Women's Mobility and Labor Supply

Experimental Evidence from Pakistan*

Robert Garlick ⓡ Erica Field ⓡ Kate Vyborny ⓡ †

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Abstract

We study whether commuting barriers constrain women's labor supply in urban Pakistan. We randomize offers of gender-segregated or mixed-gender commuting services at varying prices. Women-only transport more than doubles job application rates, while mixed-gender transport has minimal effects on men's and women's application rates. Women value the women-only service more than large price discounts for the mixed-gender service. Results are similar for baseline labor force participants and non-participants, suggesting there are many "latent jobseekers" close to the margin of participation. These findings highlight the importance of safety and propriety concerns in women's labor decisions.

Keywords: transport, mobility, gender, female labor force participation
JEL codes: J16, J22, J28, L91

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†Author order is randomized using the AEA's randomization tool with confirmation code jM-gmZ_Ig1Eo. Garlick: Duke University, robert.garlick@duke.edu; Field: Duke University, BREAD, NBER, field.erica@duke.edu; Vyborny: World Bank, kvyborny@worldbank.org.

1 Introduction

Women have substantially lower rates of labor force participation and employment than men in many parts of the world, and particularly in South Asia (ILO, 2023). This pattern has persisted despite substantial increases in women's education levels relative to men's (Heath & Jayachandran, 2018). For example, in Pakistan, 24% of women aged $\geq$25 are employed, which rises to only 29% for those with university degrees (ILO, 2025). Increasing female labor force participation and employment has the potential to raise aggregate economic output as well as women's empowerment (Agte et al., 2024; Ashraf et al., 2023; Chiplunkar & Goldberg, 2024; Duflo, 2012; Hsieh et al., 2019).

What can be done to help non-working women realize employment gains in these settings? In this paper we explore whether women's labor supply in urban Pakistan is constrained by the multidimensional challenges associated with commuting to work: risk of harassment or violence, penalties for breaking propriety by sharing public spaces with men from outside their family, and the high cost of commuting relative to women's wages. Better commuting options could boost female employment on two distinct margins. First, easier commutes could expand the set of jobs over which unemployed women search, thereby increasing the rate at which they find suitable employment. Second, some women out of the labor force may be "latent jobseekers" at the margin of participation, for whom a wider set of work options pushes them into search. For example, although only one quarter of women in Pakistan work outside the home, another quarter say they are willing to work, suggesting scope to double female labor force participation by encouraging active search (Field & Vyborny, 2016).

To study this question, we build a large job search and matching platform and conduct door-to-door recruitment in a representative set of enumeration blocks around the city of Lahore. A novel feature of our platform is that we actively register roughly equal numbers of labor force participants and non-participants at baseline, allowing us to study potentially latent jobseekers who are missing from samples of existing workers, jobseekers, or job platform users.[1] The platform provides rich administrative data on search behavior along with all jobseeker attributes visible to firms during the application process.

The platform runs a transport service that uses motorized rickshaws and 12-seat vans to pick up women from their homes, drop them at work, and then return them home each day. The service is designed to address concerns women without access to private

[1] This approach is rare in experimental research in labor economics. Poverty Action Lab (2022) reviews 29 experimental job search studies and finds that only 8 construct samples from household listings. Others have unspecified sampling frames or sample from unemployment registries or job search assistance services, which miss non-labor force participants.

transport raise about commuting to work: the door-to-door structure means that women do not need to walk to or wait at public transport nodes, reducing their risk of harassment or breaches of propriety. We randomize access to the transport service, its price, and whether the vans are women-only or mixed-gender. These additional randomizations allow us to test whether women's labor supply is sensitive to the price of commuting and whether it responds more when barriers to safety and propriety are lowered.

We study effects on one specific margin of women's labor supply: job applications. While not a comprehensive measure of labor supply, job applications are a necessary step in all subsequent stages of labor supply and employment, and many women in our sample report that commuting concerns deter them from applying for jobs. We deliberately do not study downstream outcomes like job offers and employment because these reflect both the supply-side factors that are the focus of this paper and demand-side factors that are outside the scope of our study.[2] Furthermore, downstream outcomes are not directly observed on this or most other platforms (Kircher, 2022).

We find that offering women any form of transport increases their job application rate by 70%, from a relatively low base. This increase is almost entirely driven by women-only transport, which raises the application rate by roughly 150%, compared to only 40% for mixed-gender transport. Strikingly, these effects are similar for baseline labor force participants and non-participants, suggesting that labor force participation is relatively malleable and that non-participants are dissuaded from search by commuting concerns.

Lower transport prices also increase job application rates, and the response on the price margin allows us to calibrate the value of women-only transport in monetary terms. Application decisions reveal a high pecuniary value of gender-segregated transport: offering women-only rather than mixed-gender transport increases job applications by as much as an 81% discount on the transport price. To confirm that these findings reflect gendered factors, we run an analogous experiment with men and find that offering mixed-gender transport has no detectable effect on men's job application rates, firmly establishing that the commuting barriers addressed by pick-and-drop are gender-specific.

These findings suggest that improved transport services can boost female labor supply, particularly when the service allows women to avoid interacting with men while commuting. However, back-of-the-envelope calculations suggest that the cost of running such a service is large enough relative to women's wages that market-based provision is unlikely until female employment rates are sufficiently high. Hence, given the sizable impact on labor supply and economy of scale in provision, there is a clear economic rationale

[2]Other work studying specifically labor supply rather than employment takes a similar approach (e.g. Bursztyn et al. 2020; Cefala et al. 2024).

for temporarily subsidizing gender-segregated transport in order to make it sustainable in the long run.

This paper provides the first experimental evidence about the separate importance of pecuniary versus safety and propriety factors during commutes in shaping women's job application behavior. By actively enrolling non-labor force participants, we also provide the first experimental evidence on commuting constraints to women's *participation*, an important and understudied margin for policy influence on female labor supply in many settings. We build on an extensive literature on barriers to female labor force participation and employment, reviewed by Borker (2024), Heath et al. (2024), Jayachandran (2020), and Petrongolo & Ronchi (2020). This research documents that women face risks of harassment and violence while traveling in many settings (Aguilar et al., 2019; Amaral et al., 2023; Chakraborty et al., 2018; Kondylis et al., 2025), that harassment risks are associated with lower female employment (Chakraborty et al., 2018; Sharma, 2023; Siddique, 2022), and that expanding transport infrastructure can increase women's employment, potentially through both demand and supply factors (Kwon, 2022; Martinez et al., 2020).[3] This work suggests the potential for improved transport services to shift women's labor supply but does not directly identify effects of transport services on labor supply.

Our work bridges the gap in findings from four contemporaneous papers studying women's job search, commuting, and employment using experimental and quasi-experimental variation in transport access. Chen et al. (2024) and Dasgupta & Datta (2024) show that offering free bus rides in India increases job search and employment for some types of women, while Abou Daher et al. (2025) show that access to drivers' licenses encourages female employment in Saudi Arabia. These results are consistent with our finding that women's job applications are sensitive to expected commuting costs. But these settings do not allow separate analysis of women-only and mixed-gender transport access, so cannot isolate the role of safety and propriety in constraining women's labor market outcomes. Kondylis et al. (2025) show that encouraging female commuters in Brazil to use women-only train carriages reduces their experiences of harassment and they are willing to pay higher commuting costs for this. This is consistent with our finding that women prefer women-only transport services, although they do not study labor supply effects.

We also build on research into spatial barriers to job search and employment, particularly travel costs. This work has found mixed effects of transport subsidies (Abebe et al., 2021; Banerjee & Sequeira, 2020; Caria et al., 2024; Franklin, 2018; Phillips, 2014). Our

[3] Related work shows that these concerns also affect women's education (Alba-Vivar, 2025; Borker, 2021; Cheema et al., 2022; Muralidharan & Prakash, 2017), firms' willingness to hire women (Buchmann et al., 2024), and women's choices between at-home and out-of-home work (Ho et al., 2024; Jalota & Ho, 2024).

work differs by focusing on subsidies for commuting to specific jobs rather than traveling to search for jobs, and by studying both pecuniary subsidies and a key non-pecuniary attribute of travel: women-only versus mixed-gender commuting.[4]

While we study one specific setting, the underlying barriers to women's mobility and labor supply that we demonstrate may be relevant in many settings. Harassment of female commuters is common across high- and low-income economics (ActionAid, 2016; Dominguez Gonzalez et al., 2020) and women's labor supply is more sensitive than men's to commuting times and costs across multiple countries, contributing to gender wage gaps (Farre et al., 2022; Joshi, 2024; Le Barbanchon et al., 2021). Similarly, the policies we study are live considerations, with at least 15 countries running women-only buses or train carriages (Kearl, 2015).

2 Economic Environment

We work in Lahore, a city of roughly 10 million people in Pakistan. Only 9.8% of women aged 18-60 were working in 2018 and only 11.2% were in the labor force (Table A.1). This matches the low female labor force participation across many South Asian cities, despite rising rates of female education (Heath & Jayachandran, 2018).

2.1 Jobseekers

Our platform aimed to recruit both active labor force participants and people who were not currently working or searching but were potentially open to working. To achieve this, we randomly sampled 447 enumeration blocks and conducted a door-to-door listing of all households in these blocks. This generated a sample of roughly 150,000 adults in 50,000 households, for whom we observe sex, age, education, and employment. We invited each adult member to register with our phone-based job matching service, Job Talash, described in Section 2.3. Our main analysis focuses on the 2,653 women who registered and matched with at least one job advert posted on Job Talash during the experiment. In Section 4.5, we also analyze the corresponding 5,842 men. See Appendix A for sampling details.

This recruitment strategy successfully produced a sample with widely varied labor force attachment. Table 1 shows that 11% of recruited women were employed and searching, 9% employed but not searching, 32% searching but not employed, and 48% neither employed nor searching. Only 72% had ever worked before, and mean experience conditional on working was 5.6 years. The sample also covers a wide range of demographic groups: the interdecile age range is 20-45; the shares with at most primary, junior sec-

[4] This also relates to research into gender on job search and matching platforms (Afridi et al., 2023; Jones & Sen, 2024; Kiss ⓡ al., 2023; Kuhn & Shen, 2013; Kuhn et al., 2020; Roussille, 2024; Wheeler et al., 2022).

Table 1: Jobseeker-Level Summary Statistics at Baseline

	Mean	SD	p10	p90
	(1)	(2)	(3)	(4)
Baseline labor force status:				
Employed and searching	0.110	0.312		
Employed but not searching	0.091	0.288		
Searching but not employed	0.320	0.467		
Not searching or employed	0.479	0.500		
Ever worked before	0.718	0.450		
Experience in years | Ever worked	5.63	5.91	1.00	13.00
Age in years	29.8	9.4	20.0	45.0
Highest education level:				
At most primary	0.211	0.408		
Junior secondary/matric	0.254	0.436		
Senior secondary/intermediate	0.142	0.349		
Tertiary education	0.392	0.488		
Married	0.580	0.494		
Married with children $<= 5$	0.315	0.464		
Have own private transport	0.139	0.346		
Willing to commute in public bus	0.654	0.476		
Willing to commute in rickshaw	0.801	0.399		
Willing to use our transport system	0.940	0.238		
More likely to accept job with transport	0.794	0.405		

Notes: This table shows baseline summary statistics with one observation per jobseeker in the main analysis sample used throughout the paper: all female jobseekers who receive at least one job match during the duration of the experiment ($N = 2,653$). 'At most primary education' is an indicator equal to one for jobseekers who have completed at most primary education, including jobseekers with no formal education. 'Junior secondary/matric' is an indicator variable equal to one for the jobseekers who have completed junior secondary school (called 'matric' in Pakistan). 'Senior secondary/intermediate' is an indicator variable equal to one for the jobseekers who have completed senior secondary school (called 'intermediate' in Pakistan). 'Tertiary education' is an indicator variable equal to one for the jobseekers who have completed a bachelors degree or postgraduate degree. 'Married with children ≤ 5' is an indicator variable equal to one if the jobseeker is married and has a child aged five years or younger.

ondary, secondary, and tertiary education are respectively 21, 25, 14, and 39%; 58% are married; and 31% have children aged $\leq$5. Labor force participation is higher for younger, unmarried women with more education (Table A.2).

Women in our sample self-report that transport access is a barrier to labor force participation. Only 14% have access to private transport and only 65% are willing to commute in a public bus and 80% in a private rickshaw/taxi, which might transport them alone or with a few other passengers. Meanwhile, 94% are willing to use the service we offer, which takes them directly from home to work and back, and 79% say this would increase their likelihood of taking a job.

While our household *listing* is designed to be representative of Lahore's adult female population, the sample of women who *register* on Job Talash naturally differs. Women in this sample are slightly younger, more educated, and more likely to be employed and searching than Lahore's adult female population (Table A.1). Hence, our findings do not apply to all adult women, but instead apply to the policy-relevant population of adult women who are open to searching for jobs through a matching service that actively recruits participants. We refer to those registered on the platform as "jobseekers" irrespective of their search status at baseline and their search behavior during the experiment.

2.2 Firms and Jobs

We recruited firms through a door-to-door listing in a random sample of commercial enumeration blocks around Lahore. Firms interested in Job Talash completed a baseline survey and were invited to list a job advert immediately and again every few months. During the 13-month experiment, 256 firms listed 376 adverts, but only 172 adverts listed by 120 firms were open to women.[5] We primarily analyze these 172 adverts, although we include all 376 adverts when analyzing men's job search in Section 4.5. See Appendix A for sampling details.

The average firm had 5 male and 3 female employees, although 35.9% of firms had no female employees, and 92.4% of firms were in the service sector. The job adverts cover many roles including beautician, cleaner, cook, IT, manager, salesperson, and teacher. Education requirements range from none to graduate degrees. Posted monthly salaries have interdecile range $5,000$-$30,000$ rupees.[6]

2.3 Job Talash Platform

Job Talash is a phone-based platform that registers jobseekers and firms, collects job adverts, informs jobseekers about adverts that match their profiles, and allows jobseekers to send applications to firms. The platform is designed to allow cheap, fast job postings and applications without complex or expensive technology.

Jobseekers register with their name, contact information, age, gender, education, work experience, and occupational preferences. This information populates a CV template. We

[5] Gender-segregated workplaces are common and legal in Pakistan (Gentile et al., 2023) and some other settings (Kuhn & Shen, 2013).

[6] One Pakistani rupee ≈0.03USD in purchasing power parity terms during the experiment.

contact registered firms every few months to solicit new job adverts, collecting their minimum required education and work experience. We send a text message and/or phone call to every jobseeker about every job advert that matches their education, work experience, and occupational preferences. Messages list the job title, firm name, location, and salary. If jobseekers want to apply, we send their CV to the firm (see Appendix B for details). We observe exactly the same information as both sides of the market up to and including applications. This allows us to experiment with the application process and limits the scope for analysis to be confounded by unobserved information.

The unit of observation in our analysis is a jobseeker×job advert match. We observe 54,237 matches between 2,653 female jobseekers and 172 adverts. The main outcome is the application decision. This outcome isolates labor *supply* decisions from *demand-side* decisions about how to select applicants based on gender, distance, transport access, etc.

In the control group, the average jobseeker matches with 31.7 job adverts during the 13-month study and applies to 1.1% of these. While this application rate may seem low at first blush, it is natural that few matches generate applications since a match simply means the jobseeker is interested in that occupation and has the required education and work experience. Jobseekers in any search environment will apply to only a fraction of such jobs. We show in a companion paper that, while application rates on platforms vary substantially across settings, our application rate is comparable to several platforms in countries ranging from France to Mozambique (Vyborny ⓡ al., 2024), and many studies report seemingly higher application rates because they exclude platform users submitting zero applications. Furthermore, we deliberately sample women who were economically inactive at baseline in order to study the labor force participation margin, whereas most studies of job search platforms naturally exclude such participants.

The Job Talash platform has been used to run multiple other experiments but only one of these affected participants in the transport experiment. Results are robust to controlling for treatment assignments in that other experiment (Appendix C).

3 Research Design

3.1 Treatments

We experimentally vary access to six different daily commuting services and study how these affect women's labor supply. When they receive a job advert through the platform, individuals assigned to any treatment group also receive an offer of a commuting service run by Job Talash: a van or rickshaw that will take them from home to work and back each workday. This is for commuting daily to the specific job in the advert, not any other job, nor for job applications or interviews, and is paid through a monthly subscription fee. Such "pick-and-drop" services are rare but familiar in this context: 7.8% of firms in our baseline survey offer free or fee-based transport to at least some employees.[7]

We run two different commuting services: women-only and mixed-gender. The women-only service is designed to minimize threats of harassment and breaches of propriety during commuting by avoiding all male contact between home and the workplace. The mixed-gender service provides identical cost savings and convenience but with a higher threat of harassment and breaches of propriety.

The service is offered at one of three price levels: 20, 60, and 80% discounts on the "base price". The base price is a linear function of the distance between the respondent's home and the job location, designed to approximate a market price, i.e., the break-even price when 10 out of 12 seats on the van are paid. It is equal for women-only and mixed-gender vans. It is approximately 20% lower than the cost of a private rickshaw, the status quo commute mode for many middle class female commuters. The commuting service is guaranteed for at least one year at the offered price. The average monthly base price is 5,669 Pakistani Rupees to commute a straight-line distance of 10.5km in each direction, equal to 57.8% of the average monthly salary (Table B.4).

This design creates six treatments: mixed-gender versus women-only transport, each at three different price levels. Experimental variation identifies the impacts of a convenient commuting service, with different harassment and stigma risks, at different price levels, and hence the money-metric value of lower harassment and stigma risks.

3.2 Treatment Implementation

Once a jobseeker has been randomized to any treatment, the Job Talash call center attempts to contact them to brief them about the commuting service. If they cannot be contacted during this initial round of calls, they are briefed during their next job application. The briefing describes the type of commuting service offered (mixed-gender or women-only) and explains that it will be offered for some but not necessarily all future job matches (see next subsection for reasons). The script does not specify the discount *rate* the jobseeker has been assigned. To avoid signalling other information about the job, the briefing emphasizes that the service is offered by Job Talash rather than the employer, and that the gender composition of the workplace may differ from the commuting service.

After the jobseeker has been briefed, all subsequent matches they receive include information about the commuting service. Hence, different jobseekers start receiving treatment at different times. Appendix B shows the briefing script and sample messages.

[7]We keep these firms in the experimental sample because our jobseekers do not know about firm-provided transport when they apply but results are robust to dropping these firms (Appendix E.1.)

3.3 Treatment Randomization and Receipt

The different treatments are randomized at different levels. Control versus mixed-gender versus women-only transport is randomized at the level of the enumeration block where each jobseeker lives. These are *also* randomized at the level of the enumeration block where the *jobs* are located. This geographic concentration facilitates actually running the commuting service. A jobseeker$\times$job advert match is assigned to each treatment type if and only if the jobseeker *and* job enumeration blocks are *both* randomized to that treatment type. This means that a jobseeker assigned to each treatment type is eligible to receive that transport for only some of their matches. A jobseeker assigned to control is never eligible to receive transport. Discount rates are randomized at the jobseeker level.

Table C.1 shows the shares of jobseekers and matches assigned to control and each of the six treatments. We run most analysis at the match level. Of the 54,237 matches, 84.5% are assigned to control; 10.6% are assigned to mixed-gender transport, with shares in the small/medium/large discount groups of respectively 1.0/3.2/6.4%; and 4.8% are assigned to women-only transport, with shares in the small/medium/large discount groups of respectively 0.4/1.5/3.0%. See Appendix C for details on randomization and reasons for the relative group sizes.

Table C.2 reports balance test results. The share of statistically significant differences between groups is consistent with chance, we cannot reject overall balance of covariates over all treatment groups (p=0.47), and results are robust to controlling for imbalanced covariates (Appendix E.1).

Treatment is randomized at baseline but rolled out gradually because jobseekers are only offered transport once they have been contacted and briefed. Table C.3 shows the shares of matches assigned to each treatment that are actually offered that treatment. No match is ever offered a treatment that it has not been assigned.

3.4 Estimation and Inference

We first estimate the effect of receiving an offer of any type of transport. We regress an indicator for jobseeker i applying to job j on an indicator for the match being offered any of the six treatments, instrumented by an indicator for the match being assigned to any of the six treatments. Standard errors are clustered at both the jobseeker enumeration block and the job enumeration block, the levels at which control versus mixed-gender versus women-only transport is assigned. We estimate this system with and without prespecified jobseeker- and match-level covariates, listed in Table 2. The coefficient on the 'offered any transport' indicator is an unbiased estimator of the average treatment effect on the treated jobseekers of being offered transport (the ATT/ATET).

We also estimate the ATTs of different types of treatment, e.g., mixed-gender and women-only transport, or transport at different discount levels. To do so, we replace the indicators for offered and assigned any transport with vectors of indicator variables for different types of transport offered and assigned, and we estimate a separate first-stage equation for each type of treatment.[8] See Appendix C for details.

We report all ATT estimates with the corresponding first-stage instrument strength test results. All our analyses are just-identified and use clustered standard errors, so we follow the recommendation from Andrews et al. (2019) and use F-test statistics from Kleibergen & Paap (2006) to evaluate first stage strength. The first-stage results satisfy their criteria for 'strong' instruments in all cases (Table C.4).

The estimating equations, outcome, covariates, and heterogeneity analyses are prespecified at https://www.socialscienceregistry.org/trials/2410. Appendix D describes the preanalysis plan, which analyses in the paper were not prespecified, and why some prespecified analyses become infeasible for operational reasons.

4 Results

We first discuss the average effect of offering transport of any kind at any price on female jobseekers' likelihood of applying for a vacancy. We then explore the relative importance of pecuniary and non-pecuniary considerations in women's labor supply by decomposing this average effect into the effects of being offered women-only versus mixed-gender transport at different discount rates. We finally isolate the role of gender by comparing effects between female and male jobseekers.

4.1 Average Effect of Any Transport Offer

Table 2, column 1 shows that offering any transport nearly doubles women's likelihood of applying to a match, adding 0.8pp onto the control group mean of 1.11% (s.e.=0.038, p=0.037). The estimate is virtually unchanged with controls for prespecified baseline covariates (column 2) and additional controls for quarter of year to capture aggregate time trends, jobseeker locations to capture local economic conditions, treatment assignments in the other overlapping experiment, and imbalanced baseline covariates (Appendix E.1).

Treatment does not substantially change the types of jobs that receive applications. Treated jobseekers apply to jobs that are on average 2% farther from their homes and have

[8] In our application the ATT is identical to the local average treatment effect (LATE) because no control group matches are ever offered transport (i.e. non-compliance is one-sided). Our experimental design also identifies the average treatment effects of being *assigned* to each treatment, or intention-to-treat effects (ITTs). We do not focus on ITTs because treatment receipt rates differ somewhat between treatments (Table C.3). This makes it difficult to compare ITT magnitudes between treatments. The ATTs automatically adjust for differences in treatment receipt rates, allowing us to compare magnitudes.

Table 2: Treatment Effects of Different Types of Transport Offers on Job Applications

	Apply			
	(1)	(2)	(3)	(4)
Offered any at any price	0.0080**	0.0076**		
	(0.0038)	(0.0035)		
Offered women only at any price			0.0181**	0.0163*
			(0.0090)	(0.0097)
Offered mixed at any price			0.0048	0.0048
			(0.0048)	(0.0044)
# matches	54237	54237	54237	54237
# jobseekers	2653	2653	2653	2653
# jobseeker clusters	282	282	282	282
# firm clusters	46	46	46	46
Mean outcome | T = 0	0.0111	0.0111	0.0111	0.0111
First-stage strength: F-stat	62.57	61.66	40.70	54.19
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000
Controls	No	Yes	No	Yes
Equality of treatments: p-value			0.2403	0.3208

Notes: This table shows the effects of different types of transport offers on an indicator for applying. In columns (1) and (2), the coefficient on 'Offered any transport at any price' shows the effect of being offered either women-only or mixed-gender transport at any price level. In columns (3) and (4), the coefficient on 'Offered women-only at any price' shows the effect of being offered specifically women-only transport at any price level, and the coefficient on 'Offered mixed at any price' shows the effect of being offered specifically mixed-gender transport at any price level. In all columns, the offer variables are instrumented by assignment variables, as explained in Section 3.4. Even-numbered columns include pre-specified covariates – age, education, years of work experience, and indicators for married, having children aged ≤ 5, and the job salary exceeding the jobseeker's self-reported reservation wage – using a flexible functional form explained in Appendix C and following Goldsmith-Pinkham et al. (2024). The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.

0.4% higher values of an index of desirable attributes like salary and benefits (Appendix E.2), but neither difference is close to statistically significant (p=0.83, 0.96).

The firm-side randomization means treated jobseekers are offered transport for some but not all of their matches, which might lead to within-jobseeker substitution between matches. Appendix E.3 shows that treated jobseekers submit more applications overall, net of any within-jobseeker substitution.

4.2 Gender-segregated versus Mixed-gender Transport

Female jobseekers may value the transport we offer either because it lowers commuting costs or because it provides a uniquely safe and socially sanctioned commuting option. To isolate the value of safety and propriety for encouraging female labor supply, we randomize between women-only and mixed-gender transport services. The women-only service is likely to be perceived as safer because it lowers risks of harassment by fellow commuters, and also as more socially acceptable due to prevailing norms against women and men sharing public spaces.

Table 2, column 3 shows that offering women-only transport has an effect more than three times larger than offering mixed-gender transport, suggesting that some combination of safety and propriety considerations are important for women's labor supply. Women-only transport offers increase the application probability by 1.81pp (s.e.=0.90, p=0.045), compared to only 0.48pp (s.e.=0.48, p=0.312) for mixed-gender transport offers. The 1.3pp difference is large, particularly relative to the control group mean application rate of 1.1%, but not statistically significant (p=0.24). The results are very similar when we add prespecified controls (column 4) and additional controls (Appendix E.1).

These results suggest that non-price commuting features play a key role in women's labor supply decisions. While the value of gender-segregated transport likely reflects a combination of safety and propriety considerations, separating their relative importance is difficult as it would require independent variation in safety conditional on propriety and vice versa. Instead, we conduct open-ended interviews with twenty-two jobseekers who were offered women-only transport to understand what they value about the service. Over half highlighted safety and lower harassment risks as important, while none discussed propriety considerations. This provides suggestive evidence that safety risks from other commuters are a bigger *direct* concern than stigma from breaking propriety, though breaches of propriety may contribute to harassment risks. Other respondents discussed non-gender-specific factors such as price and reliability.

4.3 Heterogeneous Treatment Effects

In Figure 1 we show heterogeneous treatment effects (HTEs) by a prespecified set of jobseeker, job, and match characteristics that likely correlate with wage elasticities and the value of transport. These estimates come with the important caveat that we have limited power to detect differences across subsamples. See Appendix E.4 for tables of the point estimates, standard errors, and adjustments for multiple hypothesis testing.

We use the HTEs to provide some suggestions about which features drive the strong preference for women-only over mixed-gender transport.[9] Both married women and women with higher safety concerns show no response at all to offers of mixed-gender transport and large but imprecisely estimated responses to offers of women-only transport (panels A–B). As the two characteristics are uncorrelated in our data (ρ=0.03), these patterns suggest roles for both safety and propriety concerns in the revealed preference for gender-segregated commuting. Jobs with above-median rather than below-median salaries see much larger effects from offers of mixed-gender transport, but not larger effects from offers of women-only transport (panel C). Interpreting this pattern is complex because salary offers are endogenous to both jobseeker and job characteristics. However, one possibility is that women feel less threatened by mixed-gender transport that is offered for well-paid jobs than for low-paid jobs, perhaps due to expectations of behavioral norms among co-commuters. Consistent with the possibility that this is about co-commuters rather than pecuniary factors, we observe this pattern only when we split the sample at the median of the aggregate salary distribution and not when we split at the medians of the jobseeker-specific salary distributions.

Importantly, the preference for women-only over mixed-gender transport is independent of many other factors: education, age, number of social contacts who work, and commuting distance (panels D–H). The widespread preference for gender-segregated commutes suggests that it is driven by a common factor such as a widespread social norm that constrains married women in any demographic group from taking safety risks or breaking propriety.

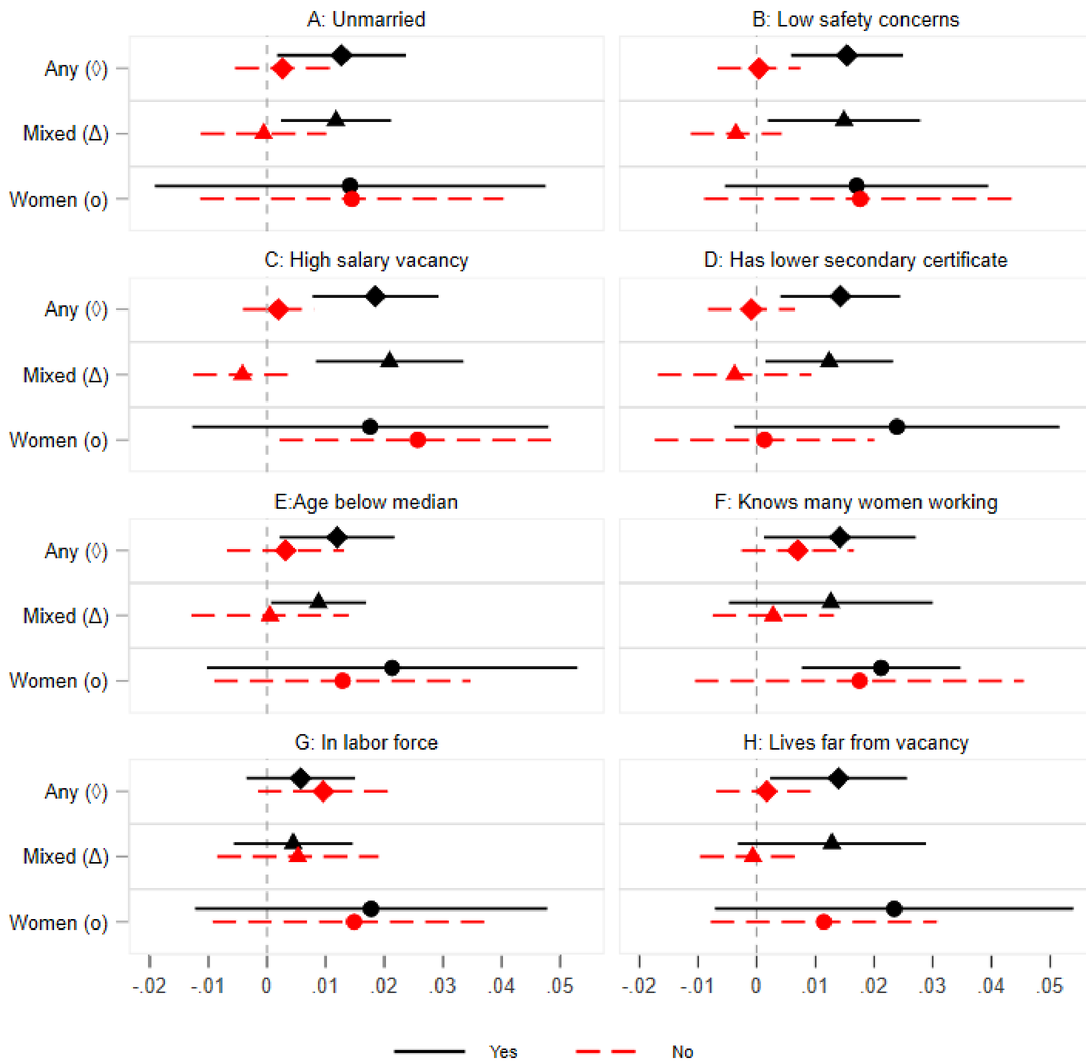
It is also striking that labor supply responses to offers of both types of transport are very similar between baseline labor force participants and non-participants (panel G).[10] This provides novel evidence that many women in this setting have relatively elastic labor force participation, responding readily to changes in the cost of labor supply. This is important because, in South Asia, the majority of women are not labor force participants. To date, we have learned relatively little about this population of latent jobseekers because they are excluded from many samples in experimental labor economics (Poverty Action Lab, 2022).[11]

[9] We also show the HTEs of offering any transport, for the sake of completeness, but we do not focus on these. Our interest is mainly on the differences between subsamples in the relative effects of offering women-only versus mixed-gender transport.

[10] We define a labor force participant as anyone employed or searching at baseline, following International Labour Organization definitions.

[11] Individual labor force participation often changes rapidly through time in both higher- and lower-income economies (Castro et al., 2024; Donovan et al., 2018), though this work does not directly examine the policy-sensitivity of participation.

Figure 1: Heterogeneous Treatment Effects of Transport Offers on Job Applications



Notes: This figure shows heterogeneous treatment effects of offering each type of transport – any transport (◇), mixed-gender transport (△), and women-only transport (○) – on job applications, using eight different dimensions of heterogeneity. Each dimension of heterogeneity is captured by a binary variable. The heterogeneous treatment effects are obtained by splitting the sample in two using the binary variable and then estimating equations (1) and (2) in each sample, including the covariates listed in Table 2. The black and red symbols show the treatment effects in the subsamples where the heterogeneity variables are respectively = 1 (Yes) and = 0 (No). The solid black and dashed red lines show 95% confidence intervals, obtained from standard errors that are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. Binary heterogeneity variables are defined as follows: "A: Unmarried" = 1 if the jobseeker was unmarried at baseline and = 0 otherwise."B: Low safety concerns" = 1 if the jobseeker has an above-median value of an inverse-covariance-weighted average of baseline self-reported perceptions of neighborhood safety, self-reported perceptions of safety for women in public spaces, and official neighborhood crime rate and = 0 otherwise. "C: High salary vacancy" = 1 if the job salary is above the sample median and = 0 otherwise. "D: Has lower secondary certificate" = 1 if the jobseeker had completed at least junior secondary school at baseline and = 0 otherwise."E: Age below median" = 1 if the jobseeker's age is below the median and = 0 otherwise. "F: Knows many women working" = 1 if the jobseeker reports knowing many women working at baseline and = 0 otherwise."G: In labor force" = 1 if the jobseeker was employed or was searching for a job at baseline and = 0 otherwise. "H: Lives far from vacancy" = 1 if the straight-line distance from the jobseeker's home to the job location is higher than the sample median and = 0 otherwise. All HTEs except age are prespecified. The safety index includes both two prespecified variables and a third variable we obtained after preregistration. See Appendix D for details. The unit of observation is the jobseeker × job match and the sample is all matches for female jobseekers during the duration of the experiment (N = 2,653).

4.4 Sensitivity of Women's Labor Supply to Commuting Costs

Our experimental design generates exogenous variation in the price at which transport is offered by randomly varying the discount rate subscribers receive on the "base price" of transport (defined in Section 3.1).

The treatment effect on applying is monotonically increasing in the size of the discount, rising from almost zero at the 20% discount to just above 1pp at the 80% discount (Table 3, columns 1–2). This price sensitivity is driven more by women-only transport: while the application rate is almost identical for mixed-gender and women-only transport at 20% discounts, it is 1.68pp higher for women-only transport at 80% discounts (columns 3–4). This difference is large relative to the control group application rate of 1.11%, although it is not statistically significant largely because the sample size in each of the six treatment groups is small (p=0.291).

Importantly, the experimental design makes possible a back-of-the-envelope comparison of the relative importance of pecuniary factors versus safety and propriety for women's labor supply. This comparison is valuable for a policymaker running mixed-gender transport, common in many parts of the world, who wants to raise women's labor supply and can allocate resources to either price discounts or providing women-only transport. We use the estimates in Table 3 columns 3–4 to fit linear application-discount relationships separately in the women-only and mixed-gender transport groups. In the mixed-gender group, a 1pp higher discount raises the application rate by 0.016pp. Hence, an 81pp higher discount rate raises the application rate by 1.33pp, equal to the effect of changing from mixed-gender to women-only transport (Table 2, column 3). This 81pp estimate suggests a high return to women-only transport relative to cheaper mixed-gender transport, although the large standard error – 110pp – means that we interpret this comparison very cautiously.

4.5 Comparing Women's and Men's Responses to Transport Offers

Although our main focus is on women's mobility and labor supply, our experimental design also allows us to compare women's and men's responses to offers of transport services. This is important because it identifies how gender shapes responses to transport offers, which would be impossible in a study of women alone.

In particular, our sample of platform registrants includes $5,842$ men living in enumeration blocks assigned to control or mixed-gender transport.[12] Men were randomized into $20, 60$, and 80% discounts on the transport base price using the same algorithm as women.

Our main finding is that men's labor supply is substantially less responsive to trans-

[12] We do not provide men-only transport because scoping work showed little demand for it in this setting.

Table 3: Price Sensitivity of Different Types of Transport Offers on Job Applications

	Apply			
	(1)	(2)	(3)	(4)
Offered any at 20% discount	-0.0037	-0.0088		
	(0.0104)	(0.0071)		
Offered any at 60% discount	0.0065	0.0071		
	(0.0057)	(0.0061)		
Offered any at 80% discount	0.0106*	0.0104**		
	(0.0055)	(0.0049)		
Offered women-only at 20% discount			-0.0036	-0.0060
			(0.0263)	(0.0243)
Offered women-only at 60% discount			0.0137	0.0146
			(0.0109)	(0.0093)
Offered women-only at 80% discount			0.0233*	0.0244*
			(0.0131)	(0.0142)
Offered mixed-gender at 20% discount			-0.0038	-0.0091
			(0.0109)	(0.0065)
Offered mixed-gender at 60% discount			0.0041	0.0031
			(0.0070)	(0.0072)
Offered mixed-gender at 80% discount			0.0065	0.0075
			(0.0065)	(0.0052)
# matches	54237	54237	54237	54237
# jobseekers	2653	2653	2653	2653
# jobseeker clusters	282	282	282	282
# firm clusters	46	46	46	46
Mean outcome | T = 0	0.0111	0.0111	0.0111	0.0111
First-stage strength: F-stat	21.18	21.27	13.86	18.98
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000
Equality of treatments: p-value	0.2844	0.0421		
Equality of women-only treatment: p-value			0.5319	0.4518
Equility of mixed-gender treatment: p-value			0.5416	0.0851
Equality of 20% discount: p-value			0.9954	0.9038
Equality of 60% discount: p-value			0.4697	0.3516
Equality of 80% discount: p-value			0.2908	0.2842
Controls	No	Yes	No	Yes

Notes: This table shows the effects of each treatment disaggregated by price discounts on an indicator for applying. In columns (1) and (2), the coefficient on 'Offered any transport at 20/60/80% discount' shows the effect of being offered either women-only or mixed-gender transport at a 20/60/80% discount. In columns (3) and (4), the coefficient on 'Offered women only at 20/60/80% discount' shows the effect of being offered specifically women-only transport at a 20/60/80% discount, and the coefficient on 'Offered mixed-gender at 20/60/80% discount' is defined analogously. In all columns, the offer variables are instrumented by assignment variables, as explained in Section 3.4. Even-numbered columns include pre-specified covariates – age, education, years of work experience, and indicators for married, having children aged $\leq$ 5, and the job salary exceeding the jobseeker's self-reported reservation wage – using a flexible functional form explained in Appendix C and following Goldsmith-Pinkham et al. (2024). The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01. More jobseekers are assigned to the larger than smaller discounts, as we explain in Appendix C, which explains why the estimated effect of a 20% discount is both less precisely estimated and slightly more sensitive to adding controls.

Table 4: Treatment Effects of Different Types of Transport Offers on Job Applications – Comparing Women and Men

	Apply			
	(1)	(2)	(3)	(4)
Offered any at any price	0.0080**		0.0080**	
	(0.0038)		(0.0038)	
Offered any at any price × Male	-0.0128***		-0.0104**	
	(0.0042)		(0.0043)	
Offered women only at any price		0.0181**		0.0181**
		(0.0090)		(0.0090)
Offered mixed at any price		0.0048		0.0048
		(0.0048)		(0.0048)
Offered mixed at any price × Male		-0.0096*		-0.0073
		(0.0051)		(0.0051)
Male Jobseeker	-0.0037**	-0.0037**	-0.0078***	-0.0078***
	(0.0015)	(0.0015)	(0.0017)	(0.0017)
# matches	365660	365660	361330	361330
# jobseeker	8495	8495	8411	8411
# jobseeker clusters	299	299	299	299
# firm clusters	62	62	62	62
Mean outcome | T = 0	0.0079	0.0079	0.0079	0.0079
First-stage strength: F-stat	46.71	37.46	40.95	33.52
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000
Weights			X	X

Notes: This table shows the gender-specific effects of each treatment on an indicator for applying. In columns (1) and (3), the coefficient on 'Offered any transport at any price' shows the effect on women of being offered either women-only or mixed-gender transport at any price level, while the coefficient on 'Offered any transport at any price × Male' shows how the effect of being offered either mixed-gender transport differs for men. In columns (2) and (4), the coefficient on 'Offered women only at any price' shows the effect on women of being offered specifically women-only transport at any price level and the coefficient on 'Offered mixed at any price' shows the effect of women being offered specifically mixed-gender transport at any price level, and the coefficient on 'Offered mixed at any price × Male' shows how the effect of being offered mixed-gender transport differs for men. In columns (3) and (4), the estimates are weighted by the jobseeker's age, years of work experience, marital status at baseline, and the gap between job ad's salary and expected salary. The unit of observation is the jobseeker × job match and the sample is all matches for female or male jobseekers during the duration of the experiment. In all columns, the offer variables are instrumented by assignment variables. Standard errors shown in parentheses are two-way clustered by jobseekers enumeration block and firm's enumeration block, the units at which the treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.

port offers than women's. Offering men mixed-gender transport increases their application rate by 1.2pp *less* than offering women any type of transport women and by 0.96pp less than offering women mixed-gender transport (Table 4, columns 1-2). Both differences are large and statistically significant (p=0.00, 0.06), suggesting that men value the transport service substantially less than women.[13] The results are similar when we reweight men to have the same distribution of observed baseline characteristics as women, which establishes that the gender difference in treatment effects is not explained by gender differences in these characteristics (columns 3-4).[14]

Taken together, these results show that men value mixed-gender transport services less than women, likely due to the fact that they have access to better outside options for transport. This highlights the relative importance for women specifically of improved transport services for commuting.

Including both women and men in the experiment raises the possibility of treatment spillovers between household members. Appendix E.5 shows that our main finding holds when we drop households with multiple members in the experiment, and that there is limited evidence of spillover effects within households with multiple members.

5 Concluding Discussion

We show that women's labor supply can be highly sensitive to variation in commuting options. Offering a reliable, home-to-work commuting service increases job application rates in our setting by 70%. Women particularly value women-only commuting services, which raise application rates by roughly 150%.

This provides both a positive and a cautionary note about the scope for commuting services to raise women's labor supply. On the one hand, such services can raise women's job application rates. On the other hand, the cost of our service is a high proportion of salaries for most jobseekers. The base price of the home-to-work commuting service exceeds half the posted salary for 42% of the job$\times$jobseeker matches on Job Talash. Even for matches with below-median commuting distance (within 10km of the jobseeker's home), the median ratio of commuting cost to posted salary is 0.24 (Figure B.2). This affordability is particularly serious for women-only transport, as low female employment and labor force participation means that women-only vans are harder to fill and hence harder to cover costs. However, this partly mirrors the high government subsidies required to

[13] The treatment effects for men are actually marginally negative but close enough to zero that we do not view this as evidence that transport offers actually discourage male applications.

[14] Control group men's application *rates* are lower than women's (bottom row of the table). But men on average match to 32.9 more jobs than women over the year of the experiment, so they submit more applications despite lower match-level application rates.

run most public transport services worldwide (Estupinan et al., 2016).

Thus, our results suggest a high potential of interventions that address barriers to physical mobility for women, either through women-only public transport or through other measures such as improved street safety (Amaral et al., 2023). These can directly raise women's labor force participation, even relative to large pecuniary subsidies. They may also have indirect effects by generating economies of scale in running women-only public transport that allow lower costs, or by showing examples of women working that challenge norms and expectations against women's work (Field et al., 2021).

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A Sample

This appendix describes the sampling process and sample characteristics for both jobseekers and firms studied in this paper. This sample is not the same as samples studied in other experiments on the Job Talash platform, as we discuss in Appendix B.

Jobseeker sample: We recruited jobseekers by randomly sampling roughly 300 enumeration blocks in the city of Lahore. Our survey team approached each dwelling in each sampled enumeration area and completed a baseline survey with roughly 150,000 adults in roughly 50,000 households. We invited all adult household members to register on the Job Talash platform. This sampling process is designed to include participants with different education levels and types of labor market attachment, including people who are neither employed nor searching. This is relatively unusual in experimental labor economics research.[15] This sample breadth is important because it allows us to study how transport access affects labor force participation.

In this paper we study the $2,653$ women and $5,842$ men who registered and matched with at least one job advert posted on Job Talash during the one year of the experiment.

We can compare our experimental sample of jobseekers to a representative sample of adults in the city of Lahore from the 2018/9 Labor Force Surveys (LFS) run by the Pakistan Bureau of Statistics (Pakistan Bureau of Statistics, 2018-2019). Table A.1 shows summary statistics for our experimental sample (column 4), for all respondents in our household listing exercise (column 3), and for two external benchmarks: data from the LFS for the entire country (column 1) and for Lahore (column 2). Women in our experimental sample are slightly younger, more likely to have secondary or higher education and to be employed, and much more likely to be searching than the adult female population of Lahore (panel A, columns 2 and 3 versus 4). We see the same patterns for men except that men in our experimental sample are *less* likely to be employed than the adult male population of Lahore (panel B, columns 2 and 3 versus 4). We deliberately do not speculate about how treatment effects might differ if we ran the same experiment in a representative sample of adults in Lahore or elsewhere. See Gentile et al. (2023) for additional discussion on selection from the household listing to registration on this platform.

Table A.2 describes the relationship between baseline labor force participation and our observed demographic characteristics: age, education, marital status, and parenthood. Women who in the labor force are on average younger, more educated, and less likely to be married or have young children, although the latter relationships are not statistically

[15] For example, of the 29 experimental job search studies reviewed by Poverty Action Lab (2022), only 8 construct samples from household listings, while another 12 sample from unemployment registries and 4 from job search assistance services, whose participants are required or strongly encouraged to search.

Table A.1: Summary Statistics for Experimental and External Comparison Samples

Panel A - Female Sample

	LFS Pakistan	LFS Lahore	HH Listing Sample	Experimental Sample
	(1)	(2)	(3)	(4)
Age	33.9	33.8	32.6	30.4
	(11.6)	(11.6)	(11.1)	(9.4)
Highest Education Level				
Less than Intermediate/High School	0.853	0.679	0.706	0.466
Completed Intermediate/High School	0.073	0.148	0.127	0.142
More than Intermediate/High School	0.074	0.173	0.159	0.392
Employed	0.242	0.098	0.081	0.200
Not employed and available for work	0.034	0.014	N/A	0.328
Searching	N/A	N/A	N/A	0.454
Searching and not employed	0.011	0.009	N/A	0.328
Applied to prospective employer	0.004	0.004	N/A	0.105
Checked at work sites, factories, markets, etc.	0.001	0.002	N/A	0.059
Sought assistance from friends, relatives, others	0.004	0.003	N/A	0.243
Placed or answered advertisements	0.002	0.000	N/A	0.069
Registered with an employment agency	0.001	0.001	N/A	0.027
Took other steps	0.004	0.000	N/A	0.004

Panel B - Male Sample

	LFS Pakistan	LFS Lahore	HH Listing Sample	Experimental Sample
	(1)	(2)	(3)	(4)
Age	34.4	34.4	33.3	30.2
	(12.2)	(11.9)	(11.4)	(9.8)
Highest education level				
Less than Intermediate/High School	0.797	0.705	0.720	0.624
Completed Intermediate/High School	0.103	0.134	0.118	0.151
More than Intermediate/High School	0.100	0.160	0.152	0.226
Employed	0.865	0.832	0.713	0.434
Not employed and available for work	0.026	0.031	N/A	0.324
Searching	N/A	N/A	N/A	0.628
Searching and not employed	0.020	0.025	N/A	0.324
Applied to prospective employer	0.009	0.013	N/A	0.136
Checked at work sites, factories, markets, etc.	0.008	0.010	N/A	0.103
Sought assistance from friends, relatives, others	0.008	0.014	N/A	0.243
Placed or answered advertisements	0.004	0.005	N/A	0.081
Registered with an employment agency	0.002	0.001	N/A	0.033
Took other steps	0.003	0.005	N/A	0.005

Notes: This table compares the sample of jobseekers in this study (column 4) to several external benchmarks: official Labor Force Survey data for the country (column 1) and Lahore (column 2), and the representative household listing of Lahore from which the experimental sample was recruited (column 3). The statistics in columns 1 and 2 are calculated from the 2018 Labor Force Survey (LFS) using post-stratification weights provided by Pakistan Bureau of Statistics. Standard deviations are shown in parentheses for all continuous variables. The indented rows show specific education levels and specific search methods used. Cells with 'N/A' mean that measure was not collected in that survey. The LFS only asked non-employed respondents about search.

Table A.2: Relationships between Baseline Labor Force Attachment and Demographics

	(1) Employed and searching	(2) Employed and not searching	(3) Searching and not employed	(4) Not searching and not employed
Age	-0.0019**	0.0002	-0.0009	0.0025
	(0.0008)	(0.0010)	(0.0015)	(0.0016)
Junior secondary/matric	0.0372**	0.0351**	-0.0206	-0.0517
	(0.0167)	(0.0170)	(0.0304)	(0.0330)
Senior secondary/intermediate	0.0674***	0.0695***	0.0190	-0.1558***
	(0.0259)	(0.0259)	(0.0401)	(0.0429)
Tertiary education	0.1047***	0.0474**	0.0872***	-0.2393***
	(0.0205)	(0.0192)	(0.0335)	(0.0346)
Married	-0.0092	-0.0371*	0.0113	0.0350
	(0.0217)	(0.0216)	(0.0369)	(0.0402)
Married with children <= 5	-0.0220	0.0174	0.0450	-0.0404
	(0.0202)	(0.0202)	(0.0341)	(0.0368)
Share of sample	0.1111	0.0905	0.3198	0.4785

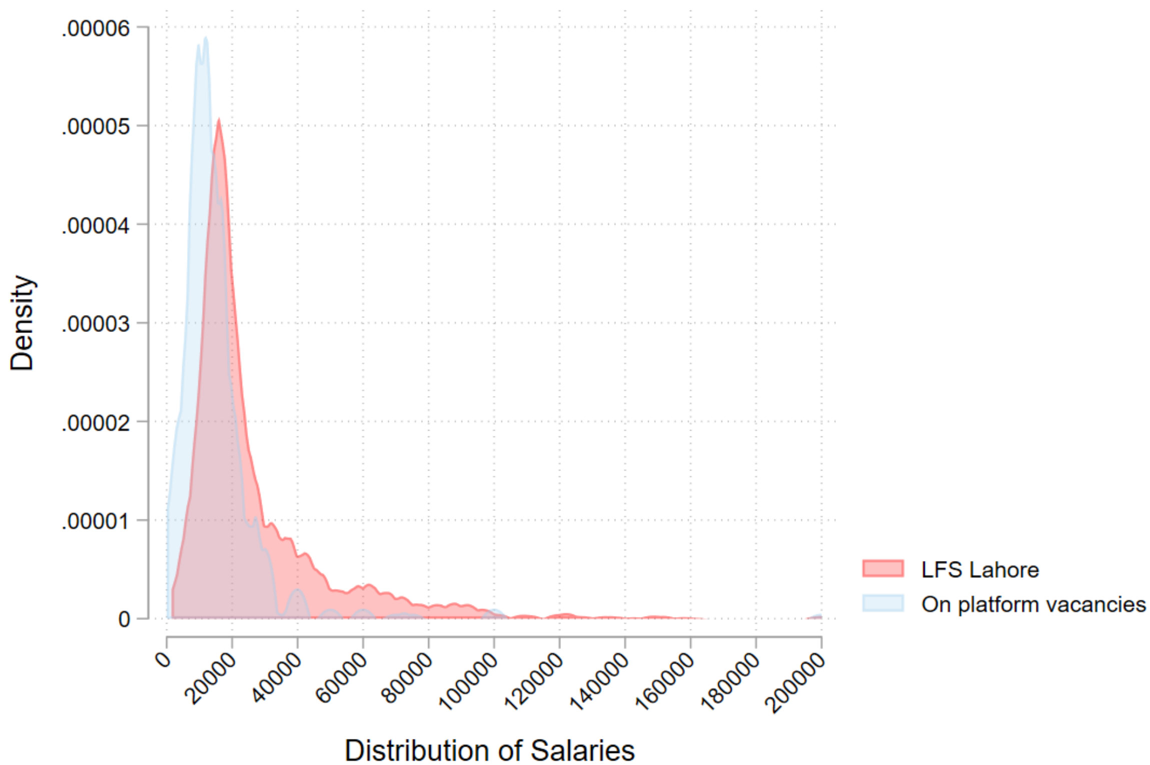
Notes: This table shows the relationships between four baseline states of labor force attachment and demographics. The unit of observation is at the jobseeker level. The sample is the 'main analysis sample:' all female jobseekers who receive at least one job match during the duration of the experiment. Columns (1) to (4) show results from regressions with four different left-hand-side variables – employed and searching, employed and not searching, searching and not employed, and not searching and not employed – and the same right-hand-side variables – age, education, marriage, and parenthood. At most primary education is the base category for the education indicator variables and is omitted. "Junior secondary/matric" is an indicator variable equal to one for the jobseekers who have completed junior secondary school (called "matric" in Pakistan). "Senior secondary/intermediate" is an indicator variable equal to one for the jobseekers who have completed senior secondary school (called "intermediate" in Pakistan). "Tertiary education" is an indicator variable equal to one for the jobseekers who have completed at least a bachelors degree. Share of sample in the final row shows the percentage of jobseekers in each of the labor force states. Heteroskedasticity-robust standard errors are shown in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

significant at conventional levels.

Firm sample: We conducted a door-to-door listing in a representative sample of enumeration blocks around Lahore, approaching all of the roughly 10,000 firms we could locate in the blocks. For firms interested in Job Talash, we conducted a baseline survey and invited them to list a job advert immediately and again every few months. During the 13-month experiment, 256 firms listed 376 adverts, but only 172 adverts listed by 120 firms were open to women. These 172 adverts are our primary analysis sample, although we include all 376 adverts when analyzing men's job search in Section 4.5.

Table A.3 shows that at baseline the average firm had 5 male and 3 female employees, although 35.9% of firms had no female employees. 92.4% of firms were in the service sector. The job adverts have a wide range of minimum education requirements, from primary or no education (23%) to tertiary education (51%). The most common roles are working in sales and marketing (16%), in salons and beauty parlors (12%), as office assistants (10%), as teachers (7%), and as managers (6%), as well as roles such as cleaners, cooks, and programmers. Posted monthly salaries have mean 18,406 and interdecile

Figure A.1: Salary Distribution for Experimental and External Comparison Sample



Notes: This figure shows the distribution of monthly salaries reported in the Labor Force Survey for Lahore in 2018 (red distribution, slightly to the right) and the distribution of salaries for vacancies posted on the Job Talash platform for the experimental sample (blue distribution, slightly to the left). Data includes both women and men. Salary values greater than 200,000 have been top-coded at 200,000. Salaries are reported in Pakistani Rupees per month. 1 Rupee $\approx$ USD 0.03 in purchasing power parity terms during the study period.

range $5,000$-$30,000$ Rupees. One Pakistani Rupee $\approx$ 0.03 USD in purchasing power parity terms during the experiment.

Figure A.1 compares the distribution of salaries for vacancies posted on the Job Talash platform to the distribution of salaries for the Lahore LFS (Pakistan Bureau of Statistics, 2018-2019). The salary distribution is somewhat higher for the LFS data. But this does not necessarily mean that vacancies on Job Talash are lower quality than average vacancies in Lahore because the data capture two different types of salaries. The LFS data capture salaries for filled jobs including jobs where incumbent workers have substantial experience with that firm, while the Job Talash data capture posted vacancies for new hires.

Table A.3: Firm- and Vacancy-Level Summary Statistics

Panel A - Firm Level

	Mean	SD	p10	p90
	(1)	(2)	(3)	(4)
No of employees:				
Total	7.95	7.63	1.00	25.00
Male	5.48	5.26	0.00	17.00
Female	2.69	4.10	0.00	12.50
Industry classification:				
Education	0.084	0.279		
Finance	0.042	0.201		
Trade	0.227	0.421		
Other services	0.571	0.497		
Non-services	0.076	0.266		

Panel B - Vacancy Level

	Mean	SD	p10	p90
	(1)	(2)	(3)	(4)
Posted salary	18,406	20,978	5,000	30,000
Offers any benefits	0.764	0.426		
Minimum required education:				
At most primary	0.233	0.424		
Junior secondary/matric	0.262	0.441		
Senior secondary/intermediate	0.000	0.000		
Tertiary education	0.506	0.501		
Occupation:				
Parlor employee	0.122	0.328		
Teacher	0.070	0.255		
Office assistant/office helper	0.099	0.299		
Manager/assistant manager	0.064	0.245		
Sales/marketing officer	0.163	0.370		
Other occupation	0.483	0.501		

Notes: Panel A includes one observation per firm, while Panel B includes one observation per vacancy. The sample consists of adverts open to women (N = 172) and firms posting adverts that are open to women (N = 120). The variables for the numbers of total, female, and male employees are winsorized at the 99% level, which explains why the mean for the total number of employees is not exactly equal to the sum of the means for female and male employees. Industry classifications are defined as dummy variables equal to 1 if the firm belongs to that sector. The posted salary variable is continuous and reflects the salary stated in the advert. The indicator variable "Offered any benefits" is an indicator equal to 1 if the vacancy provides any benefits such as a pension or healthcare. "At most primary" is an indicator variable equal to 1 for vacancies requiring no more than a primary education, including those with no formal education requirement; "Junior secondary/matric" is an indicator variable equal to 1 for vacancies that require a minimum of junior secondary education (called "matric" in Pakistan). "Senior secondary/intermediate" is an indicator variable equal to 1 for vacancies that require a minimum of senior secondary school (called "intermediate" in Pakistan). "Tertiary education" is an indicator variable equal to 1 for vacancies that require at least a bachelor's degree or a postgraduate degree. The occupation variables are each equal to 1 if the vacancy is for the corresponding occupation.

B Platform & Transport Service

Platform registration & search processes: Table B.1 compares the processes for registering, being notified about matched jobs, and applying for jobs between Job Talash and three prominent commercial job search platforms. This shows substantial overlap in the processes for learning about matched jobs and submitting applications to these jobs. The other three platforms also include search functions that allow users to learn about jobs outside the match notification system. The main difference is that Job Talash runs on text messages and phone calls, while other platforms run on websites or phone apps, making them less accessible to users with limited education or hardware access.

Briefings about transport service: Table B.2 shows the script for the phone calls used to inform treated jobseekers about the transport service. Note that the script differs for jobseekers assigned to the women-only and mixed-gender arms. The script does not mention the randomized discount rates because jobseekers are not told about their assigned discount rate; they are only told the cost of the transport service for each job to which they match.

Sample information sent to jobseekers about specific matches: Table B.3 and Figure B.1 shows sample phone scripts and text messages used to tell jobseekers about the four possible types of matches: (1) assigned to the mixed-gender transport, (2) assigned to the women-only transport, (3) not assigned any transport because the firm is not in a transport group even though the jobseeker is in a transport group, and (4) not assigned any transport because the jobseeker is in the control group. The messages differ between the third and fourth cases because in the third case, the jobseeker has been offered transport for some other matches and hence needs to be told that there is no transport for this match, while in the fourth case the jobseeker has never heard about the transport offers.

Commuting distances and costs: Table B.4 and Figure B.2 show summary statistics at the level of the jobseeker$\times$job match for commuting distance and commuting cost. These show that commuting costs are often a large share of posted salaries.

Other experiments on the Job Talash platform: The platform has been used to run multiple other experiments. No other experiments were run with these jobseekers or firms before this study's start. Jobseekers in this study participated in only one other experiment during the study duration. That experiment randomized whether jobseekers were notified about matches using only text messages or both text messages and phone calls (Vyborny ⓡ al., 2024). We show in Appendix E.1 that all main results in this paper are robust to controlling for assignments in that experiment.

Table B.1: Registration and Job Application Processes on Job Talash and Other Job Search Platforms

Platform	Job Talash (control group)	LinkedIn	Rozee	Indeed
Registration process	Complete phone call with the platform that asks about demographics, education, work experience, and occupational preferences. No fee for registration.	Complete registration on the website that requires contact information, location, education, and occupation preferences, with the option of adding more information later. No fee for registration. Can upload CV.	Complete registration on the website that requires contact information, gender, education, work experience, and occupation preferences. No fee for registration. Can upload CV.	Complete registration on the website that requires contact information, location, and gender. Can also provide information on education, work experience, and skills or upload a CV. No fee for registration.
Notification process	Notified about jobs that match education, experience, occupational preferences. Sent by text message.	Notified about jobs that match preferred job title and location. Sent by email or in the app.	Notified about jobs that match preferred experience, salary, location and optional keywords. Sent by email.	Notified about jobs that match preferred job title, salary, location and work schedule. Sent by email or in the app.
Job application process	Phone platform and ask them to send your template CV to the jobs you're interested in. No fee to apply.	If the job allows applications via LinkedIn: submit contact information, upload CV, and for some jobs answer additional job-specific questions. Otherwise redirected to the company website. No fee to apply.	If the job allows applications via Rozee: confirm contact information is correct, upload CV or submit platform-generated CV, and for some jobs answer additional job-specific questions. Otherwise redirected to the company website. No fee to apply.	Confirm contact information is correct, upload CV or submit platform-generated CV, and for some jobs answer additional job-specific questions. No fee to apply.
Other platform notes		Largest online professional networking site in the world by number of users	Largest online job search platform in Pakistan by number of users	Largest employment website in the world by number of visitors.

Notes: This table compares the processes on Job Talash for registering, being notified about matched jobs, and applying for jobs to three other prominent job search platforms. This shows substantial overlap in the processes for learning about matched jobs and submitting applications to these jobs. The other three platforms also include search functions that allow users to learn about jobs outside the match notification system. Table is reproduced verbatim from companion paper Vyborny ⓡ al. (2024).

Table B.2: Briefing Scripts Used in Phone Calls to Explain Transport Service to Treated Participants When They Are First Assigned to Treatment (Translated From Urdu)

<table>
<tr><th>Women-only Treatment Arm</th><th>Mixed-gender Treatment Arm</th></tr>
<tr><td colspan="2">${name}, Job Talash is offering a cheap and reliable transport service.</td></tr>
<tr><td>You have been selected for this exclusive pick and drop service which is being offered to only women.</td><td>You have been selected for this exclusive pick and drop service which is being offered to both men and women.</td></tr>
<tr><td colspan="2">This pick and drop service is a Job Talash initiative. It is not linked to any other company. Our van will also be taking other people from your area to the office area. The pick and drop service will be offered only for some jobs. If it is offered with a particular job, you will receive details about this transport service via SMS. Opting for the transport service is not necessary. You will still be receiving job ads regardless of your interest in this service. Registering or not registering for this service will not affect your application for jobs. This service is only applicable to the individual receiving the SMS, not for any other family member.</td></tr>
</table>

Table B.3: Briefing Scripts Used in Phone Calls to Explain Transport and Job Location for Each Job Match (Translated From Urdu)

<table>
<tr><th>Women-only Treatment Arm, Job Treated</th><th>Mixed-gender Treatment Arm, Job Treated</th><th>All Treatment Arms, Job Not Treated</th><th>Control Arm</th></tr>
<tr><td>Job Talash is offering a pick and drop service for only women to ${firmname} for ${jobtitle} position. The transport being offered is only for women who work in firms nearby but the company may have men, women or a mix of both.</td><td>Job Talash is offering a pick and drop service for both men and women to ${firmname} for ${jobtitle} position. The transport being offered is for men and women who work in firms nearby but the company may just have men, women or a mix of both.</td><td>Job Talash pick and drop service will not be offered for ${jobtitle} position at ${firmname}.</td><td>NA</td></tr>
<tr><td colspan="2">Distance from your house to ${firm_location} is ${distance}km. Monthly charges: Rs ${price}. Rickshaw fare from your house to ${firm_location} costs Rs ${rickshaw_fare} which is more than the price Job Talash is offering.</td><td colspan="2">Distance from your house to ${firm_location} is ${distance}km. Rickshaw fare from your house to ${firm_eb} costs Rs ${rickshaw_fare}.</td></tr>
</table>

Figure B.1: Sample Text Messages Sent in each Treatment Arm to Announce Job Matches (Translated from Urdu)

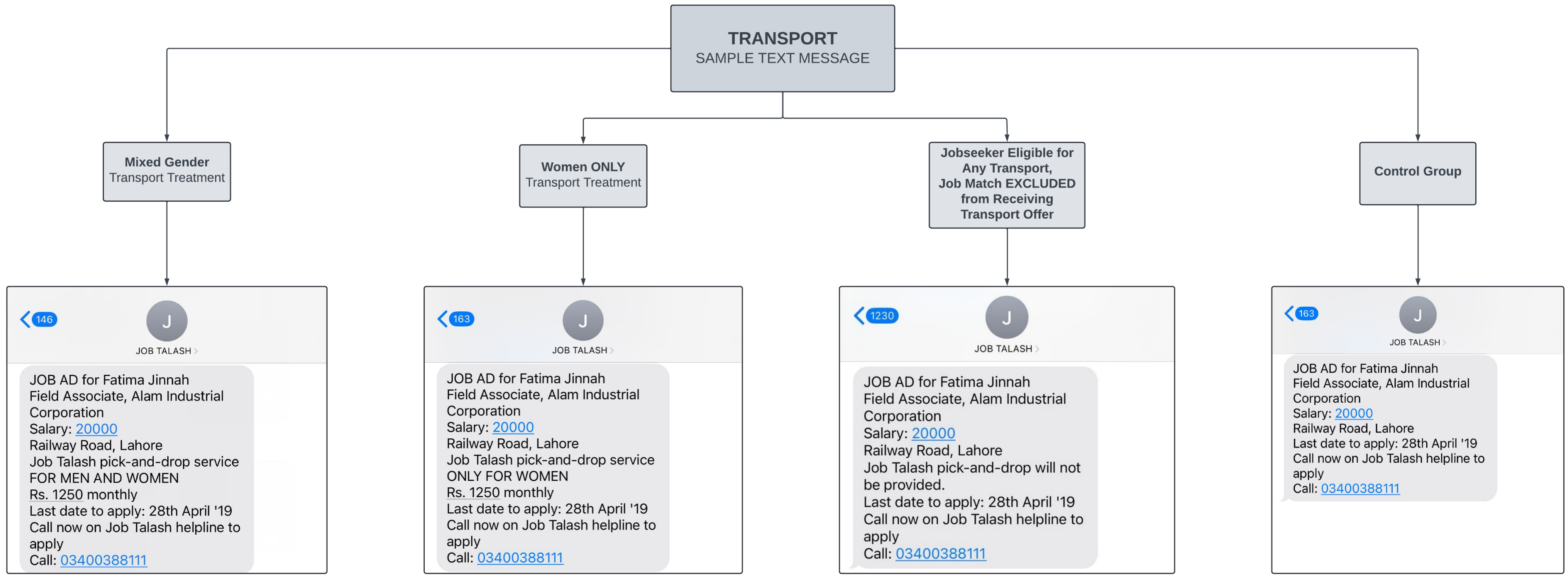


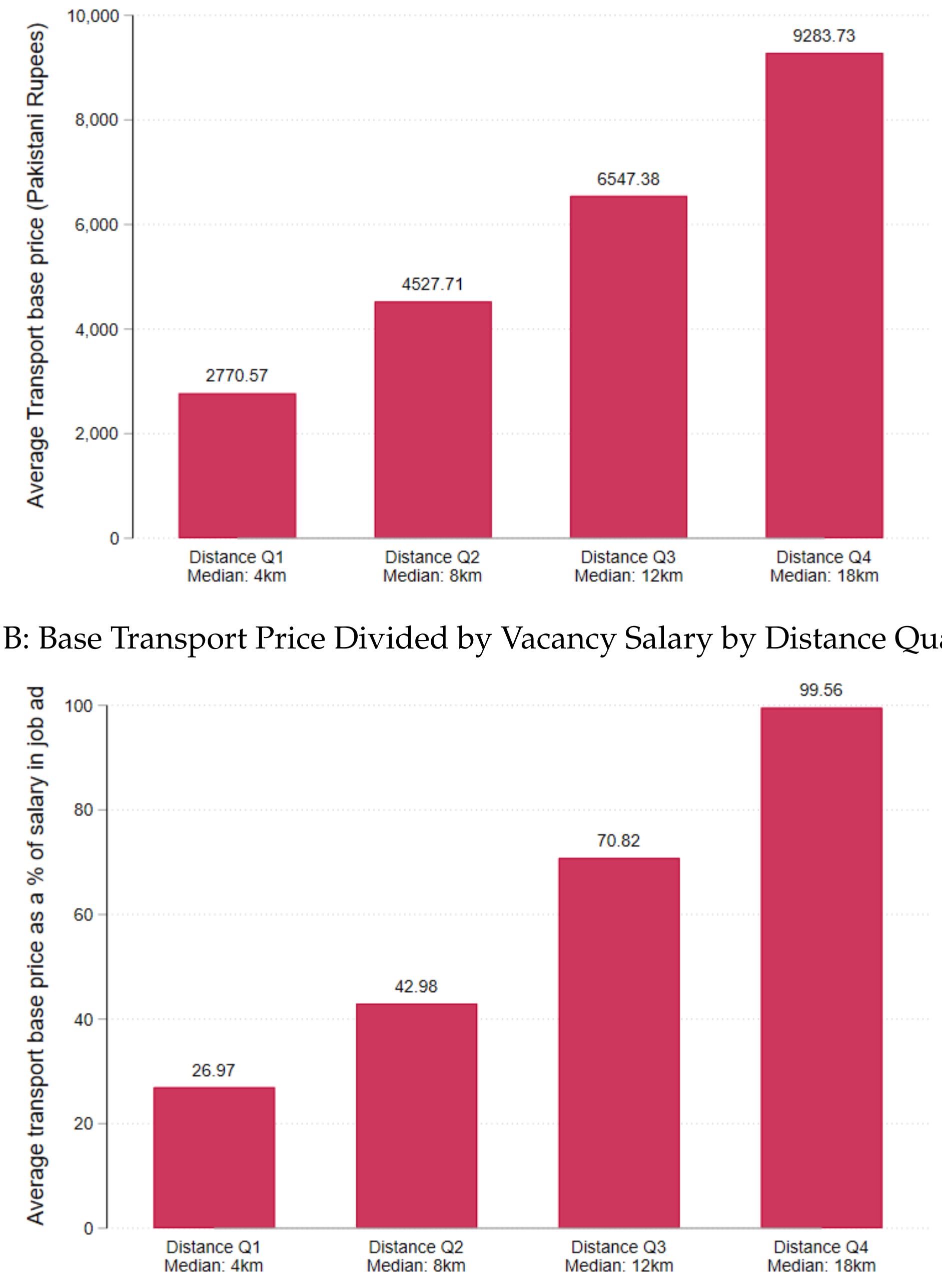
Table B.4: Match-Level Summary Statistics

	Mean	SD	p10	p90
	(1)	(2)	(3)	(4)
Commute distance	10.5	5.41	4.0	18.0
Base transport price	5,669	2,530	2,432	9,288
Base transport price as a % of posted salary	57.8	54.9	14.5	119.7

Notes: This table shows summary statistics for jobseeker $\times$ matches. 'Commute distance' is the straight-line distance in kilometres between the jobseeker's home and the job's location. 'Base transport price' is the base monthly price of the commuting service that Job Talash offers, before any randomized discounts, in Pakistani Rupees. One Rupee $\approx$ 0.03 USD in purchasing power parity at the time of the study. The third row of the table shows the base price divided by the posted salary of the job.

Figure B.2: Summary Statistics on Transport Cost

Panel A: Base Transport Price in Pakistani Rupees by Distance Quartiles



Notes: These figures show summary statistics for commuting cost at the level of the jobseeker $\times$ job match. Panel A shows the base monthly price of the transport service that Job Talash offers for commuting from the jobseeker's home to the job location in Pakistani Rupees, before any randomized discounts. Panel B shows the same measure, divided by the posted salary of the job. Data are split by quartiles of commuting distance in both panels. The number above each red bar is the mean value of commuting cost (panel A) and commuting cost divided by salary (panel B) within each quartile, while the number below each red bar is the median commuting distance within each quartile. One Rupee $\approx$ 0.03 USD in purchasing power parity at the time of the study.

C Research Design

This appendix contains additional information about the design, implementation, and analysis of the experiment.

Treatment assignments: Table C.1 shows the shares of matches (panel A) and jobseekers (panel B) assigned to the control group and each of the six possible treatments: { women-only vs mixed-gender transport } × { 20% vs 60% vs 80% discount }. The mixed-gender transport group is larger than the women-only group to facilitate the comparison of men's and women's labor supply in Section 4.5. The control group contains many more matches than the treatment groups because matches are only treated if the jobseeker *and* job are in enumeration blocks assigned to treatment, as discussed above. This also means that the share of jobseekers assigned to treatment for at least some of their matches is roughly three to four times higher than the share of treated matches.

Table C.1: Treatment Shares by Price Discount and Transport Type

	Women-only Transport Group	**Mixed-gender Transport Group**	**Control Group**
Panel A: Shares of Matches			
20% discount	0.38%	1.02%	
60% discount	1.50%	3.21%	NA
80% discount	2.96%	6.40%	
Total	4.84%	10.62%	84.54%
Panel B: Shares of Jobseekers			
20% discount	1.55%	4.07%	
60% discount	5.80%	12.63%	NA
80% discount	11.99%	25.63%	
Total	19.34%	42.33%	38.33%

Balance tests: Table C.2 shows the results from testing balance of baseline variables between the groups of women assigned to no transport offers, women-only transport, and mixed-gender transport. The results are consistent with balanced assignments: 4 out of 52 test results are statistically significant at the 10% level, and we cannot reject joint equality of all covariate means over all groups (p = 0.473). We show in Appendix E.1 that the key findings are robust to controlling for all imbalanced covariates.

Table C.2: Balance Tests

	Means			P-values			
	Control	Assigned women-only	Assigned mixed-gender	Control = assigned women-only	Control = assigned mixed-gender	Assigned women-only = assigned mixed-gender	All groups equal
Application rate	0.017	0.030	0.030	0.556	0.314	0.208	0.471
Employed	0.201	0.193	0.200	0.478	0.651	0.366	0.665
Ever worked before	0.713	0.698	0.732	0.922	0.745	0.806	0.906
Experience in years ^{P}	4.10	3.63	4.18	0.168	0.552	0.093*	0.243
Age in years ^{P}	30.1	29.5	29.6	0.618	0.319	0.773	0.598
Married ^{P}	0.604	0.582	0.557	0.801	0.098*	0.507	0.345
Married with children aged $<= 5$ ^{P}	0.321	0.325	0.304	0.115	0.921	0.345	0.354
Senior secondary/intermediate or higher ^{P}	0.491	0.554	0.564	0.211	0.088*	0.442	0.133
Have own private transport	0.145	0.154	0.128	0.802	0.055*	0.713	0.154
Low safety concerns	0.463	0.529	0.509	0.232	0.220	0.366	0.247
Knows many women working	0.287	0.270	0.334	0.791	0.247	0.121	0.343
>1 experimental participant in household	0.088	0.107	0.108	0.609	0.696	0.783	0.827
Has own phone	0.406	0.422	0.378	0.893	0.668	0.574	0.851
Joint balance over all covariates							0.473

Notes: This table shows balance tests for baseline variables. Analysis uses one observation per jobseeker and standard errors clustered by jobseeker enumeration block. Variables marked ^{P} are prespecified control variables in the treatment effects regressions. The joint balance test is implemented through a multinomial regression of the three treatment assignments on all jobseeker–level covariates with standard errors clustered by jobseeker enumeration block. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Variable definitions are: 'Application rate' is a continuous variable measuring the proportion of matches sent to a jobseeker before the experiment started that she applied to. 'Employed' is an indicator variable equal to one if the jobseeker is currently employed. 'Ever worked before' is an indicator variable equal to one if the jobseeker has ever held a job. 'Experience in years ^{P}' is a continuous variable measuring the jobseeker's total years of work experience. 'Age in years ^{P}' is a continuous variable representing the jobseeker's age in years. 'Married ^{P}' is an indicator variable equal to one if the jobseeker is married. 'Married with children aged ≤ 5 ^{P}' is an indicator variable equal to one if the jobseeker is married and has at least one child aged five or younger. 'Senior secondary/intermediate or higher ^{P}' is an indicator variable equal to one for jobseekers who have completed senior secondary school (called 'intermediate' in Pakistan) or any higher level of education. 'Have own private transport' is an indicator variable equal to one if the jobseeker owns a personal vehicle. 'Low safety concerns' is an indicator variable equal to 1 if the jobseeker has an above-median value of an inverse-covariance-weighted average of baseline self-reported perceptions of neighborhood safety, self-reported perceptions of safety for women in public spaces, and official neighborhood crime rate. 'Knows many women working' is an indicator variable equal to one if the jobseeker reports knowing many women working at baseline. '>1 experimental participant in household' is an indicator variable equal to if more than one member of the jobseeker's household participates in the experiment. 'Has own phone' is an indicator variable equal to 1 if the jobseeker has their own phone.

Treatment receipt: Table C.3 shows the shares of matches assigned to each of the six treatment arms that are offered transport. Table C.4 reports the results from a similar exercise for all of the combinations of treatments used to generate the paper's main results in Tables 2 and 3. All our analyses are just-identified and use clustered standard errors, so we follow the recommendation from Andrews et al. (2019) and use F-test statistics from Kleibergen & Paap (2006) to evaluate first stage strength. The first-stage results satisfy their criteria for 'strong' instruments in all cases.

Overall, 46% of matches assigned to transport receive transport, because it took some time to contact jobseekers and brief them about the transport service, as discussed in Section 3.2. The treatment receipt rate differs between the mixed-gender arm (51%) and the women-only arm (36%) because more call center staff time was allocated to contacting people in the mixed-gender than women-only group. The treatment receipt rate is similar for all three discount levels (46-49%). While not all matches assigned to each of the six treatment type are offered that treatment type, no match or jobseeker is ever offered a treatment type they are not assigned. No control group jobseeker or match is ever offered any form of transport, no jobseeker or match assigned to mixed-gender transport is ever offered women-only transport, and no jobseeker or match assigned to one discount level is ever offered another discount level. Throughout the paper we report average treatment effects on the treated (ATTs/ATETs) from the two-stage least squares regressions that are adjusted for differences in treatment receipt rates between treatment groups.

Table C.3: Treatment Receipt for All Six Treatment Assignments

	(1) Offered women only at 20% discount	(2) Offered women only at 60% discount	(3) Offered women only at 80% discount	(4) Offered mixed at 20% discount	(5) Offered mixed at 60% discount	(6) Offered mixed at 80% discount
Assigned women only at 20% discount	0.3786***	0.0000	-0.0000	-0.0000	-0.0000	0.0000
	(0.1169)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Assigned women only at 60% discount	0.0000	0.3596***	0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0861)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Assigned women only at 80% discount	0.0000	-0.0000	0.3524***	-0.0000	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0889)	(0.0000)	(0.0000)	(0.0000)
Assigned mixed at 20% discount	0.0000	0.0000	-0.0000	0.5344***	-0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0749)	(0.0000)	(0.0000)
Assigned mixed at 60% discount	0.0000	-0.0000	-0.0000	-0.0000	0.5175***	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0700)	(0.0000)
Assigned mixed at 80% discount	0.0000	-0.0000	0.0000	-0.0000	-0.0000	0.5091***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0650)
# observations	54237	54237	54237	54237	54237	54237
# jobseeker clusters	282	282	282	282	282	282
# firm clusters	46	46	46	46	46	46
Mean outcome | T = 0	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Controls	No	No	No	No	No	No
F-stat	10.4923	17.4550	15.7178	50.9182	54.6918	61.3111
p-value	0.0012	0.0000	0.0001	0.0000	0.0000	0.0000

Notes: This table shows the estimates from the first stage regression of assignment to each treatment type (women-only or mixed-gender) at discount rate Y% on being offered that treatment type at that discount rate. In columns (1) to (3), the coefficients on 'Assigned women only transport at Y% discount' show the estimates for being assigned women-only transport discounted by Y% from the base price. In columns (4) to (6), the coefficients on 'Assigned mixed at any Y% discount' show the estimates for being assigned mixed transport discounted by Y% from the base price. The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table C.4: Treatment Receipt for Aggregated Combinations of Treatment Assignment

	Offered Transport							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Assigned any at any price	0.4648***	0.4627***						
	(0.0588)	(0.0589)						
Assigned women only at any price			0.3567***	0.3592***				
			(0.0867)	(0.0781)				
Assigned mixed at any price			0.5141***	0.5210***				
			(0.0652)	(0.0562)				
Assigned any at 20% discount					0.4921***	0.4991***		
					(0.0674)	(0.0670)		
Assigned any at 60% discount					0.4673***	0.4710***		
					(0.0609)	(0.0622)		
Assigned any at 80% discount					0.4595***	0.4556***		
					(0.0592)	(0.0588)		
Assigned women-only at 20% discount							0.3786***	0.3902***
							(0.1173)	(0.1220)
Assigned women-only at 60% discount							0.3596***	0.3464***
							(0.0861)	(0.0964)
Assigned women-only at 80% discount							0.3524***	0.3378***
							(0.0889)	(0.0928)
Assigned mixed-gender at 20% discount							0.5344***	0.5415***
							(0.0750)	(0.0723)
Assigned mixed-gender at 60% discount							0.5175***	0.5321***
							(0.0700)	(0.0619)
Assigned mixed-gender at 80% discount							0.5091***	0.5134***
							(0.0650)	(0.0610)
# observations	54237	54237	54237	54237	54237	54237	54237	54237
# jobseeker clusters	282	282	282	282	282	282	282	282
# firm clusters	46	46	46	46	46	46	46	46
Mean outcome | T = 0	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Controls	No	Yes	No	Yes	No	Yes	No	Yes
F-stat	62.5705	61.6535	40.7046	54.1878	21.1801	21.1450	13.8648	15.1175
p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000

Notes: This table shows the estimates from the first stage regression of assignment to each specific type of transport on an indicator for being offered that specific type of transport. In columns (1) and (2), the coefficients on 'Assigned any transport at any price' show the estimates for being assigned either women-only or mixed-gender transport at any price level. In columns (3) and (4), the coefficients on 'Assigned women only at any price' show the estimates for being assigned specifically women-only transport at any price level and the coefficient on 'Assigned mixed at any price' is defined analogously for mixed-gender transport. In columns (5) and (6), the coefficients on 'Assigned any at X% discount' show the estimates of being assigned either women-only or mixed-gender transport discounted by X% from the base price. In columns (7) and (8), the coefficient on 'Assigned women-only at X% discount' show the estimates of being assigned either women-only or mixed-gender transport discounted by X% from the base price. Even-numbered columns include pre-specified covariates – age, education, years of work experience, and indicators for married, having children aged ≤ 5, and the job salary exceeding the jobseeker's self-reported reservation wage – using a flexible functional form explained in Appendix C and following Goldsmith-Pinkham et al. (2024). The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.

Estimation: We begin by estimating the average treatment effect on the treated of being offered any type of transport, by estimating the following two-stage least squares system:

$$OfferAny_{ij} = AssignAny_{ij}\alpha_1 + v_{ij} \tag{1}$$

$$Apply_{ij} = OfferAny_{ij}\beta_1 + \epsilon_{ij} \tag{2}$$

where the unit of observation is the match between jobseeker i and job j, $AssignAny_{ij}$=*1* if and only if jobseeker i and job j are both assigned any of the six treatments and =*0* otherwise, and $OfferAny_{ij}$=*1* if jobseeker i is offered any of the six treatments for job j and =*0* otherwise. The coefficient of interest is β_1 and the first-stage strength depends on α_1.

To estimate the effects of different types of transport, we replace the indicators for $AssignAny_{ij}$ and $OfferAny_{ij}$ with vectors of treatment assignments and treatment offers. For example, to estimate the effects of women-only versus mixed-gender transport, we estimate the following two-stage least squares system:

$$OfferWomen_{ij} = AssignWomen_{ij}\alpha_1 + AssignMixed_{ij}\alpha_2 + v_{ij} \tag{3}$$

$$OfferMixed_{ij} = AssignWomen_{ij}\gamma_1 + AssignMixed_{ij}\gamma_2 + v_{ij} \tag{4}$$

$$Apply_{ij} = OfferWomen_{ij}\beta_1 + OfferMixed_{ij}\beta_2 + \epsilon_{ij} \tag{5}$$

The coefficients of interest are β_1 and β_2 and the first-stage strength depends on α_1 and γ_2. $\alpha_2 = \gamma_1 = 0$ by design, because no match is ever offered a treatment that it is not assigned.

We also estimate these systems with prespecified covariates. Goldsmith-Pinkham et al. (2024) show that in regressions with multiple treatment variables, simply including covariates linearly can lead to 'contamination bias,' because it forces the relationship between the outcome and covariates to be identical across all treatment groups. To address this, we follow their recommendation to demean the covariates and include both the covariates and their interactions with the treatment variables. For example, equation (1) becomes

$$OfferAny_{ij} = AssignAny_{ij}\alpha_1 + \mathbf{\tilde{X}_{ij}}\alpha_2 + AssignAny_{ij}\mathbf{\tilde{X}_{ij}}\alpha_3 + v_{ij} \tag{6}$$

where $\mathbf{\tilde{X}_{ij}}$ is a vector of demeaned covariates. Using demeaned covariates in the interaction means that α_1 captures the effect of assignment on offers, without needing to take into account the values of α_3.[16] We use a similar approach when including covariates in the heterogeneous treatment effects analysis in Section 4.3 and the comparison of male and female treatment effects in Section 4.5, where we include the full set of interactions between treatment, gender, and demeaned covariates.

We use a slightly different approach to compare the relative importance of pecuniary versus safety and propriety factors in Section 4.4. Here we estimate:

$$\begin{aligned} Apply_{ij} &= OfferWomen_{ij}\theta_1 + OfferWomenDiscountRate_{ij}\theta_2 && (7) \\ &+ OfferMixed_{ij}\phi_1 + OfferMixedDiscountRate_{ij}\phi_2 + \epsilon_{ij} && (8) \end{aligned}$$

where $OfferWomen_{ij}$ is an indicator equal to one if jobseeker i was offered women-only transport for job j; $OfferWomenDiscountRate_{ij}$ is the discount rate of 20, 60, or 80% that she was offered for that job; and $OfferMixed_{ij}$ and $OfferMixedDiscountRate_{ij}$ are defined analogously. We instrument the four right-hand-side variables with the six treatment assignment indicators: { women-only vs mixed-gender transport } × { 20% vs 60% vs 80% discount }. The coefficient θ_2 shows the effect of a 1pp increase in the discount rate on women-only transport on the application probability, and ϕ_2 shows the analogous effect for mixed-gender transport. A policymaker running a mixed-gender transport service who wants to increase women's labor supply can then compare the effect of discounting prices on this service (ϕ_2 from equation (8)) to the effect of switching to women-only transport services ($\beta_1 - \beta_2$ from equation (5)). In particular, $\frac{\beta_1-\beta_2}{\phi_2}$ shows the increased discount rate that would generate the same effect on applications as switching from mixed-gender to women-only transport. We estimate all the relevant equations as a single stacked system to obtain a standard error on the ratio of coefficients.

[16] To see this, note that using demeaned covariates means that their values are equal to zero in expectation, $\mathbb{E}\left[\tilde{\mathbf{X}}_{\mathbf{ij}}|AssignAny_{ij}=1\right] = \mathbb{E}\left[\tilde{\mathbf{X}}_{\mathbf{ij}}|AssignAny_{ij}=0\right] = 0$. Hence all terms involving covariates are equal to zero in expectation, including the interaction terms, $\mathbb{E}\left[\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_2 + AssignAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_3|AssignAny_{ij}=1\right] = \mathbb{E}\left[\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_2 + AssignAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_3|AssignAny_{ij}=0\right] = 0$, and hence the treatment effect of $AssignAny$ on $OfferAny$ is simply $\mathbb{E}\left[OfferAny_{ij}|AssignAny_{ij}=1\right] - \mathbb{E}\left[OfferAny_{ij}|AssignAny_{ij}=0\right] = \alpha_1$. The same argument applies to the second stage regression.

D Pre-analysis Plan

The trial was preregistered at https://www.socialscienceregistry.org/trials/2410. The trial registry includes two pre-analysis plans. The first covers an initial pilot and the second covers the full trial. We follow the second pre-analysis plan with the following four exceptions.[17]

1. We preregistered job applications, interviews, hires, and transport use as outcomes. We focus only on job application in this paper. We do not have sufficient statistical power to study the downstream outcomes because applications were rarer than expected, leading to fewer interviews etc.., and because operational challenges around the COVID pandemic limited the duration of the experiment.

2. We prespecified that we would use a post double selection LASSO to include covariates in the main treatment effects analysis, following Belloni et al. (2014). However, using LASSO with the covariate-interacted specification produced unstable results, in line with recent findings by Cilliers et al. (2024). We therefore instead use the six covariates that we prespecified in the pilot pre-analysis plan: age, education, years of work experience, and indicators for married, having children aged ≤ 5, and the job salary exceeding the jobseeker's self-reported reservation wage. In practice, this change is innocuous because the main results are almost identical with and without covariates (Tables 2 and 3).

3. The experiment was meant to include an additional treatment arm: cash transfers that were labeled for commuting but could be spent on anything. This was designed to separately identify the price and income effects of the transport subsidies. However, this treatment arm was delayed and not implemented in time.

4. The following analyses that we include in the paper in response to common questions were not prespecified:

    (a) Treatment effects on job applications at the level of the jobseeker $\times$ matching round and jobseeker (Tables E.3 and E.4).

    (b) Heterogeneous treatment effects by age were not prespecified (Figure 1). We prespecified heterogeneous treatment effects by two safety proxies – self-reported

[17] We also clarify one point from the pre-analysis plan. The trial included two control groups. Both are randomly assigned at the enumeration block level, and both have identical experimental protocols, but they vary in their geographic distance to the treated enumeration blocks. We follow the pre-analysis plan in pooling these two control groups throughout the analysis.

perceptions of safety of women in public and in their own neighborhood – but we combined them into an inverse-covariance-weighted average with official neighborhood-level crime data, a measure that we only obtained after preregistration. The other six dimensions used in the heterogeneous treatment effects analysis are all prespecified.

(c) The specific approach used in the back-of-the-envelope comparison of the relative importance of pecuniary factors versus safety and propriety for women's labor supply (Section 4.4).

(d) Intrahousehold spillovers analysis (Table E.8).

E Additional Results

E.1 Robustness Checks

Table E.1: Treatment Effects of Offering Any Type of Transport on Job Applications – Robustness Checks

	Apply											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Offered any at any price	0.0080**	0.0076**	0.0090**	0.0085**	0.0076**	0.0072**	0.0077*	0.0078*	0.0082**	0.0077**	0.0071**	0.0073**
	(0.0038)	(0.0035)	(0.0044)	(0.0041)	(0.0036)	(0.0033)	(0.0040)	(0.0039)	(0.0038)	(0.0035)	(0.0035)	(0.0030)
# matches	54237	54237	43514	43514	54237	54237	54237	54237	54145	54145	54237	54237
# jobseekers	2653	2653	2645	2645	2653	2653	2653	2653	2649	2649	2653	2653
# jobseeker clusters	282	282	282	282	282	282	282	282	281	281	282	282
# firm clusters	46	46	39	39	46	46	46	46	46	46	46	46
Mean outcome | T = 0	0.0111	0.0111	0.0102	0.0102	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111
First-stage strength: F-stat	62.57	61.66	48.74	49.06	62.47	61.55	65.56	64.09	62.42	61.46	63.04	62.05
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Omit firms offering own transport	No	No	Yes	Yes	No	No	No	No	No	No	No	No
Phone call treatment FE	No	No	No	No	Yes	Yes	No	No	No	No	No	No
Time FE	No	No	No	No	No	No	Yes	Yes	No	No	No	No
Geography FE	No	No	No	No	No	No	No	No	Yes	Yes	No	No
Additional controls	No	No	No	No	No	No	No	No	No	No	Yes	Yes

Notes: This table shows the effects of offering any type of transport on an indicator for applying using different control variables and sample definitions. Columns (1) and (2) replicate the main results shown in Table 2. The sample for columns (3) and (4) is restricted to firms that did not offer a transport service at baseline. Columns (5) and (6) add controls for treatment assignments in another experiment conducted with this sample, described in Appendix B. Columns (7) and (8) add controls for the four quarters over which the year-long experiment ran, to account for time trends. Columns (9) and (10) add controls jobseeker neighborhood, to account for local economic conditions. These regressions have a slightly smaller sample size because one enumeration block was located farther away than the others and hence cannot be grouped with any of the other neighborhood fixed effects. Columns (11) and (12) include controls that are imbalanced at baseline, listed in Table C.2. In all columns, the offer variables are instrumented by assignment variables, as explained in Section 3.4. Even-numbered columns include pre-specified covariates – age, education, years of work experience, and indicators for married, having children aged ≤ 5, and the job salary exceeding the jobseeker's self-reported reservation wage – using a flexible functional form explained in Appendix C and following Goldsmith-Pinkham et al. (2024). The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.

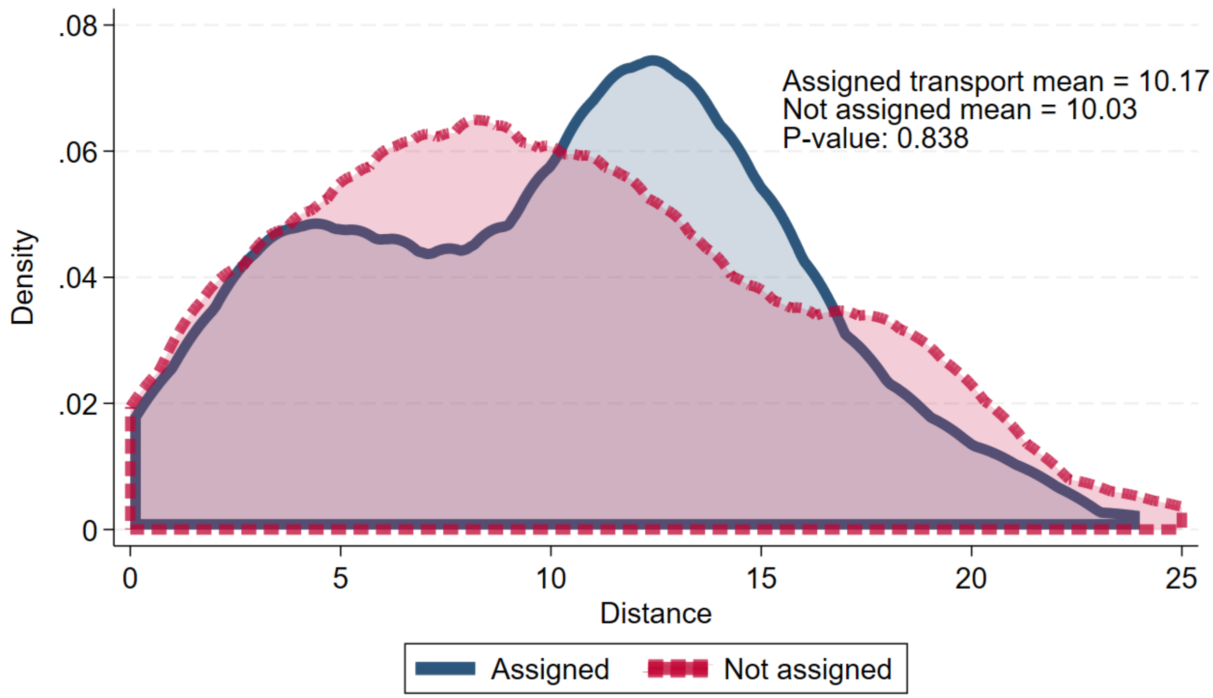
Table E.2: Treatment Effects of Offering Different Types of Transport on Job Applications – Robustness Checks

	Apply											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Offered women only at any price	0.0181**	0.0163*	0.0158	0.0145	0.0166*	0.0145	0.0193**	0.0185**	0.0181*	0.0162	0.0167**	0.0144
	(0.0090)	(0.0097)	(0.0136)	(0.0143)	(0.0087)	(0.0094)	(0.0086)	(0.0091)	(0.0091)	(0.0098)	(0.0084)	(0.0090)
Offered mixed at any price	0.0048	0.0048	0.0077	0.0074*	0.0048	0.0049	0.0040	0.0044	0.0051	0.0050	0.0040	0.0052
	(0.0048)	(0.0044)	(0.0049)	(0.0044)	(0.0045)	(0.0041)	(0.0050)	(0.0048)	(0.0047)	(0.0043)	(0.0044)	(0.0040)
# matches	54237	54237	43514	43514	54237	54237	54237	54237	54145	54145	54237	54237
# jobseekers	2653	2653	2645	2645	2653	2653	2653	2653	2649	2649	2653	2653
# jobseeker clusters	282	282	282	282	282	282	282	282	281	281	282	282
# firm clusters	46	46	39	39	46	46	46	46	46	46	46	46
Mean outcome | T = 0	0.0111	0.0111	0.0102	0.0102	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111	0.0111
First-stage strength: F-stat	40.70	54.19	34.11	45.76	40.65	54.13	42.15	55.32	40.61	54.12	40.91	54.31
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Equality of treatments: p-value	0.2403	0.3208	0.5981	0.6467	0.2872	0.3910	0.1705	0.2007	0.2566	0.3403	0.2265	0.3875
Omit firms offering own transport	No	No	Yes	Yes	No	No	No	No	No	No	No	No
Phone call treatment FE	No	No	No	No	Yes	Yes	No	No	No	No	No	No
Time FE	No	No	No	No	No	No	Yes	Yes	No	No	No	No
Geography FE	No	No	No	No	No	No	No	No	Yes	Yes	No	No
Additional controls	No	No	No	No	No	No	No	No	No	No	Yes	Yes

Notes: This table shows the effects of offering either women-only transport (row 1) or mixed-gender transport (row 2) on an indicator for applying using different control variables and sample definitions. See the footnote to Table E.1 for all details of the analysis in this table.

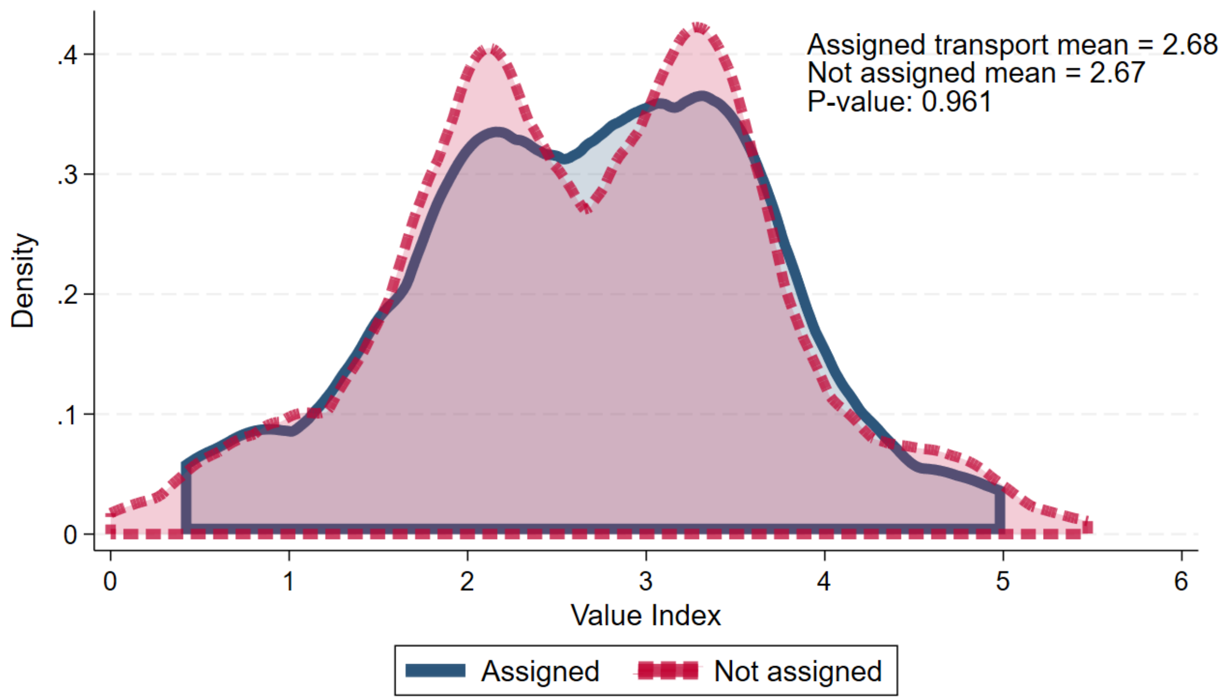
E.2 Characteristics of Matches Receiving Treated Versus Control Applications

Figure E.1: Distances from Jobseeker Home to Firm Location for Matches Receiving Job Applications in the Assigned Treatment Versus Control Groups



Notes: This figure assesses if treatment-induced applications go to jobs with longer commutes. To do this, the figure shows the density of the distance from jobseekers' homes to jobs' locations (in kilometers) of matches that receive applications, separately for matches assigned to transport and not assigned to transport. The p-value is obtained by regressing distance on an 'assigned transport' indicator within the subset of matches that receive applications, with standard errors two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. The p-value has a descriptive but not causal interpretation, as this analysis uses the selected sample of matches that receive applications.

Figure E.2: Job & Match Quality for Matches Receiving Job Applications in the Assigned Treatment Versus Control Groups



Notes: This figure assesses if treatment-induced applications go to jobs with different levels of valuable amenities / attributes. To do this, the figure shows the density of an index of job / match value for matches that receive applications, separately for matches assigned to transport and not assigned to transport. The p-value is obtained by regressing the value index on an 'assigned transport' dummy within the subset of matches that receive applications, with standard errors two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. The p-value has a descriptive but not causal interpretation, as this analysis uses the selected sample of matches that receive applications. The index is an inverse covariance-weighted average (following Anderson 2008) of seven variables capturing valuable, non-transport-related features of the job or jobseeker × job match: (1) the log of the offered salary, (2) whether any non-salary benefit is offered, such as pension payments, (3) whether the match is relevant to the jobseeker's education, (4) whether the match is relevant to the jobseeker's sector-specific experience, (5) whether flexible working hours are allowed, (6) whether the position involves short working hours, and (7) whether the posted salary exceeds the jobseeker's stated expected salary.

E.3 Treatment Effects Accounting for Substitution Between Matches

Recall from Section 3.3 that treatment is randomized for both jobseekers and firms, and that transport is only offered for matches where both the jobseeker and firm are treated. Hence treated jobseekers are assigned to receive transport offers for a random subsample of their matches. This allows us to test if offering jobseekers transport to some of their matches leads to substitution of applications away from untreated to treated matches, leaving the total number of applications unchanged. Table E.3 uses data aggregated to the jobseeker $\times$ matching round level to show that this pattern does not hold. Within a set of job matches sent simultaneously in the same matching round, a jobseeker offered transport for any one of their matches is 0.88pp more likely to apply to at least one match (column 1) and a jobseeker offered treatment for 1pp more of their matches applies to 0.01pp more of their matches (column 4). Aggregating data to the jobseeker level produces larger point estimates – respectively 1.62pp and 0.04pp – but the estimates are not statistically significant at conventional levels because the aggregation to jobseeker level reduces statistical power (Table E.4).

Table E.3: Treatment Effects at Jobseeker $\times$ Matching Round Level

	Any Applications		Prop. Applications	
	(1)	(2)	(3)	(4)
Offered any transport	0.0088*		0.0030	
	(0.0050)		(0.0040)	
Prop. offered any transport		0.0134**		0.0108**
		(0.0056)		(0.0050)
# observations	29495	29495	29495	29495
# jobseeker clusters	282	282	282	282
Mean outcome | T = 0	0.0172	0.0172	0.0146	0.0146
Controls	No	No	No	No

Notes: This table shows the effects of offering any type transport on job applications using data aggregated to the jobseeker $\times$ matching round level to account for possible substitution between treated and untreated matches that are simultaneously sent within a matching round. The outcomes are an indicator for applying to any matches in columns (1) and (2) and the share of matches applied to in columns (3) and (4). The right-hand side variables are an indicator for being offered transport for any match in columns (1) and (3) and the share of matches for which transport was offered in columns (2) and (4), instrumented by respectively an indicator for being assigned transport for match and the share of matches for which transport was assigned. The unit of observation is the jobseeker $\times$ matching round and the sample covers all female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table E.4: Treatment Effects at Jobseeker Level

	Any Applications		Prop. Applications	
	(1)	(2)	(3)	(4)
Offered any transport	0.0162		0.0005	
	(0.0187)		(0.0035)	
Prop. offered any transport		0.0932		0.0385
		(0.0905)		(0.0301)
# observations	2653	2653	2653	2653
# jobseeker clusters	282	282	282	282
Mean outcome | T = 0	0.1181	0.1181	0.0150	0.0150
Controls	No	No	No	No

Notes: This table shows the effects of offering any type transport on job applications using data aggregated to the jobseeker level to account for possible substitution between treated and untreated matches over the entire experiment. The outcomes are an indicator for applying to any matches in columns (1) and (2) and the share of matches applied to in columns (3) and (4). The right-hand side variables are an indicator for being offered transport for any match in columns (1) and (3) and the share of matches for which transport was offered in columns (2) and (4), instrumented by respectively an indicator for being assigned transport for match and the share of matches for which transport was assigned. The unit of observation is the jobseeker and the sample covers all female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.

E.4 Heterogeneous Treatment Effects

This appendix reports estimates of largely prespecified heterogeneity analysis. We report heterogeneous treatment effects over eight dimensions:

1. “Unmarried" = 1 if the jobseeker was unmarried at baseline and = 0 otherwise.

2. “Low safety concerns" = 1 if the jobseeker has an above-median value of an inverse-covariance-weighted average of baseline self-reported perceptions of neighborhood safety, self-reported perceptions of safety for women in public spaces, and official neighborhood crime rate and = 0 otherwise.

3. “High salary vacancy" = 1 if the job salary is above the sample median and = 0 otherwise.

4. “Has lower secondary certificate" = 1 if the jobseeker had completed at least junior secondary school at baseline and = 0 otherwise.

5. “Age below median" = 1 if the jobseeker’s age is below the median and = 0 otherwise. (Not prespecified but added in response to common questions.)

6. “Knows many women working" = 1 if the jobseeker reports knowing many women working at baseline and = 0 otherwise.

7. “In labor force" = 1 if the jobseeker was employed or was searching for a job at baseline and = 0 otherwise.

8. “Lives far from vacancy" = 1 if the straight-line distance from the jobseeker’s home to the job location is higher than the sample median and = 0 otherwise.

We estimate heterogeneous treatment effects for each binary heterogeneity dimension H_{ij} using:

$$\begin{aligned}
OfferAny_{ij} &= H_{ij}AssignAny_{ij}\alpha_{11} + H_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_{12} + H_{ij}AssignAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_{31} \\
&\quad + (1-H_{ij})AssignAny_{ij}\alpha_{10} + (1-H_{ij})\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_{20} + (1-H_{ij})AssignAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\alpha_{30} \qquad (9) \\
&\quad + H_{ij}\alpha_4 + v_{ij} \\
Apply_{ij} &= H_{ij}OfferAny_{ij}\beta_{11} + H_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\beta_{12} + H_{ij}OfferAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\beta_{31} \\
&\quad + (1-H_{ij})OfferAny_{ij}\beta_{10} + (1-H_{ij})\tilde{\mathbf{X}}_{\mathbf{ij}}\beta_{20} + (1-H_{ij})OfferAny_{ij}\tilde{\mathbf{X}}_{\mathbf{ij}}\beta_{30} \qquad (10) \\
&\quad + H_{ij}\beta_4 + \epsilon_{ij}
\end{aligned}$$

The coefficients of interest are the average treatment effects on treated matches in the two subsamples defined by $H_{ij} \in \{0, 1\}$, β_{10} and β_{11}, which we report in Table E.5. We use demeaned versions of all other covariates, $\tilde{\mathbf{X}}_{\mathbf{ij}}$, so that we can ignore the coefficients β_{30} and β_{31} when calculating the average treatment effects on the treated. See page 41 for a detailed explanation of this approach.

We also estimate average treatment effects on the treated matches of being offered mixed-gender and women-only transport, and report these in Tables E.6 and E.7 respectively. To estimate these, we replace the *AssignAny* and *OfferAny* variables in equation (9) and (10) with assignments and offers for women-only and mixed-gender transport, and include first-stage equations for each of $OfferWomenOnly_{ij}$ and $OfferMixedGender_{ij}$.

For each of the three types of transport offers, and each of the eight dimensions of heterogeneity, we test if treatment effects are equal between the two subsamples defined by the heterogeneity dimension. We report p-values for these tests in each of Tables E.5, E.6, and E.7 and account for multiple hypothesis testing by reporting sharpened q-values that control the false discovery rate over the eight different heterogeneity dimensions (Benjamini et al., 2006).

Results are as discussed in Section 4.3: the effects of mixed-gender transport are larger for unmarried women, women with low safety concerns, and vacancies with above-median salaries. Other than those three dimensions, effects of women-only transport are larger than effects of mixed-gender transport in all subsamples, though the differences are not generally statistically significant. And the effects of all three types of transport are similar for baseline labor force participants and non-participants, highlighting that labor supply may be relatively elastic for non-participants in this setting.

Table E.5: Heterogeneous Treatment Effects of Being Offered Any Form of Transport

	Apply							
	Unmarried?	Low safety concerns?	High salary vacancy?	Has lower secondary certificate?	Age below median?	Knows many women working?	In labor force?	Lives far from vacancy?
Treatment effect when condition = No	0.0026	0.0004	0.0019	-0.0009	0.0032	0.0070	0.0096*	0.0017
	(0.0041)	(0.0036)	(0.0031)	(0.0038)	(0.0051)	(0.0049)	(0.0057)	(0.0044)
Treatment effect when condition = Yes	0.0127**	0.0154***	0.0185***	0.0142***	0.0119**	0.0142**	0.0057	0.0139**
	(0.0056)	(0.0049)	(0.0055)	(0.0052)	(0.0050)	(0.0066)	(0.0047)	(0.0059)
Equality of treatments: p-value	0.1415	0.0040	0.0088	0.0148	0.2368	0.3519	0.5852	0.1083
Equality of treatments: q-value	0.1650	0.0330	0.0330	0.0360	0.2460	0.2770	0.2930	0.1570
Mean outcome | T = 0	0.0111	0.0111	0.0112	0.0111	0.0111	0.0106	0.0119	0.0125
First-stage strength: F-stat	34.06	31.62	31.32	33.06	30.53	33.41	39.15	42.38
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
# matches	53652	54078	47654	54237	54218	47244	48474	45311
# jobseekers	2615	2644	2653	2653	2651	2312	2380	2564
# jobseeker clusters	282	282	282	282	282	279	280	280
# firm clusters	46	46	41	46	46	46	46	44

Notes: This table shows the heterogeneous treatment effects of offering any type of transport on job applications using eight different dimensions of heterogeneity. Each dimension of heterogeneity is captured by a binary variable. The first two rows present the treatment effect estimates separately for the two subgroups defined by the heterogeneity variable, i.e., when the question in each column header has answer "no" versus "yes." See page 52 for details on estimation procedure and variable definitions * p<0.10, ** p<0.05, *** p<0.01.

Table E.6: Heterogeneous Treatment Effects of Being Offered Mixed-Gender Transport

	Apply							
	Unmarried?	Low safety concerns?	High salary vacancy?	Has lower secondary certificate?	Age below median?	Knows many women working?	In labor force?	Lives far from vacancy?
Treatment effect when condition = No	-0.0006	-0.0035	-0.0042	-0.0038	0.0005	0.0028	0.0053	-0.0007
	(0.0055)	(0.0040)	(0.0043)	(0.0067)	(0.0068)	(0.0053)	(0.0070)	(0.0046)
Treatment effect when condition = Yes	0.0118**	0.0149**	0.0209***	0.0124**	0.0088**	0.0126	0.0045	0.0128
	(0.00479)	(0.00663)	(0.00642)	(0.00555)	(0.00411)	(0.00886)	(0.00516)	(0.00817)
Equality of treatments: p-value	0.0460	0.0042	0.0009	0.0627	0.2656	0.2941	0.9123	0.1346
Equality of treatments: q-value	0.1020	0.0160	0.0080	0.1040	0.2020	0.2020	0.5070	0.1440
Mean outcome | T = 0	0.0114	0.0114	0.0116	0.0114	0.0114	0.0110	0.0123	0.0128
First-stage strength: F-stat	29.44	28.26	32.77	29.19	35.97	25.68	30.73	28.08
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
# matches	53652	54078	47654	54237	54218	47244	48474	45311
# jobseekers	2615	2644	2653	2653	2651	2312	2380	2564
# jobseeker clusters	282	282	282	282	282	279	280	280
# firm clusters	46	46	41	46	46	46	46	44

Notes: This table shows the heterogeneous treatment effects of mixed-gender transport on job applications using eight different dimensions of heterogeneity. Each dimension of heterogeneity is captured by a binary variable. The first two rows present the treatment effect estimates separately for the two subgroups defined by the heterogeneity variable, i.e., when the question in each column header has answer "no" versus "yes." See page 52 for details on estimation procedure and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

Table E.7: Heterogeneous Treatment Effects of Being Offered Women-Only Transport

	Apply							
	Unmarried?	Low safety concerns?	High salary vacancy?	Has lower secondary certificate?	Age below median?	Knows many women working?	In labor force?	Lives far from vacancy?
Treatment effect when condition= No	0.0145	0.0176	0.0257**	0.0013	0.0129	0.0175	0.0149	0.0114
	(0.0132)	(0.0136)	(0.0120)	(0.0096)	(0.0112)	(0.0143)	(0.0123)	(0.0099)
Treatment effect when condition = Yes	0.0141	0.0170	0.0176	0.0239*	0.0213	0.0212***	0.0177	0.0234
	(0.0170)	(0.0115)	(0.0155)	(0.0141)	(0.0161)	(0.0069)	(0.0153)	(0.0156)
Equality of treatments: p-value	0.9891	0.9681	0.6154	0.1886	0.6766	0.8008	0.8812	0.5381
Equality of treatments: q-value	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000	1.0000
Mean outcome | T = 0	0.0114	0.0114	0.0115	0.0114	0.0114	0.0110	0.0122	0.0128
First-stage strength: F-stat	29.44	28.26	32.77	29.19	35.97	25.68	30.73	28.08
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
# matches	53652	54078	47654	54237	54218	47244	48474	45311
# jobseekers	2615	2644	2653	2653	2651	2312	2380	2564
# jobseeker clusters	282	282	282	282	282	279	280	280
# firm clusters	46	46	41	46	46	46	46	44

Notes: This table shows the heterogeneous treatment effects of women-only transport on job applications using eight different dimensions of heterogeneity. Each dimension of heterogeneity is captured by a binary variable. The first two rows present the treatment effect estimates separately for the two subgroups defined by the heterogeneity variable, i.e., when the question in each column header has answer "no" versus "yes." See page 52 for details on estimation procedure and variable definitions. * p<0.10, ** p<0.05, *** p<0.01.

E.5 Intrahousehold Spillover Effects

Including both women and men in the experiment raises the possibility that treatment effects will spill over between household members. Both positive and negative spillovers through personal networks on job search have been found in existing work, within both households and other family or social networks (Afridi et al., 2023; Caria et al., 2018; Donald & Grosset-Touba, 2025; Kapoor & Gade, 2024).

We evaluate the possibility that our main findings might be affected by intrahousehold spillovers in two ways. First, we restrict attention to the roughly 90% of women in our sample who do not co-reside with any other participant in the experiment and hence cannot experience intrahousehold spillover effects from other household members being treated. The effect of transport offers on their job applications is only slightly lower than for the full sample, with or without including controls (Table E.8, columns 1–4). We do not interpret the slightly lower effect as evidence of spillovers because the difference is small and because women who do and do not co-reside with other participants differ in other observed characteristics. Instead, we simply interpret this as evidence that the main finding still holds in a sample where intrahousehold spillovers on job applications are unlikely.

Second, we directly estimate intrahousehold spillovers using within-household variation in treatment exposure. Assignment to control versus transport is assigned at the enumeraction block level so it does not vary at the *jobseeker* level within household. But treatment assignment does vary at the *match* level within household due to the firm-side randomization. Using this variation, we find that women's job application decisions are not influenced by whether their co-residents have been offered transport, instrumented by whether their co-residents were assigned transport (columns 5–6). However, an important caveat is that our ability to detect spillovers is limited given that only 10% of female jobseekers on the platform live in a household where another person also registers for the platform. While this limits our ability to detect spillovers econometrically, it also means that our main findings are unlikely to be affected by spillovers.

We cannot estimate intrahousehold spillover effects on applications to a specific job when multiple household members match to that job, both because these cases are very rare and because there is no intrahousehold variation in treatment status within a jobseeker $\times$ job match.

Table E.8: Within-Household Spillover Effects of Transport Offers on Job Applications

	Apply					
	(1)	(2)	(3)	(4)	(5)	(6)
Offered any at any price	0.0080**	0.0076**	0.0068*	0.0064*	0.0172*	0.0167**
	(0.0038)	(0.0035)	(0.0036)	(0.0036)	(0.0104)	(0.0080)
Offered to other HH member ever before					-0.0028	-0.0043
					(0.0074)	(0.0059)
# matches	54237	54237	48106	48106	6131	6131
# jobseekers	2653	2653	2387	2387	266	266
# jobseeker clusters	282	282	281	281	94	94
# firm clusters	46	46	46	46	46	46
Mean outcome | T = 0	0.0111	0.0111	0.0110	0.0110	0.0115	0.0115
First-stage strength: F-stat	62.57	61.66	66.70	65.35	27.24	32.17
First-stage strength: p-value	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Controls	No	Yes	No	Yes	No	Yes

Notes: This table shows the effects of each treatment on an indicator for application. In columns (1) and (2), the coefficient on 'Offered any transport at any price' shows the effect of being offered either women-only or mixed-gender transport at any price level. Columns (3) and (4) show the same treatment effect excluding the subsample of women living in households with at least one other Job Talash subscriber. In columns (5) and (6), the coefficient on 'Offered to other household member ever before' shows the effect of any other household member being offered women-only or mixed-gender transport at any price level in any previous round. These two columns are restricted to the subsample of women living in households with at least one other Job Talash subscriber. In columns (1) to (4), the indicator for being offered treatment is instrumented by an indicator for being assigned to treatment. In columns (5) and (6), the indicator for 'Offered any transport at any price' is instrumented by an indicator for being assigned to any treatment, and the indicator for 'Offered to other household member ever before' is instrumented by the proportion of job matches sent to other household members in all previous rounds that were assigned to any treatment. Even-numbered columns include pre-specified covariates – age, education, years of work experience, and indicators for married, having children aged ≤ 5, and the job salary exceeding the jobseeker's self-reported reservation wage – using a flexible functional form explained in Appendix C and following Goldsmith-Pinkham et al. (2024). The unit of observation is the jobseeker $\times$ job match and the sample is all matches for female jobseekers during the duration of the experiment. Standard errors shown in parentheses are two-way clustered by jobseeker enumeration block and job enumeration block, the units at which treatment was assigned. * p<0.10, ** p<0.05, *** p<0.01.