Glass Walls: Experimental Evidence on Access Constraints Faced by Women Pre-Analysis Plan for Heterogeneity (Non-experimental) Analysis

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

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1. Abstract

Human capital acquisition offers a pathway for many to improve their economic, social, and health outcomes. Women have historically faced systematic obstacles in accessing human capital enhancement opportunities. Based on experimental data from Pakistan, this project provides evidence on such access constraints in the context of skill acquisition.

To date, we have finished our main (experimental) analysis and found that physical distance—in terms of crossing boundaries and traveling distance—poses a significant hurdle. That analysis is based on experimentally-induced variation. The purpose of this pre-analysis plan is to discipline our analysis in the next phase since we intend to use non-experimental variation to explore the heterogeneous impact of distance on training take-up. We structure this plan as follows: Section 2 gives an overview of the treatment design, sampling strategy and key data used. Section 3 summarizes how we plan to analyze heterogeneity of treatment effects. The Appendix details the specifications we have already run and outlines our main findings to date.

2. Research Strategy

2.1 Treatment

Our study is part of our collaboration with the Punjab Skills Development Fund (PSDF). In 2011 and 2012, we conducted a baseline survey of over 11,000 households and discovered a huge latent demand for skill acquisition. Subsequent pilot training programs and follow-up surveys revealed five potential access constraints underlying a wide gap between expressed demand and actual take-up: distance, information, social norms, reliable transportation and money.

This pre-analysis plan focuses on additional heterogeneity analysis for a program entitled Skills for Market phase B (SFM-B), which rolled out in 2013-14. We randomly selected 243 villages from our original baseline survey to take part in the program and receive training in sewing and tailoring—the most demanded skills among rural women.[1] In addition, based on initial analysis

[1] We consider these 243 villages our primary sample, but PSDF also established training centers and provided services in other villages, which form our secondary sample. The current analysis uses the former and does not include the latter. In the future, we might also include the secondary sample as needed.

and focus groups conducted, we coupled the training with the following treatments to encourage program take-up:

1. Distance: We selected 108 villages to house a training center. We refer to these villages as Village Based Training (VBT) villages and the rest as non-Village Based Training (nVBT) villages.

2. Information: Because people might not fully understand the benefits of a skills training program, we randomly selected 66 villages in which we conducted hour-long all-female trainee engagement (TE) sessions to disseminate program information.

3. Social norms: Because restrictive social norms might discourage women from accessing training opportunities, we conducted community engagement (CE) sessions separately for males and females in 81 villages. Building on basic program information that TE conveyed, CE sessions discussed not only the social challenges women face to accessing the training but also how community members could help women overcome such challenges.

4. Reliable transportation: Because a lack of safe and reliable transportation compounds the physical distance constraint, we arranged group transportation (GT) for 54 nVBT villages to the corresponding training centers.

5. Money: The program sought to lessen the monetary constraint by offering a monthly stipend to all trainees for attending the training. We offered every household a base stipend of Rs. 1,500 and randomly allocated a stipend top-up. These additional stipends were as high as 4,500 PKR. To minimize fairness concerns, we introduced the stipend amount at both the village and the household level such that stipends did not vary by more than 1,000 PKR across households within a village.

2.2 Sampling

Our sampling frame comprised rural areas from the three program districts—Bahawalnagar, Bahawalpur, and Muzaffargarh—in the South of Punjab. The sample included 243 villages that collectively offered over 3000 training slots for women. Within each village, we then randomly selected 25 households to receive a training voucher and participate in survey activities.

After selecting our sample, we randomly assigned each village to 1 of 8 treatment branches based on the constraint alleviation strategies (referred to here as treatments – see table below on the specific treatments) described in section 2.1. Refer to Table 1 for a complete breakdown of the number of villages and households in each treatment branch. We completed the random allocation in multiple stages. First, we divided the 3 districts into 27 grids based on geographical proximity, each containing 9 sample villages. Second, we randomly selected 4 villages in each grid to have a training center (VBT) and 5 to have no training center (nVBT); we refer to these two primary treatment branches as the standard intervention.

All households in the standard intervention (i.e. all households in the study) received basic information about the course through a household visit. Stratifying on this primary randomization, we then further randomly assigned the 5 nVBT villages within each grid to receive either trainee engagement (TE), community engagement (CE), reliable group transport

(GT), a combination of CE and GT, or no additional treatment (standard intervention only). Among the 4 VBT villages per grid, we randomized 3 into the CE, TE, or baseline-intervention-only branch, and the fourth was randomly assigned to either the TE or baseline-intervention-only treatment branch. Note that no VBT villages received the GT treatment, as we deemed a transportation service less relevant or feasible to provide in VBT villages.

Next, we randomly assigned the total stipend amount at both the village and the household level. As noted in section 2.1, in addition to a base stipend of Rs. 1,500 per month awarded to each voucher holder with minimum course attendance of 80%, a randomly selected subset of households received a stipend top-up ranging from Rs. 500 to Rs. 4,500. Stipend variation was limited to Rs. 1,000 within a village to minimize potential perceived fairness concerns.

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

Table 1

Finally, we randomly selected a subset of our original households (from among all eight treatment arms) and offered vouchers to their neighboring households, thus inviting the neighbors of 550 (20%) of VBT and 550 (16%) of nVBT households. 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 attending training alone.

2.3 Measuring take-up and distance

Using village-level administrative data and follow-up surveys, we monitored take-up for all respondents in four stages: voucher acceptance, voucher submission, course enrollment, and course completion. In our main analysis, we coded each take-up measure as a dummy variable.

We measured distance between a respondent's home and her training center in three ways. Using GPS coordinates, we first calculated the straight-line distance from each nVBT village's centroid to the nearest VBT village's centroid. Since it was not feasible to assign the location of training center randomly within a village, we set this measure of distance to be zero for VBT villages. Second, we grouped households into small geographic units called settlements and physically measured the distance from each settlement to the training center using a motorcycle and an odometer. We also created a third measure of distance by averaging the settlement-level distance measure within each village to find the distance from the village's population centroid to the training center.

3. Treatment Heterogeneity

To date we have finished our main analysis, which we summarize in Appendix A, and identified physical distance as a significant hurdle to program take-up. We did not file a pre-analysis plan at the time since the analysis was based on variation in an unambiguously measured outcomes (e.g. take-up and course completion) induced by experimental variation that was pre-specified as explicit design features. To shed further light on potential channels underlying the per-km traveled costs and boundary effect, however, we are now turning to exploit non-experimental variation to examine the potential heterogeneity of these distance penalties' impact.

We can use a wide range of variables to examine heterogeneity. One purpose of filing this PAP now is to discipline what we do here *ex ante*. We will consider two potential approaches: (i) specifying key dimensions of heterogeneity based on likely channels behind the distance penalties and (ii) using machine-learning techniques such as Lasso to identify variables driving heterogeneous effects.

For the first approach, we will estimate all three equations below:

$$(1)\quad Y_i = \alpha + \beta_1\, VBT_i + \beta_2 GT_i + \beta_3\, Hetero_k + \beta_4\,(VBT * Hetero_k) + \rho X_i + \varepsilon_i$$

$$(2)\quad Y_i = \alpha + \beta_1\, VBT_i + \beta_2 GT_i + \beta_3\, Dist_i + \beta_4\, \text{Avg}Dist_i + \beta_5\, Hetero_k + \beta_6\,(VBT * Hetero_k) + \rho X_i + \varepsilon_i$$

$$(3)\quad Y_i = \alpha + \beta_1\, VBT_i + \beta_2 GT_i + \beta_3\, Dist_i + \beta_4\, AvgDist_i + \beta_5\, Hetero_k + \beta_6\,(VBT * Hetero_k) + \beta_7\,(Dist * Hetero_k) + \beta_8\,(AvgDist_i * Hetero_k) + \rho X_i + \varepsilon_i$$

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 VBT village; GT_i is an indicator for individual *i* living in a nVBT village with group transport treatment[2]; X_i is a matrix of individual and village-level controls, both demographic and other characteristics; and ε_i is a random error term. $Hetero_k$ represents an individual-, household- or village-level characteristic that distinguishes the sample into subgroups. These measures may also be defined as "pair-wise comparisons" between sending and receiving villages. Because we surveyed male and female household members separately at the baseline stage, we can further decompose $Hetero_k$ into two separate variables to exploit both male and female dimensions of the same questions, where applicable.

Below we pre-specify the broad categories of interest in exploring heterogeneous impact of distance penalties.[3] Under each category, we list relevant variables from our surveys, followed by our hypotheses about why heterogeneity might arise. If these variables will be constructed as indices, we list relevant survey questions in parenthesis. *Notably, we could construct some of these variables at multiple levels. Migration and mobility patterns, for instance, could be analyzed at both household and village levels*. For now, we do not pre-specify the specific functional forms (for variables and/or indices) or level of aggregation (household or village)

[2] We control for GT throughout our analysis, which means α and β's represent different information as we introduce additional treatment dummies into the models. In equation (1), for instance, α represents the mean take-up in the standard intervention nVBT villages.

[3] While we will explore both the boundary effect and per-km travelled costs, we are particularly interested in heterogeneity of the former since we feel that offers an especially powerful demonstration of distance penalties.

used. However, to ensure against any "results fishing", we will show our results are robust to different (relevant) functional forms and report multiple levels of aggregation. For assessing statistical significance, we will cluster standard errors for any individual-level regressions at the village level.

4.1 Family household obligations:

- Number of young, old or sick dependent HH members
- Women's time allocated to domestic chores (or conversely, entertainment), both in levels and as a share of waking hours.

Hypothesis: The distance penalties may be larger for women with more family obligations and more reasons to stay near home. In other words, such women may face higher distance penalties in terms of boundary effects and/or per-km travelled distance.

4.2 Economic Status:

- Household monthly income
- Consumption index (based on weekly and monthly food consumption)
- Non-business asset index (based on ownership status regarding 32 non-business assets)

Hypotheses: There are two potential (opposing) effects here. The distance penalties may be more severe on poorer females, who would find traveling between villages less affordable in terms of money and time. In contrast, to the extent that poorer women face fewer social constraints, they may in fact see lower distance penalties.

4.3 Exposure to outside world through migration/telecommunication/NGO presence:

- Migration and mobility index (based on number of HH members who have sought employment away from home village; who have expressed an interest in doing so; and who succeeded in finding employment away from home village)
- Family network and expressed willingness to visit a relative outside one's village measured on a Likert scale
- NGO presence as recorded in pre-treatment village-level focus groups.
- Facility index (based on availability of bus stops, banks, clinics, postal services, markets, schools, etc.)
- Village cellular network signal strength as recorded by enumerators at baseline.

Hypothesis: People from households or villages more exposed to the outside world may be less concerned about crossing boundaries or travelling outside; the distance penalties may be smaller for this sub-population.

4.4 Diversity:

- Village-level ethnic/religious diversity index (based on data on proportion of households in each *Quom, Biraderi* or ethnic group)
- Whether the trainee's household (or the trainee herself) belongs to a minority group

- Degree of overlap between sending and receiving villages in socio-ethnic and religious groups.

Hypotheses: In ethnically or religiously diverse villages, people may be more accustomed to facing “other” groups and therefore more readily able to cross boundaries. If so, the distance penalties would be smaller for this sub-population.

We will also look at overlap/commonality of ethnic and religious groups between sending and receiving villages, with the anticipation that distance penalties may be larger when there are more differences between sending and receiving villages.

4.5 Gender empowerment & status:

- Educational attainment, both average in village, individual level, and attainment relative to village median.
- Female influence index (based on self-assessed ability to influence household decisions about buying land, borrowing money, starting new activities and spending)
- Female confidence index (based on self-assessed ability to run one’s own business, obtain credit, manage employees, manage financial accounts, bargain with input suppliers and collect debt repayment)
- Attitude toward gender equality, measured both at village level, for individual female, and for the males in the household (based on male and female answers to a series of gender equality questions regarding girls’ education, women’s responsibilities for household chores, desired characteristics in a wife, etc.)
- Female mental health (measured by the standard K6 Distress Scale)
- Intra-household bargaining (based on the female respondent’s marital status, relationship with the household head and whether her husband resides in or works away from the household)

Hypotheses: Females who are more educated or empowered in the sense of displaying more influence, independence, confidence or more satisfaction may be less concerned about crossing boundaries and traveling; the distance penalties for this sub-population may then be smaller. These variables can be used directly or also relative to the analogous measures for men (by either controlling for the analogous male measure or defining a new measure that is women relative to men).

Alternatively, females who live with their husbands’ families may need to bargain with in-laws for approval to travel. The distance penalties may be larger for such females.

4.6 Demand for and willingness to develop skills:

- Self-assessed willingness/likelihood to enroll in skills training (based on survey questions such as “How much can you commute or would be able to commute for training?” and “If the Government is offering free training, how much would you be willing to pay per month for this?”)
- Anticipated commute time and costs for the training

- Questions regarding what one values in income-generating activities ("How important is it that your job allows you to work close to home, work indoors, have a flexible schedule, have a high social status, improve income, etc.")

Hypotheses: Women who expressed a stronger desire for skills training/income-generating activities or fewer concerns about commute time and costs would be more willing to cross boundaries/travel and therefore demonstrate lower distance penalties.

For the corresponding measures for men, it is less clear what the overall effects may be. Males who demonstrated a strong interest in training and/or seeking employment away from their home villages might also be more willing to allow women to travel. However, the opposite might also happen: men who have gone outside became more sensitive to boundaries and less willing to let women travel.

4.7 Perceived safety:

- Safety index (based on women's assessment of whether the rule of law was operative locally, whether the local crime level has increased compared to three months ago, and whether one's own family suffered from a crime in the last three months)

Hypothesis: Households/communities less concerned about safety may be more willing to allow females to travel. If so, the distance penalties would be smaller. The male and female dimensions of perceived safety could be included separately or simultaneously in regressions. Male perception of safety is arguably more influential in the context of our study.

4.8 Additional pair-wise comparisons:

In addition to the above, we could also estimate additional pair-wise comparisons where we look at attributes of both sending (villages/communities where the women reside) and receiving (villages/communities where the training center is located) locations. The general idea here is to construct measures of overlap-differences across a range of attributes (group type, village attributes, etc.) as well as relative comparisons (size, development level, etc.) to see whether these factors influence distance penalties. We have pre-specified such overlap measures along socio-ethnic and religious groups (see 4.4. above). We will find an amendment to this plan before exploring additional pair-wise metrics.

4.9 Machine learning approaches:

An alternative approach to examining heterogeneous effects is to be agnostic as to what variables to include and use lasso/machine learning methods to investigate which factors display heterogeneity of impact of distance penalties, especially the boundary effect.

While such an approach does not have the advantage of pre-specifying a set of categories based on possible channels (underlying the distance penalties), it has the advantage of allowing for a richer examination of the data. To the extent that factors identified by machine learning methods demonstrate a consistent pattern, this allows for a more flexible exploration of potential channels. To the extent feasible, we may also use such techniques to explore treatment heterogeneity. However, we will only use this method to shed light on potential channels to the extent that a consistent pattern of results emerges i.e. these techniques show significance for (sets) of variables that plausibly correspond to similar conceptual categories of interest (along the lines of the previously mentioned ones).

Appendix A

Main Specifications

In the analysis we have conducted to date, we began by estimating the effect of our primary treatment, village-based training (VBT), with the equation:

$$(1)\quad Y_i = \alpha + \beta_1 VBT_i + \beta_2 GT_i + \rho X_i + \varepsilon_i$$

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 VBT village; GT_i is an indicator for individual *i* living in a nVBT village with group transport treatment[4]; X_i is a matrix of individual and village-level controls, both demographic and other characteristics; and ε_i is a random error term. In order to account for any intra-cluster correlation and for the correlation we mechanically create through our stipend treatment design, we cluster this error at the village level. The coefficient β_1 gives the average treatment effect of placing the training center inside the village–what we call the "boundary effect."

Second, we further decomposed the effect of locating a training center in a village into two parts—an indicator for leaving the village itself (i.e. crossing the village boundary) and a continuous variable for the actual per-km distance traveled—by estimating the equation:

$$(2)\quad Y_i = \alpha + \beta_1 VBT_i + \beta_2 GT_i + \beta_3 Dist_i + \beta_4 AvgDist_i + \rho X_i + \varepsilon_i$$

where $Dist_i$ is a measure of (one of the three measures of) distance to the (closest) training center, and β_2 is the per-km traveled costs incurred by moving the training center further from a respondent's house. Recall that since the training center location was randomly assigned, the distance to the nearest training center ($Dist_i$) is also exogenous as long as we condition on the average distance between a village and all other villages within a reasonable radius ($AvgDist_i$).

Third, after establishing the effect of the VBT treatment and distance on take-up, we estimated the size of that effect in terms of economic value:

$$(3)\quad Y_i = \alpha + \beta_1 VBT_i + \beta_2 GT_i + \beta_3 Dist_i + \beta_4 AvgDist_i + \beta_5 Stipend_i + \rho X_i + \varepsilon_i$$

We determined the stipend amount needed to create the same impact on take-up as the VBT treatment by calculating β_1 / β_5 and the "marginal rate of substitution" between distance and stipend with β_3 / β_5.

Fourth, we looked beyond the impact of VBT and extended our analysis to the effects of our other treatment arms by including an additional indicator for each in our main specification. The equation used is:

[4] As in equations (1)-(3), we also controlled for GT throughout our main analysis.

$$(4)\quad Y_i = \alpha + \beta_1 VBT_i + \beta_2 Info_i + \beta_3 Comm_i + \beta_4 GT_i + \beta_5 Dist_i + \beta_6 Dist_i^2 + \beta_7 AvgDist_i + \beta_8 AvgDist_i^2 + \rho X_i + \varepsilon_i$$

where VBT_i, GT_i, $Dist_i$, and $AvgDist_i$ are the same as they appear in equations (1) and (2); $Info_i$ is an indicator for the trainee engagement treatment; and $Comm_i$ is an indicator for the community engagement treatment branch.

Results to date: Using the aforementioned models, we have identified two forms of distance penalties—the boundary effect and per-km traveled costs—across a range of take-up measures. Specifically, establishing a training center in a village increases course applications and enrollment by two to three times, and half of the access difference between in-village and out-of-village arose simply by crossing (invisible) village boundaries. In addition, our estimates suggest that counteracting the negative impact of distance penalties on program take-up would require a sizeable stipend. Finally, we found little impact of TE and CE but a sizeable effect of GT that is almost comparable (in magnitude) to the boundary effect. Interestingly the only effect of CE is that it suppresses (only) our first measure of take-up, though this adverse effect does not occur in villages that had a training center inside the village or villages (w/o a training center) that were provided group transport (GT) services.

While we did not systematically explore heterogeneity in the main analysis, we briefly explored one possible channel of heterogeneous effects: (village-level) ethnic diversity. Our analysis revealed that while the impact of ethnic diversity is generally positive on take-up rates, this effect is primarily present for villages that did not receive an in-village training center. This suggests that part of whatever constitutes the distance penalties are undone by having greater ethnicity diversity.