Glass Walls: Experimental Evidence on Constraints faced by Women in Accessing Valuable Skilling Opportunities

Ali Cheema, Asim I. Khwaja, M. Farooq Naseer, Jacob N. Shapiro*

February 23, 2025

Abstract

Experimental evidence from Pakistan shows distance poses a large and discontinuous access constraint: women with village-based training centers are four times more likely to access valued skilling opportunities. Over half the travel penalty is incurred upon crossing the village boundary. Exogenous stipend variation reveals this boundary effect is costly to offset and not explained by travel costs. Security considerations are an important factor: providing secure group transport raises take-up, while women with greater safety concerns and those traversing underpopulated areas, a proxy for insecurity, have lower take-up. The training has similar positive benefits for women with inside- and outside-village centers.

*cheema@lums.edu.pk; khwaja@hks.harvard.edu; farooqn@lums.edu.pk: jns@princeton.edu. We thank Rosemary Berberian, Olivia DaDalt, Ismail Khan, Yanchuan Liu, Kunal Mangal, German Orbegozo, Guillermo Palacios Diaz, Nikolai Schaffner, Landin Smith, and Gabriel Tourek for outstanding research assistance in Cambridge. We thank Zain Abbasi, Shahkar Ali, Danya Arif, Minahil Asim, Alaiba Faheem, Ahsan Zia Farooqi, Rehan Hasan, Hafsa Iqbal, Maheen Jahangir, Tanya Javed, Ibrahim Khan, Mahnoor Khan, Arqam Lodhi, Wafa Masood, Ayesha Mursleen, Hassan Murtza, Anum Mustafa, Rania Nasir, Minahil Niazi, Shaiza Qayyum, Nouman Rasool, Minahil Raza, Zara Riaz, Sarah Shaukat, Sahaab Sheikh, Anam Shoaib, Sameem Siddiqui, and Ahmed Sultan for outstanding research and field assistance in Lahore. We thank Faelynn Carroll and Yining Sun for outstanding research assistance in Princeton. We are grateful to Jawad Khan, Ali Sarfraz, Ali Akbar Bosan, Shoaib Mohsin, Syed Farrukh Jafri and PSDF Board member for their support. We thank seminar participants at the AALIMS-Princeton Conference, CHW/RPDS-Princeton Conference, IDRC GrOW Conference, IZA/DfID 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 research was supported by DfID Pakistan, DfID and IZA's GLM LIC program, 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 acknowledged above.

1 Introduction

An important feature of many welfare programs is to provide economic opportunities for the underprivileged. This is often done through cash transfers, employment services, and skills enhancement programs. These policies are often specifically directed towards those who have been historically excluded from state benefits—the poor, rural inhabitants, and women. But for these programs to succeed, individuals need to be able to access the benefits offered. Yet such access concerns are at times under-emphasized due to the assumption those who value the program should want and be able to access it, especially if their travel costs are compensated. However, in practice, we often see "money left on the table," even by the needy. Studies document how villagers do not obtain subsidized rice, widows fail to take advantage of monthly stipends, and women are unable to obtain vocational training, despite the large gains such programs may have (Dasgupta et al. 2015; Gupta 2017; Maitra and Mani 2017; Banerjee, Hanna, et al. 2018; Bandiera et al. 2020).

This paper uses experimental variation to estimate the nature and cost of one common access constraint—travel that requires a woman to move outside her community—and to compare the program benefits obtained by women who are able to overcome this barrier to those women who are not.[1] Specifically, we study a skills program in rural Pakistan, where women were offered free training in tailoring in response to a demand expressed by a large majority (74 percent) in program villages (Cheema, Khwaja, Naseer, and Shapiro 2012a). Working with the implementing agency and intended beneficiaries allowed us to experimentally introduce a series of interventions that test for and potentially ameliorate distance-related access barriers.[2]

We have four key results. First, we find that mobility barriers significantly limit women's access to labor market opportunities. Second, we demonstrate there is a large "village boundary effect" that is costly to compensate for financially. We argue that therefore a key compo-

[1]In the same way as "glass ceiling" is a metaphor commonly used to describe systemic obstacles that may prevent women from rising up the career ladder (BBC 2017; Bertrand 2018), this paper's title "Glass Walls" is used to refer to the systemic barriers that make it costly for women to travel beyond their own communities. The word "glass" by no means implies that these barriers are not real or tangible, but rather that they may not always be given the recognition they warrant.

[2]Causally identifying distance-related access barriers is a challenge because the locations where benefits are accessed are likely endogenous to confounding factors. Even if one could accurately identify the presence of access barriers, additional assumptions are needed in observational studies to capture the economic significance of such barriers and shed light on what factors may underlie them. Similarly, to the extent that individuals self-select into programs, estimating and comparing the causal impact of a program for those facing differing access barriers also poses an empirical challenge.

nent of these access constraints is the non-financial costs women face in (even temporarily) leaving their community. Third, we show that safety concerns likely underlie these access barriers. Fourth, we estimate the impact of the training program and find that these constraints select out women who would have benefited equally from training, compared to those women who were able to leave their village for training. We now detail each result further.

Our first main result documents the presence of a large distance-driven access barrier across a range of program take-up measures—from initial interest in applying to course enrollment and completion. Specifically, we provide a precise and causally identified estimate of distance-related access constraints by randomly allocating training centers to 108 treatment villages out of a study sample of 324 villages randomly drawn from the program regions, which generates exogenous variation in the distance between a trainee's home and the training center.[3] We find that establishing a training center in the village increases course enrollment and completion *fourfold* compared to simply offering a voucher and stipend.

Second, controlling for the direct effect of distance traveled, we show that half of the access difference between in-village and out-of-village training is generated by crossing the village boundary. This strong "boundary effect" is hard to reconcile with standard travel costs: crossing the village boundary does not induce a discontinuous jump in either distance or time; there is no formal village border one has to wait to cross, nor any toll paid at entry or exit, or other fixed costs. Furthermore, while the boundary effect also holds *within* the village for women who have to move between different settlements inside the village, there is no evidence of additional boundary effects after the first village boundary is traversed. By experimentally varying the stipend amount provided, we can also directly assess the financial compensation needed to overcome these constraints (both the boundary effect and per-kilometer travel costs) and show they are much larger than standard travel and opportunity cost of time estimates. We estimate the equivalence by comparing the increase in take-up induced by an additional stipend with the distance penalties. We find that a monthly stipend of PKR 6-8,000 is required to induce the average woman in our sample to attend training outside their village. Fully PKR 3,500-5,000 (51 percent of monthly household expenditure and 45 percent of monthly household income) of this is needed just to overcome the boundary effect—a sizable amount, especially since the course itself is free and valued. These compensatory amounts are estimated to be three times the (generous) estimates of additional travel costs (fare plus opportunity cost of travel and wait time) that the median woman in our sample would have to incur in traveling outside her village for the training.

[3]While the training centers were located in certain villages, any woman from neighboring villages could also apply for a place in the program.

Third, to shed light on underlying factors that might contribute to access constraints we estimate the causal impact of a range of interventions designed to increase program take-up — including additional trainee engagement, community engagement, and safe transport for trainees — and leverage non-experimental variation as well. We begin this analysis by examining how three additional interventions impacted take-up: a trainee engagement (TE) intervention that provided augmented information about the training to potential trainees in a group setting; a community engagement (CE) intervention that added to the TE intervention by also engaging male household members and community elders in a group setting to discuss and facilitate women's enrollment; and a group transport (GT) intervention that organized safe transport for women who did not have a training center inside their village. Despite TE and CE substantially increasing potential trainee's knowledge about the training as well their expectations regarding its quality, there is little impact on eventual program take-up. In contrast, we find a sizable effect of the GT intervention. Between one-half and two-thirds of the boundary effect can be compensated for by offering women secure group transport to the (nearest) training center outside their village.

We then combine these experimental results with additional observational variation to assess four distinct factors that may induce access barriers, especially the boundary effect: (i) program information and salience; (ii) peer and network effects; (iii) safety and security concerns; and (iv) transportation constraints. Of the four we find strong evidence for perceived safety and security considerations on two fronts. First, women who report feeling unsafe are less likely to cross a village boundary, while such safety perceptions do not impact take-up when the training is offered within the village. Second, leveraging geospatial data on population density along the (exogenous) travel paths shows that participation is lower for women who have to cross relatively underpopulated spaces (a proxy for perceived insecurity associated with commuting) to access training, and that such spaces account for a meaningful share of the boundary effect (20-40 percent depending on the estimation approach and take-up stage). The large effect associated with providing secure group transport noted above is consistent with perceived safety as a constraint.

In contrast, we find little evidence in support of the other three potential factors. One may posit that women would have more information about the course, or that it would be more salient when the training is offered inside the village. However, we test for and find no evidence of women holding differential information when the training is inside the village versus outside it. In fact, as noted above, while the TE and CE interventions generated more knowledge and raised quality perceptions of the course, they did not ameliorate the boundary effect.

One may also conjecture that training inside the village generates greater take-up because of positive network dynamics among women within these villages. We implement two tests using additional experimental variation, neither of which finds evidence for such network effects. First, an additional randomization explicitly offered peer-based incentives by providing vouchers and stipends to both a woman and her neighbor (and informing them about each other's offer). This additional "peer" intervention had no discernable effect on take-up. Second, we take advantage of the randomized stipend variation at the village level to instrument for the number of women in one's own village who took up the training. We find that the number of women who took up training in their village has no effect on their own take-up decision, nor does its inclusion affect the estimate of the boundary effect.

Finally, we examine a potential transportation services effect. Specifically, discontinuous transportation costs associated with leaving one's village to attend training may also generate boundary effects. Data from an extensive transport mapping exercise conducted in each village does not show a village boundary effect on the availability, wait times, or fares paid for the three most used public modes of transport.

Importantly, while there are no discontinuities observed in transport options for traveling inside versus outside the village, we do find that boundaries matter for what mode of travel women would prefer to use. Conditional on the distance they expect to travel, women are less likely to report walking if the course is outside their village. This provides further evidence of safety concerns associated with traveling outside the village.

Taken together, these findings suggest that the individual and societal concerns regarding safety triggered when women seek to move outside their communities can severely limit their mobility. While safety is likely not the only consideration driving the distance and boundary effects, the evidence suggests that it is a particularly important one in this context. In contrast, there is little evidence to support informational, social/network, or standard cost of travel-related factors. Encouragingly, our findings do suggest that such barriers can be partly ameliorated through a safe group transport intervention.

Finally, our fourth main result shows that access constraints select out women who would have equally benefited from training. Specifically, using experimental variation, we compare the causal impact of the training on a range of outcomes for women who access the training when it is offered inside their village versus the training impact for those (far fewer) women who can access the training when it is offered outside their village. We estimate the training returns for these populations by comparing tailoring activity and income in our treatment sample with a set of pure control villages. Using the randomized training and stipend offers to instrument for training completion, we find that trained women significantly increase their tailoring activities, with a more than five-fold increase in clothes stitched, and report better tailoring skills. They also increase earnings from tailoring by over 9.5 times (an additional PKR 300 on a base of PKR 35 over a three-month period), and show a 23 percentage point increase in the likelihood of owning a sewing machine. These effects are sustained over time: holding over six, eighteen, and thirty months after the training. Importantly, these effects are positive and statistically significant for *both* in-village trainees who did not have to overcome boundary constraints, and (the far fewer) outside-village trainees who did. Further, 'VBT Compliers', i.e., women who are unable to overcome the distance barriers to traveling outside their village and enroll if and only if the training is offered within their village do not get lower economic returns. Since we examine a range of outcomes in this section, we show the results are robust to adjusting for multiple hypothesis testing.

Our paper contributes to a literature that studies the role of distance as a barrier that prevents marginalized groups from accessing valued socioeconomic opportunities.[4] By experimentally varying distance, we complement the limited set of studies that provide causally identified estimates of distance's adverse effects on a range of outcomes, including take-up of cash transfers (Alatas et al. 2016), schooling (Burde and Linden 2013) and medical services (Thornton 2008). We further this literature by providing the first experimental evidence and estimate of a village boundary effect as well as the financial compensation required to offset it. Our results confirm the existence of spatial non-linearities in access documented in studies that use non-experimental and qualitative methods (A. Khan 1999; Gazdar 2003; Mumtaz and Salway 2005; Thakuriah, Tang, and Menchu 2011; Jacoby and Mansuri 2015).

We should also note that in contrast to Thornton (2008), who finds that barriers in their context are overcome by small cash incentives, our results show that sizeable stipends are needed to address these constraints–far larger than the amounts required to compensate for travel and the opportunity cost of time. This suggests that the efficacy of financial incentives as a tool is contingent on the shape and size of costs. It also underscores that careful empirical work is needed to reveal the nature of costs underpinning distance-related barriers within specific contexts and domains.

Our work also complements a rich experimental literature that tests the impact of different interventions in alleviating access constraints. It adds to the literature that examines the underlying factors that may contribute to distance-related access barriers faced by women.

[4] Studies show that distance impacts take-up in a diverse set of services that include medical care (Thornton 2008), banking (Ashraf, Karlan, and Yin 2006), schooling (Burde and Linden 2013; Jacoby and Mansuri 2015; Muralidharan and Prakash 2017; Fiala et al. 2022) and vocational training (Cho et al. 2013; Maitra and Mani 2017).

Here we touch on three broad areas that have received attention in the literature:

(i) Information frictions have been been highlighted as important factors in schooling and labor market decisions (see Nguyen 2008; Jensen 2010; Hoxby and Turner 2015; Wiswall and Zafar 2015). In contrast, we find that while the TE intervention augmented information and led to improved knowledge and quality perceptions about the course, it does not increase take-up or ameliorate the boundary effect. This points to the binding nature of non-informational factors as barriers.

(ii) Social barriers associated with community and household gatekeepers have been recognized as constraining women's access in contexts, such as ours, where elders and/or males are pivotal decision-makers and control resources that are necessary for women's participation.[5] There is a growing experimental literature that finds positive impacts of interventions that influence male attitudes and behavior to enable women's participation.[6] Other studies have highlighted the potential of seeding information through central nodes in community networks (Banerjee, Chandrasekhar, et al. 2019). Our CE intervention speaks to this literature as it effectively diffused information to community elders and male household members and successfully engaged them in group discussions to facilitate women's enrollment. However, we find that this intervention does not positively impact take-up. Moreover, we also do not find support for social dynamics such as those generated through positive peer effects (at the neighbor or village level) playing an important role.

(iii) Safety and security considerations have been highlighted as limiting women's mobility (Hsu 2010; Mitra-Sarkar and Partheeban 2011; Porter et al. 2011; Borker, Kreindler, and Patel 2020; Evans et al. 2021; Borker 2022; Fiala et al. 2022), often due to the risk of harassment and violence that one may face when taking public transport and in public spaces (Allen, Vanderschuren, and University of Cape Town 2016; Kondylis et al. 2020; Aguilar, Gutiérrez, and Villagrán 2021). Our findings on lower take-up for women who perceive safety concerns or have to traverse under-populated areas provide direct evidence on how security considerations can constrain valuable opportunities. We also contribute to a related literature that highlights the importance of safe and reliable transport for women (Muralidharan and Prakash 2017; Borker, Kreindler, and Patel 2020; Kondylis et al. 2020; Aguilar, Gutiérrez, and Villagrán 2021). Our study, along with Fiala et al. (2022) and Field and Vyborny (2022)'s work, provides the first experimental estimates of the impact of safe and secure transport for women on their educational and labor market outcomes. We add to these studies by con-

[5] See Field, Jayachandran, and Pande (2010), Jayachandran (2015), Thomson (2015), Bernhardt et al. (2018), and Cheema, S. Khan, et al. (2023)

[6] See Bursztyn, González, and Yanagizawa-Drott (2020) and Cheema, S. Khan, et al. (2023)

sidering impact on a range of important downstream outcomes including course completion, employment, and earnings. Our results on group transport also nuance our earlier findings about gatekeepers; while engaging males and elders in general discussions has no effect, doing so reveals a specific, but critical, constraint – secure group transport – has large effects on women's take-up.

Finally, our paper directly contributes to the understanding of the take-up and impact of vocational skills training programs. It confirms that addressing take-up in program design has the potential to dramatically increase the overall impact on target populations. [7] We also contribute to a large experimental literature that examines the economic impact of skills training programs in developing countries.[8] Our paper shows a sustained positive impact of training on tailoring outcomes more than two years after training.

Importantly, to our knowledge, ours is the first paper to rigorously estimate the causal impact of training for two different groups of women who both come from the same underlying population yet face differential access barriers. Most causal studies implement an over-subscription design where the training opportunity is randomized within the set of individuals who have applied for training. While these estimates are internally valid and important, they leave open the question as to how the training would have impacted a different group of individuals in the population.[9] This is an especially pertinent question in our context, given the significant access barriers we identify for most women in our study. Since we randomized training center placement, we are able to show that the average causal effect of skills training is similar for both in-village and (the far fewer) outside-village trainees as well as the additional 'marginal' trainees induced to get skills training when the facility is opened inside their village. Notably, in our setting, program administrators do not need to worry about potential trade-offs between raising take-up and lowering impact on the usual success metrics, as noted in the concerns about "cream-skimming" in the context of US job training programs (Anderson, Burkhauser, and Raymond 1993; Heckman and Smith 2004).

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.

[7] Studies that track self-selected applicants show course completion rates ranging from 21 percent to 95 percent, while those that consider enrollment in the general population find take-up rates from as low as five percent to 21 percent (Sandell and Rupp 1988; Bloom 1997; Maitra and Mani 2017; Bandiera et al. 2020).

[8] See McKenzie (2017) for a review of experimental evaluations of active labor market programs in developing countries (including twelve studies on vocational training programs).

[9] While several studies measure treatment effect heterogeneity, they use non-experimental variation observed within their samples and do not address the possibility that a specific subset of the target population may be systematically missing from the evaluation sample.

2 Context and Intervention

2.1 Country Context

Women in Pakistan face significant barriers to accessing education and employment opportunities and limited mobility due to socially conservative norms and safety concerns. These societal and individual considerations limit their labor force participation and agency (Naqvi, Shahnaz, and Arif 2002; World Bank Group 2022); 57.6 percent of women in Pakistan do not have primary education and only 24.5 percent of women above the age of 15 work (World Bank Group 2019; World Bank Group 2020).

The literature on Pakistan also highlights barriers to women's physical mobility as an important access constraint. High rates of harassment and violence faced by women in public spaces and the ensuing perceptions of insecurity and fear are argued to be important factors inhibiting women's mobility (Punjab Commission on the Status of Women 2018; World Bank Group 2022; Field and Vyborny 2016). These barriers are exacerbated by gender inequality in control over transport within households (World Bank Group 2019; Haerpfer et al. 2022; World Bank Group 2023), limited safe and reliable public transport options for women, and social norms that require women to seek permission from men before leaving their home for work or social reasons (Field and Vyborny 2022; World Bank Group 2022; Cheema, S. Khan, et al. 2023). While women may be more mobile within their villages, mobility outside villages remains constrained by safety concerns, limiting their access to opportunities (Gazdar 2003; Mumtaz and Salway 2005; Jacoby and Mansuri 2015).

Pakistan is not unique in these gender constraints. Appendix Figure B1 shows how Pakistan compares to other countries in MENA and South Asia along measures of female education, employment, and agency from the World Bank Gender Indicators and measures of public safety from the World Values Survey. While Pakistan is behind its regional counterparts in girls' attendance and completion of primary school, indicators of women's employment and agency tell a different story: the female labor force participation rate and the employment-to-population ratio in Pakistan are slightly above that of India and Saudi Arabia (World Bank Group 2019). Adolescent fertility rate and women's mobility score, indicators for women's agency, also place Pakistan near the regional median.[10] Cross-country comparisons on areas

[10] This score is composed of four individual indicators of women's mobility, each given 25 points and scaled to 100 to create the Mobility Indicator Score. The indicators are 1) A woman apply for a passport in the same way as a man; 2) A woman can travel outside the country in the same way as a man; 3) A woman can travel outside her home in the same way as a man; and 4) A woman can choose where to live in the same way as a man (World Bank Group 2023).

like violence against women, show that Pakistan is not an outlier. Pakistan is in the middle range for reports of sexual harassment and street violence among regional counterparts and large majority-Islamic states (Haerpfer et al. 2022).

2.2 Program Background

The skills training program we study was implemented by the Pakistan Skills Development Fund (PSDF), a not-for-profit company set up as part of the Punjab Economic Opportunities Program (PEOP)—a partnership between the Government of Punjab and the UK Department for International Development (DfID) that aimed to increase the employability and earnings of low-income and vulnerable families by augmenting human capital through vocational training.

The program design was informed by prior work by the Center for Economic Research in Pakistan (CERP) that revealed low take-up rates for vocational courses, especially for women (see Appendix A for details). Such low take-up was surprising given the high reported demand for training—over 90 percent of the households nominated a female member who wanted to receive the training—as well as strong expectations that this training would lead to increased skills and returns. This suggests that women were likely facing barriers to realizing their demand and that these access barriers were costly, which makes understanding and alleviating these constraints important.

An important focus in this paper is therefore exploring the unique constraints presented by distance-related barriers to *women's* mobility and skills acquisition. Rather than compare differences in access across women and men, we felt it would be more instructive to compare women who experience different (and experimentally induced) constraint alleviation strategies. This allowed us to better isolate the mechanisms that may be behind these effects by holding constant unobservable variables that are common among women (but may be different for men).[11] This decision is also informed by findings from our prior work and from the literature which reports the existence of large distance-related penalties for women but finds

[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, it is informative to use 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 range of design variations for women skill building programs where each variation is designed in consideration of an underlying factor/mechanism. Within a fixed budget, there is an inherent trade-off between going deeper into mechanisms for women versus comparing cross-gender differences in access constraints. Given the robust literature on the additional constraints women face, we preferred the former approach and focused on understanding women's constraints and how to alleviate them.

muted effects for men in the same settings (Jacoby and Mansuri 2015; Field and Vyborny 2022). These findings are mirrored in our earlier baseline surveys (conducted from 2011-13), which find that women are less likely to report they would travel to a nearby town over the next two days/months compared to men (33 percent and 56 percent for women versus 53 percent and 78 percent for men). While both women and men report equal interest in going for training in the near future (over 95 percent each), only roughly a third of women report being willing to travel more than two hours relative to nearly half of men who do so. Finally, in an earlier study for PSDF, we offered training courses to a small sample of both men and women. While distance was not randomly assigned in this prior work, there was a strong negative relationship for rural women between physical distance and course enrollment and completion, controlling for a host of individual-level characteristics such as monthly income, education, and employment status. In contrast, distance was not statistically significant for men and the point estimates of the distance penalty for women were around 10 times larger than that for men (see Appendix Table B1).

2.3 Intervention Design

Below we describe the main program, as well as five experimental variations introduced to study the impact of alleviating the constraints revealed through our qualitative work.

The training program focused on tailoring and included modules on basic literacy, numeracy, and financial skills.[12] The training was delivered over a four-month period, five to six days per week in the morning, typically from 9 am to 1 pm, and each trainee was required to maintain an attendance rate 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. The courses were implemented by established training service providers selected through a competitive process. Trainees reported the training was high quality in post-treatment surveys: 55 percent reported that the quality of the course content, training conduct, and facilities was high or very high; 69 percent of trainees said the course met or exceeded their expectations; and 74 percent reported that the training helped them improve their tailoring skills.

To better understand the low take-up rates in prior pilot studies, a series of field visits were carried out to elicit qualitative feedback on barriers women face in accessing skills training

[12]Initially a wider range of vocational skills training was offered. However, with the vast majority of women picking tailoring, PSDF chose to focus on that skill. The additional literacy and numeracy components were added as pilot work revealed most women who desired such training lacked the requisite skills needed for tailoring (writing down orders, taking measurements, preparing budgets, opening a bank account, etc.). So rather than make those a precondition and lower access, PSDF included them as part of their training.

as well as ways of alleviating these constraints. We interviewed male and female household members and influential community members (Cheema, Khwaja, Naseer, and Shapiro 2012b). This exercise informed a small subsequent pilot study, which evaluated the impact of two interventions (Cheema, Khwaja, Naseer, Shapiro, et al. 2013): (a) women's engagement in villages without a training center through meetings that stressed the training's usefulness and discussed ways to manage household concerns; and (b) combining women's engagement with village-based training to test the additional effect of alleviating distance-related barriers.[13] The pilot and field visits highlighted five primary constraints to resolve: distance, money, information, societal concerns, and safe and reliable transportation.[14] Each of these constraints were then directly addressed through the following program variants designed in partnership with training providers and PSDF:

Distance - Given the importance of distance, a subset of program villages were randomly selected to house a training center in the village itself. As a result, households in these villages were, on average, closer to their training center than households in other villages: the median travel distance for trainees in villages with and without a training center was 1.1 km and 9.25 km, respectively. We will refer to the former sample villages as village-based training (VBT) villages and the latter as outside-village training.

Financial Constraints - For rural women, participation in the training program may imply additional travel costs or potential income loss due to the opportunity cost of time allocated to the training program. Lack of financial compensation for such costs was the second-most cited reason for course dropout in prior program rollouts (Cheema, Khwaja, Naseer, Shapiro, et al. 2013). To address this, every trainee was offered a base monthly stipend of PKR 1,500. To rigorously test the impact of these stipends, a randomly selected subset of villages and households were provided additional stipends of up to PKR 4,500, resulting in a final variation in monthly stipend amounts from PKR 1,500 to 6,000 (see section 3.1 for details of randomization). These are sizable amounts constituting 13-52 percent of average monthly HH income and 15 to 60 percent of monthly average household expenditure. Stipends were disbursed four times and were only given to individuals still enrolled in the program with a minimum attendance rate of 80 percent.[15]

[13] Details about the pilot and what was and was not incorporated from it in the final design of the main program are provided in Appendix A.

[14] Interestingly, while we expected child care would be an important issue, qualitative field visits demonstrated little demand by women for such a service. Women were either confident that their own family members could take care of their children or, when they did not have such help, not comfortable with it being provided by non-family members.

[15] To make payments easy for trainees stipend top-ups were provided in four monthly installments through EasyPaisa, a mobile payment service that allows withdrawal free of charge at retail outlets. Our team helped

Information - Pre-treatment interviews with the sample population revealed that there was interest and need among potential applicants to be informed about the skills being taught, the quality of the training provider, and the logistics of the training. To address this issue, the program design first ensured that the basic intervention carried out in all villages (hereafter the "standard information intervention") provided a substantial amount of information to all individuals in our survey sample. In particular, after our baseline surveys were conducted and the training center locations were established in villages, each sample household was provided with a packet containing information about the nearest training center, the type and duration of the training, enrollment forms, and stipend information. The exact same protocol was followed in villages whether they had a training center in the village or not.

PSDF worked with local training organizations to design an additional trainee engagement (TE) intervention administered in a randomly selected subset of villages where we increased the informational content and salience of the course even further by also organizing a group discussion with potential trainees. In TE villages, after the standard information had been delivered, sample households were invited to learn about the program in an hour-long, female-only information session. Two to three days later, these group sessions disseminated information regarding course content and quality, female instructor credentials, course timings and duration, training center facility standards, and application submission protocols. Sessions shared the success stories of three trainees from previous trainings. These testimonies emphasized the lifelong value of the tailoring course, showing how past trainees used their skills to earn or save money by making higher-quality clothes for themselves, their families and neighbors, and by teaching fellow villagers how to stitch. Led by experienced field staff from the training organizations, the sessions included a question-and-answer period for attendees to ask logistical or informational questions. Attendees were given details regarding a three-day Open Period, during which they could visit the training center to see the facilities, meet the trainers, and ask any remaining questions about the course. Finally, a few days later, each household invited to the sessions received a follow-up visit to redistribute written information and answer additional questions. The TE treatment was designed to address the kinds of information gaps identified as barriers to training by our preliminary work and the literature (Nguyen 2008; Jensen 2010; Wiswall and Zafar 2015)

households set up accounts when necessary, made calls to ensure households received their top-ups, maintained a helpline to resolve issues, and hand-delivered withdrawal codes to households that did not have a mobile phone. Control over money is often a concern in such settings. At the end-line survey, 91 percent of trainees reported having either a large (54 percent) or moderate (37 percent) influence over where the money was spent. Trainees did not always directly retrieve the money: 44 percent of women reported that their spouse/fiancé did so and 25 percent, their parents.

Societal Constraints - Gender norms often situate community elders and male family members as important gatekeepers for women's participation (Field and Vyborny 2016; Cheema, S. Khan, et al. 2023)). In many cases, these individuals not only have the authority to deny permission, but they also control key resources (transport, information, and accompaniment) that can enable women's participation. Such barriers were often mentioned in our fieldwork and surveys, with household heads citing safety and social concerns as important factors behind a reluctance to have female household members apply for skills training. PSDF worked with local training organizations to address this constraint by introducing a community engagement (CE) intervention in a randomly selected sample of villages. Analogous to the TE intervention, the CE one was conducted after the standard information intervention had been delivered to each sample household. The CE intervention entailed conducting two separate 75- to 90-minute community-level information sessions for males and females. These sessions were attended by male household members and male and female community elders in addition to the potential trainees. Besides providing information, the CE sessions encouraged male and female participants to discuss any societal challenges that women face in accessing and benefiting from the training, as well as ways in which the community members could facilitate female members to enroll in training. Experienced field staff, who were trained community mobilizers from the training organizations, moderated the conversation. Community members and meeting attendees were also offered free transportation to the training center during the Open Period so that everyone (potential trainees and respected community members) could see that the facilities were indeed appropriate and safe. This was in contrast to the trainee engagement (TE) villages where meeting attendees were only informed about the dates when the center would be open for a visit (no free transport) and were reminded later about these Open Period dates. As with TE, subsequent follow-up visits redistributed written information and answered any additional questions. This treatment was designed to address potential barriers due to community-level constraints, and was analogous to interventions that aim to enhance participation and take-up by addressing social concerns and seeding information through community networks.[16]

Safe and Reliable Transport - A lack of safe and reliable transportation as well as norms surrounding what is considered to be appropriate means of travel may compound the physical distance constraint. This may be especially relevant in rural areas which tend to have more conservative norms and lower population density. Indeed, in focus groups conducted during our design phase, male household members often cited such concerns and would refuse per-

[16]See Banerjee, Chandrasekhar, et al. (2019), Bursztyn, González, and Yanagizawa-Drott (2020), and Cheema, S. Khan, et al. (2023).

mission for women to visit training centers in other villages unless they were accompanied by others. In our end-line surveys, we probed travel constraints further, asking women how far they could travel: if (i) they are alone; (ii) with a female relative; or (iii) with a male relative. Only 7 percent said they could travel any distance if alone, 42 percent said they could do so if traveling with other women, and 74 percent could do so if traveling with a male relative.[17] In the CE meetings in villages without a training center, the discussion in both women's and men's meetings was dominated by the safety risks associated with women traveling outside their village and the safety associated with different transport options and of traveling alone versus going in a group.

To alleviate such transportation concerns, free group transportation (GT) to the training centers was offered in a randomly selected subset of villages. Care was taken to ensure that the transportation was seen as safe, reliable, and socially acceptable by the villagers. Based on focus group feedback, this transport primarily consisted of women traveling in small groups of three or more on "qingchis" (a common type of auto-rickshaw) using male drivers from the same community the women were from. This was implemented by first holding a group meeting with male household heads where they nominated local drivers and suggested specific arrangements (e.g. pick-up times and locations). Female household members provided feedback on proposed arrangements during the voucher delivery visit. A final meeting with household members helped finalize these arrangements. Once the transport fees and terms of service were agreed with the nominated driver, households were informed about the finalized group transport facility, including the driver's name, mode of transport, pick-up and drop-off locations, and schedules. This service was not offered in VBT villages as the distances were short for group transportation to be as salient.

Appendix Table 1 summarizes the key elements of the different interventions described above.

3 Experimental Design, Data and Empirical Strategy

3.1 Sample and Experimental Design

Our sample frame comprised rural areas from the three districts in southern Punjab (Bahawalnagar, Bahawalpur, and Muzaffargarh). These are fairly typical of the country's agrarian regions, though slightly poorer than the typical district in Punjab. We randomly select two

[17]Of course, these responses could be influenced by treatment. We provide them here as an illustration of how traveling with others (especially men) could mitigate transportation constraints.

types of villages: "treatment villages" in which we introduce eight combinations of the interventions described in Section 2.2; and control villages. Within each village, we conducted a complete census listing of households using village field maps from the provincial statistics bureau before randomly selecting households for the study.

Our final sample size of 324 villages and 8,175 households was based on power simulations that drew on data from our pilot studies (e.g. the intracluster correlations in take-up in the pilot study). It allows us to detect 0.2 standard deviation differences in take-up across all eight different intervention combinations at a 5 percent significance level with 80 percent power as well as 0.3 standard deviation impacts on household consumption given the likely take-up rates.[18] Appendix Figure 1 provides a breakdown of the number of villages and households selected in each randomization branch.

We conducted the randomization in multiple stages. First, we divided the three districts into 27 total grids based on geographical proximity, each containing a total of 12 villages including nine treatment villages. Four of the nine treatment villages in each grid were then randomly selected to have a training center in the village (VBT). We then randomly assigned the five outside-village training villages to receive either trainee engagement (TE), community engagement (CE), group transport (GT), a combination of CE and GT, or the standard information intervention only. The four VBT villages were randomized 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. As noted previously, no VBT villages were randomized into the GT intervention.

All sample households in the nine treatment villages got the standard information intervention (course information, enrollment vouchers, etc.), as well as a randomized stipend offer, while households in a subset of villages got the additional (TE, CE and/or GT) village-level interventions. All sampled households in the control villages were surveyed but did not receive any information about the courses so they could provide a clean comparison for evaluating the impact of training on economic outcomes (Section 4.5).[19] Appendix Table

[18] Under assumed take-up rates for different treatments and a take-up ICC of 0.10 observed in the pilot study, our power simulations showed that a cross-section of 243 treatment villages and 6,100 households provided at least 80 percent power to detect a 0.2 s.d. change in take-up across different interventions (e.g., an 8 percentage point increase compared to a base take-up rate of 20 percent). With an additional set of 81 control villages, power simulations showed that we can detect a 0.3 s.d. impact on log-consumption spending per capita when comparing VBT and outside-village arms against pure control villages using an IV specification with three rounds of post-training data to calculate the LATE. Controlling for the baseline outcome value, as done in the ANCOVA specification, improved the power on training's impact beyond 90 percent.

[19] While any individual from control villages could directly enroll in the training program, given the spatial spread among sample villages, it may not have been feasible to attend the training. Unsurprisingly, no one from our control village sample even applied for training. Therefore we exclude control villages from our take-up

B2 shows the treatment and control samples are well-balanced on baseline variables (marital status, literacy, household size, etc.). Appendix Table B3 shows these same variables are balanced across the eight take-up treatment arms.

We also randomly assigned the total stipend amount at both the village and the household level for households in the treatment sample. In addition to a base stipend of PKR 1,500 per month, a randomly selected subset of households in each treatment village received an additional stipend top-up as high as PKR 4,500.[20] We determined this range through analysis of previous pilot data, which indicated that stipends in this range were the most cost-effective at increasing take-up and would not trigger equity concerns.[21] Appendix Figure 2 reports the total number of households that received each level of stipend top-up. Note that while stipend amounts were allocated randomly, the probability of being assigned each amount varied throughout the range of possible amounts. Moreover, additional stipend top-ups were provided to about 60 percent of the treatment households.

In addition, we randomly selected a subset of our original households (from among all eight treatment arms) and offered a voucher to one of their neighboring households. 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, it consented to be interviewed, and it contained an eligible female household member.[22]

analysis and only utilize them when we estimate the impact of the training.

[20]We first randomly selected the 10 households in each village to receive only the base stipend. We then randomized the remaining households in each village into one of eight "stipend buckets" of 500, 1,000, ... , and 4,000 additional PKR/month. In each bucket we further randomized households into low, medium, or high, where the low households received 500 less than the bucket amount, the medium got the bucket amount, and the high received 500 more than the bucket amount. The different stipend amounts are well-balanced on the same variables checked for other treatments.

[21]Field interviews suggested that households were comfortable with stipend variation as long as each individual received a minimum stipend and any extra amount was determined through a fair ballot process. A review of the literature also supports this observation (Blount (1995) and Bolton, Brandts, and Ockenfels (2005)). To further ensure our process was viewed as fair, the stipend variation was randomized in stages and the outcome provided in a sealed, marked envelope opened in the household head's presence. There were no reported cases of discontent regarding the difference in stipend values, which should reduce any concerns that those allocated a smaller stipend were less likely to enroll because they perceived the allocation as unfair.

[22]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. While these additional neighboring households were selected to receive vouchers after the original households, all vouchers were delivered at the same time 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 percent) of VBT households and 550 (16 percent) of outside-village training households.

3.2 External Validity

While our experimental design provides internally valid causal estimates, a related question is the extent to which our results generalize to other settings. We assess the external validity of our results based on the four conditions laid out in List (2020): selection, attrition, naturalness, and scaling.

1. Selection: An important design feature of our study is that we randomly sample villages from each of the three districts and then sample households randomly from a census of all households in these villages. Therefore our study population is, by construction, representative of the underlying rural population in the selected districts and also of our target population (women who are able and willing to receive training). Furthermore, the selected districts are not an outlier relative to other districts in the country or to other low- and middle-income countries in the region across a range of commonly reported demographic indicators in the cross-country Multiple Indicators' Cluster Survey (MICS) data.[23] Pakistan is also comparable to other countries on key measures of female empowerment and safety according to World Bank Gender Indicators and the World Values Survey (Appendix Figure B1).

2. Attrition: Our baseline survey refusal rates are less than two percent. For the take-up analysis, our attrition rate (voucher submission) is 4 percent. For the impact analysis, we conducted three additional rounds of survey over a 3-year period with an average attrition rate of 14.9 percent and only 5.5 percent if we consider individuals who were never surveyed in any of the three post-training rounds. This is in line with the expectations given the time period involved and is also lower than that in many evaluations of other skills training schemes elsewhere: 15 percent in Adoho et al. (2014), 18.5 percent in Attanasio, Kugler, and Meghir (2011), 18 percent in Bandiera et al. (2020), 38 percent in Card et al. (2011), and 25 percent in Maitra and Mani (2017).

3. Naturalness: We study an intervention by the public entity responsible for providing

[23] We utilize MICS reports, at https://mics.unicef.org/surveys, for the years 2010-2017 to compare women from our sample districts with women from 16 other low- and middle-income countries in South Asia and Middle East and North Africa (MENA), and the rest of Africa on several indicators pertaining to: i) female literacy and gender parity in education, ii) fertility rate and incidence of early marriage, and iii) access to reproductive health services. On all eight indicators, women from the sample districts were not outliers when compared to others in the South Asia region. In all except two health indicators (antenatal coverage and access to skilled birth attendants), the same applies when comparing our sample to Middle Eastern and African countries. Country-level data on female labor force participation, extracted for the sample period from the World Bank Open Data at https://data.worldbank.org, shows that Pakistan's female labor force participation (22.9 percent) was the lowest in the South Asia region, but similar to the five MENA countries (median: 25.8 percent).

such training, the Punjab Skills Development Fund (PSDF), that contracted providers who are active in the space through standard competitive procurement processes. The setting is thus standard for formal vocational training programs and our interventions indeed reflect the choices and time frames that individuals would make in a natural setting. The added features from our study are baseline and end-line surveys of households and the use of a waitlist ballot when there were excess applications at any center. Trainees and the training organizations knew that there was an ongoing study, though there were no financial incentives for the training organizations to comply with the study protocols.

4. Scaling: The fact that the program we evaluated is an at-scale rollout of the Provincial Government's main skilling program means that essentially all the features of the program (outside of the randomization and experimental evaluation design) are implemented at scale and the final results are representative of an at-scale benefit-cost calculus. In fact, concurrent to our study, PSDF was running similarly designed trainings across a wider range of skills in southern Punjab. As defined by the PSDF Board, the target population, the set of potential training organizations, and the mechanisms for procuring their services and monitoring the quality of training delivery were applicable everywhere.

3.3 Data Collection

Our data comes from three sources: household surveys, administrative data, and a distance-mapping exercise. Appendix A provides a timeline of surveys and field visits (Appendix Table A1) as well as a brief summary of data collected. The baseline household survey, conducted two months before the start of the training, collected information on demographic and outcome variables about the household and the nominated female member. During subsequent household visits for the intervention rollout, we conducted surveys to verify voucher acceptance and to ensure that households had been informed of all treatment activities within their village. The first follow-up household survey (six months after the training concluded) verified take-up status and collected information on the impact of the course on the trainee and her household. The subsequent two follow-up surveys (18 and 30 months after training) help capture the longer-term impacts of the training and increase the statistical power of our impact analysis (McKenzie 2012).

Throughout the intervention, our team and the training service providers collected 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.

We measured program take-up in four stages of increasing commitment: (i) voucher acceptance, (ii) voucher submission, (iii) course enrollment, and (iii) course completion.[24] The first was collected during a household visit ("voucher delivery") after the baseline survey and indicates whether a household nominated a specific member for training. The second captures whether the household then submitted their voucher at the training center during the open enrollment period. The last two measures capture whether the individual actually showed up when the course started and if they eventually completed the course.[25]

Given that distance to training centers is one of our key explanatory variables, we conducted an extensive distance mapping exercise to accurately measure the route each respondent would take from the cluster of houses where her home is located (i.e., co-locational neighborhoods in this context) to the nearest training center. We recorded both the distance to the training center and the time and cost of travel for multiple modes of transportation (e.g. wait time for a bus and fare for the route). We measured distance in three different ways. First, a "straight-line distance" from each outside-village training village's centroid to the nearest VBT village's centroid based on GPS. Since it was not feasible to assign training centers randomly *within* a village, we set this measure of distance to zero for VBT villages. Since a "straight-line" measure underestimates the actual distance a trainee would need to travel, we also constructed a "cluster-level travel distance" based on grouping households into geographic clusters and physically measuring the distance from each cluster to the training center by a surveyor on a motorcycle (for details on this surveying procedure, see Appendix A). Since the training center location *within* the village is not randomly assigned, this second measure may create an endogeneity problem (for example, if rich households have the center located closer to them). In order to address this, we constructed our third (and preferred) measure, "travel distance," which averages the cluster-level travel distance measure within each village to find the distance from the village's population centroid to the training cen-

[24] See Appendix A for further details on these measures.

[25] Training was open to any woman in the village (whether she was a voucher holder or not). For the training centers that had more applicants than they could accept, a randomized ballot was used to generate enrollment rosters and wait lists. Enrollment status for individuals who never had a chance to get off the waitlist (less than 10 percent of our sample) is defined to be missing since we cannot assume what their enrollment status would have been had they been given a chance to enroll. Since the (wait list) order was randomized (and the individuals are effectively excluded from our sample), this does not affect our analysis.

ter. By averaging the cluster-level travel distance, this third measure removes any parts of distance that could be endogenous within the village, while still allowing us to construct a non-zero travel distance measure, even for VBT villages.

Appendix Table 2 provides summary statistics. The average household in our sample has a monthly income of PKR 11,000 and has between six and seven members. 70 percent of the prospective trainees are married and only 34 percent have any formal education. Additionally, 33 percent are involved in paid work, and 33 percent have any ability to stitch. These basic statistics show that our course offered an opportunity with high potential value for our sample.

Appendix Table 2 also includes our three main distance measures. While average distances to a training center are not that large (a 3.2 km straight-line distance including villages where the training center is in the village; 5.8 km excluding them), there is still sufficient variation to estimate distance effects on take-up rates. Traveled (measured) distance is 1.5 times larger than straight-line distance on average.

Appendix Table 2 includes our main outcome variables. While voucher acceptance rates are reasonably high at 63 percent, class completion rates are quite low. Only 22 percent of the population completed the course. This average masks substantial variation across villages, a point that we will explore in more detail below.

We additionally present descriptive statistics on baseline values of key impact variables. At baseline, only 1 percent of our sample earns income from tailoring while a higher fraction (5 percent) are engaged in stitching activities. On average, a woman only spends 16.2 minutes in a day in tailoring, and this is reflected in low earnings; the average tailoring income was a modest PKR 35.58 (USD 0.36) over a period of three months prior to the survey, although there is significant variation with some women earning more than PKR 10,000 (USD 100). While 44 percent of households own a sewing machine, the low levels of engagement suggest that the reference period either had relatively lean demand or that women are primarily engaged in stitching as an infrequent and informal activity, rather than an avenue for income generation. Further, variables measuring household influence, business confidence, and gender-role perceptions (constructed from additive indices) suggest that women in our sample experienced low levels of empowerment at baseline.

3.4 Empirical Strategy

We first outline our strategy for estimating the impact of the various interventions on take-up rates, and then discuss how we estimate the impact of the training program.

Estimating Impact on Take-Up Rates:

Because of random assignment to different treatments, we interpret the differences in take-up rates between treatment branches as the causal impact of the interventions.[26] We first estimate the effect of our primary treatment intervention, village-based training (VBT), with

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

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-level controls measured at baseline; and ε_i is a random error term. In order to account both 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 β_1 gives the average treatment effect of placing the training center inside the village. Since VBT_i is randomly assigned, we do not require X_i for an unbiased estimate of β_1, but adding controls can help provide tighter standard errors. We present results from specifications with and without X_i.[27]

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. We do so by estimating,

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

where $Dist_i$ is a measure of distance to the closest training center, β_1 now isolates the boundary effect, and β_2 captures the per-km travel 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 exogenous as long as we condition on the average distance between a village and all other villages in our sample ($AveDist_i$).[28] We run variations of this specification, including higher-order polynomials in

[26]As we noted previously, we do not include pure control villages in the take-up analysis given there was no voucher delivery there (no one received even the standard information treatment). Moreover, as we find that no one applied for the course from our sampled households in control villages, including them in the sample would not change our findings in comparing how take-up is affected by the various interventions introduced.

[27]As noted previously, it did not make sense to provide group transport in VBT villages. Therefore our treatment design is not fully cross-randomized (see Appendix Figure 1), and in order to correctly estimate the VBT effect, we need to control for the group transport treatment. We do so in all specifications but suppress reporting it for expositional clarity except when we explicitly examine the impact of different design variations.

[28]To see why the $AveDist_i$ control is needed, consider an example of three villages being jointly randomized (one to VBT, two to outside-village training). 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

distance as well as discrete distance bins, 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 $Dist_i$.

Since our design introduced exogenous variation in stipend, we can estimate the impact of money on take-up and compare it to the impact of VBT to determine the economic magnitude of the effect. To do so we estimate:

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

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 also extend our analysis to the effects of our other treatment arms by including an additional indicator for each arm in our main specification in the equation:

$$Y_i = \alpha + \beta_1 VBT_i + \beta_2 TE_i + \beta_3 CE + \beta_4 GT_i + \beta_5 Dist_i + \beta_6 Dist_i^2 + \beta_7 AveDist_i + \beta_7 AveDist_i^2 + \rho X_i + \varepsilon_i, \tag{4}$$

where VBT_i, $Dist_i$, and $AveDist_i$ are the same as they appear in Equation 2; TE_i is an indicator for the trainee engagement (TE) intervention, CE_i is an indicator for the community engagement (CE) intervention; and GT_i is an indicator for the group transport (GT) intervention. It is worth mentioning that α in this specification now represents the mean take-up in the outside-village training standard information intervention villages (refer to Appendix Figure 1) so that each β on an intervention indicator represents the difference in take-up between those villages that received these additional interventions compared to the standard information treatment villages (controlling for distance and whether a village received a training center or not).

Estimating the Impact of Training:

VBT treatment, the respondents in the villages within 1 km of each other have a higher probability of having the training center being within 1 km of their home. To the extent that the farther away village varies on other characteristics (e.g. income, industry, etc.) that may 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). We checked the robustness of our results by using the average distance to all villages within 5 km, 10 km, and 20 km as well as averaging the distance to all sample villages within 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.

We estimate the impact of training among the women induced to complete training through the following specification that now also includes the control villages:

$$Y_{it} = \alpha_0 + \beta_1 Complete_{it} + \beta_2 BaselineY_{it} + \lambda_i + \gamma_t + u_{it}. \tag{5}$$

Here, Y_{it} measured at different points in time (t = 2, 3, 4) represents a particular outcome of interest. The indicator variable in this equation, $Complete_{it}$, is a dummy variable indicating whether an individual i completed the skills training in period t. Since this is an endogenous variable, we instrument for course completion with the randomized encouragement treatments (whether they received a training center, group transport, or just the information about training, as well as indicator variables for each randomized stipend level) and estimate a local average treatment effect (LATE). Hence, β_1 captures the average impact of skills training for women who complete the training in response to our take-up interventions. Our preferred method of evaluation, analysis of covariance (ANCOVA), has improved statistical power as compared to the difference-in-difference estimator (McKenzie 2012); we control for baseline round one values of the outcomes using $BaselineY_{it}$. We control for the randomization strata using grid fixed effects λ_i, round fixed effects γ_t, and u_{it} is the error term.[29] For assessing statistical significance, we cluster standard errors at the village level.

The above regression provides the overall LATE averaging the skills training impact across compliers of all the different interventions employed to raise take-up in this study. Next, we measure training impact separately for those women who only needed to commute within village to access training and those who had to commute across villages to do so. We do this by estimating equation (5) in two different sub-samples comparing, respectively, VBT and outside-village treatment samples relative to control. In the regression comparing VBT to control we use VBT and stipend dummies to instrument for training completion. In the regression estimating the effect for outside-village compliers we instrument using group transport, information and stipend level dummies. Finally, our setting naturally lends itself to measuring the effect among those additionally enrolled women who only acquire skills training when it is offered inside their village and would otherwise not travel beyond the village boundary for training. For this, we re-estimate (5) restricting the data to only the VBT and Outside-Village samples without transport (ie. only the information treatment arms: basic information, TE, CE). In keeping with the recent econometric literature on the use of mul-

[29]From the second follow-up survey (18 months after treatment) onwards we introduced an additional market linkage treatment to a randomized subset of the trainees. Since that is an additional intervention we are analyzing in ongoing work, for the purposes of this paper, we control for the market linkage treatment so as to be able to isolate the pure training impact that we are interested in for this paper.

tiple instruments (Mogstad, Torgovitsky, and Walters 2021), we limit the instrument set for endogenous training completion in these last regressions to the VBT dummy because stipend is cross-randomized and independent of the other treatments when the sample does not include control villages. The resulting β_1 captures the LATE on 'VBT Compliers' measuring the average impact of skills training for women who get training if and only if the training is offered within their village.[30]

Since we consider multiple outcome variables, we show the results are robust to adjusting for multiple hypothesis testing using both Bonferroni-Holm and Sidak-Holm approaches. We measure impact on five categories of outcomes: tailoring engagement; tailoring skills; tailoring income; household economic outcomes; and influence and engagement. While this study was conducted before pre-registration plans were a standard practice, our measures of tailoring outcomes all draw on our prior work incorporating outcomes of interest to the Punjab skills development program (Cheema, Khwaja, Naseer, Shapiro, et al. 2013). We expected a positive impact on all tailoring outcomes. Because additional skills would enable women to generate income and save money on clothing, we expected to see direct outcome on earnings and also that households may acquire more assets, particularly tailoring ones, and reduced expenditures on clothing. In terms of within-household dynamics, while this is less clear, to the extent that the training empowers women this could increase household influence and business confidence as women could contribute more to the household. Government service usage and civic engagement measures were included since we believed they may be affected based on the experience of having the government deliver a valuable training experience. We were agnostic as to the influence on gender roles, but felt it important to measure.

[30]Note that the estimation sample now does not have control and transport (GT) arms. Since the information treatments did not have an effect on take-up (and thus no 'first-stage' effect on course completion), we collapse the remaining treatment arms into VBT and Outside-Village, and thus compare the randomized treatments with the maximum difference in take-up. In this restricted sample with a randomized binary instrument (VBT), the usual LATE theorem assumptions hold. To see why the monotonicity assumption holds, recall that each treatment household in the sample (regardless of the specific treatment type) was provided with the list of four nearest training centers that, in the case of VBT, included the name of their own village as well. Thus, while VBT treatment provided a training facility inside villages, the households still had the option to enroll in one of the other training facilities opened nearby for this scheme. So, in a sense, the VBT treatment "expanded" the choice set by making training feasible inside the village boundary without constraining the option of going to a different training facility. Thus, all else equal, it would be rational to expect households who complete training when the center is located outside their village (z=0) to also get training when it is offered in the village (z=1).

4 Results

4.1 Distance Constraints and the Boundary Effect

We start by establishing the critical role that distance plays in women's decisions to pursue skills-enhancement opportunities. Table 1 Panel A first examines the impact on take-up rates of having a training center in one's village. We find large positive effects on all four take-up measures: voucher acceptance, submission, course enrollment, and completion. The odd number columns present our basic specification, and the even number columns add additional controls. As the measures of take-up move from intent to enrollment to completion, we find increasingly substantial impacts in both the absolute magnitude of the effect and its relative size. For voucher acceptance (i.e., an individual expresses intent to take a course), women in VBT villages have a 22 percentage point higher take-up than counterparts in outside-village training villages (column 1), which reflects a nearly 36 percent increase compared to outside-village training villages (the comparison group). Women in VBT villages have 32 percentage point higher voucher submission rates (more than double the control mean), 34 percentage point higher course enrollment rates, and 27 percentage point higher course completion rates (these effects represent a three to fourfold increase relative to the control group). As the mean travel distance of a training center for outside-village training 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.

Each of the four take-up measures represents different decisions at the household level. The first, voucher acceptance, captures whether the household nominated an individual for training when the voucher was delivered. While access considerations may be relevant at such an early stage, households may not be fully thinking through all the ramifications of the travel situation as there is no financial or commitment cost at this point; accepting a voucher does not obligate the individual to attend the course. In our comparison villages (outside-village training with standard information) 61 percent of potential trainees accepted their vouchers. Voucher submission, the next stage, is a more meaningful and costlier measure since it entails going to a training center two weeks before the course starts to submit the voucher. In our comparison villages, the largest drop happens at this stage: going from 61 percent who accepted the voucher to 24 percent who submit. When we asked individuals who did not submit a voucher why that was the case, three-fourths of the individuals reported that the head of household ultimately decided not to give permission for them to attend training. This suggests the intra-household discussions were not complete at the stage of accepting the voucher. After these first two stages, there are subsequent drops (12 percent enroll and 8 percent complete the course), suggesting that even if one is aware of access concerns up front, the actual experience may reveal over time that the costs are more significant than expected, leading one to drop out. Even for individuals who overcome household objections and general perceptions earlier on, the experience of actually enrolling and traveling to class every day may make these concerns more salient to them and their household members, leading to attrition.

Recall from Section 3.4 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 traveled allows us to separately identify the continuous per-km travel costs and any boundary effect (a penalty paid simply for leaving one's village for the training).[31] Such boundary effects, unlike per-km costs, are not readily explained by standard costs of travel since there are no economic tariffs charged for crossing village boundaries or other such discontinuities at the boundary. Panels B to C look at the straight-line distance of the closest training center to the outside-village training village's geographical centroid (this distance measure uses the respective GPS coordinates and is defined as zero for households within VBT villages).[32] Panel B introduces a linear control for distance, while Panel C adds a quadratic term to allow for a concave per-km travel cost function. Both panels demonstrate that the distance penalties increase with distance; for example, Panel B shows that class completion rates drop by two percentage points for each additional km. However, even after accounting for distance, the village boundary effect persists, ranging from 9-23 percentage points for different take-up measures (a slightly smaller effect than Panel A's specification without distance). There is a persistent additional effect of (crossing) the village boundary above and beyond the economic costs of traveling captured through the per-km measure. This finding of a clear boundary effect provides causal evidence in support of a set of papers that use evidence from non-experimental and qualitative methods to argue that spatial non-linearities in access restrict women to opportunities within their own villages and neighborhoods (A. Khan 1999; Mumtaz and Salway 2005; Porter et al. 2011; Thakuriah, Tang, and Menchu 2011; Jacoby and Mansuri 2015).

To provide graphical intuition for these results, Figure 1 plots each take-up measure on distance, clearly showing the drop at the village boundary as well as the additional effect of

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

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

distance on take-up for women traveling from other villages. Note that the non-parametric fit in the graph suggests that the boundary effect is likely to remain robust to different functional forms of the distance term.

A possible concern in our results so far is the possible overestimation of both the intercept term as well as the per-km travel costs due to using the straight-line measure of distance which is by definition, a lower bound to true travel distance.

To generate a more precise measure of the actual distance traveled, we conducted a field exercise where surveyors measured the distance physically traveled using the actual routes that a villager would most likely take (details in Section 3.3 and Appendix A). Since we also utilize distance traveled inside the village (with a training center) this measure is defined and non-zero for both VBT and outside-village training villages.[33]

Running our specifications using this more precise measure of actual distance we get smaller coefficients on distance traveled (Panel D), as we should (given that travel distance is on average 1.5 times greater than straight-line distance), and more precisely estimated functional form (Panel E). Our core results are unchanged: there is a large distance penalty and the boundary effect remains large at 13-22 percentage points when we control for linear measured distance and 11-18 percentage points when we control for quadratic measured distance. Across specifications between one-third and one-half of the total distance penalty is paid right at the point of leaving the village. We retain this more accurate measure of distance in our subsequent specifications.

4.1.1 Robustness to Functional Form

Table 2 shows that both the per-km travel costs and boundary effect are robust to a range of more flexible functional forms. Panel A of Table 2 uses a log specification for travel distance, often used in the literature on commuting (Heblich, Redding, and Sturm 2020) and shows that our results are effectively unchanged from those in Table 1 (Panels D and E). Panel B allows for polynomial forms up to a 5th order (controlling for a similar 5th

[33] As detailed in Section 3.3, the underlying measure is the physically traveled distance between households in a given geographical cluster (i.e., a small set of households located right next to each other) in a village. Recall the training center location was randomized at the village but not cluster level (i.e., we randomly selected which village received a training center but did not specify the exact location within the village that received it, as this was not logistically feasible). Since location *within* a village is not randomly assigned, directly using the "cluster-level" distance measure can result in an endogeneity issue; for example, poorer households in the village may live farther away from the training center. In order to address such concerns, the measure used in our analysis - "travel distance" - averages the cluster-level distance measure *within* each village to find the distance from the village's population centroid to the training center. In practice, both measures give very similar results suggesting that the endogeneity concern is not important in our setting (see Appendix Table B4).

order polynomial in $AveDist_i$). This exercise tests whether a highly flexible functional form in distance would substantially reduce the boundary effect estimated in Table 1. It does not; the VBT coefficient is largely unchanged. Moreover, since the higher-order terms in the polynomial are not individually significant, we conclude that the underlying relationship between distance and travel is best estimated as quadratic.

Panel C takes an alternative approach. Rather than assuming a smooth functional form in distance, Panel C flexibly controls for travel distance bin fixed effects. To do this, we first divide individuals from outside-village training villages into decile bins based on their village's average travel distance to the training center. We exclude VBT villages 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 (from both VBT and outside-village training villages) into a given travel 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. This more demanding specification shows similar boundary effects to the main regressions in Table 1 along all four stages of take-up.

Finally, Panel D of Table 2 takes these checks yet a step further by implementing what is akin to a regression discontinuity style design. Note that this is not needed for causal inference: distance is exogenous given our intervention design, so we obtain correct causal inference in our basic specification. However, to further minimize concerns about the true functional form of distance and its implication for the measured boundary effect, we restrict the comparison to those villages where a training center is located less than 4 km from the population center, either within the village boundary or outside (i.e., within the first two travel distance bins), so we are comparing households that face similar (and relatively small) travel distance to the training center. We also control for travel 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 D shows that the boundary effect remains robust and is 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 outside-village training villages with similar travel distances. This final test provides further evidence of how robust the boundary effect is.[34]

[34]One possible concern in our RD design is that we may be comparing (VBT and outside-village training) villages with very different areas (smaller outside-village training villages) and/or households who may be living at different distances from the center of the village (i.e., those who travel less but still cross a boundary may live at the periphery of their village). In Appendix Table B5 we find that controlling for village area, perimeter, and distance of the households to the village center, does not affect our RD results suggesting that these concerns

4.1.2 Additional Boundaries

While our results so far demonstrate the large, negative effect of crossing a village boundary on take-up rates, the village boundary is one of several boundaries women may have to cross when leaving their households. In this section, we explore additional boundaries. Our results for within-village boundaries employ non-experimental variation while for boundaries outside (between) villages they make use of experimental variation arising from our design.

Within Village Boundaries: A typical village has several settlements—smaller groupings of households that signify sub-communities in the village—separated by empty or agricultural land; the median village in our sample has eight settlements.[35] Therefore, settlements present a natural and potentially salient boundary. Using the same strategy as described in Section 4.1, we can estimate the impact of crossing a settlement border to reach a training center in addition to the effect of crossing the village border. Table 3 does this by including an additional indicator variable for a training center located within the individual's settlement (SBT).

Panel A shows that there is an additional SBT effect for all outcomes except voucher acceptance. Positioning the training center in a woman's own settlement leads to a 9 to 12 percentage point higher take-up rate (for voucher submission, class enrollment, and completion) over and above the 21 to 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 33 percentage points higher enrollment (21 for the in-village effect and an additional 12 for the in-settlement effect). Panel B includes linear cluster-level travel distance controls to better isolate the settlement and village boundary effects and the per-km costs.[36] Panel A in Appendix Table 3 shows similar results when using a quadratic specification. Overall, the suggestive evidence of a settlement boundary effect is strongest and most robust for our final measures of take-up: course enrollment and completion.

Outside Village Boundaries: Apart from boundaries *within* a village, there are additional boundaries outside one's village. In particular, if a woman has to pass through multiple villages on her way to a training center, each additional village may present another boundary that could influence her take-up.

Given our experimental design, the number of village borders between each pair of send-

are not important in practice.

[35] We use settlement definitions used in the national census exercise conducted by the Federal Bureau of Statistics of Pakistan.

[36] Recall that the cluster-level distance measure is based on a smaller (than settlement) grouping of households identified by our data collectors. Using it as the distance control allows to introduce finer variation.

ing and receiving villages is also random. To explore the role of village borders, we used Google Maps to identify the likely routes that a woman could take to reach the closest VBT village and counted the number of villages that she would encounter en route (inclusive of her destination village).

Panels C to D of Table 3 present the results of regressing program take-up on the number of boundaries one has to cross to get to the training center (Panel D controls for distance in addition to the number of boundaries crossed). For ease of interpretation, we set the training villages (the VBT group) as the omitted category (hence the sign of the boundary effects will be reversed) and we separate the villages without a training center based on how many village borders a woman would have to cross before reaching the training facility. We find that the negative effect on take-up shows up on crossing the first village boundary and there is no consistent additional effect of subsequent boundaries. In other words, it is the action of *leaving* one's village that has a negative relationship with program take-up. Panel B in Appendix Table 3 shows similar results when using a quadratic specification in distance. While in our primary table we only consider one versus two or more borders, our results are similar if we separately consider the impact of crossing additional borders.[37]

Together, our results present a nuanced picture. Once a woman leaves her village, the distance traveled still matters (take-up drops with distance) but additional (village) boundaries do not seem to have a detectable adverse impact. This provides further evidence that the discontinuous distance penalties we observe arise from concerns that are generated as a woman exits the confines and safety of her community/village.

4.2 Economic Significance of the Boundary and Distance Constraints

Our experimental design allows us to leverage exogenous individual-level 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 each of our four different measures are impacted by an increase in stipend amounts. Using the resulting estimate of the causal impact of money paid as stipends on individual take-up rates, we can then calculate how much extra stipend must be offered to induce a similar take-up rate change as the

[37] Appendix Table B6 also shows results where we divide the villages without a training center into roughly five equally sized bins, where we separately consider the effects of crossing one, two, three, four, five, and more borders. Note that these bins are "nested" for the sake of readability. Thus the first indicator "Crossed 1st Boundary" will take a value of 1 for all villages that did not have a training center (i.e., what we referred to as outside-village training villages before). Therefore each subsequent measure captures the *additional* impact (if any) of crossing an *additional* border—which is what we are in fact interested in isolating. While we use travel distance in these tables, our results are similar if we use straight-line distance.

distance and boundary effects.

Panel A of Table 4 shows the causal impact of stipends on take-up rates by including the exogenously assigned monthly stipend amount in our primary specification. A PKR 1,000 (~$10) increase in the monthly stipend raises take-up rates by four to five percentage points for the four increasingly demanding take-up measures.

Panel B then translates the stipend effect into the monthly stipend amount needed to replicate the full effect of having in-village training. Women in the average village would have to be paid an additional PKR 6,308-7,951 per month to achieve the same level of take-up as women who had a training center in their village. This additional monthly stipend corresponds to 66-84 percent of average monthly household expenditures reported in our pre-training survey and would imply an additional transfer of PKR 25K-32K to each individual over the four-month training period.

Panel C separates the implied economic value of VBT treatment into the financial transfers needed to overcome the boundary effect and the per-km costs, using coefficients from Table 1, Panel D and Table 4, Panel A. We find that the additional stipend necessary to induce a woman to simply cross a village boundary is PKR 3,686-5,212 per month, approximately the same amount as the median monthly household non-food expenditures in our pre-treatment survey.[38]

Once past the boundary, she would then require PKR 273-402 per additional km traveled. Since we account for distance in this estimation (Table 1, Panel D), the boundary-crossing compensation does not represent compensation for standard travel or time costs, but rather an economic measure of the additional and discontinuous access barriers faced by women in our context.[39] To our knowledge this is the first precise estimate of the economic magnitude of such access barriers in the literature.[40]

[38]While it may seem surprising that course completion requires a lower subsidy than enrollment, we should note that this is conceptually possible for both selection and experiential effects. In terms of selection, it is possible that those who need to be incentivized to enroll versus those who need to be incentivized to *remain* enrolled are different populations. The former may include individuals who are less willing to travel, so the weighted average of the compensation costs is higher for the former group than the latter. Second, to the extent that starting to take the course itself can lead to a positive experience or be habit-forming, it could indeed be the case that the compensation needed to have the average individual enroll may be higher than what it takes to guarantee completion.

[39]These estimates are even larger if we include the settlement boundary effect we noted in Section 4.1.2. Appendix Table B7 uses the estimates from Panels A and B in Table 3 to provide the equivalent economic magnitude of crossing the village and settlement boundaries. For example, using Panel A column 8 shows that a household must be paid 7,689 PKR a month (5,119 for the in-village effect and an additional 2,570 for the in-settlement effect) to allow a woman to attend a training that is both outside her settlement and village.

[40]Interestingly, the amount women require in compensation to leave the village in our rural setting is similar to the amount they will pay for a safer travel route in an urban context in the same region (Borker, Kreindler,

The boundary effect already implies that these costs are not readily reconciled with standard economic costs of travel and opportunity cost of time. We also compare the stipend compensation estimated in Table 4 with plausible estimates of the opportunity cost of travel based on the distance mapping exercise which captured the commute and wait times, as well as fares, for travel to the nearest training center using public transport facilities (bus, qingchi/auto-rickshaw, and motorbike). We estimate the opportunity cost of travel for the median woman in our sample as: $median\_fare$ + ($connecting\_time$ + $travel\_time$)$\times$$peak_wages$ + $waiting\_time$ $\times$$peak_wages$.[41]

This exercise shows that the stipend compensation amounts required to bring the outside of village enrollment and completion into line with the inside of village rates are substantially larger than generous estimates of travel costs (fare and time) when using public modes of transport. The median woman in our sample would incur additional costs of more than PKR 2,100 per month if she were to travel outside her village for training using public qingchi, one of the most common modes of transport, compared to walking to training in her own village.[42] Even having included generous assumptions on the opportunity cost of time (valuing wait and travel time for each trip at the hourly wage during peak labor season), these total travel and wait costs are approximately one-third of the compensatory stipend estimates obtained in Table 4.

Moreover, our results suggest that even the $per-km$ travel compensation, over and above the boundary-crossing compensation, may be hard to reconcile with travel costs on public transportation. In order to see this, Appendix Table B8 presents reported data on actual fares per trip paid for different modes of transport. columns 1 to 3 show the *additional* per km fare that needs to be paid for the three public transport modes, which at PKR 57-73 per km traveled each month are substantially smaller than the PKR 273-402 per km extra compensation we estimated in Panel C of Table 4.

While we estimated the compensatory amount was 3-4 times the cost of *public* transport

and Patel 2020). Our estimates of the compensation required to cross the village boundary range from 16.64 USD/month to 23.53 USD/month, after controlling for distance. These values bound the estimates in Borker, Kreindler, and Patel (2020) which finds that women in Delhi are on average willing to incur an additional expense of 250 USD per year (20.8 USD per month) to travel by a route that is one standard deviation safer.

[41]To estimate *fare* we regress reported fares on distance for measured routes and use the estimated fare at the median travel distance from a connecting point to a training center for those who need to travel outside their villages (5.8 km). Connecting time, travel time, and waiting time are the median values measured in the distance mapping exercise. Peak wages of 30 rupees/hour are the median hourly wage reported during harvest season, the period of peak labor demand. For simplicity, we assume travel to training 25 days per month (training was six days per week).

[42]The median opportunity cost for outside-of-village travel by public qingchi was approximately PKR 2,750 per month, and for in-village training the opportunity cost of walking was PKR 650 per month.

(fare plus opportunity cost of wait and travel time), it was closer to the cost of travel via a *private* mode of transport. Specifically, using the fare estimates from column 4 in Appendix Table B8, along with valuing commute time at the prevailing wage rate, we estimate that the median woman in our sample would incur additional costs of around PKR 5,000-6,000 a month if she were to travel to training on a private motorbike. This is much closer to the PKR 6.5-8K monthly stipend compensation we obtained in Table 4. We will return to this comparison when we examine (next sub-section) the impact of the safe group transport (GT) intervention - which offered an affordable semi-private means of transport to the training center.

4.3 Addressing and Understanding the Access Constraint

We begin our analysis of mechanisms by first examining three interventions designed to alleviate access barriers that could arise from informational, social, and transportation concerns that are exacerbated when training is outside one's village. In this section we directly look at the impact of: (1) trainee engagement (TE) sessions conducted in each village to further increase knowledge and salience of what the training involved; (2) a community engagement (CE) exercise to enable societal-level discussions by also inviting male and female community elders and male household members to a village-level meeting to discuss concerns and ways to facilitate potential trainees; and (3) providing secure and reliable group transportation (GT) for women through group consultations with their male household heads to ameliorate transportation concerns of attending training outside their villages. We focus on the extent to which these interventions ameliorated access constraints and raised program take-up. The following section then further discusses the efficacy of these interventions (or lack thereof) explicitly in terms of the potential channels at play in generating the distance and boundary effects documented above.

Table 5 presents the impact of each of these treatments on our four take-up measures and allows us to contrast them with the per-km distance and boundary effects. Across all four take-up measures, we find that the trainee engagement (TE) intervention does not increase take-up, nor does its inclusion impact the magnitude of the distance penalty or the boundary effect. Moreover, because TE was cross-randomized with village-based training, we can interact it with the VBT dummy to check whether information provision under TE was especially effective when the training was outside one's own village. As shown in Appendix Table 4, we find no evidence that TE was more helpful when the training was outside the village. To check whether the TE intervention provided information beyond the standard information treatment we collected data on a range of questions about the course before the first take-up decision (voucher acceptance). Appendix Table 5 looks at the impact of the TE intervention, showing that TE had strong positive impacts on prospective trainees' likelihood of visiting the training center, their knowledge about the course, and the ex-ante expected quality of course content, trainers, and facilities. While the TE intervention succeeded in imparting more knowledge and generating excitement, the fact that it was not instrumental in raising take-up suggests that informational constraints were not as binding.

Recall that the community engagement (CE) intervention added engagement with village elders and male household members in an effort to address any questions and concerns they might have. As with the TE intervention, it also substantially increases knowledge about the course and its expected quality (see Appendix Table 5). Nevertheless, we again find that CE does not help raise program take-up. In fact, while CE did not have any impact on voucher submission, class enrollment or completion, it had a fairly large but *negative* impact on voucher acceptance (9 to 10 percentage points). When we look at the fully interacted model (Appendix Table 4), we find that this negative impact of CE on voucher acceptance is driven entirely by villages where the training was outside the village: CE suppresses voucher acceptance by over 19 percentage points in such villages. While the fact that this treatment did not improve eventual course completion is disheartening from a policy perspective, these results are quite revealing in interpreting the access barriers we find.[43] First, they demonstrate that social factors are at play given that TE had no overall negative effect and CE, which simply added wider community members to the engagement, did. Second, given that the negative impact only occurs when the training course was located outside the village suggests that the social concerns were related specifically to a woman leaving her village for the training (as opposed to social concerns regarding another aspect of the training). Third, the fact that the negative impact of CE does not arise for subsequent stages of take-up suggests the meetings

[43] We should acknowledge that a stronger form of community engagement, perhaps one which lasted over a longer period and was more involved, might have been impactful. That said, these meetings were organized and delivered by local organizations that routinely conduct such mobilizations and follow best practices. Our results in Appendix Table 5 show that CE did increase knowledge and expectations regarding the training. Moreover, qualitative field observations of TE and CE suggest that these meetings were well attended and led to robust discussions on a host of issues that ranged from course content, to the types of clothes it would enable them to stitch, to the uses they could put these skills to. There was also a vibrant discussion among women and men in CE on the challenges of balancing women's everyday household responsibilities with the demands of the course and the kinds of support women would need to attend the course. In villages without a training center, the discussion in women's and men's meetings was dominated by the safety risks associated with women traveling outside their village and the safety associated with different transport options and of traveling alone versus going in a group. Our results therefore offer a sobering reminder that even when one is able to engender a robust discussion of issues faced, addressing social barriers, especially those that may entail changing restrictive social attitudes, is a difficult and costly exercise that may take months if not years to materialize.

raised concerns that these women would have faced subsequently in any case (even before they were able to submit a voucher); thus, the CE treatment dissuaded the subset of women who would have ultimately dropped out from even accepting the voucher. We expand further on this discussion and what it reveals about underlying channels in the subsequent section.

Finally, we turn to the constraints that arise from transportation concerns. We find that the secure group transport (GT) intervention has a large, positive impact on all but the first stage of take-up (Table 5). For course completion, the GT impact is roughly two-thirds the size of the village boundary effect.[44] As the GT treatment is only offered in villages that did not have a training center, we take this as strong evidence that providing appropriate group transport goes a long way in compensating for the penalty that women faced when crossing the village boundary.[45]. The GT impact is also statistically significantly larger than TE or CE for all the measures of take-up.[46] Moreover, the importance of such dedicated transport is consistent with our previous results where we found the amount of stipend women needed to compensate them for travel outside the village was similar to private transport options, which also provide a dedicated, safe, reliable, and socially acceptable mode of transport.

We can gain further insights into the boundary effect by examining interactions between GT and other randomized interventions. While we did not offer GT in VBT villages, we assigned both GT and CE simultaneously to some villages. Appendix Table 4 shows that the interaction between group transport and community engagement is positive and marginally significant at the voucher acceptance stage (p-value of 0.12). Recall that the negative impact of community meetings at the voucher acceptance stage was not present for women in VBT

[44]We note that while GT impacts take-up positively in the later stages of take-up (voucher submission, course enrollment, and class completion) it does not have a statistically significant effect on voucher acceptance, the first stage of take-up. Each subsequent stage of take-up represents an increasing level of commitment, and group transport may have entailed further (social) considerations. While individuals were aware that they had the option of group transport available even at the time of voucher acceptance, it is likely they did not fully internalize the specificities of the transport and hence the effect on voucher acceptance may be muted. In particular, the transport was run by a locally designated driver approved by both males and females in the community. This aspect, which gave it further safety and social acceptability, along with specific knowledge of which other trainees were accompanying the trainee on the transport (those living nearby) may have become more salient in subsequent take-up stages which is why we may see a significant effect on voucher submission, course enrollment, and completion and not on voucher acceptance.

[45]This result is also consistent with the end-line survey results discussed in Section 2.3 that six times more women said they could travel any distance if they went with other women (42 percent) than if they went alone (7 percent).

[46]For voucher acceptance, all three treatments are statistically different from each other: the p-values (using column 2 estimates) of the comparisons are as follows: CE versus TE 0.10; TE versus GT 0.07; and CE versus GT 0.01. For voucher submission, course enrollment, and course completion, we cannot reject equality between CE and TE. However, the GT impact is statistically larger when compared to either TE or CE: the p-values for voucher submission GT versus TE is 0.08 and GT versus CE is 0.03; for enrollment GT versus TE is 0.01 and GT versus CE is 0.01; and for completion GT versus TE is 0.001 and GT versus CE is 0.0003.

villages. Analogously, we see that this negative effect of community engagement on take-up is also mitigated for villages that received group transport. Community engagement only negatively impacted voucher acceptance in villages that received neither a training center in the village nor reliable transport, suggesting that providing *either* in-village training or secure group transport mitigated whatever objections to training were made salient in the community engagement meetings.

4.4 Examining Potential Channels

We now shed further light on the evidence for and against different factors that may underlie these distance barriers. In order to do so we interpret our previous results and marshal new evidence that explicitly looks into four potential channels: information and salience; peer and network effects; safety and security; and transportation effects.[47] We note at the outset that our intent is not to claim that a single factor drives the distance and boundary effects, but rather to see which appears most important in this setting.

Information and Salience: The distance and boundary effects may arise if having a training center in one's own village provides better information or increases the training's salience. We first note that, as detailed in 2.3, the standard information treatment carried out in all villages provided a substantial amount of information. As one test for whether there was an informational advantage for trainees in villages with training centers, we used follow-up surveys run after the introduction of the course to check whether individuals were more aware of the training being provided if it was in their village and if so, whether they could correctly identify the location of the closest training center. Over 98 percent of our respondents knew the training center's location and were able to correctly identify it, while there was no statistical difference in this information between VBT and outside-village training villages.[48]

[47]We do not include a subsection on burden of care because, as noted in Section 2.3, we found little demand for childcare in our pre-treatment field visits and observed no difference in its importance for women who had to travel for training versus those in villages that received training centers. Despite this, we conducted one test of whether household care burdens could lead to a boundary effect, i.e., whether such concerns are more binding when traveling outside one's village, by interacting the VBT dummy with multiple ways of measuring the household dependency ratio (# of dependents / household size) in our core specification from Table 2. We found no evidence for heterogeneity in the estimated boundary effect, the difference between VBT and outside-village training in take-up rates at all stages appears to be uncorrelated with the household dependency ratio (regressions not shown). This result suggests that in our context the burden of care was not a key factor in generating the boundary effect.

[48]Interestingly, the analysis shows that while there is a quantitatively very small but statistically weak effect (p-values of 14-17 percent) with distance (the likelihood of an individual correctly reporting which village the training center is located in drops by 0.23 percentage points for each additional km that that village is away from

The lack of impact for the TE and CE interventions on take-up suggests that other forms of informational advantages are unlikely to explain the boundary effect. In fact, the TE and CE interventions generated more knowledge about course content and left a more positive impression of the quality of the course than simply having the course in the village did (Appendix Table 5).

It is also possible that simply having a course run in a village and interacting with the program trainees could raise the salience of the course. However, this cannot explain the boundary effects at the initial stages of take-up; for example, accepting and submitting a voucher which occurred before any course activity started.

Furthermore, we asked women who had not applied to the course the reasons why they did not do so. There was no statistically significant difference between VBT and other villages in terms of whether women cited not feeling they would get valuable skills, not knowing someone else applying in the village, or in their perceptions of course quality or expected returns from the course. There does not seem to have been an informational or salience advantage of having the course offered inside one's village.

Finally, our non-experimental finding that settlement boundaries also matter suggests that informational factors may be less salient. While settlements typically tend to be physically separated (see Appendix Figure B2 for Google Earth images of sample villages), they seem to be informationally connected. This is both based on qualitative fieldwork and the fact that we find no difference in respondents' knowledge of the training center and its precise location based on whether the training center is located in one's own settlement or not. As a further test of connectivity, we use data from our baseline survey on whether respondents are aware of others who may have used one of seven different services (public or private health centers, schools, police, courts, sanitation, and utilities). If informational flows were poorer across than within settlements, then we would expect that the individual response would be more correlated to those in their settlement than to those in other settlements in the same village. We find that this is not the case.[49] Despite settlements being physically disconnected and

their own village), there is no effect of crossing the village boundary in this knowledge (see Appendix Table 6).

[49] Specifically, for each respondent we construct the percentage of (the seven) services the person reports they know someone else used. We then regress this individual measure on the average response for the same question given by individuals in the person's (i) own settlement (excluding the person themselves) and (ii) own village (excluding all individuals in their own settlement). We find own knowledge is highly predicted by both settlement and village knowledge - with a coefficient of 0.44 on own settlement average and 0.40 on own village (excluding own settlement) average. Importantly, both are statistically indistinguishable, i.e., we cannot reject that the two coefficients are statistically the same (p-value is 0.73). This test suggests that an individual's own knowledge is quite responsive to the knowledge of those in their own settlements and villages and that there is no statistical difference in this knowledge responsiveness when comparing settlements and villages.

seeing boundary effects, they seem to be informationally connected, suggesting that informational differences are likely not a major factor inducing boundary effects.[50]

Peer and Network Effects: The success of in-village training and of GT suggests that there may be positive peer and network effects that could generate access advantages for in-village training.

We can directly test for group effects using experimental variation in two distinct ways. First, we included an individual-level randomization explicitly designed to induce peer effects by providing a voucher and stipend to one neighbor for a randomly selected subset of women. We find no evidence that the neighbor's offer positively impacted an individual's take-up decision (Table 6), suggesting that peer effects are either not present or require a stronger change in other's attendance to generate. Second, we take advantage of the fact that while stipends varied at the individual level, there was also random variation in stipends across villages. The fact that stipend provision impacts course take-up means that villages that were randomly assigned a higher average stipend have an exogenously larger incentive to take up the training. We can therefore instrument how many total women took up the training at each relevant stage with the randomized average stipend top-up provided in the village. Table 7 shows that an individual's decision to accept/submit/enroll/complete is not affected by the number of other women in their village who accepted/submitted/enrolled/completed, and we find no qualitative change in the boundary effect either.[51]

Both results show that peer and social effects, including those related to social learning and salience, are not likely to be driving the boundary effect.

Safety and Security: To the extent that leaving the village exposes women to less populated and potentially unsafe and unmonitored areas, the boundary effect could be driven by

[50]One may be concerned that the training itself, or the training organizations and their training staff, may work differently for those inside- and outside- the village. However, we were careful to ensure this was not the case. Not only were training providers randomly assigned at the village-grid level but each training center typically had both trainees who attended from inside the village and outside it (therefore our take-up analysis comparisons are not between different training centers but rather between women who came from different villages to these centers). Appendix Table B9 shows that the training organization identifier as well as a set of trainer attributes are balanced across our experimental interventions. Moreover, the field management staff were centrally recruited and trained; interactions between training staff and applicants were scripted and monitored so there was no difference in how women who came from within the village where the training center was located and those who came from outside were treated. As direct evidence of this, we also checked in with all respondents who tried to enroll whether they faced any issues. We did not see any difference between VBT and outside-village training women on knowledge of course, respondents' ex-ante ratings of course quality (Appendix Table 5), or in reasons provided by respondents for not enrolling in the course.

[51]Appendix Table B10 in the online appendix also shows the ITT version of this table which directly includes randomized individual and average village-level stipends. Consistent with the IV specification, we see that the boundary effect is unchanged and that in fact, once we control for the stipend an individual receives (and that is highly predictive), there is no impact on their take-up decision of the average stipend provided in their village.

real or perceived safety issues. This is consistent with GT having an impact since it provides safe and secure transport for women. It is also consistent with qualitative field observations of TE and CE meetings which show that the discussion among attendees in villages without a center was dominated by concerns for women's safety associated with traveling outside their village, unless it was in groups, and an assessment of the safety risks of different transport options. This issue was rarely reported in villages with a training center. We conduct two direct tests to see if such concerns may be important in generating the access barriers we observe.

First, we check whether women who reported they were more concerned with safety issues in our baseline surveys show a larger boundary effect. As Table 8 shows, women who self-reported safety concerns pre-treatment are 7 to 10 percentage points less likely to take up the training when it is outside their village. However, when the training is in their own village, these women show no difference in take-up rates compared to other women.[52] Safety concerns only appear to matter when the training is outside the village.[53]

Second, we use an external and measurable proxy for insecurity. The literature on gender-based violence underlines how verbal and physical assault by strangers occurs most often when women are alone (Hossain, Mahajan, and Sekhri 2022; Simic 2021), and the risk of violence is higher when facilities are located far from home and women must traverse isolated, open, and secluded places (Jewkes and Abrahams 2002; Bapat and Agarwal 2003; Moser and McIlwaine 2004; McIlwaine 2013). Accordingly, we use underpopulated spaces as a proxy for the risk of physical insecurity for women. To identify such spaces, we use WorldPop geospatial population data which draws on census data and a range of physical features to predict the population density of each 100 m $\times$100 m grid cell on Earth (Stevens et al.

[52]The coefficient on the interaction term between safety concerns and the in-village training dummy is positive and comparable in magnitude to the negative coefficient on the safety concerns variable itself. We do not find similar effects for male perceptions of safety or reported crime rates in the community.

[53]In light of the large and robust boundary effect, we realized examining such heterogeneous boundary effects could provide additional insight into the mechanisms at play and therefore filed an analysis plan (see https://www.socialscienceregistry.org/trials/4068) before examining these results to discipline the analysis. Interestingly, the only pre-specified variable that seems to be part of the boundary effect (i.e., has an impact in outside-village training villages but not in VBT villages and therefore is a factor that is likely related to crossing the village boundary) was women's perception of safety. While a range of other variables (like women's stated desire to enroll, socioeconomic status, household size, and agency within the household) affected take-up in VBT villages, they did not show any differential impact in outside-village training villages i.e., they do not display the same pattern as women's safety perception where the sign on the interaction term with the VBT dummy is of equal magnitude but opposite sign to the main effect. This suggests that these variables, while important for take-up in general, were unlikely to be related to concerns raised when crossing the village boundary (regressions not shown).

2015).[54] To characterize insecure paths we define a dummy variable as equal to one when the straight-line path from the cluster-level centroid to the nearest training center crosses at least 500 meters of a continuous underpopulated space.[55] We define grid cells as underpopulated if their predicted population is below the median population density observed along all travel paths in our sample.[56]

Table 9 includes this variable in our primary specifications from Table 1 to examine how its inclusion affects the boundary effect. To make the comparison straightforward, we report in the bottom part of Table 9 the percentage change in the main VBT coefficients from controlling for underpopulated travel segments.[57]

Across all the specifications, we find that crossing empty space has a large negative effect on take-up, and noticeably reduces the boundary effect, even when controlling for distance very flexibly. For example, columns 7-8 in Panel A show that having to travel through an underpopulated segment depressed course completion rates by 9-10 percentage points and reduces the estimated impact of inside-village training by 22 percent. Panel C shows that the boundary effect drop ranges from 19-41 percent depending on how we control for straight-line distance. The fact that including this proxy for security exposure on actual travel paths attenuates the boundary effect strongly suggests that concerns due to traversing underpopulated areas are quite important. These results are also robust to using 250 meters to define the underpopulated dummy (Appendix Table B12)

Including these measures in Tables 5 and 6 (see Appendix Tables B13 and B14) shows the same pattern, as does including it in our analysis of the settlement boundary effect and RD design.[58] Importantly, including both the underpopulated segment dummy variable and

[54]The population density raster has a three-arc second resolution (approximately 100 m at the equator).

[55]Since we do not have the actual traveled paths charted on a digital map (as measured by the travel distance variable), we can only construct these measures for straight-line distance measures. To the extent that this generates a noisier proxy for underpopulated segments on the actual travel path a woman would have to take, we believe our estimates will be attenuated, and therefore likely provide underestimates of the importance of this underpopulation factor.

[56]The median is calculated from the distribution of the mean population density of each path. The median value used as the cutoff for our dummy is 3.44 people per 100 square meters. The average number of people per cell in our sample region is 3.8. To give a sense of cardinality, the mean population densities of Lahore and Karachi, two of the largest cities, are 39 and 29 people per 100 square meters, respectively. Using this definition there are 3,012 households (59 percent of the sample) that must cross at least 500 meters of underpopulated space on the path to the nearest training center.

[57]Appendix Table B11 re-estimates Table 1 (not including the underpopulated travel paths dummy) using the same restricted sample as shown here in Table 9. The sample size is reduced from the main table as not all observations had GPS data. We report reduction from the Appendix Table B11 coefficients as that is the appropriate comparison to make. Standard F-tests for nested models show that including the underpopulated dummy in the main model results in a statistically significant increase in model fit in all regressions.

[58]As before we compare the change in the boundary effect using the estimates in the restricted sample of

average population density on the straight-line path shows only the former having an effect (Appendix Table B17), suggesting this effect is specific to crossing underpopulated spaces.[59]

We should note that our safety results are not specific enough to test whether these concerns are held by the women trainees, their families, or communities at large. Such distinctions are hard to isolate conceptually since what may start as a community concern may eventually get internalized by individuals and households or vice versa. In survey questions about barriers to taking the course asked prior to any take-up decision, women from villages with training centers were far less likely to cite disapproval from their own household members, extended family, other women, and individuals in the village, suggesting that all may matter (see Appendix Table B18). And in surveys after voucher submission, women in VBT villages were 9.2 percentage points less likely to cite other household members not wanting her to apply as the reason for not submitting a voucher. Overall, it is likely that individual, household, and community perceptions of safety are all at play here.

Transportation Constraints: To test for discontinuities in access or availability of transport modes within versus outside the village we leverage data on estimated average wait, travel, and connecting times for each mode of transport from our distance mapping exercise for each of three public transport modes (bus, qingchi, and motorbike). After controlling for distance, there is no additional effect of crossing the village boundary in terms of (i) availability; (ii) average wait time for a route that would allow travel to the nearest training center; (iii) connecting times if any connections were needed along the route; and (iv) the average fare paid for the route (see Appendix Table 7). This null suggests that our boundary effect for course take-up is unlikely to be driven by any differential public transport access.

While we find no evidence of discontinuities in the transport choices available to women at the village boundary, we do find different preferences over travel modes. Before women decided on their course choices, we asked them what mode of transport they would likely use if they were to attend the training. Since the location of the training center was provided to them at the time, including whether it was in their village or outside it and how to get to the center, women responded with the specific mode they would likely use for the actual location they were considering. As Appendix Table 8 shows, there is a clear boundary effect

households for which we have GPS coordinates. Appendix Tables B15 and B16 reproduce tables 2 and 3, respectively, using the restricted sample.

[59]To see why our measure and mean density both measure different features of the world imagine two paths with five segments. Along path A the second and third segments have a population density of 0.5, while the rest have a population density of eight. On path B, all segments have a population density of five. Even though the mean population density for both paths is five, the two segments would be entirely different in terms of security for women. The first would require them to travel through underpopulated space and thus be exposed to higher risk.

in their desired mode of transport:[60] The likelihood that a woman intended to walk to the training center is decreasing in distance, as one would naturally expect, and there is an additional statistically significant boundary effect; after adjusting for distance traveled, women are significantly more likely to say they would walk if the training center is located inside their village.[61]

In summary, our analysis of the impact of the additional randomized interventions, the importance of secure group transportation, and the results in this section provide strong evidence that travel-related security concerns contribute to the observed boundary effect. There is less evidence for informational, peer/network, and transport availability-related factors.

4.5 Training Impact: Overall and Differential

We now turn to estimating the impact of the training. Since we offered training vouchers to a randomly selected set of women in the village (as opposed to randomizing within women who applied for training as an over-subscription design would do) we can estimate: (i) the overall training impact for those induced to enroll by our different take-up treatments; (ii) the impact for those who only needed to commute within village; (iii) the impact for those who had to commute across villages; and (iv) the impact for those women who attended within village but would not have attended if the training were outside their village. As mentioned in section 3.4, we instrument for course completion with whether a village received a training center, group transport, or information, as well as a set of dummy variables for the randomized

[60]While our data has multiple modes of transport ranging from walking, bicycling, taking a private motorbike/qingchi, and taking public transport (bus/qingchi), in reality the commonly used transports are split between walking, and private motorbike and qingchi. We, therefore, focus in our analysis on the decision to walk or not as that presents the biggest cost contrast with other modes.

[61]A potential concern with this interpretation is that there is a mechanical heuristic/physical constraint that creates a discontinuity, and this coincides with the village border i.e., as long as the distance is less than "X" kms one can walk, but beyond that one has to take some other form of transport. Since our sample villages vary a fair bit in size and households also vary in terms of how far from the village border they live, we do not think this mechanical effect is likely (since the distance to the village border will constitute a fairly large band and not present a sharp discontinuity at a fixed distance). columns 3 and 4 of Panel A in Appendix Table 8 test directly for this by taking advantage of our smaller RD-sample of villages where a training center is located less than 4 km from the population center (either within the village boundary or outside) and show that the boundary effect, while slightly smaller, remains robust. Panels B-D take these checks even further by adding more demanding distance controls (logarithm, quadratics, and even discrete distance bins to capture further discontinuities). These additional checks show that even in the limited RD sample and with such extensive distance controls, the boundary effect for the desired transport mode remains. Finally, we can also replicate Table 3 and Appendix Table 3 by seeing if the intended mode of transport also shows additional boundary effects. Appendix Table B19 shows that results are very similar to before. We do see a boundary effect for both settlement and village boundaries (walking is less likely to be preferred when one has to cross either) and this effect only shows up when crossing the first village boundary and not for subsequent village boundaries.

stipend level.

Table 10 first presents the impact of the training on women in our overall sample (pooling together three rounds of surveys that capture the impact six, 18, and 30 months after training)[62] along a range of outcomes (column 1). Panel A shows that the training increased women's engagement with tailoring: it leads to a 9.0 percent increase in any tailoring activity, an extra 22 minutes a day in time spent tailoring, and results in 1.5 more clothes stitched in the last three months. While these effects may seem modest in an absolute sense, they are quite large relative to the baseline values of these variables provided in column 5. As a multiple of these baseline values, women who received training increased tailoring activity by 2.8 times, hours stitched per day by 2.4 times, and number of clothes stitched by 5.7 times. Panel B shows that women also report improved designing and sewing skills (18 and 40 percentage points). This increase in activity and skills translates directly into greater earnings. Panel C shows that the training increases the probability women earned income from tailoring by 8.4 percentage points (a seven-fold increase), and make an extra PKR 301 over a three-month period (an over nine-fold increase relative to the baseline control mean). Close to two-thirds of this income increase comes from selling to non-relatives, showing that women were able to sell outside their family networks as well. The relative earnings effect compares well with the effects of other vocational training programs reported in the experimental literature (McKenzie 2017).

There is some evidence that the increased stitching could have reduced own expenditures (Panel D) as women may have stitched clothes for their own household members. Though these effects are large in magnitude, they are statistically weaker (p-value of 0.18 for monthly expenditures and of 0.14 for clothing expenditures). While there is little evidence that these earnings were large enough to impact overall asset ownership, what is noteworthy is that there is a 23 percentage point increase in the likelihood of owning a sewing machine (a 53 percent increase over baseline ownership).

Finally, Panel E considers impacts on a range of non-economic factors. For tractability, we combine the outcomes here along a range of indices, but provide impact on each subcomponent of these indices in Appendix Tables B20 and B21. We find that training worsened women's perception of their influence in household decision-making. While this may sound counter-intuitive, it could be the case that the training induced women to attempt to exercise their influence more in conversations with household members about attending the course,

[62] Appendix Table 9 separates out these effects in column 1 for the three post periods. We generally find similar patterns over time. If anything, stitching engagement and earnings tend to increase over time, so we prefer to provide the pooled estimates.

making and selling clothes, etc. and that this revealed to women that in fact, they had less influence than they may have otherwise thought. This is consistent with the negative effect being somewhat larger for women who had to travel outside their village to receive the training (column 3) versus those who did not (column 2). To the extent that these women had to overcome greater hurdles, they may have had more (contentious) discussions on what they could and could not do, revealing the limits on their decision making and influence. Furthermore, Appendix Tables B20 and B21 decomposes the effect on the household decision making index reported in Table 10 into its sub-components and shows that the negative effect on the index is mostly driven by trainees' perception on whether they can influence their husband on new activities.[63] This is again consistent with women who had to overcome greater hurdles having to push harder, including initiating new activities using their newly acquired skills, and realizing the opposition they faced there.[64]

We find no significant overall effects across a large set of civic engagement questions, though the sub-component analysis in the appendix shows positive impacts on political knowledge (whether they correctly identified the president and chief minister), and women's sense of whether they could influence the government.

We now turn to separately measuring the training impact for women in VBT villages versus those who were able to complete the course even though they had to commute outside their village to do so. This is an important question since one could posit that the latter, who had to overcome distance and boundary access barriers may have been either more motivated or selected in a way that they expected higher program benefits. From a policy perspective this is also important: while providing in-village training opportunities did increase take-up substantially, it could be that the additional women who were induced to enroll by lowering access barriers obtained lower benefits which may not be as attractive from a benefit-cost

[63] An examination of the specific questions in the women's household influence index shows that the negative effects are mostly driven by questions that concern whether women can influence their husbands to start new activities related to tailoring. While there is little impact on the index of business confidence, the one question in this index that shows a positive effect is managing financial accounts, which women likely had to do when selling clothes they produced. Similarly, there is no significant impact on perceptions about gender roles, on government service usage, or on civic engagement. The component analysis, however, shows that there is a significant positive effect on using education and electricity services, as well as correctly identifying political heads of the state, possessing citizenship cards or believing in the citizens' ability to influence their government, and a negative significant effect on using courts or being a member of a political party or NGO.

[64] Interestingly, examining impact differences in perceptions of household influence between those who received the TE and/or CE treatment relative to those women who did not suggests that the TE/CE treatments attenuated the negative effect somewhat. While this evidence (not reported) is tentative as it lacks adequate power, it appears that even though these treatments did not effect ultimate course enrollment or completion rates, the group discussions they enabled could have moderated some of the negative perceptions women felt about their ability to influence household decisions.

point of view.

In order to examine these questions, we separately estimate the impact of training for women in VBT versus outside-village training (columns 2 and 3 in Table 10), as well as those women who were able to attend only because the training center was in their village (column 4 which compares outcomes in villages that received only information to those in the VBT arms). As mentioned in section 3.4, we instrument for course completion with dummy variables for the randomized stipend levels (in columns 2 and 3) as well as whether a village received a training center (in column 2) or whether it received information about training and group transport (in column 3). Note that in column 4 we only use the VBT treatment instrument and drop the stipend instruments to isolate the effect for VBT compliers.[65]

The LATE estimates for women who completed training in VBT and Outside-Village training (OVT) samples are similar to the overall average treatment effects reported in column 1. Panels A, B, and C show that the women who complete training from either treatment arm become more engaged in production of clothes for sale and report higher skills. The training also increases their income from tailoring and a sizable fraction of those earnings come from selling clothes to people outside their familial networks. Likewise, in panels D and E, trainees from VBT and OVT are more likely to own a sewing machine (by 22 percentage points and 35 pp, respectively) and they both report a negative effect on the household influence index. Where the impact differs is also interesting. OVT trainees who had to travel outside their village to acquire skills training report a statistically significant impact on: asset index (0.289), gender role perceptions (0.103), and civic engagement (0.042).[66] Meanwhile, VBT trainees report lower spending, especially on clothing expenses, as a result of training.

Among the additional set of 'marginal' trainees who take up training only when it is offered in their village, we see a similarly positive and significant impact of skills training on production, machine ownership, and income (column 4). This is an important policy-relevant finding which suggests that lowering barriers to accessing skills training attracts women who benefit from the program and should be a priority while designing such programs. In addition, this group reports lower clothing expenses, reduced perception of influence in the household, and increased usage of government services.

Because we measure impact on 17 different outcomes in Table 10, there may be a concern about significant results arising by random chance causing Type-1 error probabilities to deviate from the nominal significance levels. In the appendix, we adjust the p-values to account

[65] Stipend is uncorrelated with the other instruments by construction in the column 4 sub-sample.

[66] The positive asset effect is driven largely by sewing machines and becomes weaker when we exclude them from the household assets.

for multiple hypothesis testing (MHT), controlling the family-wise error rate (the probability of incorrectly rejecting at least one of the 17 hypotheses) at the nominal significance level, and report two sets of MHT-adjusted p-values (Bonferroni-Holm and Sidak-Holm) for each outcome variable in Appendix Table 10. The adjusted p-values account for the fact that we are testing 17 null hypotheses (not one) and can be reliably compared with the usual statistical significance thresholds.

Adjusting for MHT does not qualitatively change our results on production, skills, and income: the trainees are more skilled, more engaged in production, and earn more as a result of skills training. In the overall sample, only 1 out of the 11 statistically significant outcomes becomes insignificant at the 10% level after accounting for multiple hypothesis testing (asset index). In evaluation sub-samples (columns 2 to 4) that were less powered to begin with (e.g. due to lower take-up in outside village training) MHT adjustment makes a larger difference. For VBT (in-village training), 3 out of the 12 statistically significant outcomes become insignificant at a 10% level (all from panels D and E: household expenditure, log clothing expense, household influence index); for Outside-village training, 2 out of the previously 12 significant outcomes become insignificant at 10% after adjusting the p-values for multiple hypothesis testing (from panels D and E: asset index and civic engagement), and for VBT Compliers 5 of the 12 statistically significant outcomes move from a p-value less than 10% to on above that threshold.

5 Conclusion

Our paper highlights the importance of access constraints that women face in emerging economies, especially those related to travel outside of their communities. We find that these barriers are large and not readily reconcilable with standard costs of travel. We document a stark boundary effect, whereby training take-up for women falls substantially when they cross the village boundary. As women continue past the boundary, they also experience per-km travel costs substantially greater than standard economic costs would imply. Our results suggest that these large costs are likely generated by individual and societal constraints that women face, especially regarding safety, when leaving their own community.

These barriers have important welfare and distributional consequences for rural women and their households. We find that the skills training has substantial benefits for trainees, and that these benefits are similar for the additional women who are able to participate in the training once training barriers are alleviated. This has important policy implications as it suggests that the women who are unable to overcome access barriers are not selecting out because their realized benefits are lower, but simply that they face large non-economic costs and hence excluding them leaves large unrealized benefits on the table. Because the constraints we identify are not specifically connected to knowledge acquisition, our work also suggests that the same access issues women face in acquiring skills may also prevent them from deploying skills. In ongoing work, we are further exploring how connecting female trainees post-training to external-to-village markets can increase their returns.

Our analysis also highlights a critical program design trade-off. Distributing training and other services to small rural villages is expensive as one loses economies of scale and has to pay for more travel and distribution of training inputs. Yet without substantially compensating women for the additional costs of travel we have highlighted, take-up will be quite low outside the immediate area around a training facility and thus many women will miss beneficial skilling opportunities. By cross-randomizing service accessibility and stipend, implementers can quantify such trade-offs to make better informed program design decisions.

More broadly, our paper also shows that while it may be quite hard to change access constraints in the short run, there is room to work around them. Working with the community to address their concerns regarding female mobility through meetings and discussion had limited impact in our setting. But providing a community-vetted and safe transport service substantially mitigated the boundary effect. Preliminary cost-benefit calculations suggest that our project costs are quite comparable in achieving similar take-up rates whether we set up a training center in a village or arrange appropriate group transport for them to do so (the latter is a bit higher). Paying women an additional stipend to travel to another village is substantially more expensive (about 30-40 percent higher). In other settings, the tradeoff between in-village training and group transport will surely depend on the economies of scale each has. Whether the increased mobility from safe transport generates other longer-term benefits, or changes in norms and attitudes regarding female mobility in the long run, is a topic we hope to shed further light on in subsequent work.

References

Adoho, F., S. Chakravarty, D. T. Korkoyah, M. K. Lundberg, and A. Tasneem (2014). “The impact of an adolescent girls employment program: The EPAG project in Liberia.” In: *World Bank Policy Research Working Paper* 6832.

Aguilar, A., E. Gutiérrez, and P. S. Villagrán (2021). “Benefits and unintended consequences of gender segregation in public transportation: Evidence from Mexico City’s subway system.” In: *Economic Development and Cultural Change* 69.4, pp. 1379–1410.

Alatas, V., R. Purnamasari, M. Wai-Poi, A. Banerjee, B. A. Olken, and R. Hanna (2016). “Self-targeting: Evidence from a field experiment in Indonesia.” In: *Journal of Political Economy* 124.2, pp. 371–427.

Allen, H., M. Vanderschuren, and the University of Cape Town (2016). “Safe and Sound.” In: *The FIA Foundation, London: International Research on Women’s Personal Safety on Public Transport* FIA Foundation Research Series.6.

Anderson, K. H., R. V. Burkhauser, and J. E. Raymond (1993). “The effect of creaming on placement rates under the Job Training Partnership Act.” In: *ILR Review* 46.4, pp. 613–624.

Ashraf, N., D. Karlan, and W. Yin (2006). “Deposit collectors.” In: *Advances in Economic Analysis & Policy* 6.2.

Attanasio, O., A. Kugler, and C. Meghir (2011). “Subsidizing vocational training for disadvantaged youth in Colombia: evidence from a randomized trial.” In: *American Economic Journal: Applied Economics* 3.3, pp. 188–220.

Bandiera, O., N. Buehren, R. Burgess, M. Goldstein, S. Gulesci, I. Rasul, and M. Sulaiman (Jan. 2020). “Women’s Empowerment in Action: Evidence from a Randomized Control Trial in Africa.” In: *American Economic Journal: Applied Economics* 12.1, pp. 210–59.

Banerjee, A., R. Hanna, J. Kyle, B. A. Olken, and S. Sumarto (2018). “Tangible information and citizen empowerment: Identification cards and food subsidy programs in Indonesia.” In: *Journal of Political Economy* 126.2, pp. 451–491.

Banerjee, A., A. G. Chandrasekhar, E. Duflo, and M. O. Jackson (2019). “Using gossips to spread information: Theory and evidence from two randomized controlled trials.” In: *The Review of Economic Studies* 86.6, pp. 2453–2490.

Bapat, M. and I. Agarwal (2003). “Our needs, our priorities; women and men from the slums in Mumbai and Pune talk about their needs for water and sanitation.” In: *Environment and urbanization* 15.2, pp. 71–86.

BBC (2017). “100 Women: ’Why I invented the glass ceiling phrase’.” In: *BBC News*. URL: https://www.bbc.co.uk/news/world-42026266.

Bernhardt, A., E. Field, R. Pande, N. Rigol, S. Schaner, and C. Troyer-Moore (2018). “Male social status and women’s work.” In: *AEA Papers and Proceedings*. Vol. 108. American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203, pp. 363–367.

Bertrand, M. (2018). “Coase Lecture - The Glass Ceiling.” In: *Economica* 85.338, pp. 205–231.

Bloom, D. (1997). *After AFDC: Welfare-to-Work Choices and Challenges for States*. Tech. rep. Manpower Demonstration Research Corp., New York, NY.

Blount, S. (1995). “When Social Outcomes Aren’t Fair: The Effect of Causal Attributions on Preferences.” In: *Organizational Behavior and Human Decision Processes* 63.2, pp. 131–144. ISSN: 0749-5978.

Bolton, G. E., J. Brandts, and A. Ockenfels (2005). “Fair Procedures: Evidence from Games Involving Lotteries.” In: *The Economic Journal* 115.506, pp. 1054–1076.

Borker, G., G. Kreindler, and D. Patel (July 2020). *Safety in Numbers? Free Public Transportation for Women and Women’s Safety*. AEA RCT Registry.

Borker, G. (2022). *Constraints to Women’s Use of Public Transport in Developing Countries, Part I: High Costs, Limited Access, and Lack of Comfort*.

Burde, D. and L. L. Linden (2013). “Bringing education to Afghan girls: A randomized controlled trial of village-based schools.” In: *American Economic Journal: Applied Economics* 5.3, pp. 27–40.

Bursztyn, L., A. L. González, and D. Yanagizawa-Drott (2020). “Misperceived social norms: Women working outside the home in Saudi Arabia.” In: *American economic review* 110.10, pp. 2997–3029.

Card, D., P. Ibarrarán, F. Regalia, D. Rosas-Shady, and Y. Soares (2011). “The labor market impacts of youth training in the Dominican Republic.” In: *Journal of Labor Economics* 29.2, pp. 267–300.

Cheema, A., A. Khwaja, M. F. Naseer, and J. Shapiro (2012a). *Household and Community Surveys: Baseline Household Report for Skills*. Center for Economic Research in Pakistan.

— (2012b). *Skills Intervention Report: Results of first round of voucher disbursement and strategies for improving uptake*. Center for Economic Research in Pakistan.

Cheema, A., A. Khwaja, M. F. Naseer, J. Shapiro, S. Siddiqui, and S. Sheikh (2013). *Skills Intervention Report: Results from SFM 2012-13 Phased Evaluation*. Center for Economic Research in Pakistan.

Cheema, A., S. Khan, A. Liaqat, and S. K. Mohmand (2023). “Canvassing the gatekeepers: A field experiment to increase women voters’ turnout in Pakistan.” In: *American Political Science Review* 117.1, pp. 1–21.

Cho, Y., D. Kalomba, A. M. Mobarak, and V. Orozco-Olvera (2013). “Gender differences in the effects of vocational training: Constraints on women and drop-out behavior.” In: *World Bank Policy Research Working Paper* 6545.

Dasgupta, U., L. Gangadharan, P. Maitra, S. Mani, and S. Subramanian (2015). “Choosing to be trained: Do behavioral traits matter?” In: *Journal of Economic Behavior & Organization* 110.C, pp. 145–159.

Evans, D. K., S. Hares, P. A. Holland, A. M. Acosta, et al. (2021). *Adolescent Girls’ Safety in and Out of School: Evidence on Physical and Sexual Violence from Across Sub-Saharan Africa*. Center for Global Development.

Fiala, N., A. Garcia-Hernandez, K. Narula, and N. Prakash (2022). “Wheels of change: Transforming girls’ lives with bicycles.” In: *CESifo Working Paper*.

Field, E., S. Jayachandran, and R. Pande (2010). “Do traditional institutions constrain female entrepreneurship? A field experiment on business training in India.” In: *American Economic Review Papers and Proceedings* 100, pp. 125–129.

Field, E. and K. Vyborny (2016). “Female labor force participation in Asia: Pakistan country study.” In: *Asian Development Bank Economics Working Paper Series*.

— (2022). “Women’s Mobility and Labor Supply: Experimental Evidence from Pakistan.” In: *Asian Development Bank Economics Working Paper Series* 655.

Gazdar, H. (2003). “Gendered spaces and economic and social stagnation.” In: *Strengthening Participatory Organization: SPO Discussion Paper Series*.

Gupta, S. (2017). *Perils of the Paperwork: The Impact of Information and Application Assistance on Welfare Program Take-Up in India*. Job Market Paper.

Haerpfer, C., R. Inglehart, A. Moreno, C. Welzel, K. Kizilova, J. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, and B. Puranen (2022). “World Values Survey Wave 7 (2017-2022) Cross-National Data-Set.” In: *World Values Survey Association*.

Heblich, S., S. J. Redding, and D. M. Sturm (2020). “The making of the modern metropolis: evidence from London.” In: *The Quarterly Journal of Economics* 135.4, pp. 2059–2133.

Heckman, J. J. and J. A. Smith (2004). “The determinants of participation in a social program: Evidence from a prototypical job training program.” In: *Journal of Labor Economics* 22.2, pp. 243–298.

Hossain, M. A., K. Mahajan, and S. Sekhri (2022). “Access to toilets and violence against women.” In: *Journal of Environmental Economics and Management* 114, p. 102695.

Hoxby, C. M. and S. Turner (May 2015). “What High-Achieving Low-Income Students Know about College.” In: *American Economic Review* 105.5, pp. 514–17.

Hsu, H.-P. (2010). “How does fear of sexual harassment on transit affect women’s use of transit.” In: *Women’s Issues Transp* 46, pp. 85–94.

Jacoby, H. G. and G. Mansuri (2015). “Crossing boundaries: How social hierarchy impedes economic mobility.” In: *Journal of Economic Behavior & Organization* 117, pp. 135–154.

Jayachandran, S. (2015). “The roots of gender inequality in developing countries.” In: *Annual Review of Economics* 7, pp. 63–88.

Jensen, R. (2010). “The (perceived) returns to education and the demand for schooling.” In: *The Quarterly Journal of Economics* 125.2, pp. 515–548.

Jewkes, R. and N. Abrahams (2002). “The epidemiology of rape and sexual coercion in South Africa: an overview.” In: *Social science & medicine* 55.7, pp. 1231–1244.

Khan, A. (1999). “Mobility of women and access to health and family planning services in Pakistan.” In: *Reproductive health matters* 7.14, pp. 39–48.

Kondylis, F., A. Legovini, K. Vyborny, A. M. T. Zwager, and L. Cardoso De Andrade (2020). “Demand for safe spaces: Avoiding harassment and stigma.” In: *World Bank Policy Research Working Paper* 9269.

List, J. A. (2020). *Non est disputandum de generalizability? a glimpse into the external validity trial*. Tech. rep. National Bureau of Economic Research.

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

McIlwaine, C. (2013). “Urbanization and gender-based violence: exploring the paradoxes in the global South.” In: *Environment and Urbanization* 25.1, pp. 65–79.

McKenzie, D. (2012). “Beyond baseline and follow-up: The case for more T in experiments.” In: *Journal of development Economics* 99.2, pp. 210–221.

— (2017). “How effective are active labor market policies in developing countries? a critical review of recent evidence.” In: *The World Bank Research Observer* 32.2, pp. 127–154.

Mitra-Sarkar, S. and P. Partheeban (2011). “Abandon All Hope, Ye Who Enter Here: Understanding the Problem of Eve Teasing in Chennai, India.” In: *Transportation Research Board Conference Proceedings*. Vol. 2. 46.

Mogstad, M., A. Torgovitsky, and C. R. Walters (Nov. 2021). “The Causal Interpretation of Two-Stage Least Squares with Multiple Instrumental Variables.” In: *American Economic Review* 111.11, pp. 3663–98. DOI: 10.1257/aer.20190221. URL: https://www.aeaweb.org/articles?id=10.1257/aer.20190221.

Moser, C. O. and C. McIlwaine (2004). *Encounters with violence in Latin America: urban poor perceptions from Colombia and Guatemala*. Psychology Press.

Mumtaz, Z. and S. Salway (2005). "I never go anywhere: extricating the links between women's mobility and uptake of reproductive health services in Pakistan." In: *Social science & medicine* 60.8, pp. 1751–1765.

Muralidharan, K. and N. Prakash (2017). "Cycling to School: Increasing Secondary School Enrollment for Girls in Indias." eng. In: *American Economic Journal: Applied Economics* 9.3, pp. 321–350. ISSN: 1945-7782.

Naqvi, Z. F., L. Shahnaz, and G. M. Arif (2002). "How do women decide to work in Pakistan?" In: *The Pakistan Development Review* 41.4, pp. 495–513.

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

Porter, G., K. Hampshire, A. Abane, A. Tanle, A. Munthali, E. Robson, M. Mashiri, G. Maponya, and S. Dube (2011). "Young people's transport and mobility in sub-Saharan Africa: The gendered journey to school." In: *Documents danalisi geografica.* 57.1, pp. 61–79.

Punjab Commission on the Status of Women (2018). "Overcoming Barriers to Woman Mobility in Punjab (Policy Brief)." In: URL: `https://pcsw.punjab.gov.pk/system/files/overcoming.pdf`.

Sandell, S. and K. Rupp (1988). *Who Is Served in JTPA Programs: Patterns of Participation and Intergroup Equity*. Tech. rep. National Commission for Employment Policy, Washington, D.C.

Simic, A. (2021). "Feeling Safe and Secure from Gender-Based Violence: Highly Educated, Middle-Class Indian Women Migrants' Gendered Understandings of Integration in the UK." In: *Journal of international migration and integration* 22.1, pp. 69–86.

Stevens, F. R., A. E. Gaughan, C. Linard, and A. J. Tatem (Feb. 2015). "Disaggregating Census Data for Population Mapping Using Random Forests with Remotely-Sensed and Ancillary Data." In: *PLoS ONE* 10.2, e0107042.

Thakuriah, P. V., L. Tang, and S. Menchu (2011). "Young womens transportation and labor market experiences." In: *Transportation Research Board Conference Proceedings*. Vol. 2. 46. Transportation Research Board.

Thomson, S. (2015). "18 Countries Where Women Need Their Husbands' Permission to Work." In: *World Economic Forum. November*. Vol. 10.

Thornton, R. (2008). "The demand for, and impact, of learning HIV status." In: *American Economic Review* 98.5, pp. 1829–1863.

Wiswall, M. and B. Zafar (2015). “How Do College Students Respond to Public Information about Earnings?” In: *Journal of Human Capital* 9.2, pp. 117–169.

World Bank Group (2019). *World Bank Gender Indicators, June 2019*. World Bank Open Data. URL: `https://data.worldbank.org`.

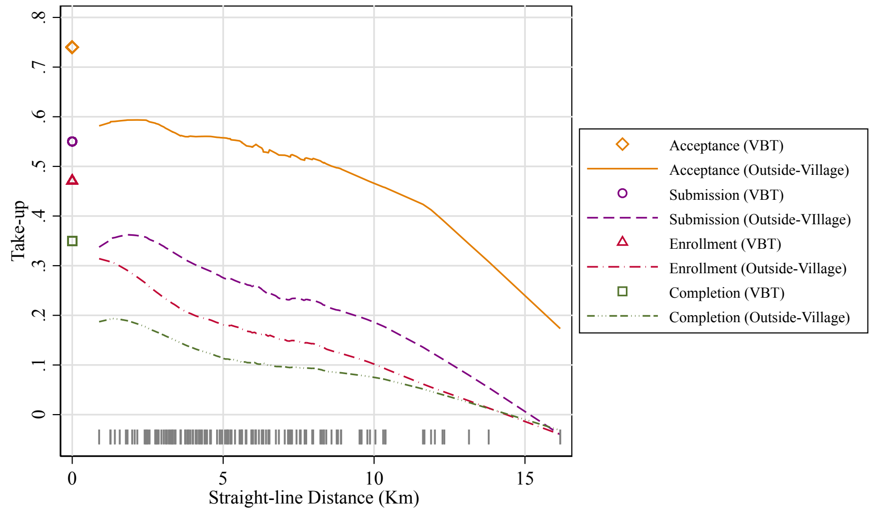
— (2020). *World Bank Gender Indicators, June 2020*. World Bank Open Data. URL: `https://data.worldbank.org`.

— (2022). *From Swimming in Sand to High and Sustainable Growth: A Roadmap to Reduce Distortions in the Allocation of Resources and Talent in the Pakistani Economy*. World Bank.

— (2023). *Women, business and the law 2023: A decade of reform*. World Bank.

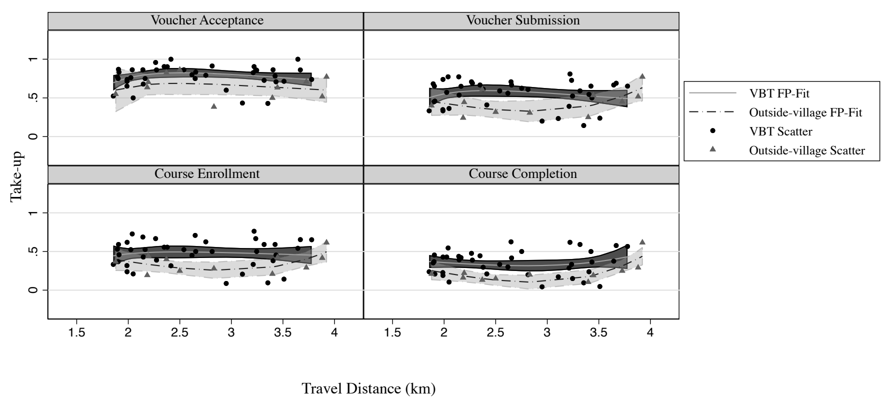
Main Tables and Figures

Figure 1: Effect of Distance on Take-up



Notes: The figure plots the mean of each take-up measure for VBT villages. For villages assigned to training outside village, it shows the local linear smooth plot of each take-up measure as a function of straight-line distance to the nearest VBT village. The grey vertical lines at the bottom of the chart show the distribution of distance values for the outside-village training set.

Figure 2: Mean Take-Up by Distance (Travel Distance to Training Center <= 4 Km)



Notes: The figure implements the graphical counterpart to the analysis in Table 2 Panel D. It shows the relationship of distance to takeup at each stage of the program for within- and outside-village training villages. Points plot the village-level mean of each take-up measure against the village-level mean travel distance for all villages within 4 km of a training center. The lines are the corresponding local linear smooth plots for each type of village.

Table 1: Effect of VBT on Take-Up

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Village Access Effect								
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: Separating Boundary and Linear Distance Effects								
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 (10 km)	-0.189***	-0.217***	-0.223***	-0.228***	-0.208***	-0.207***	-0.146***	-0.151***
	(0.069)	(0.068)	(0.049)	(0.049)	(0.040)	(0.039)	(0.033)	(0.031)
Panel C: Separating Boundary and Quadratic Distance Effects								
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 (10 km)	0.181	0.232	-0.191	-0.061	-0.317**	-0.183	-0.272**	-0.162
	(0.203)	(0.191)	(0.177)	(0.179)	(0.160)	(0.162)	(0.124)	(0.128)
(Straight-Line Distance)^2	-0.257**	-0.310**	-0.022	-0.116	0.075	-0.017	0.086	0.008
	(0.128)	(0.120)	(0.102)	(0.104)	(0.092)	(0.094)	(0.071)	(0.074)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Distance Effects								
Village Based Training	0.129***	0.129***	0.170***	0.187***	0.204***	0.225***	0.172***	0.193***
	(0.039)	(0.038)	(0.037)	(0.038)	(0.033)	(0.033)	(0.026)	(0.026)
Travel Distance (10 km)	-0.115***	-0.121***	-0.192***	-0.185***	-0.171***	-0.160***	-0.128***	-0.117***
	(0.042)	(0.041)	(0.031)	(0.032)	(0.026)	(0.027)	(0.021)	(0.022)
Panel E: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.145***	0.165***	0.109**	0.140***	0.150***	0.180***	0.124***	0.156***
	(0.044)	(0.041)	(0.045)	(0.044)	(0.041)	(0.040)	(0.034)	(0.033)
Travel Distance (10 km)	-0.056	0.015	-0.428***	-0.372***	-0.385***	-0.338***	-0.315***	-0.264***
	(0.103)	(0.097)	(0.099)	(0.095)	(0.094)	(0.090)	(0.080)	(0.077)
(Travel Distance)^2	-0.029	-0.067	0.114***	0.092**	0.104**	0.087**	0.090**	0.071**
	(0.052)	(0.050)	(0.044)	(0.043)	(0.042)	(0.040)	(0.036)	(0.034)
Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on treatment and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. The comparison group is Outside-village (Standard Information intervention). Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 2: Take-Up - Alternative Distance Controls

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Separating Boundary and Logarithmic Distance Effects								
Village Based Training	0.159***	0.170***	0.153***	0.176***	0.188***	0.213***	0.157***	0.185***
	(0.039)	(0.038)	(0.040)	(0.039)	(0.038)	(0.037)	(0.033)	(0.032)
Log. Travel Distance	-0.033*	-0.029*	-0.091***	-0.085***	-0.082***	-0.076***	-0.064***	-0.055***
	(0.017)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.013)	(0.014)
Panel B: Separating Boundary and Polynomial Distance Effects								
Village Based Training	0.139***	0.166***	0.112**	0.146***	0.158***	0.191***	0.127***	0.164***
	(0.045)	(0.042)	(0.048)	(0.046)	(0.044)	(0.042)	(0.037)	(0.036)
Travel Distance (10 km)	0.415	0.295	-0.281	-0.338	-0.424	-0.471	-0.366	-0.446
	(0.508)	(0.501)	(0.482)	(0.479)	(0.449)	(0.444)	(0.375)	(0.368)
(Travel Distance)^2	-1.567	-0.801	-0.155	0.192	0.621	0.872	0.526	0.991
	(1.707)	(1.706)	(1.495)	(1.488)	(1.304)	(1.305)	(1.079)	(1.065)
(Travel Distance)^3	1.927	0.807	0.136	-0.280	-0.961	-1.217	-0.790	-1.378
	(2.170)	(2.192)	(1.808)	(1.800)	(1.530)	(1.540)	(1.269)	(1.267)
(Travel Distance)^4	-1.013	-0.398	0.009	0.197	0.611	0.705	0.501	0.784
	(1.134)	(1.153)	(0.914)	(0.912)	(0.760)	(0.770)	(0.631)	(0.637)
(Travel Distance)^5	0.187	0.072	-0.013	-0.042	-0.125	-0.136	-0.102	-0.149
	(0.204)	(0.209)	(0.161)	(0.162)	(0.133)	(0.135)	(0.110)	(0.112)
Panel C: Separating Boundary and Non-Parametric Distance Effects								
Village Based Training	0.097**	0.132***	0.108**	0.143***	0.156***	0.191***	0.127***	0.167***
	(0.042)	(0.040)	(0.046)	(0.046)	(0.041)	(0.039)	(0.037)	(0.035)
Bin 2	-0.139***	-0.091*	-0.176***	-0.150***	-0.119***	-0.099**	-0.091**	-0.065
	(0.050)	(0.051)	(0.048)	(0.048)	(0.045)	(0.044)	(0.042)	(0.039)
Bin 3	0.047	0.103*	-0.037	-0.009	-0.061	-0.038	-0.079*	-0.047
	(0.051)	(0.057)	(0.057)	(0.063)	(0.049)	(0.054)	(0.045)	(0.049)
Bin 4	-0.179***	-0.151**	-0.205***	-0.195***	-0.157***	-0.151***	-0.117**	-0.097*
	(0.067)	(0.062)	(0.069)	(0.065)	(0.059)	(0.055)	(0.058)	(0.053)
Bin 5	-0.063	-0.023	-0.217***	-0.193***	-0.187***	-0.158***	-0.161***	-0.129***
	(0.063)	(0.060)	(0.063)	(0.063)	(0.052)	(0.052)	(0.042)	(0.040)
Bin 6	-0.152**	-0.127*	-0.219***	-0.193***	-0.189***	-0.161***	-0.156***	-0.130***
	(0.072)	(0.070)	(0.067)	(0.067)	(0.055)	(0.055)	(0.047)	(0.048)
Bin 7	-0.111*	-0.080	-0.319***	-0.267***	-0.260***	-0.203***	-0.211***	-0.152***
	(0.064)	(0.069)	(0.068)	(0.067)	(0.055)	(0.055)	(0.047)	(0.046)
Bin 8	-0.134**	-0.110*	-0.271***	-0.259***	-0.279***	-0.269***	-0.231***	-0.223***
	(0.065)	(0.059)	(0.051)	(0.051)	(0.043)	(0.045)	(0.040)	(0.042)
Bin 9	-0.283***	-0.252***	-0.374***	-0.345***	-0.326***	-0.295***	-0.250***	-0.214***
	(0.082)	(0.076)	(0.058)	(0.058)	(0.046)	(0.046)	(0.044)	(0.044)
Bin 10	-0.201***	-0.185**	-0.315***	-0.295***	-0.252***	-0.232***	-0.202***	-0.176***
	(0.077)	(0.076)	(0.060)	(0.061)	(0.051)	(0.051)	(0.043)	(0.043)
Panel D: Boundary Effects in a Regression Discontinuity Design								
Village Based Training	0.206***	0.239***	0.104***	0.150***	0.136***	0.196***	0.111***	0.166***
	(0.029)	(0.030)	(0.032)	(0.034)	(0.033)	(0.035)	(0.031)	(0.033)
Travel Distance (10 km)	-0.073	0.034	-0.413***	-0.332***	-0.336***	-0.282***	-0.252***	-0.182**
	(0.069)	(0.072)	(0.075)	(0.080)	(0.077)	(0.082)	(0.072)	(0.078)
Panel A-C Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Panel D Obs.	3250	2956	3250	2956	2955	2679	2955	2679
Mean of Comparison Group	0.714	0.714	0.452	0.481	0.235	0.258	0.118	0.129
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment and alternative distance controls. Distance variables, excluding distance bins, are scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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 (1 km). The following are the distances corresponding to each Bin: Bin 1, 1.8 km. Bin 2, 4.3 km. Bin 3, 5.8 km. Bin 4, 6.9 km. Bin 5, 8.4 km. Bin 6, 9.7 km. Bin 7, 11.2 km. Bin 8, 12.9 km. Bin 9, 15.3 km. Bin 10, 18.3 km. Controls include other treatment dummies, stipend amount dummies, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Take-Up - Additional Boundaries

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Within Village Boundaries: Settlement								
Panel A: Overall Village and Settlement Access Effects								
Village Based Training	0.222***	0.227***	0.277***	0.289***	0.283***	0.296***	0.207***	0.224***
	(0.034)	(0.034)	(0.033)	(0.034)	(0.030)	(0.031)	(0.025)	(0.026)
Settlement Based Training	0.009	0.005	0.093***	0.087***	0.109***	0.106***	0.122***	0.113***
	(0.027)	(0.027)	(0.031)	(0.032)	(0.030)	(0.031)	(0.028)	(0.029)
Panel B: Separating Village and Settlement Boundaries and Linear Distance Effects								
Village Based Training	0.140***	0.140***	0.147***	0.157***	0.169***	0.184***	0.120***	0.139***
	(0.038)	(0.037)	(0.039)	(0.039)	(0.035)	(0.035)	(0.028)	(0.029)
Settlement Based Training	-0.011	-0.013	0.059*	0.054*	0.081***	0.078**	0.102***	0.094***
	(0.028)	(0.028)	(0.032)	(0.032)	(0.030)	(0.031)	(0.028)	(0.029)
Cluster-level Travel Distance (10 km)	-0.119***	-0.125***	-0.188***	-0.189***	-0.168***	-0.166***	-0.128***	-0.124***
	(0.036)	(0.035)	(0.028)	(0.028)	(0.025)	(0.024)	(0.019)	(0.020)
Outside Village Boundaries: Number of Villages Crossed								
Panel C: Effect of Crossing Village Boundaries								
Crossed 1st Boundary	-0.138***	-0.165***	-0.287***	-0.309***	-0.314***	-0.337***	-0.247***	-0.270***
	(0.053)	(0.055)	(0.049)	(0.050)	(0.046)	(0.042)	(0.039)	(0.036)
Crossing 2 or more Boundaries	-0.094*	-0.069	-0.039	-0.027	-0.029	-0.017	-0.028	-0.017
	(0.055)	(0.056)	(0.047)	(0.049)	(0.044)	(0.041)	(0.038)	(0.036)
Panel D: Separating Village Boundaries and Linear Distance Effects								
Crossed 1st Boundary	-0.093*	-0.113**	-0.204***	-0.225***	-0.241***	-0.265***	-0.196***	-0.220***
	(0.054)	(0.055)	(0.051)	(0.053)	(0.047)	(0.045)	(0.040)	(0.038)
Crossing 2 or more Boundaries	-0.050	-0.022	0.047	0.054	0.051	0.056	0.033	0.038
	(0.056)	(0.058)	(0.048)	(0.051)	(0.043)	(0.043)	(0.039)	(0.038)
Travel Distance (10 km)	-0.103**	-0.116***	-0.202***	-0.197***	-0.182***	-0.173***	-0.136***	-0.126***
	(0.043)	(0.043)	(0.032)	(0.032)	(0.026)	(0.027)	(0.021)	(0.023)
Panel A Obs.	4841	4841	4841	4841	4841	4841	4841	4841
Panels B Obs.	4691	4691	4691	4691	4691	4691	4691	4691
Panels C - D Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of Comparison Group	0.745	0.769	0.540	0.563	0.446	0.463	0.321	0.336
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on treatment, additional boundaries, and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all specifications. Cluster-Level Travel Distance (in Panel B) is the measured distance from the respondent's cluster boundary to the training center. 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, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Within outcomes observations change due to missingness in control variables. The top two panels have fewer observations than the bottom two because of missing values on Cluster-Level Travel Distance. 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. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Economic Magnitude of the Take-Up Effect: Implied VBT-Cash Trade-off

	(1) Voucher Acceptance	(2) Voucher Submission	(3) Class Enrollment	(4) Class Completion
Panel A: Stipend Effect				
Stipend (000s in PKR)	0.035***	0.046***	0.043***	0.043***
	(0.006)	(0.006)	(0.005)	(0.005)
Panel B: Economic Magnitude of Overall Village Access Effect				
VBT Magnitude (in PKR)	6308***	7050***	7951***	6497***
	(1301)	(1049)	(1154)	(878)
Panel C: Economic Magnitudes of Boundary and Linear Distance Effects				
VBT Magnitude (PKR)	3686***	4040***	5212***	4495***
	(1161)	(951)	(997)	(800)
Distance Magnitude (PKR per 10 km)	3431***	4023***	3693***	2731***
	(1386)	(836)	(744)	(587)
Obs.	5348	5348	4900	4900
Mean of Comparison Group	0.625	0.254	0.129	0.081

Notes: Panel A reports OLS regressions of take-up variables on stipend level, treatment, and controls. Distance variables scaled to 10 km units for ease of coefficient readability. Controls include other treatment dummies, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Panels B and C report economic magnitudes which are derived by dividing the VBT coefficients by the stipend coefficients. The same thing is done with the distance coefficient. The distance magnitude coefficient in Panel C shows the economic magnitude of the treatment effect per 10 km, in PKR. Panels B and C use the same specifications used in Table 4, Panel D, which include a group transport control and average distance. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Impact of Interventions on Take-Up

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Village Based Training	0.136***	0.165***	0.109**	0.140***	0.152***	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	0.013	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 (10 km)	-0.048	0.015	-0.424***	-0.372***	-0.382***	-0.338***	-0.311***	-0.264***
	(0.099)	(0.097)	(0.097)	(0.095)	(0.091)	(0.090)	(0.077)	(0.077)
(Travel Distance)^2	-0.037	-0.067	0.112***	0.092**	0.103**	0.087**	0.088**	0.071**
	(0.050)	(0.050)	(0.043)	(0.043)	(0.040)	(0.040)	(0.034)	(0.034)
Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on treatments and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Average Distance and Average 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 stipend amount dummies, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Take-Up - Effect by Neighbor Treatment Status

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Neighbour Impact - Overall Village Access Effect								
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: Travel Distance								
Panel B: Neighbour Impact - Boundary Effect								
Village Based Training	0.139***	0.138***	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 (10 km)	-0.114***	-0.120***	-0.192***	-0.185***	-0.170***	-0.160***	-0.128***	-0.117***
	(0.042)	(0.041)	(0.031)	(0.032)	(0.026)	(0.027)	(0.021)	(0.022)
Obs.	5872	5348	5872	5348	5392	4900	5392	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: OLS regressions of uptake variables on VBT treatment and distance. Neighbor is a dummy variable marking respondents who also had a neighbor invited to enroll in the program. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all regressions. Controls include other treatment dummies, stipend amount dummies, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Accounting for Total Take-Up in Village (IV Estimates)

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Village Access Effect								
Village Based Training	0.239***	0.208***	0.283***	0.281***	0.261***	0.273***	0.209***	0.228***
	(0.083)	(0.066)	(0.070)	(0.068)	(0.065)	(0.073)	(0.049)	(0.056)
Total Women with [Take-Up Var] in Village	-0.002	0.004	0.005	0.006	0.010	0.010	0.011	0.009
	(0.013)	(0.010)	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)
Distance Measure 1: Straight-Line Distance								
Panel B: Separating Boundary and Linear Distance Effects								
Village Based Training	0.113*	0.104**	0.163***	0.171***	0.169***	0.182***	0.148***	0.164***
	(0.058)	(0.052)	(0.048)	(0.048)	(0.044)	(0.049)	(0.035)	(0.041)
Total Women with [Take-Up Var] in Village	-0.002	0.004	0.006	0.007	0.010	0.010	0.011	0.009
	(0.013)	(0.010)	(0.008)	(0.008)	(0.008)	(0.009)	(0.007)	(0.009)
Straight-Line Distance (10 km)	-0.199*	-0.165*	-0.189***	-0.176***	-0.150***	-0.148***	-0.104***	-0.108***
	(0.103)	(0.085)	(0.063)	(0.061)	(0.052)	(0.057)	(0.036)	(0.042)
Panel C: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.226***	0.223***	0.190***	0.211***	0.169***	0.191***	0.143***	0.165***
	(0.084)	(0.073)	(0.064)	(0.064)	(0.053)	(0.057)	(0.040)	(0.045)
Total Women with [Take-Up Var] in Village	0.001	0.007	0.007	0.008	0.011	0.011	0.011	0.010
	(0.011)	(0.009)	(0.008)	(0.007)	(0.008)	(0.009)	(0.007)	(0.009)
Straight-Line Distance (10 km)	0.238	0.288*	-0.077	-0.008	-0.146	-0.102	-0.115	-0.095
	(0.194)	(0.162)	(0.158)	(0.146)	(0.146)	(0.145)	(0.109)	(0.118)
(Straight-Line Distance)^2	-0.291**	-0.304***	-0.075	-0.113	-0.002	-0.030	0.008	-0.008
	(0.134)	(0.111)	(0.085)	(0.083)	(0.074)	(0.075)	(0.056)	(0.062)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Distance Effects								
Village Based Training	0.148**	0.133***	0.166***	0.170***	0.175***	0.188***	0.147***	0.167***
	(0.058)	(0.047)	(0.051)	(0.048)	(0.049)	(0.052)	(0.039)	(0.043)
Total Women With [Take-Up Var] in Village	-0.004	0.003	0.003	0.005	0.008	0.008	0.008	0.007
	(0.015)	(0.011)	(0.009)	(0.009)	(0.009)	(0.010)	(0.008)	(0.009)
Travel Distance (10 km)	-0.126	-0.099	-0.171***	-0.156***	-0.133***	-0.126***	-0.096***	-0.093***
	(0.077)	(0.061)	(0.054)	(0.052)	(0.043)	(0.048)	(0.029)	(0.034)
Panel E: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.169**	0.164***	0.117**	0.131***	0.139***	0.154***	0.115***	0.137***
	(0.067)	(0.054)	(0.049)	(0.046)	(0.048)	(0.049)	(0.037)	(0.040)
Total Women With [Take-Up Var] in Village	-0.004	0.003	0.002	0.004	0.006	0.007	0.008	0.007
	(0.015)	(0.011)	(0.010)	(0.009)	(0.010)	(0.011)	(0.008)	(0.010)
Travel Distance (10 km)	-0.049	0.022	-0.387***	-0.329**	-0.305***	-0.281**	-0.237***	-0.219**
	(0.121)	(0.098)	(0.143)	(0.130)	(0.116)	(0.119)	(0.090)	(0.097)
(Travel Distance)^2	-0.037	-0.059	0.101**	0.082*	0.080*	0.073*	0.067*	0.061*
	(0.058)	(0.049)	(0.050)	(0.047)	(0.041)	(0.041)	(0.034)	(0.036)
Panel A Obs.	5872	5348	5872	5348	5392	4900	5392	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: IV regressions of uptake variables on VBT treatment, distance, and total number of women who took up training in the village (endogenous), instrumenting the total women for each take-up measure with the randomized mean stipend in the village. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Table 8: Heterogeneous Effect on Take-Up 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***	0.123***	0.156***	0.180***	0.216***	0.247***	0.191***	0.223***
	(0.044)	(0.045)	(0.045)	(0.046)	(0.043)	(0.042)	(0.037)	(0.036)
Woman Feels Unsafe	-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 $\times$ Woman Feels Unsafe	0.093*	0.111*	0.081	0.069	0.084	0.069	0.094	0.099
	(0.054)	(0.060)	(0.067)	(0.072)	(0.068)	(0.072)	(0.062)	(0.066)
Travel Distance (10 km)	-0.112**	-0.116**	-0.204***	-0.189***	-0.191***	-0.169***	-0.154***	-0.130***
	(0.047)	(0.050)	(0.037)	(0.039)	(0.034)	(0.035)	(0.028)	(0.030)
Obs.	2948	2667	2948	2667	2680	2418	2680	2418
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, distance, and the interaction between VBT and a binary indicator of the respondent's reporting feeling unsafe. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control are included in all specifications. Controls include other treatment dummies, stipend amount dummies, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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 due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table 9: Accounting for Underpopulated Travel Paths

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Village Access Effect								
Village Based Training	0.173***	0.180***	0.281***	0.297***	0.283***	0.294***	0.215***	0.225***
	(0.033)	(0.035)	(0.035)	(0.034)	(0.033)	(0.033)	(0.026)	(0.026)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.081***	-0.079**	-0.089***	-0.087***	-0.105***	-0.108***	-0.093***	-0.097***
	(0.029)	(0.032)	(0.032)	(0.032)	(0.031)	(0.031)	(0.025)	(0.025)
Distance Measure 1: Straight-Line Distance								
Panel B: Separating Boundary and Linear Distance Effects								
Village Based Training	0.061	0.054	0.158***	0.173***	0.166***	0.184***	0.134***	0.147***
	(0.050)	(0.049)	(0.044)	(0.043)	(0.040)	(0.039)	(0.031)	(0.030)
Straight-Line Distance (10 km)	-0.190***	-0.218***	-0.212***	-0.215***	-0.201***	-0.193***	-0.141***	-0.138***
	(0.068)	(0.065)	(0.050)	(0.047)	(0.040)	(0.037)	(0.034)	(0.030)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.075***	-0.068**	-0.083***	-0.077**	-0.100***	-0.099***	-0.090***	-0.091***
	(0.028)	(0.031)	(0.031)	(0.031)	(0.030)	(0.030)	(0.025)	(0.025)
Panel C: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.138*	0.150**	0.144**	0.198***	0.115*	0.172***	0.086*	0.135***
	(0.074)	(0.069)	(0.068)	(0.068)	(0.063)	(0.061)	(0.049)	(0.046)
Straight-Line Distance (10 km)	0.073	0.110	-0.258	-0.130	-0.378**	-0.234	-0.304**	-0.176
	(0.209)	(0.194)	(0.174)	(0.173)	(0.154)	(0.151)	(0.122)	(0.120)
(Straight-Line Distance)^2	-0.182	-0.225*	0.032	-0.059	0.122	0.028	0.112	0.025
	(0.137)	(0.127)	(0.101)	(0.101)	(0.088)	(0.088)	(0.068)	(0.069)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.074***	-0.068**	-0.083***	-0.076**	-0.101***	-0.099***	-0.090***	-0.091***
	(0.028)	(0.031)	(0.031)	(0.031)	(0.030)	(0.030)	(0.025)	(0.025)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Distance Effects								
Village Based Training	0.086**	0.089**	0.147***	0.169***	0.164***	0.187***	0.126***	0.149***
	(0.042)	(0.042)	(0.041)	(0.041)	(0.037)	(0.037)	(0.030)	(0.029)
Travel Distance (10 km)	-0.118***	-0.126***	-0.186***	-0.179***	-0.166***	-0.151***	-0.124***	-0.109***
	(0.042)	(0.041)	(0.031)	(0.032)	(0.025)	(0.025)	(0.020)	(0.020)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.066**	-0.061*	-0.067**	-0.063**	-0.085***	-0.086***	-0.078***	-0.082***
	(0.028)	(0.031)	(0.029)	(0.030)	(0.028)	(0.029)	(0.023)	(0.024)
Panel E: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.104**	0.123***	0.092*	0.125***	0.118***	0.150***	0.088**	0.123***
	(0.048)	(0.045)	(0.048)	(0.048)	(0.045)	(0.044)	(0.035)	(0.035)
Travel Distance (10 km)	-0.049	0.014	-0.414***	-0.363***	-0.357***	-0.305***	-0.283***	-0.215***
	(0.107)	(0.101)	(0.098)	(0.097)	(0.090)	(0.087)	(0.073)	(0.072)
(Travel Distance)^2	-0.033	-0.068	0.109**	0.089**	0.092**	0.074*	0.076**	0.052*
	(0.053)	(0.051)	(0.042)	(0.042)	(0.039)	(0.038)	(0.031)	(0.031)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.070**	-0.069**	-0.056**	-0.054*	-0.075***	-0.078***	-0.070***	-0.076***
	(0.028)	(0.031)	(0.028)	(0.029)	(0.027)	(0.028)	(0.022)	(0.023)
Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Mean of Comparison Group	0.600	0.613	0.225	0.237	0.108	0.115	0.065	0.068
%Δ VBT Panel A (Relative to Table 4, Restricted Sample)	-23.77	-22.38	-17.42	-16.18	-19.91	-19.57	-22.49	-22.25
%Δ VBT Panel B (Relative to Table 4, Restricted Sample)	-43.03	-42.03	-24.46	-20.38	-27.22	-23.86	-29.44	-26.55
%Δ VBT Panel C (Relative to Table 4, Restricted Sample)	-25.96	-21.24	-27.23	-18.62	-36.69	-25.56	-40.95	-28.57
%Δ VBT Panel D (Relative to Table 4, Restricted Sample)	-30.56	-27.10	-20.69	-16.77	-22.88	-19.96	-26.10	-22.96
%Δ VBT Panel E (Relative to Table 4, Restricted Sample)	-22.50	-18.61	-20.59	-14.99	-21.18	-17.25	-25.01	-19.83
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, distance and the underpopulated dummy. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, any clothes stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. Observations change relative to Table 4 as not all households had GPS data to map their paths. The variable *Dummy: 500m Segment $\leq$ 50th %ile Pop. Density* is equal to 1 when the path has 500 meters or more in which the population density is below the median. Paths are calculated from the cluster centroid to the nearest training center. All percentage changes relative to Table 4 with the restricted sample are significant at the 95%. These are calculated using a nested model F-test, testing the inclusion of the dummy. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

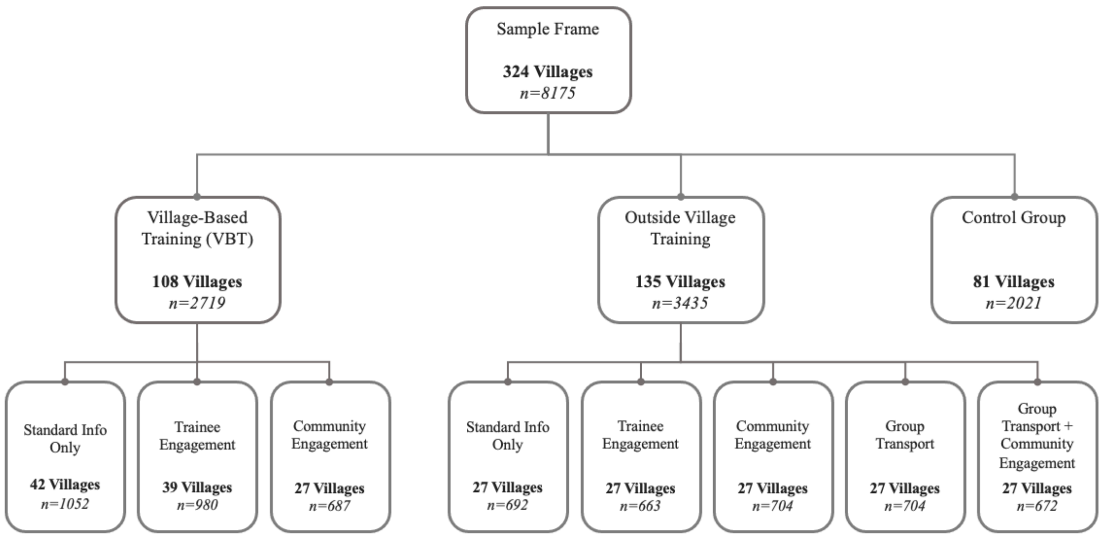
Table 10: Impact of Skills Training

	(1) Overall Impact	(2) VBT	(3) Outside Village	(4) VBT Compliers	(5) Baseline Mean
Panel A. Tailoring Engagement					
Engagement in Any Tailoring	0.090***	0.082***	0.127***	0.091***	0.050
	(0.013)	(0.014)	(0.026)	(0.022)	
Number of Minutes Per Day Spent on Tailoring	22.024***	19.754***	25.481***	27.071***	16.204
	(3.979)	(4.502)	(7.560)	(6.223)	
Number of Clothes Stitched (3-months)	1.506***	1.275***	2.156***	1.513**	0.321
	(0.438)	(0.486)	(0.768)	(0.653)	
Panel B. Tailoring Skills					
Self-Assessment of Designing Skills	0.184***	0.172***	0.229***	0.163***	0.087
	(0.023)	(0.024)	(0.046)	(0.033)	
Self-Assessment of Sewing Skills	0.399***	0.409***	0.375***	0.377***	0.153
	(0.028)	(0.030)	(0.054)	(0.042)	
Panel C. Tailoring Earnings					
Earns Income From Tailoring	0.084***	0.081***	0.088***	0.074***	0.014
	(0.013)	(0.014)	(0.023)	(0.020)	
Tailoring Earnings in PKR (3-months)	300.854***	247.854***	422.046***	307.849***	35.576
	(83.763)	(91.077)	(148.703)	(119.419)	
Tailoring Earnings From Non-Relatives in PKR (3-months)	204.439***	175.664***	306.823***	162.421*	25.725
	(55.749)	(59.408)	(114.905)	(83.042)	
Panel D. Household (HH) Level Outcomes					
Log of Average Monthly Expenditure	-0.044	-0.065*	0.005	-0.021	9.101
	(0.033)	(0.035)	(0.065)	(0.045)	
Log of Expenditure on Clothes	-0.192	-0.240*	0.400	-0.367**	7.782
	(0.129)	(0.136)	(0.283)	(0.184)	
Asset Index	0.092*	0.056	0.289**	0.013	-0.112
	(0.054)	(0.055)	(0.124)	(0.090)	
Ownership of Sewing Machine	0.231***	0.221***	0.347***	0.189***	0.438
	(0.043)	(0.044)	(0.087)	(0.068)	
Panel E. Influence & Engagement					
Household Influence	-0.066***	-0.059**	-0.146***	-0.065**	0.462
	(0.023)	(0.024)	(0.052)	(0.033)	
Business Confidence	0.022	0.017	0.033	-0.006	0.461
	(0.022)	(0.023)	(0.048)	(0.032)	
Gender-role Perceptions	0.032	0.029	0.103**	0.030	0.710
	(0.020)	(0.021)	(0.042)	(0.029)	
Government Services Usage	0.026	0.021	0.008	0.055**	0.301
	(0.017)	(0.018)	(0.036)	(0.026)	
Civic Engagement	0.005	0.003	0.042*	-0.004	0.287
	(0.011)	(0.011)	(0.023)	(0.016)	
Observations	19070	11703	12386	11169	19226
Base Group	Control	Control	Control	OVT-I	
Stipend Instruments	Yes	Yes	Yes	No	

Notes: IV estimates of the impact of skills training. Outcome variables are in rows. Columns 1-3 report the LATE averaged across different complier groups, pooling 3 rounds of post-training surveys. Controls for survey round, baseline values of the outcome variable, and grid fixed effects are included in all specifications. Column 1 reports IV estimates (2SLS) comparing all treatment groups to the control group; training completion is instrumented by the randomized treatments (VBT, OVT-Transport, OVT-Information (no transport) and eight stipend dummies). Columns 2 and 3 restrict the sample to control and specific treatment subsets (VBT and OVT) using stipend along with the relevant treatment dummies, VBT (Col 2) or OVT-transport, OVT-Information (Col 3) respectively, as instruments to estimate the LATE for a subset comprising the compliers of that treatment and stipend. Column 4 reports the impact on VBT compliers by comparing VBT to Outside-Village training arms (without group transport) using the VBT dummy as the only instrument. Column 5 reports the baseline mean value of the outcome variable. Panel E variables are additive indices, re-scaled to the 0-1 interval. As there was no baseline information for Panel B variables, we display the post-periods' control group mean in (5). Standard errors are clustered at the village level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

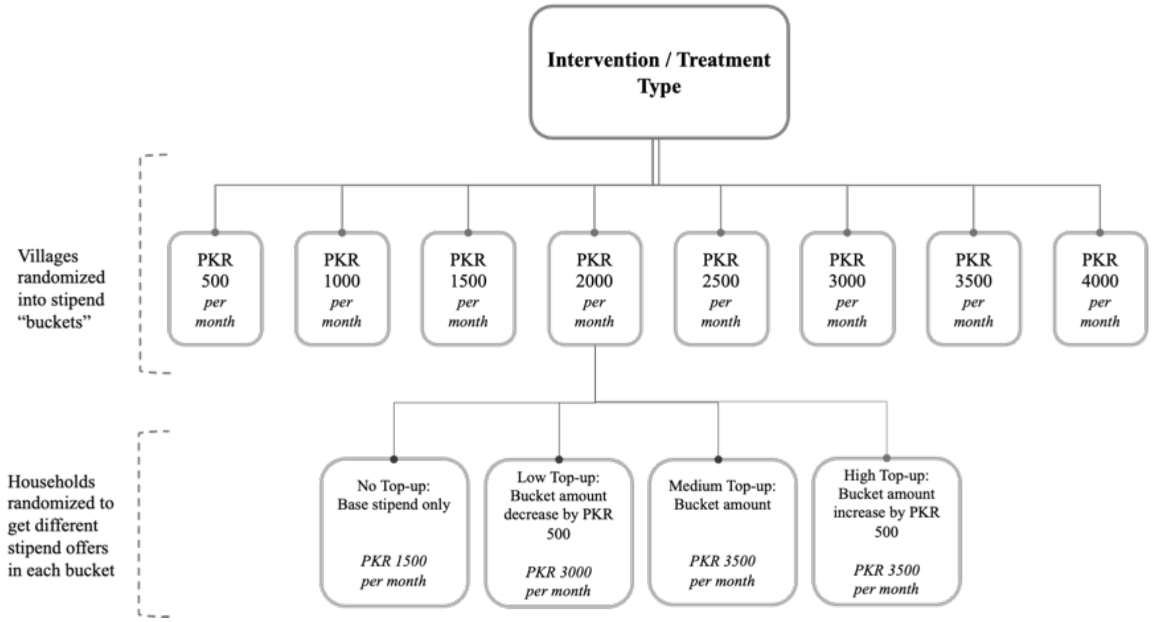
Main Appendix Tables and Figures

Appendix Figure 1: Experimental Design



Notes: The figure shows the randomization design for all treatment groups. The VBT-Standard Information and VBT-Trainee Engagement arms are slightly larger in order to increase the overall VBT sample (we allocated fewer village to VBT-Community Engagement as that intervention was costlier).

Appendix Figure 2: Stipend Randomization



Notes: The figure shows randomized stipend allocation at the village and household level. Each village in a given intervention/treatment was first allocated into one of eight stipend buckets. For each stipend bucket, 40 percent of households were randomly allocated the "No Top-up" bin, and the remaining 60 percent of households were equally divided between the Low, Medium and High Top-up bins.

Appendix Table 1: Intervention Details

	At the community level	**Provision of information materials to sample households (only)**					**Information sessions, meetings and group discussions**			**Provision of group transport**		**Voucher Offered**
Interventions	Leaflets providing basic course information and nearest training center locations	Provide oral as well as printed information materials about information session, PSDF, and the courses. Printed info included:					Short 60-minute information session held with sample women to inform them about the training program (incl pictures of training centers, as well as products and testimonials of successful graduates) and address queries	Long 75-90 minute information session held with women (from sample and respected community members) with same objective as the short session, plus discuss constraints to enrolment and	Long 75-90 minute information session held with men (from sample and respected community members) with same objective as the short session, plus discuss constraints to enrolment and possible solutions	Free group transport facility offered to trainees from the village	Male household members from the sample invited to attend a meeting to discuss, and agree upon, feasible transport arrangements for women	Offer of a voucher, filled in the name of an interested and eligible (sample) household member, that would grant her priority in admission over other applicants
		Course Booklet	List of Nearest Training Centers	Blank Enrollment Form	Stipend Envelope	Invitation Cards for Information (and/or group transport) Sessions						
Standard Information	✓	✓	✓	✓	✓							✓
Trainee Engagement	✓	✓	✓	✓	✓	✓	✓					✓
Community Engagement	✓	✓	✓	✓	✓	✓		✓	✓			✓
Group Transport (only outside-village instances)	✓	✓	✓	✓	✓	✓				✓	✓	✓

Notes: This table lists activities undertaken in various interventions. Other than group transport (GT), these activities were common across villages regardless of whether the center was located within the village (VBT) or outside the village. In addition, for outside-village interventions, a visit to the training center was facilitated by providing a reminder that it was open for visits (outside-village trainee engagement) and providing transport (outside-village community engagement)

Appendix Table 2: Summary Statistics

	Mean	Std. Dev.	Min	Max
Household Variables:				
Monthly Income (000s in PKR)	11.56	7.00	0.00	150.00
Size	6.57	2.87	1.00	31.00
Punjabi	0.47	0.50	0.00	1.00
Asset Index	-0.00	0.96	-1.13	9.56
Trainee Variables:				
Married	0.69	0.46	0.00	1.00
Has Formal Education	0.34	0.47	0.00	1.00
Paid Work	0.33	0.47	0.00	1.00
Able to Stitch	0.33	0.47	0.00	1.00
Village Distance Variables:				
Straight-Line Distance (Km)	3.22	3.64	0.00	16.17
Cluster-level Travel Distance (Km)	6.14	5.59	0.04	36.20
Travel Distance (Km)	6.10	5.29	0.17	24.21
Take-Up Outcomes:				
Voucher Acceptance	0.63	0.48	0.00	1.00
Voucher Submission	0.40	0.49	0.00	1.00
Class Enrollment	0.30	0.46	0.00	1.00
Class Completion	0.22	0.41	0.00	1.00
Baseline Impact Outcomes:				
Engagement in Any Tailoring	0.05	0.22	0.00	1.00
Number of Minutes Per Day Spent on Tailoring	16.20	89.41	0.00	1200.00
Number of Clothes Stitched (3-months)	0.32	3.34	0.00	125.00
Earns Income From Tailoring	0.01	0.12	0.00	1.00
Tailoring Earnings in PKR (3-months)	35.58	472.56	0.00	15250.00
Tailoring Earnings From Non-Relatives in PKR (3-months)	25.73	393.55	0.00	15250.00
Log of Average Monthly Expenditure	9.10	0.46	6.62	11.16
Log of Expenditure on Clothes	7.78	2.42	0.00	11.51
Asset Index	-0.11	0.89	-1.21	8.75
Ownership of Sewing Machine	0.44	0.50	0.00	1.00
Household Influence	0.46	0.35	0.00	1.00
Business Confidence	0.46	0.31	0.00	1.00
Gender-role Perceptions	0.71	0.27	0.00	1.00
Government Services Usage	0.30	0.19	0.00	1.00
Civic Engagement	0.29	0.13	0.00	0.81

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 representing the share of our sample belonging to that category. Straight-line distance is the distance from each outside-village's centroid to the nearest VBT village's centroid based on GPS. Cluster-level Travel Distance is the physically measured distance from each cluster to the training center by a surveyor on a motorcycle. Travel Distance is the measured distance from the population centroid of the village to the training center. Baseline statistics of skills impact outcome variables are provided in the last panel.

Appendix Table 3: Take-Up - Additional Boundaries (Quadratic Specifications)

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
Panel A: Within Village Boundaries: Quadratic Cluster Distance Effect								
Village Based Training	0.135***	0.139***	0.094**	0.110***	0.117***	0.135***	0.078***	0.098***
	(0.039)	(0.038)	(0.039)	(0.039)	(0.036)	(0.037)	(0.029)	(0.030)
Settlement Based Training	-0.008	-0.007	0.028	0.025	0.048	0.046	0.072**	0.063**
	(0.030)	(0.029)	(0.033)	(0.033)	(0.032)	(0.032)	(0.030)	(0.031)
Cluster-Level Travel Distance (10 km)	-0.135**	-0.121*	-0.445***	-0.428***	-0.428***	-0.418***	-0.342***	-0.340***
	(0.066)	(0.063)	(0.068)	(0.066)	(0.064)	(0.062)	(0.051)	(0.054)
(Cluster-Level Travel Distance)^2	0.008	-0.001	0.118***	0.111***	0.119***	0.116***	0.098***	0.100***
	(0.028)	(0.027)	(0.027)	(0.027)	(0.026)	(0.025)	(0.021)	(0.022)
Panel B: Outside Village Boundaries: Quadratic Distance Effects								
Crossed 1st Boundary	-0.111**	-0.147***	-0.147***	-0.181***	-0.190***	-0.222***	-0.151***	-0.185***
	(0.055)	(0.055)	(0.056)	(0.057)	(0.052)	(0.049)	(0.044)	(0.042)
Crossing 2 or More Boundaries	-0.052	-0.026	0.057	0.062	0.061	0.064	0.040	0.043
	(0.056)	(0.057)	(0.049)	(0.052)	(0.044)	(0.044)	(0.041)	(0.040)
Travel Distance (10 km)	-0.035	0.026	-0.451***	-0.397***	-0.410***	-0.365***	-0.331***	-0.282***
	(0.105)	(0.099)	(0.098)	(0.094)	(0.093)	(0.090)	(0.082)	(0.079)
(Travel Distance)^2	-0.033	-0.069	0.119***	0.097**	0.109***	0.093**	0.094***	0.075**
	(0.052)	(0.050)	(0.044)	(0.043)	(0.042)	(0.040)	(0.036)	(0.035)
Panel A Obs.	4841	4841	4841	4841	4841	4841	4841	4841
Panel B Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, additional boundaries, and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all specifications. Cluster-Level Travel Distance is the measured distance from the respondent's cluster boundary to the training center. 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 4: Take-Up - Full Treatment Breakdown

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Village Based Training	0.042	0.081	0.081	0.117**	0.128**	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***	-0.194***	-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	-0.025	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**	0.154**	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	0.014	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 (10 km)	-0.096	-0.033	-0.436***	-0.383***	-0.400***	-0.353***	-0.316***	-0.263***
	(0.098)	(0.096)	(0.100)	(0.098)	(0.093)	(0.092)	(0.078)	(0.079)
(Travel Distance)^2	-0.015	-0.045	0.119***	0.098**	0.111***	0.094**	0.092***	0.073**
	(0.050)	(0.050)	(0.045)	(0.044)	(0.042)	(0.041)	(0.035)	(0.035)
Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: OLS regressions of uptake variables on VBT treatment and distance. Distance variables scaled to 10 km units for ease of coefficient readability. 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, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 5: Treatment Impact on Trainee Behavior, Knowledge, and Expectations

	Visit	Knowledge			Quality		
	(1) Did you visit the training center	(2) Info session for men held to inform about training	(3) Info session for women held to inform about training	(4) Knowledge on the course content	(5) Rank of quality of the course content	(6) Rank of quality of the course trainers	(7) Rank of quality of the training facilities
Village Based Training	0.174***	-0.031	-0.007	0.064	0.164	0.152	0.213
	(0.053)	(0.033)	(0.028)	(0.065)	(0.215)	(0.209)	(0.194)
Community Engagement	0.365***	0.817***	0.925***	0.179***	0.816***	0.780***	0.788***
	(0.029)	(0.029)	(0.019)	(0.040)	(0.133)	(0.133)	(0.129)
Trainee Engagement	0.301***	0.062***	0.905***	0.228***	0.754***	0.733***	0.702***
	(0.034)	(0.016)	(0.021)	(0.041)	(0.149)	(0.145)	(0.143)
Group Transport	0.017	0.103***	0.006	0.138***	-0.030	0.013	0.026
	(0.032)	(0.036)	(0.028)	(0.052)	(0.157)	(0.160)	(0.152)
Travel Distance (10 km)	-0.391***	-0.017	-0.051	0.005	-0.025	-0.075	-0.123
	(0.117)	(0.085)	(0.077)	(0.159)	(0.523)	(0.525)	(0.460)
(Travel Distance)^2	0.132**	-0.012	0.020	-0.068	-0.070	-0.101	-0.046
	(0.052)	(0.039)	(0.034)	(0.076)	(0.249)	(0.255)	(0.211)
Obs.	5571	5259	5335	5571	4394	4192	4274
Mean Outcome Var	0.347	0.330	0.579	2.074	3.127	3.084	3.099
Controls							

Notes: OLS regressions of treatment arms and distance. Distance variables scaled to 10 km units for ease of coefficient readability. The outcome in column 1 is a dummy that comes from the question: *Did you visit your nearest training center?* The outcome in column 2 is a dummy that comes from the question: *Was an information session/meeting for men held in your village to inform men about the training program?* The outcome in column 3 is a dummy that comes from the question: *Was an information session for women held in your village to inform women about the training program?* The outcome in column 4 comes from the question: *What will the course train you in and can you tell us a bit about the content to be covered in the course?* and ranks knowledge from 1 to 3, from non knowing at all, to knowing well. The outcome in column 5 is a likert scale variable [1-5] that comes from the question: *How would you rank the quality of the course content?* The outcome in column 6 is a likert scale variable [1-5] that comes from the question: *How would you rank the quality of the trainers?* The outcome in column 7 is a likert scale variable [1-5] that comes from the question: *How would you rank the quality of the training facilities?* Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 6: Examining Boundary Effects on Information about Training Centers

	Were you aware of a training course being provided in village or nearby?		Provided the correct village name in which training was being provided	
	(1)	(2)	(3)	(4)
Village Based Training	0.002	-0.000	0.001	-0.002
	(0.010)	(0.010)	(0.011)	(0.011)
Travel Distance (10 km)	-0.023	-0.022	-0.023	-0.022
	(0.015)	(0.016)	(0.015)	(0.016)
Obs.	5873	5348	5873	5348
Mean Outcome Var	0.986	0.988	0.986	0.987
Controls		X		X

Notes: OLS regressions of knowledge of training center on VBT and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 7: Examining Boundary Effects for Public Transport Modes

	Public Mode Available			Wait time (mins)			Connecting time (mins)			One Way Fare (PKR)		
	(1) Motorbike	(2) Bus	(3) Qingchi	(4) Motorbike	(5) Bus	(6) Qingchi	(7) Motorbike	(8) Bus	(9) Qingchi	(10) Motorbike	(11) Bus	(12) Qingchi
Village Based Training	-0.022	-0.025	-0.126	6.175	-15.109	-8.112	-2.297	-1.121	-3.048	-7.291	-5.225	-1.799
	(0.070)	(0.091)	(0.098)	(5.172)	(10.805)	(7.463)	(5.058)	(3.175)	(2.987)	(6.942)	(4.115)	(3.563)
Travel Distance (10 km)	-0.005	0.057	-0.067	7.590	-8.745	-4.688	6.591	10.526***	11.186***	5.332	13.657**	8.587**
	(0.056)	(0.083)	(0.092)	(5.758)	(8.059)	(5.492)	(4.685)	(3.572)	(3.093)	(5.726)	(6.299)	(3.857)
Obs.	4639	4639	4639	637	1494	1776	637	1494	1776	637	1494	1776
Mean Outcome Var	0.137	0.319	0.378	22.286	36.149	34.352	9.818	17.421	15.457	36.727	24.903	25.065

Notes: OLS regressions of public transport variables on VBT and distance. Distance variables scaled to 10 km units for ease of coefficient readability. These measures were constructed based on the distance mapping exercise. Each travel route (to the nearest training center) was mapped for the relevant transport mode with enumerators actually taking these routes and estimating fares and average wait, travel and connecting times for each mode of transport by talking with drivers and passengers at the relevant stops for each mode. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 8: Examining Boundary Effects on Trainee Preference to Walk to Center

	Intended to Walk Full Sample		Intended to Walk RDD Sample	
	(1)	(2)	(3)	(4)
Panel A: Separating Boundary and Linear Distance Effects				
Village Based Training	0.505***	0.506***	0.227***	0.231***
	(0.041)	(0.042)	(0.059)	(0.054)
Travel Distance (10 km)	-0.154***	-0.161***	-1.225***	-1.238***
	(0.031)	(0.032)	(0.124)	(0.123)
Panel B: Separating Boundary and Logarithmic Distance Effects				
Village Based Training	0.439***	0.441***	0.235***	0.238***
	(0.046)	(0.047)	(0.057)	(0.053)
Log Travel Distance	-0.382***	-0.393***	-1.513***	-1.527***
	(0.064)	(0.065)	(0.148)	(0.146)
Panel C: Separating Boundary and Polynomial Distance Effects				
Village Based Training	0.322***	0.330***	0.251***	0.252***
	(0.053)	(0.052)	(0.063)	(0.058)
Travel Distance (10 km)	-0.853***	-0.850***	-1.764***	-1.727***
	(0.136)	(0.133)	(0.355)	(0.337)
(Travel Distance)^2	0.339***	0.337***	1.144	1.042
	(0.066)	(0.064)	(0.756)	(0.701)
Panel D: Separating Boundary and Non-Parametric Distance Effects				
Village Based Training	0.373***	0.378***	0.317***	0.323***
	(0.059)	(0.059)	(0.075)	(0.072)
Bin 2	-0.314***	-0.317***	-0.332***	-0.328***
	(0.063)	(0.064)	(0.071)	(0.068)
Bin 3	-0.309***	-0.326***		
	(0.072)	(0.070)		
Bin 4	-0.275***	-0.278***		
	(0.073)	(0.073)		
Bin 5	-0.357***	-0.362***		
	(0.064)	(0.064)		
Bin 6	-0.352***	-0.348***		
	(0.064)	(0.063)		
Bin 7	-0.313***	-0.321***		
	(0.068)	(0.069)		
Bin 8	-0.320***	-0.327***		
	(0.069)	(0.069)		
Bin 9	-0.355***	-0.362***		
	(0.065)	(0.065)		
Bin 10	-0.341***	-0.357***		
	(0.063)	(0.062)		
Obs.	5873	5348	3250	2956
Mean of Comparison Group	0.095	0.093	0.464	0.468
Controls		X		X

Notes: OLS regressions of Walking Intention variable on VBT and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Within outcomes observations change due to missingness in control variables. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table 9: Impact of Skills Training over time

	(1) Overall Impact	(2) Round 2	(3) Round 3	(4) Round 4
A. Tailoring Engagement				
Engagement in Any Tailoring	0.090***	0.100***	0.054***	0.116***
	(0.013)	(0.020)	(0.019)	(0.022)
Number of Minutes Per Day Spent on Tailoring	22.024***	23.945***	15.794***	26.161***
	(3.979)	(6.058)	(5.077)	(6.417)
Number of Clothes Stitched (3-months)	1.506***	0.910**	1.596***	2.162**
	(0.438)	(0.357)	(0.525)	(0.909)
B. Tailoring Skills				
Self-Assessment of Designing Skills	0.184***	0.197***	0.186***	0.170***
	(0.023)	(0.033)	(0.035)	(0.031)
Self-Assessment of Sewing Skills	0.399***	0.480***	0.484***	0.229***
	(0.028)	(0.037)	(0.041)	(0.036)
C. Tailoring Earnings				
Earns Income From Tailoring	0.084***	0.070***	0.122***	0.065***
	(0.013)	(0.017)	(0.022)	(0.018)
Tailoring Earnings in PKR (3-months)	300.854***	162.400**	378.304***	395.549**
	(83.763)	(65.430)	(111.619)	(170.189)
Tailoring Earnings From Non-Relatives in PKR (3-months)	204.439***	95.073**	281.549***	266.575**
	(55.749)	(47.209)	(81.650)	(109.317)
D. Household (HH) Level Outcomes				
Log of Average Monthly Expenditure	-0.044	-0.068*	-0.010	-0.045
	(0.033)	(0.037)	(0.045)	(0.041)
Log of Expenditure on Clothes	-0.192	-0.264	-0.302*	0.015
	(0.129)	(0.232)	(0.163)	(0.202)
Asset Index	0.092*	0.059	0.054	0.161**
	(0.054)	(0.067)	(0.068)	(0.068)
Ownership of Sewing Machine	0.231***	0.224***	0.259***	0.214***
	(0.043)	(0.057)	(0.051)	(0.051)
E. Influence & Engagement				
Household Influence	-0.066***	-0.041	-0.068**	-0.086***
	(0.023)	(0.031)	(0.033)	(0.029)
Business Confidence	0.022	0.046	-0.024	0.039
	(0.022)	(0.036)	(0.034)	(0.032)
Gender-role Perceptions	0.032	0.080**	0.002	0.015
	(0.020)	(0.038)	(0.034)	(0.028)
Government Services Usage	0.026	0.029	0.037	0.010
	(0.017)	(0.025)	(0.025)	(0.019)
Civic Engagement	0.005	0.007	0.005	0.005
	(0.011)	(0.017)	(0.015)	(0.017)
Observations	19070	7020	5752	6298

Notes: IV estimates of the impact of skills training. Outcome variables are in rows. Training completion is instrumented by the randomized treatments (VBT, OVT-Transport, OVT-Information (no transport) and eight stipend dummies). Column (1) reports the overall impact relative to the control group, using data from all 3 post-training rounds. Columns (2), (3), and (4) report the same regresion restricting to round 2, 3, and 4 data, respectively. The skills training intervention took place from March to Jun of 2014. Round 2 was conducted in December 2014, Round 3 in December 2015 and Round 4 in January 2017. Round 3 has less observations due to a cost-cutting sample reduction that was introduced by design. Panel E variables are additive indices, re-scaled to the 0-1 interval. Standard errors are clustered at the village level. $*p < 0.10, **p < 0.05, ***p < 0.01$.

Appendix Table 10: Impact of Skills Training - Multiple Hypothesis Testing

	Coef	p	Bonferroni-Holm	Sidak-Holm	Coef	p	Bonferroni-Holm	Sidak-Holm
	(1) Overall Impact				(2) VBT			
Engagement in Any Tailoring	0.090	0.000	0.000	0.000	0.082	0.000	0.000	0.000
Number of Minutes Per Day Spent on Tailoring	22.024	0.000	0.000	0.000	19.754	0.000	0.000	0.000
Number of Clothes Stitched (3-months)	1.506	0.001	0.005	0.005	1.275	0.009	0.078	0.076
Self-Assessment of Designing Skills	0.184	0.000	0.000	0.000	0.172	0.000	0.000	0.000
Self-Assessment of Sewing Skills	0.399	0.000	0.000	0.000	0.409	0.000	0.000	0.000
Earns Income From Tailoring	0.084	0.000	0.000	0.000	0.081	0.000	0.000	0.000
Tailoring Earnings in PKR (3-months)	300.854	0.000	0.003	0.003	247.854	0.007	0.065	0.063
Tailoring Earnings From Non-Relatives(3-months)	204.439	0.000	0.003	0.003	175.664	0.003	0.034	0.034
Log of Average Monthly Expenditure	-0.044	0.180	0.658	0.502	-0.065	0.060	0.421	0.352
Log of Expenditure on Clothes	-0.192	0.138	0.658	0.502	-0.240	0.077	0.464	0.383
Asset Index	0.092	0.092	0.643	0.491	0.056	0.307	0.921	0.667
Ownership of Sewing Machine	0.231	0.000	0.000	0.000	0.221	0.000	0.000	0.000
Household Influence	-0.066	0.004	0.029	0.029	-0.059	0.016	0.125	0.118
Business Confidence	0.022	0.322	0.658	0.540	0.017	0.470	0.939	0.719
Gender-role Perceptions	0.032	0.110	0.658	0.502	0.029	0.169	0.846	0.604
Government Services Usage	0.026	0.128	0.658	0.502	0.021	0.224	0.898	0.638
Civic Engagement	0.005	0.644	0.658	0.644	0.003	0.783	0.939	0.783
	(3) Outside Village				(4) VBT Compliers			
Engagement in Any Tailoring	0.127	0.000	0.000	0.000	0.091	0.000	0.000	0.000
Number of Minutes Per Day Spent on Tailoring	25.481	0.001	0.009	0.009	27.071	0.000	0.000	0.000
Number of Clothes Stitched (3-months)	2.156	0.005	0.050	0.049	1.513	0.020	0.204	0.186
Self-Assessment of Designing Skills	0.229	0.000	0.000	0.000	0.163	0.000	0.000	0.000
Self-Assessment of Sewing Skills	0.375	0.000	0.000	0.000	0.377	0.000	0.000	0.000
Earns Income From Tailoring	0.088	0.000	0.002	0.002	0.074	0.000	0.002	0.002
Tailoring Earnings in PKR (3-months)	422.046	0.005	0.050	0.049	307.849	0.010	0.109	0.104
Tailoring Earnings From Non-Relatives(3-months)	306.823	0.008	0.061	0.059	162.421	0.050	0.370	0.315
Log of Average Monthly Expenditure	0.005	0.945	1.000	0.969	-0.021	0.647	1.000	0.984
Log of Expenditure on Clothes	0.400	0.158	0.631	0.497	-0.367	0.046	0.370	0.315
Asset Index	0.289	0.020	0.118	0.112	0.013	0.889	1.000	0.991
Ownership of Sewing Machine	0.347	0.000	0.001	0.001	0.189	0.006	0.069	0.067
Household Influence	-0.146	0.005	0.050	0.049	-0.065	0.046	0.370	0.315
Business Confidence	0.033	0.487	1.000	0.865	-0.006	0.856	1.000	0.991
Gender-role Perceptions	0.103	0.014	0.098	0.094	0.030	0.305	1.000	0.838
Government Services Usage	0.008	0.824	1.000	0.969	0.055	0.033	0.293	0.258
Civic Engagement	0.042	0.062	0.309	0.273	-0.004	0.792	1.000	0.991

Notes: This table reproduces the results of the skills training impact, with multiplicity (family-wise) adjusted p-values. Outcome variables are listed in first column. For each impact comparison, we report the original (unadjusted) p-values, followed by adjusted p-values using Bonferroni-Holm and Sidak-Holm methodology testing 17 hypotheses for each column set.

Supplementary Appendices

Appendix A: Program and Data Details

Early Pilot Work: Understanding Access Constraints

The design of the program we study in this paper was based on our prior work with the Punjab Skills Development Fund (PSDF). The first major undertaking of our collaboration was a large-scale baseline survey exercise of over 11,000 households in the program region. This exercise aimed to understand the demand for skills and the specific access constraints faced by potential program participants. To develop a holistic understanding of the local skills and labor markets, we conducted village and employer surveys in each of the program districts in addition to the household surveys. The exercise revealed significant latent demand for skills acquisition from both households and employers. Over 92 percent of households indicated their willingness to nominate at least one male and female member for skills training. Among those nominated, 96 percent of men and 97 percent of women reported a desire to acquire skills, and two-thirds of households reported 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 the 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, Khwaja, Naseer, and Shapiro 2012a). This is consistent with the positive results on economic impact in this paper. Moreover, individuals also recognized non-economic returns to basic skills, such as enhanced degree of political engagement, ability to exercise political rights, and health status.

Based on the high demand for, and expectations of high return from, skills training, PSDF launched its first program Skills for Employability (SFE) in late 2011. SFE offered a variety of training courses to both (urban and rural) men and women. Despite the large expressed demand for training, CERP's evaluation revealed low take-up. Take-up was particularly low for females. Only 7 percent of women offered vouchers for training ended up enrolling in courses, and only 3 percent of women completed the course (Cheema, Khwaja, Naseer, and Shapiro 2012b). Even fewer women who enrolled came from poor and vulnerable households and/or lived far from the training centers. Through field visits and analytical work, we found that physical distance to the training center was one of the main reasons for the lack of enrollment in skills training programs for women. Almost half of the targeted trainees who refused to participate in the SFE program identified distance as the primary constraint.

Using the lessons learned from the first training rollout (the SFE program), PSDF launched a small pilot in 2012-13, specifically targeting rural women in 52 of the villages originally surveyed in the 2011 baseline surveys. In the pilot, they offered training courses in tailoring, rural dairy products, and home decoration. The pilot was designed to specifically address distance-related barriers and constraints arising due to household and societal concerns. The interventions introduced in the pilot to address these constraints included: (a) women's engagement in villages without a training center through individual and group meetings that were designed to encourage them to participate in training by stressing its usefulness and by using discussion and guidance to figure out ways to manage household concerns and (b) combining women's engagement with village-based training to test the additional effect of reducing the distance to training. In addition to these two interventions, the pilot also included an information arm that provided women information about the training provider, center location, stipend amounts, the content, duration, and timing of the course (Cheema, Khwaja, Naseer, Shapiro, et al. 2013).

Initial results showed 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 engagement meetings, 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, signaling 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 main program studied in the paper.

The main program that we study in this paper was designed and rolled out in 2013-14 in a larger sample of villages with additional design variations to address the constraints identified by these earlier pilots. It built on the pilot and used exogenous variation in the location of training centers to evaluate the impact of distance-related access constraints on women's take-up at scale. Unlike the pilot, the main program studied in this paper includes a standard information intervention in both VBT and outside-village training villages. It also introduced two modifications of the women's engagement arm from the pilot by unbundling this intervention. The new trainee engagement (TE) arm used group meetings for women to increase the information content and the salience of training. The community engagement (CE) arm organized separate information sessions for: (a) male and female members of the community with participation from male and female village elders; and (b) male and female heads of sample households. The main aim of CE was to invite participants to identify constraints to women's enrollment training as well as their potential solutions and to encourage male members to facilitate women's access to training. We decided to include group sessions for male family members and village elders because findings from the earlier studies showed that social concerns of household heads adversely affected women's access to training. Unlike the pilot, the TE and CE arms were introduced in both villages with and without training centers. A separate group transport arm was also introduced in villages without training centers in the main program because findings from earlier studies showed that the lack of availability of cost-effective and safe transport options was a major constraint for women's enrollment in villages without a training center. Finally, the main program included exogenous stipend variations at the village level to quantify the distance penalty in monetary terms.

Data Details and Sources

The Figure below provides a timeline of data collection followed by details of each data collection exercise.

Appendix Table A1: Timeline of Data Collection Activities

Activity	Period
Baseline Survey	Oct - Nov 2013
Information Visit	Dec 2013 - Jan 2014
Voucher Delivery Visit	Feb 2014
Voucher Submission Lists	Feb 2014
Initial Enrollment Lists	Feb 2014
Monthly Attendance Audits	Mar - Jul 2014
First Follow-Up Tracker Survey	Nov - Dec 2014
Cluster Level Distance Survey	Aug 2015
Second Follow-Up Tracker Survey	Dec 2015
Endline Survey	Jan 2017

- **Household Baseline Survey:** The baseline survey was conducted in the full sample before villages were randomized into treatment and control. The survey collected data on pre-treatment demographic characteristics of households, as well as solicited 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 household to the nearest training center.

- **Information Visit:** During this initial visit in all treatment villages (only), sample households were provided with course booklets highlighting the nearest center locations and informed about the training program and stipend. Specifically, household members were informed that the government had launched this training scheme for women in their area, hired the best training organizations for this purpose, and that they could participate in this training for free by nominating one eligible member of their household to receive a voucher. This visit confirmed the information on all female members who were age-eligible to participate in the training program. The households were then given a blank enrollment form for the upcoming training and informed about any additional treatments (such as engagement meetings or the stipend top-up amount) according to the household's randomized treatment status.

- **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, 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 told that due to a limited number of seats, the voucher does not ensure a spot in the course, but it will help with 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 that she wanted to attend and accepted the offer of provisional course enrollment. We recorded acceptance rates at the time of voucher 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. 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 applications (including vouchers) was then given to us by each training center. This generated our voucher submission outcome—a measure of 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 submissions with respondents during the follow-up survey.

- **Initial Enrollment Lists:** As the training was open to all eligible women in the village, we also received applications from self-applicants outside our sample (i.e., women who opted to register themselves for training without targeted information or engagement). Since the number of submitted vouchers and applications often exceeded the training center capacity (20 students per center), we conducted a random ballot to ensure a transparent allocation of slots to applicants without compromising the evaluation. Applicants were therefore given a randomized sort order and categorized as either "admitted" (enrolled in the program) or "wait-listed" (trainees who we kept as a backup in case admitted trainees dropped out) with our voucher household applicants given priority. A week 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 trainee's attendance. 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 contacted these newly admitted students and sent the training centers an updated roster in order to ensure the wait list order was properly followed. This detailed field activity allowed us to track those respondents who ultimately chose to enroll which was later confirmed during the follow-up survey. This forms our third measure of take-up—course enrollment.

- **Monthly Attendance Audits:** Once the class enrollment lists were finalized at the end of the 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 trainees who met the attendance criteria, but these visits additionally provided detailed information on how long each respondent remained in the program and their eventual course completion status. Consequently, we can easily identify which trainees had satisfactory attendance (80 percent) through the course's completion. We also confirmed each individual's class completion status through the follow-up survey. This provided our fourth and final measure of take-up.

- **Household Follow-up Surveys:** To measure the impact of skills training, three follow-up surveys were conducted. These surveys were scheduled a year apart from each other to prevent any seasonal variation in women's income from affecting our results.[67] Five months after all training activities had ended, we revisited each household to administer the first follow-up survey. The main purpose of this survey was to gather updated information on respondent's post-treatment outcomes which are being used to measure the training program's impact. However, we also used this opportunity to ask respondents about their take-up status. We use this information to confirm the status determined from the administrative data gathered above.

- **Cluster-level Distance Survey:** The survey was designed 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 clusters, and a map was then made of each village demarking these clusters. Routes were then traced on each map for all means of transport: private modes (walk, cycle, motorcycle, a rickshaw-like vehicle called qingchi, and car), public modes (bus, qingchi, and motorcycle), and group transport. Refer to Appendix Figure A1 below for an example of a map.

  Following the paths marked on the maps, enumerators measured the distance from each cluster 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 means of transport.

  The approach to calculate distance varied by the means of transport and the type of cluster. Three types of clusters were identified: clusters within a VBT village that contained the training center (special clusters); clusters that did not host a training center and belong to a VBT village (non-special clusters); and clusters from outside-village training villages.

[67] The sample size for the second follow-up survey (round 3) was cut by randomly dropping 12.5 percent of households from that round to manage the evaluation costs. The target sample for survey rounds 2 and 4 was the same as the round 1 baseline survey.

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

2. Non-special clusters: Distance by private transportation is measured from the cluster boundary to the training center of the village. In the case of public transportation, we calculated the distance in tranches: i) first connecting route: cluster 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. Clusters from outside-village training villages: Distance by private transportation was calculated in tranches and then added up: i) from the cluster boundary to the boundary of the outside-village training village where the cluster is in, ii) from the outside-village training 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: cluster 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: cluster 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 means of public transport.[68] 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, qingchi, and motorcycle stops, which were measured by having enumerators ask two individuals waiting at each stop what their average wait times were.

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

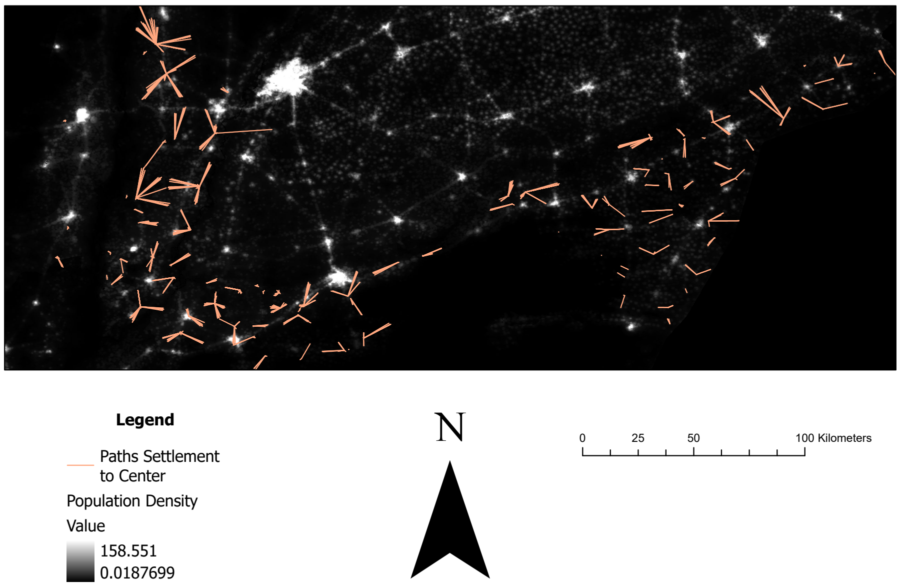
Appendix Figure A1: Map for Cluster-level Distance Survey

- **Population Density Data:**

  The population density data was downloaded from WorldPop's Pakistan data page. The data provide estimated total number of people in 2013 per three arc-second grid cell, approximately 100 m × 100 m at the equator. Estimates are from a Random Forest-based dasymetric redistribution of census data using on a range of physical features (Stevens et al. 2015). The minimum value of the raster is imputed to the cells that have no information (e.g cells that mostly cover a water feature). Appendix Figure A2 shows straight-line paths from cluster centroids to the nearest training center overlaid on the population density raster.

  We use these data to calculate the mean population density along the straight-line paths from the cluster-level centroids to the nearest training center. Then, to characterize risky/insecure paths we define a dummy variable equal to one when the path has at least either 500 or 250 meters of an underpopulated segment, which we define as path segments through cells with population density below the median population density observed along all travel paths in our sample.

Appendix Figure A2: Cluster-Center Paths and Population Density Raster



Training Cost Calculations

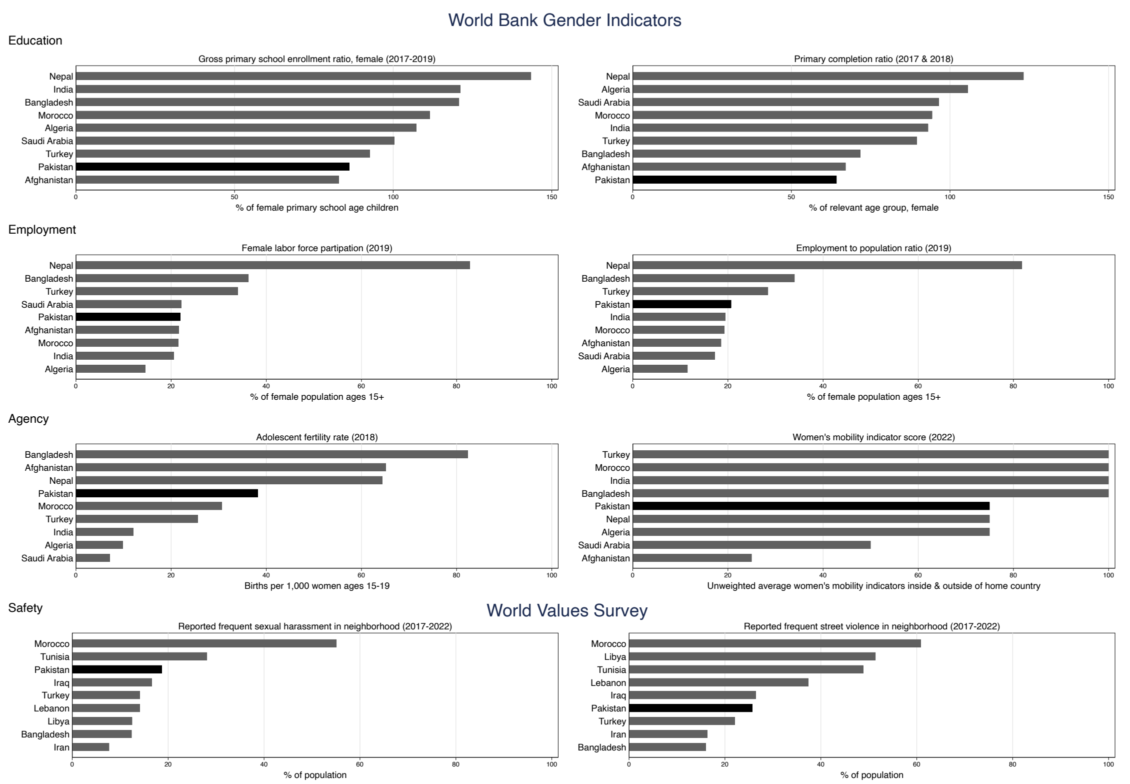
Costs for training combine four general categories: training expenditures; mobilization, advertising, and communication costs; operational costs; and monitoring and evaluation costs. Our implementing partner PSDF aggregated across multiple training service providers (TSP) to calculate total costs by category (See Cheema et al., 2019, Table 11). We obtain the cost per trainee by dividing those costs by the number of participants who completed the training. Training was provided six days per week for four months.

Training expenditures include rent, utilities, security, trainer salaries (including travel), materials, sewing machines, baseline trainees stipends of PKR 1,500 per month, and furniture. Mobilization and related costs include home visits, printed materials, advertising, and organizing community information sessions. Operational costs include rent, equipment depreciation, and electricity. Monitoring and evaluation costs include TSP and PSDF expenditures on day-to-day programmatic oversight.

Group transport was organized by the community and paid for by the survey firm. Costs varied by village based on distance to the nearest training center, ranging from PKR 6,000 to 28,000 per month. Using the fare estimates from column 4 in Appendix Table B8, along with valuing commute time at the prevailing wage rate, we estimate that the median woman in our sample would incur additional costs of around PKR 5,000-6,000 a month if she were to travel to the training on a private motorbike. Average group transport cost per participant was estimated at PKR 13,770 per four-month training session.

Appendix B: Additional Figures and Tables

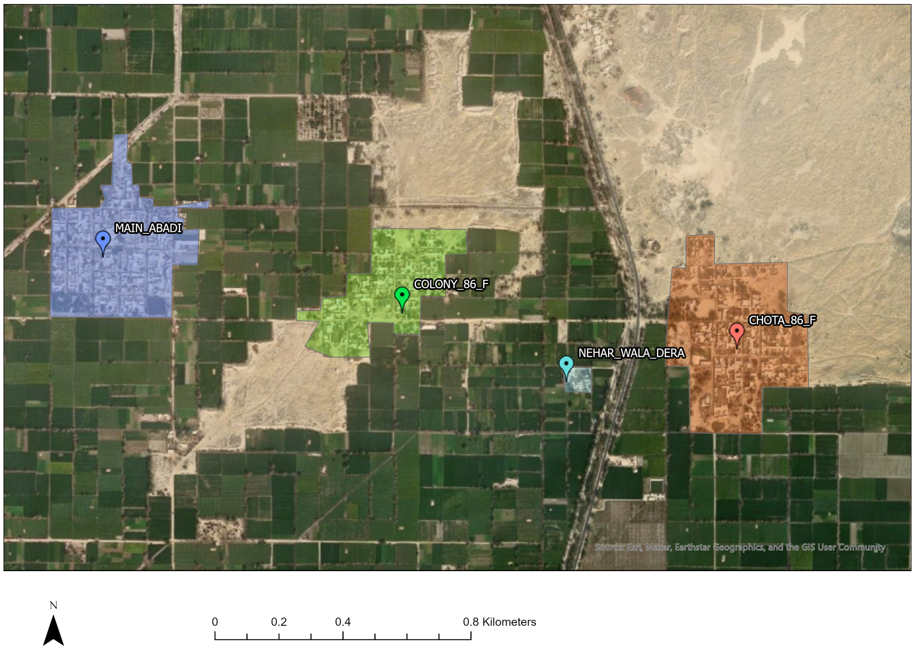
Appendix Figure B1: Comparing Gender Equity Measures Across South Asia and MENA



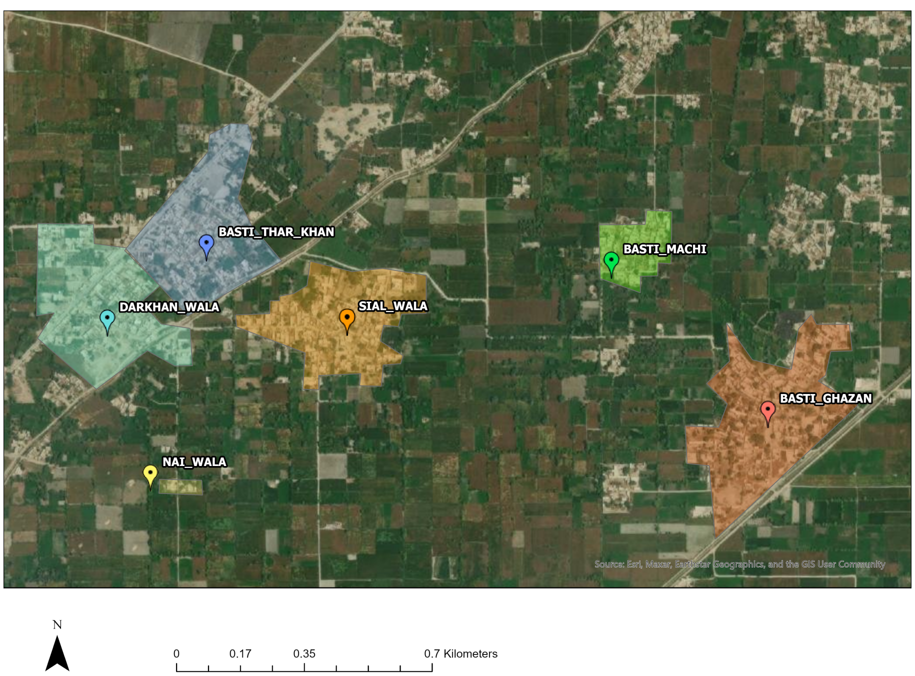
Notes: These figures present data from World Bank's Gender Indicators (rows 1-3) and the World Values Survey (row 4) that examines a variety of indicators across countries including Pakistan. The women's mobility score (row 3, column 2) is composed of four individual indicators of women's mobility, each given 25 points and scaled to 100. The indicators are (i) A woman can apply for a passport in the same way as a man; (ii) A woman can travel outside the country in the same way as a man; (iii) A woman can travel outside her home in the same way as a man; and (iv) A woman can choose where to live in the same way as a man.

Appendix Figure B2: Google Maps Image of Settlements

Panel A



Panel B



Notes: The figures show two sample villages and highlight the distinct settlements in each village

Appendix Table B1: Pilot Study - Effect of Distance Constraints on Enrollment for Women and Men, Non-Random Assignment

	Enrolled (Full Sample)		Stayed (Full Sample)	
	(1)	(2)	(3)	(4)
Rural Respondents Only				
Panel A: Linear Distance				
Min Dist to Eligible Course in 100KM	-0.1066**	-0.0095	-0.0901**	-0.0121
	(0.0428)	(0.0137)	(0.0391)	(0.0119)
Asset Index	0.0209	0.0277	0.0019	0.0218
	(0.0225)	(0.0303)	(0.0165)	(0.0247)
Monthly HH Income in 1000s	-0.0015	-0.0004	-0.0006	0.0001
	(0.0016)	(0.0013)	(0.0013)	(0.0011)
Does Paid Work	0.0043	0.0666*	-0.0176	0.0412
	(0.0300)	(0.0360)	(0.0290)	(0.0253)
HH Size	0.0142**	-0.0083*	0.0123*	-0.0045
	(0.0054)	(0.0048)	(0.0061)	(0.0033)
Has Formal Educ	0.0093	0.0332	0.0188	0.0061
	(0.0401)	(0.0360)	(0.0364)	(0.0305)
Gender	Women	Men	Women	Men
Obs.	237	271	237	271
Mean of Take-up	0.051	0.077	0.038	0.052

Notes: OLS regressions of SFE takeup variables on distance. Each specification also controls for one's monthly stipend level, whose coefficients are omitted to keep the table concise. Missing values of control variables are replaced with zero, and relevant dummy indicators are created and included in the regressions. Moving from Enrolled to Stayed observations change due to respondents who were randomly balloted out after submission. Standard errors clustered at the village level reported in parentheses.

Appendix Table B2: Treatment Balance Table

	Take-Up Balance		Impact Balance	
	Outside-Village Mean	VBT vs Outside-Village	Control Mean	Treatment vs Control
Trainee Attributes				
Age	29.670	0.919**	29.618	0.465
		(0.376)		(0.377)
Married	0.688	0.015	0.691	0.004
		(0.015)		(0.015)
Has Basic Literacy	0.415	0.024	0.412	0.013
		(0.038)		(0.037)
Has Formal Education	0.348	-0.017	0.333	0.007
		(0.026)		(0.025)
Does Paid Work	0.315	0.021	0.315	0.010
		(0.026)		(0.026)
Stitched Last Month	0.052	0.006	0.049	0.005
		(0.009)		(0.008)
In Good or Very Good Physical Health	0.817	0.006	0.822	-0.002
		(0.019)		(0.020)
PCA Influence over Domestic Decisions	-0.030	0.060	-0.067	0.064
		(0.046)		(0.040)
PCA Influence over Business Decisions	0.025	-0.068	-0.041	0.035
		(0.063)		(0.056)
Likely or Very Likely to Enroll in Training	0.728	-0.014	0.736	-0.014
		(0.020)		(0.019)
Household Attributes				
Monthly Income (000s)	11.581	-0.045	14.150	-2.589***
		(0.304)		(0.375)
Monthly Expenditure (000s)	9.997	-0.048	9.754	0.222
		(0.286)		(0.242)
Asset Index	-0.002	-0.004	-0.029	0.025
		(0.055)		(0.049)
Household Size	6.602	-0.081	6.602	-0.037
		(0.090)		(0.092)
Village Attributes				
Number of NGOs at Work	1.004	-0.035	0.928	0.061
		(0.125)		(0.127)
Has Access to Public Transport Stops	0.569	0.001	0.675	-0.105*
		(0.065)		(0.062)
Has Access to Non-Transport Facilities	0.675	0.034	0.700	-0.010
		(0.061)		(0.060)
Total Number of Signal Bars	16.438	0.157	16.315	0.193
		(0.687)		(0.717)
Bus Available	0.337	0.030	0.396	-0.045
		(0.063)		(0.063)
Qingchi Available	0.411	-0.057	0.515	-0.130**
		(0.063)		(0.064)

Note: Table shoes balance between different treatment groups. Columns (1) and (2) show balance between the two main Take-Up treatment groups, VBT and Outside-Village Training. Columns (3) and (4) show balance between the pure control group and the treatment group (all VBT + Outside-Village). Standard errors clustered at the village level reported in parentheses; $*p < 0.10, **p < 0.05, ***p < 0.01$

Appendix Table B3: Treatment Balance Table – All Treatment Arms

	Outside-Village (Standard Information only) *versus*							
	(1) Mean	(2) VBT (Info)	(3) VBT + TE	(4) VBT + CE	(5) Outside-Village + TE	(6) Outside-Village + CE	(7) Outside-Village + GT	(8) Outside-Village + CE + GT
Trainee Attributes								
Age	29.217	-1.672	-0.979	-1.475	-0.801	0.232	-0.972	-0.752
		(0.684)	(0.736)	(0.727)	(0.684)	(0.776)	(0.830)	(0.759)
Married	0.685	-0.018	-0.009	-0.032	-0.021	0.012	-0.022	0.014
		(0.030)	(0.030)	(0.032)	(0.032)	(0.038)	(0.034)	(0.039)
Has Basic Literacy	0.404	-0.011	-0.034	-0.074	-0.002	-0.040	0.009	-0.020
		(0.073)	(0.074)	(0.076)	(0.083)	(0.081)	(0.083)	(0.077)
Has Formal Education	0.365	0.030	0.027	0.049	0.041	0.016	0.008	0.020
		(0.054)	(0.054)	(0.055)	(0.063)	(0.065)	(0.064)	(0.061)
Does Paid Work	0.281	-0.066	-0.048	-0.052	-0.046	-0.025	-0.012	-0.090
		(0.049)	(0.047)	(0.052)	(0.054)	(0.052)	(0.050)	(0.054)
Stitched Last Month	0.054	-0.010	-0.006	0.008	0.026	-0.001	-0.028	0.016
		(0.016)	(0.018)	(0.018)	(0.015)	(0.017)	(0.027)	(0.015)
In Good or Very Good Physical Health	0.821	0.003	0.001	-0.014	0.012	-0.010	0.031	-0.011
		(0.035)	(0.034)	(0.031)	(0.037)	(0.029)	(0.041)	(0.032)
PCA Influence over Domestic Decisions	-0.065	-0.137	-0.014	-0.147	-0.079	0.012	-0.097	-0.012
		(0.093)	(0.092)	(0.097)	(0.099)	(0.106)	(0.102)	(0.107)
PCA Influence over Business Decisions	-0.125	-0.007	-0.113	-0.152	-0.085	-0.217	-0.229	-0.220
		(0.129)	(0.122)	(0.133)	(0.141)	(0.129)	(0.127)	(0.128)
Likely or Very Likely to Enroll in Training	0.738	0.036	0.040	-0.022	0.018	-0.010	0.007	0.033
		(0.034)	(0.036)	(0.036)	(0.035)	(0.033)	(0.040)	(0.038)
Household Attributes								
Monthly Income (000s)	11.90	0.07	0.79	0.21	0.63	0.38	0.14	0.47
		(0.65)	(0.53)	(0.63)	(0.55)	(0.63)	(0.66)	(0.57)
Monthly Expenditure (000s)	10.06	0.13	0.01	0.23	-0.15	0.01	-0.02	0.484
		(0.60)	(0.63)	(0.68)	(0.66)	(0.63)	(0.67)	(0.67)
Asset Index	0.04	0.01	0.07	0.08	0.10	0.08	-0.02	0.06
		(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.13)	(0.12)
Household Size	6.75	0.24	0.23	0.19	0.05	0.23	0.19	0.26
		(0.17)	(0.18)	(0.18)	(0.22)	(0.21)	(0.17)	(0.20)
Village Attributes								
Number of NGOs at Work	1.056	0.156	0.012	0.086	0.099	-0.044	0.153	0.053
		(0.248)	(0.271)	(0.287)	(0.265)	(0.255)	(0.271)	(0.286)
Has Access to Public Transport Stops	0.620	0.134	-0.022	0.023	0.024	0.089	0.080	0.062
		(0.124)	(0.124)	(0.136)	(0.136)	(0.137)	(0.137)	(0.137)
Has Access to Non-Transport Facilities	0.759	0.098	-0.038	0.106	0.051	0.279	0.117	-0.024
		(0.111)	(0.104)	(0.125)	(0.121)	(0.128)	(0.126)	(0.115)
Total Number of Signal Bars	16.177	0.470	-1.296	-0.532	-0.954	-1.053	1.560	-0.882
		(1.308)	(1.345)	(1.429)	(1.370)	(1.380)	(1.499)	(1.385)
Bus Available	0.363	0.001	-0.030	0.022	-0.056	-0.018	0.173	0.028
		(0.121)	(0.123)	(0.133)	(0.135)	(0.135)	(0.122)	(0.133)
Qingchi Available	0.481	0.121	0.192	0.042	0.078	0.169	0.063	0.041
		(0.124)	(0.122)	(0.138)	(0.137)	(0.133)	(0.139)	(0.139)

Notes: Table shows balance between the Outside-Village group with only the Standard Information intervention, and all other treatment arms. Column (1) reports the mean of the base group. TE, CE, and GT stand for Trainee Engagement, Community Engagement, and Group Transport interventions. The remaining columns test whether the given treatment group mean is different from the baseline group. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Appendix Table B4: Take-up Results - Robustness to Using 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: Separating Boundary and Linear Distance Effects								
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 (10 km)	-0.120***	-0.124***	-0.197***	-0.195***	-0.180***	-0.175***	-0.143***	-0.134***
	(0.035)	(0.035)	(0.028)	(0.028)	(0.025)	(0.025)	(0.020)	(0.021)
Panel B: Separating Boundary and Quadratic Distance Effects								
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 (10 km)	-0.142**	-0.115**	-0.479***	-0.442***	-0.477***	-0.447***	-0.409***	-0.382***
	(0.060)	(0.058)	(0.065)	(0.064)	(0.062)	(0.062)	(0.053)	(0.055)
(Cluster-Level Travel Distance)^2	0.010	-0.004	0.131***	0.116***	0.138***	0.127***	0.124***	0.116***
	(0.026)	(0.026)	(0.027)	(0.027)	(0.026)	(0.026)	(0.022)	(0.023)
Obs.	5641	5135	5641	5135	5172	4698	5172	4698
Mean of Comparison Group	0.609	0.618	0.231	0.243	0.108	0.115	0.070	0.074
Controls		X		X		X		X

Notes: OLS regressions of uptake variables on VBT treatment and cluster-level distance. Cluster-Level Distance is the measured distance from the respondent's cluster boundary to the training center's cluster. Distance variables scaled to 10 km units for ease of coefficient readability, excluding distance bins. 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, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B5: Regression Discontinuity with Additional Controls

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
Panel A: RD-Style Design (Vill. Size + Vill. Perimeter + Distance to Vill. Center Controls)								
Village Based Training	0.152***	0.209***	0.070*	0.139***	0.105***	0.188***	0.082**	0.159***
	(0.035)	(0.035)	(0.037)	(0.039)	(0.039)	(0.040)	(0.036)	(0.038)
Travel Distance (10 km)								
Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of Comparison Group	0.689	0.696	0.432	0.449	0.200	0.214	0.067	0.071
Controls		X		X		X		X

Notes: OLS regressions of uptake variables on VBT treatment and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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. Controls include other treatment dummies, stipend amount dummies, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B6: Take-Up - Impact of Multiple Village Boundaries

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Crossing Villages Access Effect								
Crossed 1st Boundary	-0.137**	-0.163***	-0.284***	-0.306***	-0.310***	-0.333***	-0.244***	-0.267***
	(0.053)	(0.055)	(0.049)	(0.050)	(0.046)	(0.042)	(0.039)	(0.036)
Additional Impact of 2nd Boundary	-0.114*	-0.086	0.021	0.021	0.035	0.032	0.019	0.014
	(0.066)	(0.065)	(0.058)	(0.060)	(0.051)	(0.050)	(0.042)	(0.040)
Additional Impact of 3rd Boundary	0.045	0.053	-0.033	-0.024	-0.040	-0.029	-0.029	-0.015
	(0.056)	(0.054)	(0.048)	(0.049)	(0.039)	(0.041)	(0.030)	(0.032)
Additional Impact of 4th Boundary	-0.015	-0.026	-0.006	0.011	-0.005	0.009	-0.006	0.008
	(0.066)	(0.069)	(0.061)	(0.061)	(0.048)	(0.049)	(0.037)	(0.037)
Additional Impact of 5th Boundary	-0.017	-0.026	-0.075	-0.087	-0.069	-0.076*	-0.050	-0.058*
	(0.065)	(0.067)	(0.059)	(0.058)	(0.045)	(0.045)	(0.034)	(0.031)
Distance Measure 2: Travel distance								
Panel B: Separating Multiple Boundaries and Linear Distance Effects								
Crossed 1st Boundary	-0.072	-0.095*	-0.197***	-0.218***	-0.239***	-0.261***	-0.196***	-0.218***
	(0.055)	(0.056)	(0.052)	(0.054)	(0.047)	(0.045)	(0.040)	(0.038)
Additional Impact of 2nd Boundary	-0.117*	-0.086	0.035	0.033	0.055	0.049	0.042	0.033
	(0.063)	(0.063)	(0.055)	(0.058)	(0.048)	(0.048)	(0.041)	(0.041)
Additional Impact of 3rd Boundary	0.082	0.088*	0.006	0.014	-0.013	-0.001	-0.015	0.000
	(0.050)	(0.049)	(0.041)	(0.043)	(0.033)	(0.036)	(0.026)	(0.029)
Additional Impact of 4th Boundary	0.009	-0.007	0.015	0.029	0.008	0.020	-0.002	0.012
	(0.066)	(0.069)	(0.057)	(0.057)	(0.044)	(0.046)	(0.035)	(0.036)
Travel Distance (10 km)	-0.156***	-0.158***	-0.220***	-0.215***	-0.187***	-0.182***	-0.135***	-0.130***
	(0.049)	(0.048)	(0.037)	(0.037)	(0.030)	(0.031)	(0.025)	(0.026)
Additional Impact of 5th Boundary	0.045	0.038	0.016	0.002	0.009	0.000	0.008	-0.002
	(0.065)	(0.066)	(0.057)	(0.055)	(0.043)	(0.044)	(0.033)	(0.031)
Distance Measure 2: Travel distance								
Panel C: Separating Multiple Boundaries and Quadratic Distance Effects								
Crossed 1st Boundary	-0.090	-0.130**	-0.142**	-0.175***	-0.190***	-0.220***	-0.153***	-0.184***
	(0.056)	(0.056)	(0.057)	(0.058)	(0.053)	(0.050)	(0.044)	(0.043)
Additional Impact of 2nd Boundary	-0.117*	-0.087	0.041	0.039	0.063	0.055	0.048	0.037
	(0.063)	(0.063)	(0.056)	(0.059)	(0.048)	(0.049)	(0.042)	(0.042)
Additional Impact of 3rd Boundary	0.077	0.080	0.012	0.019	-0.008	0.004	-0.010	0.005
	(0.050)	(0.049)	(0.041)	(0.043)	(0.032)	(0.035)	(0.025)	(0.028)
Additional Impact of 4th Boundary	0.009	-0.006	0.017	0.030	0.011	0.022	-0.001	0.012
	(0.066)	(0.069)	(0.056)	(0.057)	(0.043)	(0.046)	(0.035)	(0.035)
Additional Impact of 5th Boundary	0.048	0.045	0.003	-0.009	-0.003	-0.010	-0.002	-0.010
	(0.064)	(0.066)	(0.057)	(0.055)	(0.042)	(0.043)	(0.032)	(0.031)
Travel Distance (10 km)	-0.082	-0.012	-0.463***	-0.412***	-0.410***	-0.371***	-0.327***	-0.285***
	(0.109)	(0.102)	(0.100)	(0.096)	(0.096)	(0.092)	(0.085)	(0.081)
$(\text{Travel Distance})^2$	-0.036	-0.072	0.119***	0.097**	0.109**	0.093**	0.094**	0.076**
	(0.053)	(0.051)	(0.044)	(0.042)	(0.042)	(0.040)	(0.036)	(0.035)
Obs.	5873	5348	5873	5348	5393	4900	5393	4900
Mean of VBT (Info) Group	0.745	0.769	0.540	0.563	0.446	0.463	0.321	0.336
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on dummies of each of the boundaries crossed. The dummies are 'nested,' so the first indicator 'Crossed 1st Bound' is = 1 for all villages which did not have a training center. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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. Controls include other treatment dummies, stipend amount dummies, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Appendix Table B7: Economic Magnitude of Settlement Boundary and Distance

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Economic Magnitudes								
Panel A Magnitudes:								
VBT	6222***	6489***	6055***	6169***	6575***	6737***	4771***	5119***
	(1383)	(1432)	(1055)	(1064)	(1110)	(1115)	(800)	(835)
SBT	254	144	2026***	1853**	2529***	2400***	2801***	2570***
	(755)	(765)	(731)	(746)	(775)	(801)	(723)	(752)
Panel B Magnitudes:								
VBT	4042***	4204***	3423***	3636***	4201***	4544***	2864***	3345***
	(1225)	(1268)	(1004)	(1011)	(1036)	(1056)	(758)	(797)
SBT	306	401	1384*	1256***	2002**	1926**	2446***	2259***
	(819)	(836)	(752)	(764)	(794)	(819)	(731)	(767)
Cluster-level Travel Dist. (10 km)	3423***	3750***	4376***	4371***	4164***	4092***	3056***	2969***
	(1251)	(1315)	(841)	(860)	(769)	(781)	(537)	(585)
Panel A Obs.	5797	5285	5797	5285	5321	4841	5321	4841
Panel B Obs.	5631	5127	5631	5127	5163	4691	5163	4691
Controls		X		X		X		X

Notes: Economic magnitudes derived by dividing the VBT, SBT, or distance coefficient by the stipend coefficient, based on OLS regressions of take-up variables on treatment and distance in Table 6, Panel A and Panel B. Group Transport dummy control included in all specifications, and an Average Distance control is included with the same functional form as distance. Distance variables scaled to 10 km units for ease of coefficient readability. Travel Distance is the measured distance from the population centroid of the village to the training center. Controls include other treatment dummies, stipend amount, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Within outcomes observations change due to missing 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. 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 Table B8: Travel Costs and Distance

	Bus Fare	Public Motorcycle Fare	Public Qingchi Fare	Private Motorcycle Fare
	(1)	(2)	(3)	(4)
Distance and One-Way Fare (Linear)				
Travel Distance (10 km)	16.879***	15.023***	12.940***	40.461***
	(5.013)	(3.818)	(1.788)	(14.766)
Constant	10.335***	18.451***	11.755***	90.089***
	(1.225)	(2.065)	(0.714)	(10.614)
Obs.	505	255	593	255
Average Travel Fare	19.317	26.216	18.272	111.000
R^2	0.325	0.279	0.457	0.097

Notes: Bus/public motorcycle/public qingchi fare represents the price a driver would charge for taking a passenger to complete a relevant segment. Private motorcycle/qingchi fare is the price a public transport driver would charge if he take a passenger to complete the same relevant segment in a private capacity. Standard errors clustered at the village level reported in parentheses. Distance variables scaled to 10 km units for ease of coefficient readability. Travel distance measures commute distance via a particular mode of public transport between one station and another, excluding connecting distance to and from stations. Standard errors are clustered at village level; *p < 0:10; **p < 0:05; ***p < 0:01.

Appendix Table B9: Treatment Balance on Trainer Attributes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	TSP Code	Age	Married	Has children	Educated to higher secondary or above	Was employed before training	Engages in stitching	Sells stitched clothes	Has taught before	Is village resident
VBT + TE	-0.075	0.883	-0.093	0.018	-0.050	0.012	0.022	0.123	0.048	-0.106
	(0.405)	(1.696)	(0.112)	(0.110)	(0.112)	(0.112)	(0.042)	(0.109)	(0.105)	(0.110)
VBT + CE	-0.100	1.736	-0.056	0.003	-0.144	0.052	0.048	0.167	-0.008	-0.229**
	(0.449)	(2.056)	(0.125)	(0.121)	(0.124)	(0.123)	(0.033)	(0.118)	(0.114)	(0.112)
Outside-Village (Info)	-0.157	3.624*	-0.042	0.092	0.140	0.098	-0.017	-0.038	0.027	-0.104
	(0.461)	(1.972)	(0.121)	(0.121)	(0.111)	(0.119)	(0.055)	(0.121)	(0.113)	(0.122)
Outside-Village + TE	-0.125	1.176	-0.082	-0.015	-0.007	0.048	0.010	0.155	0.075	0.008
	(0.443)	(1.967)	(0.118)	(0.115)	(0.118)	(0.119)	(0.050)	(0.113)	(0.112)	(0.120)
Outside-Village + CE	-0.341	0.958	-0.012	0.098	0.149	0.037	-0.030	0.085	-0.029	-0.041
	(0.454)	(1.909)	(0.122)	(0.120)	(0.112)	(0.121)	(0.063)	(0.121)	(0.114)	(0.123)
Outside-Village + GT	0.034	0.619	-0.187	-0.173	0.096	0.183	-0.032	-0.085	0.220*	-0.151
	(0.437)	(1.972)	(0.120)	(0.107)	(0.118)	(0.114)	(0.065)	(0.124)	(0.119)	(0.118)
Outside-Village + CE + GT	0.083	1.491	0.016	0.082	0.071	0.092	0.017	0.077	0.055	-0.061
	(0.435)	(1.970)	(0.123)	(0.122)	(0.118)	(0.119)	(0.045)	(0.121)	(0.116)	(0.122)
Obs	5873	5754	5873	5873	5873	5873	5873	5873	5873	5873
Mean of Comparison Group	3.27	27.61	0.51	0.38	0.59	0.55	0.95	0.54	0.30	0.47

Note: Table shows balance between different treatment groups on Trainer Service Provider (TSP) code and and attributes of inidividual trainer assigned to each class. Information on trainer attributes was collected in a survey done after the courses started. Comparison group is VBT (Standard Info only). TE stands for Trainee Enagement, CE for Community Engagement and GT for Group Transport. Standard errors are clustered at village level; $*p < 0.10, **p < 0.05, ***p < 0.01$.

Appendix Table B10: Take-Up - Impact of Individual 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: Overall Village Access Effect								
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 (000s in PKR)	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 (000s in PKR)	-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: Separating Boundary and Linear Straight-Line Distance Effects								
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 (000s in PKR)	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 (000s in PKR)	-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 (10 km)	-0.190***	-0.217***	-0.224***	-0.227***	-0.207***	-0.206***	-0.145***	-0.150***
	(0.070)	(0.068)	(0.048)	(0.048)	(0.038)	(0.038)	(0.030)	(0.031)
Panel C: Separating Boundary and Quadratic Striaght-Line Distance Effects								
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 (000s in PKR)	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 (000s in PKR)	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 (10 km)	0.245	0.231	-0.103	-0.069	-0.222	-0.192	-0.178	-0.169
	(0.205)	(0.195)	(0.179)	(0.179)	(0.161)	(0.161)	(0.122)	(0.127)
(Straight-Line Distance)^2	-0.302**	-0.310**	-0.084	-0.110	0.011	-0.009	0.022	0.012
	(0.130)	(0.123)	(0.103)	(0.103)	(0.093)	(0.094)	(0.070)	(0.074)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Travel Distance Effects								
Village Based Training	0.135***	0.128***	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 (000s in PKR)	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 (000s in PKR)	-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 (10 km)	-0.111***	-0.121***	-0.186***	-0.186***	-0.165***	-0.159***	-0.123***	-0.117***
	(0.042)	(0.041)	(0.031)	(0.031)	(0.025)	(0.026)	(0.020)	(0.022)
Panel E: Seprating Boundary and Quadratic Travel Distance Effects								
Village Based Training	0.154***	0.164***	0.122***	0.139***	0.162***	0.181***	0.137***	0.157***
	(0.044)	(0.042)	(0.044)	(0.043)	(0.040)	(0.040)	(0.033)	(0.033)
Trainee Stipend (000s in PKR)	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 (000s in PKR)	-0.006	-0.009	0.002	-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 (10 km)	-0.040	0.013	-0.405***	-0.377***	-0.360***	-0.342***	-0.289***	-0.266***
	(0.103)	(0.097)	(0.097)	(0.095)	(0.092)	(0.090)	(0.078)	(0.078)
(Travel Distance)^2	-0.035	-0.066	0.106**	0.093**	0.094**	0.089**	0.081**	0.072**
	(0.052)	(0.050)	(0.043)	(0.043)	(0.041)	(0.040)	(0.035)	(0.035)
Obs.	5872	5348	5872	5348	5392	4900	5392	4900
Mean of Comparison Group	0.614	0.625	0.241	0.254	0.121	0.129	0.076	0.081
Controls		X		X		X		X

Notes: OLS regressions of uptake variables on VBT treatment and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B11: Table 1 Specifications Restricted to Table 9 Sample

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Village Access Effect - Restricted Sample								
Village Based Training	0.226***	0.231***	0.340***	0.354***	0.353***	0.366***	0.277***	0.289***
	(0.031)	(0.031)	(0.028)	(0.027)	(0.024)	(0.023)	(0.020)	(0.019)
Distance Measure 1: Straight-Line distance								
Panel B: Separating Boundary and Linear Distance Effects - Restricted Sample								
Village Based Training	0.107**	0.093**	0.209***	0.217***	0.229***	0.241***	0.190***	0.200***
	(0.048)	(0.046)	(0.040)	(0.039)	(0.035)	(0.034)	(0.027)	(0.025)
Straight-Line Distance (10 km)	-0.196***	-0.226***	-0.218***	-0.225***	-0.208***	-0.206***	-0.148***	-0.150***
	(0.068)	(0.065)	(0.049)	(0.047)	(0.040)	(0.037)	(0.033)	(0.029)
Panel C: Separating Boundary and Quadratic Distance Effects - Restricted Sample								
Village Based Training	0.187***	0.190***	0.198***	0.243***	0.181***	0.231***	0.145***	0.189***
	(0.071)	(0.066)	(0.066)	(0.066)	(0.061)	(0.060)	(0.046)	(0.044)
Straight-Line Distance (10 km)	0.076	0.105	-0.254	-0.135	-0.372**	-0.241	-0.299**	-0.182
	(0.208)	(0.195)	(0.176)	(0.177)	(0.158)	(0.158)	(0.122)	(0.122)
(Straight-Line Distance)^2	-0.188	-0.228*	0.025	-0.062	0.113	0.024	0.104	0.022
	(0.137)	(0.128)	(0.103)	(0.103)	(0.090)	(0.092)	(0.069)	(0.070)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Distance Effects - Restricted Sample								
Village Based Training	0.124***	0.122***	0.185***	0.203***	0.212***	0.234***	0.171***	0.193***
	(0.040)	(0.039)	(0.038)	(0.038)	(0.033)	(0.033)	(0.027)	(0.026)
Travel Distance (10 km)	-0.126***	-0.134***	-0.194***	-0.188***	-0.176***	-0.164***	-0.134***	-0.121***
	(0.042)	(0.041)	(0.031)	(0.031)	(0.025)	(0.025)	(0.020)	(0.020)
Panel E: Separating Boundary and Quadratic Distance Effects - Restricted Sample								
Village Based Training	0.134***	0.152***	0.115**	0.147***	0.149***	0.182***	0.117***	0.153***
	(0.046)	(0.043)	(0.047)	(0.046)	(0.043)	(0.043)	(0.035)	(0.034)
Travel Distance (10 km)	-0.094	-0.030	-0.450***	-0.397***	-0.407***	-0.357***	-0.329***	-0.266***
	(0.104)	(0.098)	(0.102)	(0.099)	(0.096)	(0.093)	(0.078)	(0.076)
(Travel Distance)^2	-0.016	-0.051	0.123***	0.102**	0.111***	0.094**	0.094***	0.071**
	(0.051)	(0.050)	(0.044)	(0.044)	(0.042)	(0.040)	(0.034)	(0.033)
Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Mean of Comparison Group	0.600	0.613	0.225	0.237	0.108	0.115	0.065	0.068
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment and distance. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all Specifications, and an Average Distance control 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. The restricted sample is composed of observations for which we have GPS data, which we use to map the paths from the cluster centroids to the nearest training center. This is the same sample as in Table 10. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B12: Table 9 Specifications Defining the Dummy with 250 Meters

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
No Distance Measure								
Panel A: Overall Village Access Effect								
Village Based Training	0.185***	0.191***	0.289***	0.303***	0.294***	0.305***	0.221***	0.230***
	(0.033)	(0.034)	(0.034)	(0.033)	(0.031)	(0.031)	(0.025)	(0.025)
Dummy: 250m Segment $\leq$ 50th %ile Pop. Density	-0.067**	-0.067**	-0.084***	-0.084***	-0.096***	-0.100***	-0.091***	-0.097***
	(0.028)	(0.030)	(0.030)	(0.030)	(0.029)	(0.029)	(0.024)	(0.024)
Distance Measure 1: Straight-Line Distance								
Panel B: Separating Boundary and Linear Distance Effects								
Village Based Training	0.071	0.061	0.163***	0.175***	0.175***	0.190***	0.138***	0.149***
	(0.050)	(0.049)	(0.043)	(0.042)	(0.039)	(0.038)	(0.030)	(0.029)
Straight-Line Distance (10 km)	-0.193***	-0.221***	-0.214***	-0.218***	-0.203***	-0.197***	-0.144***	-0.141***
	(0.067)	(0.065)	(0.050)	(0.048)	(0.040)	(0.037)	(0.034)	(0.030)
Dummy: 250m Segment $\leq$ 50th %ile Pop. Density	-0.062**	-0.059**	-0.079***	-0.077***	-0.092***	-0.094***	-0.089***	-0.093***
	(0.027)	(0.029)	(0.029)	(0.029)	(0.028)	(0.028)	(0.023)	(0.024)
Panel C: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.148**	0.156**	0.148**	0.199***	0.122*	0.177***	0.088*	0.136***
	(0.074)	(0.069)	(0.068)	(0.068)	(0.062)	(0.061)	(0.048)	(0.046)
straight-line distance (10 km)	0.068	0.103	-0.265	-0.137	-0.385**	-0.242	-0.311**	-0.183
	(0.209)	(0.195)	(0.175)	(0.174)	(0.155)	(0.153)	(0.122)	(0.121)
(Straight-Line Distance)^2	-0.180	-0.223*	0.035	-0.056	0.125	0.031	0.115*	0.029
	(0.137)	(0.128)	(0.102)	(0.101)	(0.088)	(0.089)	(0.069)	(0.070)
Dummy: 250m Segment $\leq$ 50th %ile Pop. Density	-0.062**	-0.059**	-0.079***	-0.076***	-0.093***	-0.094***	-0.090***	-0.093***
	(0.027)	(0.029)	(0.029)	(0.029)	(0.028)	(0.028)	(0.023)	(0.024)
Distance Measure 2: Travel Distance								
Panel D: Separating Boundary and Linear Distance Effects								
Village Based Training	0.097**	0.098**	0.153***	0.173***	0.173***	0.195***	0.131***	0.153***
	(0.042)	(0.041)	(0.041)	(0.040)	(0.036)	(0.036)	(0.029)	(0.028)
Travel Distance (10 km)	-0.120***	-0.127***	-0.186***	-0.179***	-0.166***	-0.152***	-0.124***	-0.108***
	(0.042)	(0.041)	(0.031)	(0.032)	(0.025)	(0.026)	(0.020)	(0.021)
Dummy: 250m Segment $\leq$ 50th %ile Pop. Density	-0.053*	-0.050*	-0.062**	-0.061**	-0.076***	-0.080***	-0.077***	-0.083***
	(0.027)	(0.030)	(0.028)	(0.028)	(0.027)	(0.027)	(0.022)	(0.023)
Panel E: Separating Boundary and Quadratic Distance Effects								
Village Based Training	0.114**	0.132***	0.097**	0.129***	0.126***	0.158***	0.093***	0.128***
	(0.047)	(0.044)	(0.048)	(0.047)	(0.044)	(0.044)	(0.035)	(0.034)
Travel Distance (10 km)	-0.055	0.011	-0.414***	-0.360***	-0.359***	-0.304***	-0.279***	-0.209***
	(0.107)	(0.102)	(0.099)	(0.097)	(0.091)	(0.089)	(0.074)	(0.074)
(Travel Distance)^2	-0.031	-0.067	0.109**	0.087**	0.092**	0.073*	0.074**	0.048
	(0.053)	(0.051)	(0.043)	(0.042)	(0.039)	(0.038)	(0.032)	(0.032)
Dummy: 250m Segment $\leq$ 50th %ile Pop. Density	-0.056**	-0.058**	-0.051*	-0.052*	-0.066**	-0.072***	-0.069***	-0.077***
	(0.028)	(0.030)	(0.027)	(0.027)	(0.026)	(0.026)	(0.022)	(0.023)
Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Mean of Comparison Group	0.600	0.613	0.225	0.237	0.108	0.115	0.065	0.068
%Δ VBT Panel A (Relative to Table 4, Restricted Sample)	-18.22	-17.64	-15.13	-14.45	-16.68	-16.71	-20.28	-20.38
%Δ VBT Panel B (Relative to Table 4, Restricted Sample)	-33.97	-34.65	-22.13	-19.32	-23.54	-21.26	-27.50	-25.46
%Δ VBT Panel C (Relative to Table 4, Restricted Sample)	-20.95	-17.86	-25.44	-18.19	-32.74	-23.45	-39.46	-28.24
%Δ VBT Panel D (Relative to Table 4, Restricted Sample)	-21.95	-20.13	-17.45	-14.85	-18.49	-16.63	-23.23	-20.92
%Δ VBT Panel E (Relative to Table 4, Restricted Sample)	-15.12	-13.01	-15.76	-12.13	-15.36	-12.91	-20.36	-16.47
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, distance and the underpopulated dummy. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. Observations change relative to Table 1 as not all households had GPS data to map their paths. The variable *Dummy: 250m Segment $\leq$ 50th %ile Pop. Density* is equal to 1 when the path has 250 meters or more in which the population density is below the median. Paths are calculated from the cluster centroid to the nearest training center. All percentage changes relative to Table 1 with the restricted sample are significant at the 95%. These are calculated using a nested model F-test, testing the inclusion of the dummy. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B13: Accounting for Underpopulated Travel Paths (Table 2 Specifications)

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Separating Boundary and Proportional Distance Effects								
Village Based Training	0.077*	0.083*	0.115***	0.140***	0.136***	0.163***	0.105***	0.132***
	(0.045)	(0.044)	(0.043)	(0.043)	(0.039)	(0.039)	(0.031)	(0.031)
Log. Travel Distance	-0.212***	-0.217***	-0.371***	-0.353***	-0.329***	-0.298***	-0.250***	-0.213***
	(0.077)	(0.075)	(0.059)	(0.060)	(0.049)	(0.050)	(0.039)	(0.040)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.063**	-0.058*	-0.059**	-0.055*	-0.077***	-0.080***	-0.072***	-0.077***
	(0.028)	(0.032)	(0.029)	(0.029)	(0.028)	(0.029)	(0.023)	(0.024)
Panel B: Separating Boundary and Polynomial Distance Effects								
Village Based Training	0.092*	0.120**	0.088*	0.124**	0.122**	0.157***	0.088**	0.127***
	(0.050)	(0.047)	(0.052)	(0.051)	(0.047)	(0.047)	(0.038)	(0.037)
Travel Distance (10 km)	0.734	0.604	0.079	0.038	-0.097	-0.152	-0.212	-0.251
	(0.522)	(0.524)	(0.490)	(0.492)	(0.454)	(0.447)	(0.383)	(0.369)
(Travel Distance)^2	-2.494	-1.680	-1.153	-0.887	-0.152	0.100	0.176	0.548
	(1.761)	(1.759)	(1.514)	(1.508)	(1.315)	(1.300)	(1.083)	(1.037)
(Travel Distance)^3	2.936	1.750	1.227	0.916	-0.185	-0.417	-0.425	-0.902
	(2.228)	(2.235)	(1.817)	(1.806)	(1.530)	(1.517)	(1.252)	(1.206)
(Travel Distance)^4	-1.474	-0.824	-0.502	-0.364	0.265	0.339	0.327	0.552
	(1.157)	(1.167)	(0.912)	(0.908)	(0.753)	(0.751)	(0.614)	(0.598)
(Travel Distance)^5	0.261	0.140	0.073	0.051	-0.069	-0.075	-0.072	-0.108
	(0.208)	(0.210)	(0.160)	(0.160)	(0.131)	(0.131)	(0.106)	(0.104)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.076***	-0.077**	-0.065**	-0.062**	-0.088***	-0.088***	-0.074***	-0.081***
	(0.029)	(0.032)	(0.028)	(0.029)	(0.027)	(0.028)	(0.023)	(0.024)
Panel C: Separating Boundary and Non-Linear Distance Effects								
Village Based Training	0.036	0.074	0.074	0.110**	0.107**	0.142***	0.083**	0.122***
	(0.049)	(0.049)	(0.054)	(0.054)	(0.046)	(0.045)	(0.040)	(0.037)
Bin 2	-0.161***	-0.113**	-0.145***	-0.130**	-0.088**	-0.077*	-0.061	-0.038
	(0.050)	(0.051)	(0.051)	(0.051)	(0.044)	(0.043)	(0.039)	(0.035)
Bin 3	0.011	0.070	-0.060	-0.035	-0.079	-0.059	-0.085*	-0.055
	(0.052)	(0.060)	(0.060)	(0.068)	(0.050)	(0.056)	(0.044)	(0.047)
Bin 4	-0.215***	-0.185***	-0.223***	-0.213***	-0.162***	-0.154***	-0.109*	-0.088*
	(0.067)	(0.061)	(0.069)	(0.064)	(0.061)	(0.055)	(0.060)	(0.052)
Bin 5	-0.090	-0.048	-0.230***	-0.210***	-0.192***	-0.165***	-0.157***	-0.123***
	(0.065)	(0.063)	(0.068)	(0.068)	(0.056)	(0.056)	(0.043)	(0.039)
Bin 6	-0.189***	-0.162**	-0.231***	-0.208***	-0.182***	-0.154***	-0.141***	-0.110**
	(0.071)	(0.070)	(0.071)	(0.070)	(0.056)	(0.055)	(0.048)	(0.047)
Bin 7	-0.142**	-0.109	-0.328***	-0.280***	-0.267***	-0.210***	-0.207***	-0.143***
	(0.066)	(0.070)	(0.073)	(0.072)	(0.057)	(0.056)	(0.049)	(0.046)
Bin 8	-0.144**	-0.121**	-0.250***	-0.242***	-0.260***	-0.246***	-0.210***	-0.197***
	(0.065)	(0.059)	(0.055)	(0.054)	(0.043)	(0.046)	(0.041)	(0.043)
Bin 9	-0.312***	-0.283***	-0.378***	-0.352***	-0.322***	-0.292***	-0.238***	-0.200***
	(0.085)	(0.079)	(0.060)	(0.059)	(0.045)	(0.045)	(0.043)	(0.042)
Bin 10	-0.226***	-0.204***	-0.318***	-0.300***	-0.253***	-0.230***	-0.202***	-0.172***
	(0.077)	(0.075)	(0.060)	(0.061)	(0.050)	(0.050)	(0.041)	(0.040)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.060**	-0.060*	-0.075**	-0.071**	-0.096***	-0.097***	-0.085***	-0.087***
	(0.030)	(0.033)	(0.030)	(0.031)	(0.028)	(0.029)	(0.023)	(0.023)
Panel D: Boundary Effects in a Regression Discontinuity Design								
Village Based Training	0.186***	0.219***	0.077**	0.130***	0.099***	0.166***	0.073**	0.135***
	(0.033)	(0.034)	(0.036)	(0.038)	(0.037)	(0.039)	(0.035)	(0.037)
Travel Distance (10 km)	-0.076	0.053	-0.267***	-0.193**	-0.159*	-0.111	-0.112	-0.026
	(0.086)	(0.088)	(0.094)	(0.098)	(0.096)	(0.101)	(0.090)	(0.095)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.031	-0.040	-0.090***	-0.087***	-0.125***	-0.123***	-0.113***	-0.116***
	(0.026)	(0.027)	(0.029)	(0.029)	(0.030)	(0.031)	(0.028)	(0.029)
Panel A-C Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Panel D Obs.	2732	2498	2732	2498	2477	2254	2477	2254
Mean of Comparison Group	0.692	0.699	0.436	0.452	0.206	0.220	0.079	0.085
%Δ VBT Panel A (Relative to Table 5, Restricted Sample)	-29.70	-25.62	-20.81	-16.09	-22.56	-19.04	-26.13	-22.03
%Δ VBT Panel B (Relative to Table 5, Restricted Sample)	-24.22	-19.42	-22.05	-15.82	-21.41	-17.10	-24.32	-18.99
%Δ VBT Panel C (Relative to Table 5, Restricted Sample)	-50.31	-33.08	-38.34	-28.34	-35.07	-28.94	-38.19	-29.83
%Δ VBT Panel D (Relative to Table 5, Restricted Sample)	-30.56	-27.10	-20.69	-16.77	-22.88	-19.96	-26.10	-22.96
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, alternative distance controls and the underpopulated dummy. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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. The following are the distances corresponding to each Bin: Bin 1, 1.8 km. Bin 2, 4.3 km. Bin 3, 5.8 km. Bin 4, 6.9 km. Bin 5, 8.4 km. Bin 6, 9.7 km. Bin 7, 11.2 km. Bin 8, 12.9 km. Bin 9, 15.3 km. Bin 10, 18.3 km. Controls include other treatment dummies, stipend amount dummies, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. The variable *Dummy: 500m Segment $\leq$ 50th %ile Pop. Density* is equal to 1 when the path has 500 meters or more in which the population density is below the median. Paths are calculated from the cluster centroid to the nearest training center. All percentage changes relative to Table 5 with the restricted sample are significant at the 90%. These are calculated using a nested model F-test, testing the inclusion of the dummy. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B14: Accounting for Underpopulated Travel Paths (Table 3 Specifications)

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Within Village Boundaries: Settlement								
Panel A: Overall Village and Settlement Access Effects								
Village Based Training	0.180***	0.183***	0.252***	0.265***	0.247***	0.259***	0.170***	0.185***
	(0.037)	(0.037)	(0.040)	(0.040)	(0.037)	(0.038)	(0.030)	(0.031)
Settlement Based Training	-0.013	-0.011	0.072**	0.072**	0.083**	0.084**	0.102***	0.094***
	(0.031)	(0.030)	(0.035)	(0.036)	(0.034)	(0.035)	(0.030)	(0.031)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.087***	-0.082**	-0.077**	-0.072**	-0.091***	-0.089***	-0.075***	-0.076***
	(0.031)	(0.033)	(0.031)	(0.032)	(0.030)	(0.030)	(0.023)	(0.024)
Panel B: Separating Village and Setllement Boundaries and Linear Distance Effects								
Village Based Training	0.107**	0.103**	0.138***	0.149***	0.148***	0.163***	0.092***	0.110***
	(0.042)	(0.040)	(0.043)	(0.042)	(0.039)	(0.039)	(0.030)	(0.031)
Settlement Based Training	-0.024	-0.021	0.049	0.051	0.064*	0.067*	0.088***	0.082***
	(0.032)	(0.031)	(0.035)	(0.036)	(0.033)	(0.034)	(0.030)	(0.031)
Cluster-Level Travel Distance	-0.112***	-0.125***	-0.179***	-0.184***	-0.158***	-0.157***	-0.123***	-0.119***
	(0.039)	(0.037)	(0.028)	(0.028)	(0.024)	(0.023)	(0.017)	(0.018)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.074**	-0.066**	-0.056*	-0.046	-0.073***	-0.067**	-0.061***	-0.060***
	(0.030)	(0.032)	(0.029)	(0.029)	(0.027)	(0.028)	(0.022)	(0.023)
Outside Village Boundaries: Number of Villages Crossed								
Panel C: Overall Crossing Villages Access Effect								
Crossed 1st Boundary	-0.090	-0.122**	-0.252***	-0.277***	-0.261***	-0.282***	-0.191***	-0.212***
	(0.056)	(0.059)	(0.055)	(0.055)	(0.053)	(0.048)	(0.043)	(0.039)
Crossing 2 or more Boundaries	-0.097*	-0.068	-0.035	-0.023	-0.025	-0.014	-0.027	-0.015
	(0.054)	(0.056)	(0.050)	(0.051)	(0.047)	(0.043)	(0.040)	(0.036)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.077***	-0.075**	-0.087***	-0.086***	-0.104***	-0.108***	-0.092***	-0.096***
	(0.029)	(0.032)	(0.032)	(0.032)	(0.031)	(0.031)	(0.025)	(0.025)
Panel D: Separating Multiple Boundaries and Linear Distance Effects								
Crossed 1st Boundary	-0.049	-0.075	-0.180***	-0.206***	-0.199***	-0.223***	-0.147***	-0.172***
	(0.055)	(0.057)	(0.053)	(0.054)	(0.049)	(0.046)	(0.041)	(0.038)
Crossing 2 or more Boundaries	-0.053	-0.021	0.048	0.053	0.051	0.053	0.030	0.034
	(0.054)	(0.057)	(0.049)	(0.052)	(0.045)	(0.043)	(0.040)	(0.038)
Travel Distance (10 km)	-0.106**	-0.121***	-0.197***	-0.191***	-0.178***	-0.164***	-0.132***	-0.116***
	(0.044)	(0.043)	(0.032)	(0.032)	(0.025)	(0.026)	(0.021)	(0.022)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.065**	-0.060*	-0.068**	-0.064**	-0.086***	-0.088***	-0.079***	-0.083***
	(0.028)	(0.031)	(0.029)	(0.030)	(0.028)	(0.029)	(0.023)	(0.024)
Panel A Obs.	4248	4248	4248	4248	4248	4248	4248	4248
Panels B Obs.	4128	4128	4128	4128	4128	4128	4128	4128
Panels C - D Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Mean of Comparison Group	0.751	0.776	0.557	0.579	0.457	0.473	0.324	0.338
%Δ VBT Panel A (Relative to Table 6, Restricted Sample)	-19.66	-18.31	-13.38	-11.93	-15.61	-14.64	-18.04	-16.97
%Δ SBT Panel A (Relative to Table 6, Restricted Sample)	-213.35	-191.62	-22.96	-21.39	-23.81	-22.86	-17.29	-18.35
%Δ VBT Panel B (Relative to Table 6, Restricted Sample)	-23.12	-21.09	-15.16	-11.43	-17.77	-14.67	-22.38	-18.42
%Δ SBT Panel B (Relative to Table 6, Restricted Sample)	424.32	388.19	-23.10	-18.54	-23.48	-20.60	-15.64	-15.83
%Δ Cross. 1st Boundary Panel C (Relative to Table 6, Restricted Sample)	-33.54	-25.89	-17.07	-14.93	-19.15	-17.77	-22.23	-20.52
%Δ Cross. 1st Boundary Panel D (Relative to Table 6, Restricted Sample)	-41.98	-29.06	-17.06	-13.63	-19.11	-16.54	-22.63	-19.59
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, additional boundaries, distance and the underpopulated dummy. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all specifications. Cluster-Level Travel Distance (in Panels B and C) is the measured distance from the respondent's cluster boundary to the training center. 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Economic magnitudes derived by dividing the VBT, SBT, or distance coefficient by the stipend coefficient. Within outcomes observations change due to missingness in control variables. The top three panels have fewer observations than the bottom three because of missing values on Cluster-Level Travel Distance. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. The variable *Dummy: 500m Segment $\leq$ 50th %ile Pop. Density* is equal to 1 when the path has 500 meters or more in which the population density is below the median. Paths are calculated from the cluster centroid to the nearest training center. All percentage changes are relative to their counterparts in Table 6 using the same restricted sample and are significant at the 95%. These are calculated using a nested model F-test, testing the inclusion of the dummy. Whenever the coefficient is not significant in the restricted sample, the percentage change is suppressed from the table and an x is shown instead. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B15: Table Take-up with Alternative Distance Controls - Restricted to Table B13 Sample

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Separating Boundary and Logarithmic Distance Effects								
Village Based Training	0.109**	0.111***	0.145***	0.167***	0.176***	0.202***	0.142***	0.169***
	(0.043)	(0.041)	(0.041)	(0.041)	(0.036)	(0.036)	(0.029)	(0.029)
Log Travel Distance	-0.230***	-0.236***	-0.389***	-0.371***	-0.353***	-0.326***	-0.272***	-0.241***
	(0.076)	(0.074)	(0.059)	(0.059)	(0.049)	(0.050)	(0.039)	(0.040)
Panel B: Separating Boundary and Polynomial Distance Effects								
Village Based Training	0.122***	0.149***	0.113**	0.148***	0.155***	0.190***	0.116***	0.157***
	(0.047)	(0.043)	(0.051)	(0.049)	(0.047)	(0.046)	(0.038)	(0.037)
Travel Distance (10 km)	0.450	0.328	-0.161	-0.184	-0.416	-0.461	-0.482	-0.534
	(0.520)	(0.512)	(0.494)	(0.489)	(0.462)	(0.448)	(0.381)	(0.361)
(Travel Distance)^2	-1.932	-1.128	-0.677	-0.443	0.460	0.699	0.694	1.097
	(1.752)	(1.732)	(1.524)	(1.505)	(1.338)	(1.310)	(1.089)	(1.032)
(Travel Distance)^3	2.444	1.258	0.810	0.519	-0.709	-0.939	-0.869	-1.380
	(2.223)	(2.216)	(1.833)	(1.809)	(1.561)	(1.538)	(1.269)	(1.214)
(Travel Distance)^4	-1.279	-0.626	-0.337	-0.204	0.468	0.545	0.500	0.741
	(1.157)	(1.161)	(0.921)	(0.911)	(0.770)	(0.763)	(0.625)	(0.606)
(Travel Distance)^5	0.232	0.111	0.048	0.028	-0.098	-0.105	-0.097	-0.136
	(0.208)	(0.210)	(0.162)	(0.160)	(0.134)	(0.133)	(0.108)	(0.106)
Panel C: Separating Boundary and Non-Parametric Distance Effects								
Village Based Training	0.073*	0.111***	0.120**	0.154***	0.165***	0.200***	0.134***	0.174***
	(0.043)	(0.042)	(0.051)	(0.050)	(0.044)	(0.042)	(0.040)	(0.036)
Bin 2	-0.172***	-0.123**	-0.159***	-0.141***	-0.109**	-0.094**	-0.079*	-0.054
	(0.050)	(0.051)	(0.053)	(0.052)	(0.048)	(0.046)	(0.042)	(0.038)
Bin 3	0.013	0.073	-0.057	-0.031	-0.076	-0.055	-0.083*	-0.052
	(0.054)	(0.061)	(0.062)	(0.069)	(0.054)	(0.060)	(0.049)	(0.051)
Bin 4	-0.215***	-0.183***	-0.222***	-0.211***	-0.164**	-0.155***	-0.111*	-0.089
	(0.068)	(0.062)	(0.071)	(0.066)	(0.064)	(0.058)	(0.062)	(0.055)
Bin 5	-0.091	-0.048	-0.230***	-0.210***	-0.195***	-0.167***	-0.159***	-0.125***
	(0.064)	(0.061)	(0.067)	(0.067)	(0.056)	(0.056)	(0.044)	(0.040)
Bin 6	-0.193***	-0.166**	-0.236***	-0.213***	-0.191***	-0.163***	-0.148***	-0.119**
	(0.073)	(0.071)	(0.072)	(0.071)	(0.057)	(0.058)	(0.050)	(0.050)
Bin 7	-0.140**	-0.106	-0.326***	-0.278***	-0.266***	-0.208***	-0.206***	-0.141***
	(0.066)	(0.070)	(0.073)	(0.072)	(0.058)	(0.057)	(0.051)	(0.048)
Bin 8	-0.148**	-0.126**	-0.255***	-0.248***	-0.267***	-0.257***	-0.217***	-0.207***
	(0.065)	(0.059)	(0.056)	(0.055)	(0.046)	(0.048)	(0.042)	(0.044)
Bin 9	-0.319***	-0.289***	-0.387***	-0.359***	-0.336***	-0.304***	-0.250***	-0.210***
	(0.085)	(0.079)	(0.061)	(0.059)	(0.047)	(0.046)	(0.044)	(0.043)
Bin 10	-0.232***	-0.212***	-0.325***	-0.309***	-0.264***	-0.245***	-0.212***	-0.185***
	(0.078)	(0.076)	(0.061)	(0.062)	(0.051)	(0.052)	(0.043)	(0.042)
Panel D: Boundary Effects in a Regression Discontinuity Design								
Village Based Training	0.197***	0.234***	0.110***	0.161***	0.143***	0.208***	0.114***	0.175***
	(0.032)	(0.033)	(0.035)	(0.037)	(0.036)	(0.038)	(0.034)	(0.036)
Travel Distance (10 km)	-0.119	-0.003	-0.395***	-0.314***	-0.336***	-0.283***	-0.271***	-0.189**
	(0.078)	(0.080)	(0.085)	(0.089)	(0.087)	(0.091)	(0.081)	(0.086)
Panel A-C Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Panel D Obs.	2732	2498	2732	2498	2477	2254	2477	2254
Mean of Comparison Group	0.692	0.699	0.436	0.452	0.206	0.220	0.079	0.085
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment and alternative distance controls on restricted sample. All distance variables, except distance bins, are scaled to 10 km for readability. Group Transport dummy control included in all specifications, and an Average Distance control 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. The following are the distances corresponding to each Bin: Bin 1, 1.8 km. Bin 2, 4.3 km. Bin 3, 5.8 km. Bin 4, 6.9 km. Bin 5, 8.4 km. Bin 6, 9.7 km. Bin 7, 11.2 km. Bin 8, 12.9 km. Bin 9, 15.3 km. Bin 10, 18.3 km. Controls include other treatment dummies, stipend amount dummies, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. The restricted sample is composed of observations for which we have GPS data, which we use to map the paths from the cluster centroids to the nearest training center. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B16: Take-up with Additional Boundaries - Restricted to Table B14 Sample

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Within Village Boundaries: Settlement								
Panel A: Overall Village and Settlement Access Effects - Restricted Sample								
Village Based Training	0.223***	0.224***	0.291***	0.301***	0.292***	0.303***	0.207***	0.222***
	(0.036)	(0.035)	(0.037)	(0.037)	(0.033)	(0.034)	(0.027)	(0.028)
Settlement Based Training	0.011	0.012	0.094***	0.092**	0.109***	0.109***	0.123***	0.115***
	(0.030)	(0.029)	(0.036)	(0.036)	(0.035)	(0.036)	(0.030)	(0.031)
Panel B: Separating Village and Setllement Boundaries and Linear Dist. Effects - Restricted Sample								
Village Based Training	0.139***	0.130***	0.162***	0.168***	0.180***	0.191***	0.119***	0.135***
	(0.041)	(0.039)	(0.041)	(0.041)	(0.037)	(0.037)	(0.029)	(0.030)
Settlement Based Training	-0.005	-0.004	0.064*	0.063*	0.084**	0.084**	0.104***	0.098***
	(0.031)	(0.030)	(0.036)	(0.036)	(0.034)	(0.034)	(0.030)	(0.031)
Cluster-Level Travel Dist. (10 km)	-0.118***	-0.133***	-0.184***	-0.190***	-0.165***	-0.165***	-0.129***	-0.126***
	(0.039)	(0.037)	(0.028)	(0.028)	(0.024)	(0.023)	(0.017)	(0.018)
Outside Village Boundaries: Number of Villages Crossed								
Panel C: Overall Crossing Villages Access Effect - Restricted Sample								
Crossed 1st Boundary	-0.135**	-0.164***	-0.303***	-0.326***	-0.323***	-0.343***	-0.246***	-0.266***
	(0.052)	(0.055)	(0.050)	(0.052)	(0.048)	(0.044)	(0.040)	(0.037)
Crossing 2 or more Boundaries	-0.103*	-0.076	-0.042	-0.032	-0.034	-0.026	-0.035	-0.026
	(0.053)	(0.056)	(0.048)	(0.050)	(0.046)	(0.043)	(0.039)	(0.036)
Panel D: Separating Multiple Boundaries and Linear Dist. Effects - Restricted Sample								
Crossed 1st Boundary	-0.085	-0.105*	-0.217***	-0.238***	-0.246***	-0.268***	-0.190***	-0.214***
	(0.053)	(0.055)	(0.052)	(0.054)	(0.048)	(0.045)	(0.041)	(0.038)
Crossing 2 or more Boundaries	-0.056	-0.024	0.045	0.050	0.048	0.049	0.027	0.030
	(0.054)	(0.057)	(0.048)	(0.052)	(0.045)	(0.044)	(0.041)	(0.038)
Travel Distance (10 km)	-0.113**	-0.129***	-0.204***	-0.200***	-0.188***	-0.176***	-0.141***	-0.128***
	(0.044)	(0.043)	(0.032)	(0.031)	(0.025)	(0.026)	(0.021)	(0.021)
Panel A Obs.	4248	4248	4248	4248	4248	4248	4248	4248
Panels B Obs.	4128	4128	4128	4128	4128	4128	4128	4128
Panels C - D Obs.	5083	4647	5083	4647	4665	4252	4665	4252
Mean of Comparison Group	0.751	0.776	0.557	0.579	0.457	0.473	0.324	0.338
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, additional boundaries, and distance in the restricted sample. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all specifications. Cluster-Level Travel Distance (in Panels B and C) is the measured distance from the respondent's cluster boundary to the training center. 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Within outcomes observations change due to missingness in control variables. The top three panels have fewer observations than the bottom because of missing values on Cluster-Level Travel Distance. Moving from Submission to Enroll/Complete, observations change because respondents had to be randomly balloted out after submission due to course capacity constraints. The restricted sample is composed of observations for which we have GPS data, which we use to map the paths from the cluster centroids to the nearest training center. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B17: Boundaries and Underpopulated Travel Paths Matter, Population Density Does Not

	Voucher Acceptance		Voucher Submission		Class Enrollment		Class Completion	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Village Access Effect, Population Density								
Village Based Training	0.175***	0.178***	0.266***	0.276***	0.257***	0.260***	0.191***	0.197***
	(0.035)	(0.036)	(0.038)	(0.037)	(0.035)	(0.035)	(0.028)	(0.027)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.098***	-0.099***	-0.071**	-0.074**	-0.098***	-0.109***	-0.076***	-0.088***
	(0.032)	(0.035)	(0.036)	(0.035)	(0.034)	(0.033)	(0.027)	(0.027)
Mean Population Density	-1.468	-1.902	-0.026	-0.515	-2.370	-3.201*	-1.056	-1.367
	(2.070)	(2.078)	(1.843)	(1.702)	(2.007)	(1.858)	(1.741)	(1.622)
Panel B: Separating Boundary and Linear Distance Effects, Population Density								
Village Based Training	0.090**	0.090**	0.141***	0.159***	0.146***	0.162***	0.111***	0.130***
	(0.042)	(0.042)	(0.043)	(0.042)	(0.039)	(0.039)	(0.031)	(0.031)
Travel Distance (10 km)	-0.122***	-0.129***	-0.180***	-0.172***	-0.160***	-0.144***	-0.116***	-0.099***
	(0.044)	(0.042)	(0.032)	(0.032)	(0.026)	(0.026)	(0.020)	(0.021)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.082***	-0.079**	-0.048	-0.047	-0.078**	-0.086***	-0.062**	-0.073***
	(0.031)	(0.034)	(0.033)	(0.034)	(0.032)	(0.032)	(0.025)	(0.026)
Mean Population Density	-0.807	-1.231	0.930	0.360	-1.686	-2.565	-0.606	-0.978
	(2.395)	(2.231)	(2.102)	(1.868)	(1.935)	(1.822)	(1.722)	(1.639)
Panel C: Separating Boundary and Quadratic Distance Effects, Population Density								
Village Based Training	0.095**	0.113**	0.085*	0.117**	0.101**	0.129***	0.079**	0.111***
	(0.047)	(0.044)	(0.049)	(0.048)	(0.046)	(0.045)	(0.035)	(0.035)
Travel Distance (10 km)	-0.103	-0.027	-0.428***	-0.360***	-0.360***	-0.294***	-0.258***	-0.185**
	(0.114)	(0.106)	(0.106)	(0.106)	(0.096)	(0.094)	(0.074)	(0.075)
(Travel Distance)^2	-0.009	-0.048	0.117**	0.090**	0.094**	0.071*	0.067**	0.041
	(0.055)	(0.052)	(0.045)	(0.045)	(0.041)	(0.040)	(0.032)	(0.032)
Dummy: 500m Segment $\leq$ 50th %ile Pop. Density	-0.083***	-0.082**	-0.039	-0.041	-0.071**	-0.081**	-0.057**	-0.070***
	(0.031)	(0.034)	(0.032)	(0.033)	(0.031)	(0.032)	(0.025)	(0.026)
Mean Population Density	-0.830	-1.320	1.230	0.528	-1.450	-2.423	-0.432	-0.896
	(2.399)	(2.187)	(2.227)	(1.949)	(1.993)	(1.838)	(1.758)	(1.641)
Obs.	4175	3801	4175	3801	3824	3471	3824	3471
Mean of Comparison Group	0.600	0.613	0.225	0.237	0.108	0.115	0.065	0.068
Controls		X		X		X		X

Notes: OLS regressions of take-up variables on VBT treatment, distance, the underpopulated dummy and mean population density. Distance variables scaled to 10 km units for ease of coefficient readability. Group Transport dummy control included in all specifications, and an Average Distance control 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. 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. Observations change relative to Table 4 as not all households had GPS data to map their paths. The variable *Dummy: 500m Segment $\leq$ 50th %ile Pop. Density* is equal to 1 when the path has 500 meters or more in which the population density is below the median. The variable *Mean Pop. Density* is the average populaion density on each path. The units are 1000 people per 100 meters. Paths are calculated from the cluster centroid to the nearest training center. Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B18: Impact of Interventions on Community Disapproval

	Disapproval of own HH members on enrollment		Disapproval of tribe, biraderi, extended family on enrollment		Disapproval of other women in vill on enrollment		Disapproval of other people in vill on enrollment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Village Based Training	-0.398***	-0.432***	-0.303**	-0.341**	-0.259*	-0.288**	-0.223	-0.265*
	(0.138)	(0.137)	(0.135)	(0.134)	(0.140)	(0.138)	(0.136)	(0.135)
Community Engagement	0.175**	0.198**	0.177**	0.192**	0.137*	0.138*	0.158**	0.147*
	(0.086)	(0.087)	(0.081)	(0.082)	(0.079)	(0.079)	(0.075)	(0.075)
Trainee Engagement	0.155*	0.206**	0.200**	0.228***	0.147*	0.165**	0.176**	0.194**
	(0.091)	(0.090)	(0.084)	(0.085)	(0.083)	(0.082)	(0.082)	(0.082)
Group Transport	0.036	0.034	0.021	0.014	0.027	0.016	0.025	0.015
	(0.117)	(0.119)	(0.106)	(0.108)	(0.105)	(0.105)	(0.101)	(0.101)
Travel Distance (10 km)	0.105	0.006	0.299	0.207	0.261	0.155	0.253	0.120
	(0.290)	(0.288)	(0.286)	(0.283)	(0.295)	(0.293)	(0.285)	(0.288)
(Travel Distance)^2	0.134	0.174	0.050	0.079	0.046	0.090	0.055	0.108
	(0.135)	(0.135)	(0.134)	(0.134)	(0.141)	(0.145)	(0.135)	(0.141)
Obs.	5571	5115	5571	5115	5571	5115	5571	5115
Mean Outcome Var	2.234	2.204	2.252	2.224	2.148	2.127	2.172	2.154
Controls		X		X		X		X

Notes: OLS regressions of treatment arms and distance. Distance variables scaled to 10 km units for ease of coefficient readability. The outcomes are likert scale variables that range from 1 (strongly approve) to 5 (strongly disapprove), and come from the question: *How do you think that attending the tailoring course at the nearest location available to you will be viewed by some of the following groups. Even if you are not planning to take the course, it is important for us to know what you THINK about other people's views on your enrolment if you were to attend it.* Standard errors clustered at the village level reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B19: Examining Additional Boundary Effects on Trainee Preference to Walk to Center

	Intention to Walk		Actual Walk	
	(1)	(2)	(3)	(4)
Within Village Boundaries: Settlement				
Panel A: Village and Settlement Access Effect				
Village Based Training	0.466***	0.477***	0.630***	0.626***
	(0.035)	(0.036)	(0.032)	(0.034)
Settlement Based Training	0.288***	0.282***	0.198***	0.203***
	(0.034)	(0.035)	(0.032)	(0.033)
Panel B: Separating Boundary and Linear Cluster-Level Travel Distance Effects				
Village Based Training	0.380***	0.385***	0.521***	0.510***
	(0.042)	(0.042)	(0.039)	(0.041)
Settlement Based Training	0.265***	0.259***	0.172***	0.178***
	(0.034)	(0.034)	(0.031)	(0.032)
Cluster-Level Travel Distance (10 km)	-0.130***	-0.139***	-0.149***	-0.157***
	(0.026)	(0.028)	(0.023)	(0.024)
Panel C: Separating Boundary and Quadratic Cluster-Level Travel Distance Effects				
Village Based Training	0.298***	0.308***	0.428***	0.423***
	(0.044)	(0.044)	(0.042)	(0.044)
Settlement Based Training	0.207***	0.202***	0.105***	0.112***
	(0.033)	(0.034)	(0.030)	(0.031)
Cluster-Level Travel Distance (10 km)	-0.539***	-0.536***	-0.612***	-0.606***
	(0.087)	(0.090)	(0.085)	(0.086)
(Cluster-Level Travel Distance)^2	0.187***	0.183***	0.212***	0.207***
	(0.036)	(0.038)	(0.037)	(0.037)
Outside Village Boundaries: Number of Villages Crossed				
Panel D: Overall Crossing Villages Access Effect				
Crossed 1st Boundary	-0.548***	-0.549***	-0.706***	-0.706***
	(0.059)	(0.062)	(0.039)	(0.040)
Additional Impact of Crossing Two Boundaries or More	-0.088	-0.094*	-0.037	-0.036
	(0.054)	(0.057)	(0.033)	(0.034)
Panel E: Separating Multiple Boundaries and Linear Distance Effects				
Crossed 1st Boundary	-0.487***	-0.485***	-0.635***	-0.627***
	(0.061)	(0.064)	(0.040)	(0.041)
Additional Impact of Crossing Two Boundaries or More	-0.025	-0.030	0.025	0.028
	(0.053)	(0.055)	(0.029)	(0.030)
Travel Distance (10 km)	-0.148***	-0.155***	-0.152***	-0.164***
	(0.030)	(0.031)	(0.029)	(0.029)
Panel F: Separating Multiple Boundaries and Quadratic Distance Effects				
Crossed 1st Boundary	-0.322***	-0.326***	-0.490***	-0.484***
	(0.071)	(0.071)	(0.046)	(0.047)
Additional Impact of Crossing Two Boundaries or More	-0.001	-0.006	0.047*	0.049*
	(0.055)	(0.056)	(0.025)	(0.027)
Travel Distance (10 km)	-0.853***	-0.848***	-0.773***	-0.788***
	(0.136)	(0.133)	(0.120)	(0.117)
(Travel Distance)^2	0.339***	0.337***	0.299***	0.303***
	(0.066)	(0.064)	(0.055)	(0.054)
Panel A Obs.	5285	5285	5285	5285
Panels B - C Obs.	5127	5127	5127	5127
Panels D - F Obs.	5873	5348	5873	5348
Controls		X		X

Notes: OLS regressions of intention to walk and actual walk variables on VBT treatment and distance. Distance variables are scaled to 10 km units for ease of coefficient readability. Group Transport dummy and Average Distance control included in all specifications. Cluster-Level Travel Distance (in Panels B and C) is the measured distance from the respondent's cluster boundary to the training center. 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, household assets, household income, stitched last month, individual skill/employment/education/marital status, as well as indicators of female empowerment. Within outcomes observations change due to missingness in control variables. The top three panels have fewer observations than the bottom three because of missing values on Cluster-Level Travel Distance. 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. * p<0.10, ** p<0.05, *** p<0.01

Appendix Table B20: Impact on Disaggregated Indices of Influence and Engagement (1/2)

	(1) Overall Impact	(2) VBT	(3) Outside Village	(4) VBT Compliers	(5) Baseline Mean
Household Influence					
Index	-0.066***	-0.059**	-0.146***	-0.065**	0.462
	(0.023)	(0.024)	(0.052)	(0.033)	
Doesn't need permission for new activity	0.012	0.004	0.005	0.021	0.101
	(0.017)	(0.019)	(0.030)	(0.025)	
Can influence husband (daughter school dropout)	0.019	0.028	0.015	0.049	0.880
	(0.039)	(0.042)	(0.091)	(0.065)	
Can influence husband (new activity)	-0.051	-0.039	-0.145*	-0.036	0.816
	(0.037)	(0.038)	(0.077)	(0.058)	
Can influence husband (buy sewing machine)	-0.018	-0.001	-0.061	-0.024	0.912
	(0.033)	(0.035)	(0.071)	(0.050)	
Can influence husband (spend on child clothes)	-0.013	0.015	-0.094	-0.010	0.927
	(0.035)	(0.038)	(0.083)	(0.050)	
Business Confidence					
Index	0.022	0.017	0.033	-0.006	0.461
	(0.022)	(0.023)	(0.048)	(0.032)	
Can run own business	0.017	-0.002	-0.030	-0.019	0.452
	(0.036)	(0.037)	(0.074)	(0.055)	
Can obtain credit for business	0.025	0.010	0.128*	-0.041	0.276
	(0.034)	(0.035)	(0.071)	(0.048)	
Can manage employees	0.040	0.024	0.066	0.004	0.306
	(0.030)	(0.030)	(0.063)	(0.046)	
Can manage financial accounts	0.072**	0.065*	0.082	0.065	0.309
	(0.034)	(0.036)	(0.070)	(0.053)	
Can bargain	0.001	0.007	0.002	-0.040	0.512
	(0.034)	(0.035)	(0.075)	(0.046)	
Can collect debt	0.042	0.020	0.086	0.022	0.539
	(0.033)	(0.035)	(0.079)	(0.048)	
Can influence HH borrowing decision	-0.008	0.009	-0.015	-0.019	0.688
	(0.033)	(0.035)	(0.066)	(0.047)	
Can influence HH land buying decision	-0.013	0.007	-0.042	-0.013	0.685
	(0.033)	(0.035)	(0.068)	(0.049)	
Gender-role Perceptions					
Index	0.032	0.029	0.103**	0.030	0.710
	(0.020)	(0.021)	(0.042)	(0.029)	
Women are better at managing daily affairs	0.030	0.022	0.036	0.075*	0.771
	(0.032)	(0.033)	(0.069)	(0.044)	
Men and women should study till same level	0.057	0.046	0.166**	0.072	0.567
	(0.037)	(0.039)	(0.082)	(0.053)	
Women should take paid employment	0.029	0.032	0.092	-0.003	0.876
	(0.027)	(0.028)	(0.058)	(0.037)	
Women should work outside	0.020	0.020	0.112	0.027	0.732
	(0.040)	(0.043)	(0.081)	(0.060)	
Base Group	Control	Control	Control	OVT-I	
Stipend Instruments	Yes	Yes	Yes	No	

Notes: IV estimates of the impact of skills training. Outcome variables are in rows. Columns 1-3 report the LATE averaged across different complier groups, pooling 3 rounds of post-training surveys. Controls for survey round, baseline values of the outcome variable, and grid fixed effects are included in all specifications. Column 1 reports IV estimates (2SLS) comparing all treatment groups to the control group; training completion is instrumented by the randomized treatments (VBT, OVT-Transport, OVT-Information (no transport) and eight stipend dummies). Columns 2 and 3 restrict the sample to control and specific treatment subsets (VBT and OVT) using stipend along with the relevant treatment dummies, VBT (Col 2) or OVT-transport, OVT-Information (Col 3) respectively, as instruments to estimate the LATE for a subset comprising the compliers of that treatment and stipend. Column 4 reports the impact on VBT compliers by comparing VBT to Outside-Village training arms (without group transport) using the VBT dummy as the only instrument. Column 5 reports the baseline mean value of the outcome variable. Standard errors are clustered at the village level. $*p < 0.10, **p < 0.05, ***p < 0.01$

Appendix Table B21: Impact on Disaggregated Indices of Influence and Engagement (2/2)

	(1) Overall Impact	(2) VBT	(3) Outside Village	(4) VBT Compliers	(5) Baseline Mean
Government Services Usage					
Index	0.026	0.021	0.008	0.055**	0.301
	(0.017)	(0.018)	(0.036)	(0.026)	
Used Govt health centers	0.001	-0.013	-0.004	0.003	0.710
	(0.032)	(0.032)	(0.073)	(0.048)	
Used Govt education services	0.081**	0.067*	0.061	0.106*	0.513
	(0.038)	(0.039)	(0.091)	(0.058)	
Used Police services	-0.019	-0.007	-0.042*	-0.009	0.038
	(0.012)	(0.012)	(0.024)	(0.016)	
Used Court services	-0.028***	-0.026**	-0.041*	-0.025	0.027
	(0.011)	(0.012)	(0.023)	(0.015)	
Used Govt sanitation services	0.005	-0.004	0.102	0.024	0.103
	(0.026)	(0.025)	(0.073)	(0.045)	
Used electricity company services	0.113**	0.110**	0.008	0.222***	0.418
	(0.048)	(0.049)	(0.090)	(0.070)	
Civic Engagement					
Index	0.005	0.003	0.042*	-0.004	0.287
	(0.011)	(0.011)	(0.023)	(0.016)	
Member of political party	-0.015**	-0.015**	-0.023*	-0.012	0.010
	(0.006)	(0.007)	(0.012)	(0.008)	
Participated in protest	0.000	0.002	-0.001	0.001	0.010
	(0.003)	(0.003)	(0.006)	(0.005)	
See yourself as part of community	0.007	0.013	0.008	-0.063	0.598
	(0.047)	(0.048)	(0.096)	(0.074)	
Correctly identified President	0.062**	0.036	0.151**	0.089**	0.089
	(0.029)	(0.030)	(0.064)	(0.042)	
Correctly identified CM Punjab	0.067*	0.032	0.168**	0.118**	0.183
	(0.036)	(0.036)	(0.077)	(0.058)	
Important: Pakistan is governed by elected reps	-0.013	0.010	0.016	-0.072*	0.858
	(0.028)	(0.030)	(0.058)	(0.040)	
Important: courts are independent	-0.052*	-0.035	-0.017	-0.082*	0.793
	(0.029)	(0.033)	(0.055)	(0.042)	
Important: individuals express political views	-0.034	-0.025	-0.056	-0.063	0.774
	(0.037)	(0.040)	(0.070)	(0.052)	
Important: work on political issues	0.026	0.049	0.065	-0.004	0.801
	(0.033)	(0.036)	(0.061)	(0.049)	
Important: property rights are secure	-0.038	-0.026	-0.041	-0.033	0.554
	(0.039)	(0.041)	(0.083)	(0.055)	
Member of NGO/civil welfare org	-0.013**	-0.010*	-0.028**	-0.008	0.011
	(0.005)	(0.005)	(0.012)	(0.008)	
Donated to NGO/civil welfare org	-0.005	-0.008	-0.017*	0.006	0.009
	(0.004)	(0.005)	(0.009)	(0.004)	
Helped community dispute resolution	0.000	-0.003	0.002	0.008	0.020
	(0.007)	(0.007)	(0.013)	(0.009)	
Helped neighbors with harvesting	0.010	0.001	-0.007	0.042	0.090
	(0.020)	(0.021)	(0.042)	(0.027)	
Made any charities	0.038	0.010	0.195***	0.026	0.491
	(0.032)	(0.035)	(0.070)	(0.049)	
(Rescaled) Household members with CNIC	0.009*	0.009*	0.031***	0.003	0.107
	(0.005)	(0.005)	(0.012)	(0.006)	
(Rescaled) How much people can affect govt?	0.041*	0.034	0.043	0.047	0.357
	(0.022)	(0.023)	(0.047)	(0.034)	
Base Group	Control	Control	Control	OVT-I	
Stipend Instruments	Yes	Yes	Yes	No	

Notes: IV estimates of the impact of skills training. Outcome variables are in rows. Columns 1-3 report the LATE averaged across different complier groups, pooling 3 rounds of post-training surveys. Controls for survey round, baseline values of the outcome variable, and grid fixed effects are included in all specifications. Column 1 reports IV estimates (2SLS) comparing all treatment groups to the control group; training completion is instrumented by the randomized treatments (VBT, OVT-Transport, OVT-Information (no transport) and eight stipend dummies). Columns 2 and 3 restrict the sample to control and specific treatment subsets (VBT and OVT) using stipend along with the relevant treatment dummies, VBT (Col 2) or OVT-transport, OVT-Information (Col 3) respectively, as instruments to estimate the LATE for a subset comprising the compliers of that treatment and stipend. Column 4 reports the impact on VBT compliers by comparing VBT to Outside-Village training arms (without group transport) using the VBT dummy as the only instrument. Column 5 reports the baseline mean value of the outcome variable. Standard errors are clustered at the village level. $*p < 0.1, **p < 0.05, ***p < 0.01$