0.020) | (0.020) | (0.017) | (0.017) | (0.015) | (0.016) |
| VBT × Very Likely to Enroll | 0.032 | 0.032 | 0.084*** | 0.070** | 0.099*** | 0.088*** | 0.104*** | 0.097*** |
| | (0.029) | (0.029) | (0.029) | (0.028) | (0.029) | (0.028) | (0.028) | (0.028) |
| Travel Distance | -0.011*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5692 | 5348 | 5692 | 5348 | 5225 | 4900 | 5225 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

### Heterogeneity by Perception of Safety

Table C29: Effect by Male Perception of Safety

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.109** | 0.124*** | 0.146*** | 0.177*** | 0.192*** | 0.228*** | 0.164*** | 0.202*** |
| | (0.045) | (0.043) | (0.046) | (0.045) | (0.046) | (0.044) | (0.039) | (0.038) |
| Male Doesn't Feel Safe | 0.072** | 0.099*** | 0.037 | 0.078* | 0.003 | 0.035 | -0.010 | 0.018 |
| | (0.033) | (0.037) | (0.036) | (0.042) | (0.025) | (0.029) | (0.025) | (0.029) |
| VBT × Male Doesn't Feel Safe | -0.029 | -0.049 | -0.043 | -0.094 | -0.006 | -0.040 | 0.046 | 0.013 |
| | (0.048) | (0.050) | (0.054) | (0.058) | (0.049) | (0.051) | (0.048) | (0.051) |
| Travel Distance | -0.013*** | -0.013*** | -0.022*** | -0.020*** | -0.021*** | -0.019*** | -0.016*** | -0.014*** |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.004) | (0.004) | (0.003) | (0.003) |
| Obs. | 2840 | 2543 | 2840 | 2543 | 2591 | 2311 | 2591 | 2311 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C30: Effect by Female Perception of Safety

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.116*** | 0.124*** | 0.156*** | 0.181*** | 0.216*** | 0.247*** | 0.191*** | 0.223*** |
| | (0.044) | (0.045) | (0.045) | (0.046) | (0.043) | (0.042) | (0.037) | (0.036) |
| Female Doesn't Feel Safe | -0.068* | -0.082* | -0.098*** | -0.086** | -0.086*** | -0.071** | -0.079*** | -0.073*** |
| | (0.037) | (0.043) | (0.034) | (0.037) | (0.029) | (0.031) | (0.023) | (0.026) |
| VBT × Female Doesn't Feel Safe | 0.093* | 0.111* | 0.081 | 0.069 | 0.085 | 0.069 | 0.094 | 0.099 |
| | (0.054) | (0.060) | (0.067) | (0.072) | (0.068) | (0.072) | (0.062) | (0.066) |
| Travel Distance | -0.011** | -0.012** | -0.020*** | -0.019*** | -0.019*** | -0.017*** | -0.015*** | -0.013*** |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.003) | (0.004) | (0.003) | (0.003) |
| Obs. | 2948 | 2667 | 2948 | 2667 | 2680 | 2418 | 2680 | 2418 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Heterogeneity by Socioeconomic Status

Table C31: Effect by Household Monthly Income – Continous Measure

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.123*** | 0.128*** | 0.162*** | 0.185*** | 0.200*** | 0.224*** | 0.170*** | 0.192*** |
| | (0.039) | (0.038) | (0.038) | (0.038) | (0.033) | (0.033) | (0.027) | (0.026) |
| STD Ln HH Monthly Income | -0.023** | -0.022** | -0.027*** | -0.030*** | -0.015** | -0.017** | -0.012** | -0.018*** |
| | (0.010) | (0.010) | (0.008) | (0.009) | (0.007) | (0.008) | (0.006) | (0.006) |
| VBT × STD Ln HH Monthly Income | 0.022 | 0.036** | 0.013 | 0.025* | 0.001 | 0.012 | 0.010 | 0.021* |
| | (0.014) | (0.014) | (0.013) | (0.014) | (0.012) | (0.013) | (0.011) | (0.012) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5692 | 5348 | 5692 | 5348 | 5225 | 4900 | 5225 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C32: Effect by Household Monthly Income – Binary Measure

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.147*** | 0.157*** | 0.190*** | 0.221*** | 0.225*** | 0.257*** | 0.195*** | 0.223*** |
| | (0.040) | (0.039) | (0.039) | (0.039) | (0.033) | (0.034) | (0.028) | (0.028) |
| HH Income in Village's Bottom 30% | 0.019 | 0.013 | 0.028* | 0.032** | 0.025* | 0.026* | 0.021* | 0.027** |
| | (0.019) | (0.020) | (0.015) | (0.016) | (0.014) | (0.014) | (0.012) | (0.013) |
| VBT × HH Income in Village's Bottom 30% | -0.055** | -0.072** | -0.067*** | -0.091*** | -0.062*** | -0.084*** | -0.062*** | -0.077*** |
| | (0.027) | (0.028) | (0.023) | (0.024) | (0.023) | (0.026) | (0.023) | (0.024) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5692 | 5348 | 5692 | 5348 | 5225 | 4900 | 5225 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C33: Effect by Household Asset Index – Binary Measure

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.136*** | 0.139*** | 0.187*** | 0.211*** | 0.226*** | 0.252*** | 0.198*** | 0.221*** |
| | (0.039) | (0.039) | (0.038) | (0.039) | (0.033) | (0.034) | (0.027) | (0.027) |
| HH Asset Index in Village's Bottom 30% | 0.002 | -0.013 | 0.025 | 0.026 | 0.020 | 0.019 | 0.014 | 0.015 |
| | (0.017) | (0.018) | (0.016) | (0.017) | (0.015) | (0.017) | (0.012) | (0.014) |
| VBT × HH Asset Index in Village's Bottom 30% | -0.033 | -0.026 | -0.071*** | -0.073*** | -0.077*** | -0.083*** | -0.086*** | -0.087*** |
| | (0.025) | (0.025) | (0.025) | (0.026) | (0.026) | (0.027) | (0.025) | (0.026) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.018*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5692 | 5348 | 5692 | 5348 | 5225 | 4900 | 5225 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C34: Effect by Education Status

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.137*** | 0.134*** | 0.178*** | 0.188*** | 0.212*** | 0.227*** | 0.184*** | 0.199*** |
| | (0.038) | (0.038) | (0.038) | (0.038) | (0.034) | (0.033) | (0.027) | (0.026) |
| Has Never Been to School | 0.119*** | 0.136*** | 0.046* | 0.058** | 0.031 | 0.046** | 0.009 | 0.025 |
| | (0.033) | (0.033) | (0.025) | (0.026) | (0.020) | (0.022) | (0.017) | (0.019) |
| VBT × Has Never Been to School | -0.097** | -0.091** | -0.055 | -0.042 | -0.049 | -0.041 | -0.051 | -0.046 |
| | (0.041) | (0.040) | (0.039) | (0.040) | (0.036) | (0.037) | (0.031) | (0.033) |
| Travel Distance | -0.013*** | -0.014*** | -0.020*** | -0.020*** | -0.017*** | -0.017*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5872 | 5348 | 5872 | 5348 | 5392 | 4900 | 5392 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Heterogeneity by Ethnic Diversity

Table C35: Effect by Standardized Ethnic Fragmentation Index

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.130*** | 0.129*** | 0.169*** | 0.186*** | 0.204*** | 0.225*** | 0.172*** | 0.193*** |
| | (0.038) | (0.038) | (0.037) | (0.038) | (0.033) | (0.033) | (0.026) | (0.026) |
| STD Ethnic Fragmentation Index | -0.017 | -0.014 | 0.021 | 0.020 | 0.020* | 0.020* | 0.013 | 0.012 |
| | (0.019) | (0.019) | (0.013) | (0.014) | (0.011) | (0.012) | (0.008) | (0.009) |
| VBT × STD Ethnic Fragmentation Index | -0.010 | -0.007 | -0.045** | -0.038* | -0.046** | -0.039* | -0.036** | -0.030* |
| | (0.023) | (0.023) | (0.022) | (0.022) | (0.021) | (0.022) | (0.016) | (0.017) |
| Travel Distance | -0.011*** | -0.012*** | -0.019*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

Table C36: Effect by Standardized Ethnic Difference Index

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.116*** | 0.118*** | 0.161*** | 0.179*** | 0.196*** | 0.218*** | 0.163*** | 0.185*** |
| | (0.037) | (0.037) | (0.037) | (0.038) | (0.032) | (0.032) | (0.026) | (0.026) |
| STD Inter-village Ethnic Difference Index | -0.042*** | -0.045*** | -0.004 | -0.008 | -0.000 | -0.004 | 0.000 | -0.005 |
| | (0.012) | (0.013) | (0.009) | (0.010) | (0.009) | (0.011) | (0.007) | (0.008) |
| VBT × STD Inter-village Ethnic Difference Index | -0.001 | 0.007 | -0.019 | -0.007 | -0.026 | -0.016 | -0.027* | -0.021 |
| | (0.021) | (0.022) | (0.023) | (0.023) | (0.020) | (0.021) | (0.016) | (0.017) |
| Travel Distance | -0.012*** | -0.012*** | -0.020*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5758 | 5246 | 5758 | 5246 | 5282 | 4802 | 5282 | 4802 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Heterogeneity by Village Connectivity

Table C37: Effect by Availability of Transport Facilities

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.153*** | 0.148*** | 0.193*** | 0.209*** | 0.225*** | 0.245*** | 0.193*** | 0.211*** |
| | (0.048) | (0.047) | (0.047) | (0.047) | (0.042) | (0.042) | (0.033) | (0.033) |
| Availability of Transport Facilities | 0.048 | 0.045 | 0.040 | 0.035 | 0.040* | 0.036 | 0.052*** | 0.045** |
| | (0.038) | (0.037) | (0.029) | (0.030) | (0.023) | (0.024) | (0.018) | (0.019) |
| VBT × Availability of Transport Facilities | -0.045 | -0.037 | -0.045 | -0.043 | -0.040 | -0.040 | -0.042 | -0.037 |
| | (0.047) | (0.047) | (0.046) | (0.046) | (0.042) | (0.043) | (0.035) | (0.036) |
| Travel Distance | -0.012*** | -0.012*** | -0.019*** | -0.019*** | -0.017*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

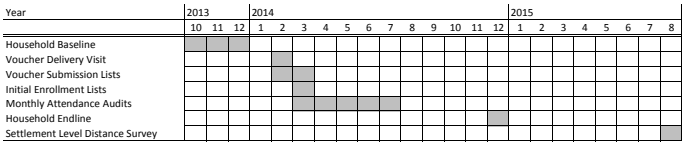
Table C38: Effect by Availability of Non-transport Facilities

| | Voucher Acceptance | | Voucher Submission | | Class Enrollment | | Class Completion | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Village Based Training | 0.186*** | 0.184*** | 0.187*** | 0.206*** | 0.233*** | 0.253*** | 0.200*** | 0.216*** |
| | (0.047) | (0.047) | (0.051) | (0.050) | (0.044) | (0.043) | (0.036) | (0.035) |
| Availability of Non-transport Facilities | 0.081** | 0.082** | 0.029 | 0.026 | 0.043* | 0.041 | 0.040** | 0.033* |
| | (0.038) | (0.037) | (0.031) | (0.031) | (0.024) | (0.025) | (0.018) | (0.019) |
| VBT × Availability of Non-transport Facilities | -0.097** | -0.095** | -0.029 | -0.032 | -0.049 | -0.048 | -0.047 | -0.038 |
| | (0.048) | (0.048) | (0.050) | (0.049) | (0.045) | (0.044) | (0.038) | (0.038) |
| Travel Distance | -0.012*** | -0.013*** | -0.019*** | -0.019*** | -0.018*** | -0.016*** | -0.013*** | -0.012*** |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) | (0.002) | (0.002) |
| Obs. | 5873 | 5348 | 5873 | 5348 | 5393 | 4900 | 5393 | 4900 |
| Controls | | X | | X | | X | | X |

*Notes*: OLS regressions of uptake variables on treatment and distance. Group Transport dummy and Average Distance control included in all specifications. Controls include other treatment dummies, stipend amount dummies, stipend bucket dummies, household assets, and household income. Travel Distance is the measured distance from the population centroid of the village to the training center. Within outcomes observations change due to missingness in control variables. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. Standard errors clustered at the village level reported in parentheses.

## Appendix E: Data Sources

Figure E1: Time of Data Collection Activities




- **Household Baseline Survey:** During this initial visit households were provided with course and training booklets in order to inform them about the training program. Additionally, Each household was given a survey to collect pre-treatment demographic characteristics of households, as well as solicit nominations from each household for a member to receive training. Additional questions were asked of nominated individuals concerning their demographic characteristics, as well as questions related to their previous experience with stitching. We also recorded the Geo-coordinates of each household, which allows us to measure the straight-line distance from the house to the nearest training center.

- **Voucher Delivery Visit:** After treatment activities had been concluded, we revisited each household to deliver training vouchers to the respondent nominated in the baseline survey. During this visit each household was given a short survey through which we recorded whether or not they accepted their voucher, our first measure of take-up. Respondents were also asked about the various treatment activities that had occurred in their village in order to ensure that activities had been properly carried out and advertised. Households that wished to switch their nominated member were allowed to do so at this point. For these households an additional baseline survey was conducted with the new nominated member to collect their pre-treatment demographic characteristics.

- **Voucher Submission Lists:** Households that accepted their voucher were told to submit their vouchers within a two-week time frame to their training center of choice. A list of all submitted vouchers was then given to us by each training center. These lists give us our second measure of take-up, voucher submission.

- **Initial Enrollment Lists:** For training centers which had more applicants than they could accept, we conducted a random ballot to generate enrollment rosters and wait-lists. For the first 12 days of class each training center provided us with a student attendance list. Admitted students who were not attending class were removed from the roster, and those on the wait-list were admitted. Each day we contact newly admitted students and sent the training centers an updated roster in order to ensure the wait-list order was properly followed. These detailed lists not only allow us to track which were respondents were admitted through the ballot, but also track those respondents who ultimately chose to enroll. This is our third measure of take-up.

- **Monthly Attendance Audits:** After class rosters had been finalized through the process detailed above, we performed monthly audits to track student attendance. This was primarily done to ensure that only those students who were still attending class received their stipends, but it also useful for us as a means of tracking which respondents completed the course (our final measure of take-up).

- **Household End-line Survey:** Five months after all training activities had ended we revisited each household to administer a final end-line survey. The main purpose of this survey was to gather updated information of respondents post-treatment demographic characteristics, which will be used in another study to measure the impact of the training program. However, we also used to opportunity to ask respondents about each of their take-up statuses. We use this information to confirm the statuses determined from the administrative data gathered above.

- **Settlement Level Distance Survey:** The survey was conducted aiming to measure distance from households' location to the closest training centers accounting for the actual routes used to travel between villages. Households were grouped into pre-existing geographic clusters called settlements, and a map was then made of each village demarking these settlements. Routes were then traced on each map for all means of transport: private modes (walk, cycle, motorcycle, qingchi and car), public modes (bus, qingchi and motorcycle) and group transport. Refer to Figure 5 below for an example of a map.

  Following the paths marked on the maps, enumerators measured the distance from each settlement to the training center using a motorcycle and an odometer. However, when there was evidence that the route taken using a motorcycle would differ from the one using another private mode, we also computed the distance for that specific mean of transport.

  The approach to calculate distance varied depending on the mean of transport and the type of settlement. Three types of settlements were identified: settlements within a VBT village that contained the training center (special settlements); settlements that did not host a training center and belong to a VBT village (non-special settlements); and settlements from nVBT villages.

1. Special settlements: To measure the distance to the center location by private transportation, the enumerators selected four random and geographically dispersed households in the settlement and measured their distance to the training center. The settlement-level distance consisted in the average of these four distances. As these settlements hosted the training center, there was no public transport needed and hence no corresponding measure of distance.

2. Non-special settlements: Distance by private transportation is measured from the settlement boundary to the training center of the village. In the case of public transportation, we calculated the distance in tranches: i) first connecting route: settlement boundary to the nearest bus/motorcycle/qingchi stop; ii) route taken by bus/motorcycle/qingchi to the drop off point; and iii) second connecting route: from the drop off point to the training center.

3. Settlements from nVBT villages: Distance by private transportation was calculated in tranches and then added up: i) from the settlement boundary to the boundary of the nVBT village where the settlement is in, ii) from the nVBT village boundary to a VBT village boundary ; and iii) from the VBT village boundary to the training center. In a similar manner, distance by public transportation consists of the sum of three legs: i) first connecting route: settlement boundary to the nearest bus/motorcycle/qingchi stop, ii) route taken by bus/motorcycle/qingchi to the drop off point, iii) second connecting route: from the drop off point to the training center. For Group Transport, we calculated two tranches and then added them up: i) connecting route: settlement boundary to the pick-up point in the village; and ii) route taken by the Group transport provider from the pick-up point to the training center.

To get a better sense of transportation costs, we calculated the cost of fuel and the fare for using each mean of public transport.[33] We also estimated the time cost of commuting by converting the distance into time terms for each mode of transport. In the case of public transport time calculations, we included estimates of waiting times at bus stops.

Figure E2: Map for Cluster-level Distance Survey

[33]We calculated the cost of fuel by asking to the nearest fuel supplier in each village. To estimate the fare for each public transport (bus, qingchi and motorcycle) we asked the corresponding driver about the fare for one-way relevant segment of the journey.