# BRINGING WORK HOME:

## FLEXIBLE WORK ARRANGEMENTS AS GATEWAY JOBS FOR WOMEN IN WEST BENGAL *

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**Abstract**

There is a large latent workforce in developing countries that consists of hundreds of millions of women who prefer to have paid work and yet are out of the labor force. Often, available job opportunities are incompatible with traditional gender roles that encourage women to stay at home. In a randomized experiment with 1,670 households, we partner with a jobs platform to offer short-term data work to women who are out of the labor force. We find three main results. First, flexible work-from-home jobs are highly effective at bringing women into paid work. Job flexibility more than triples take up from 15% for an office job to 48% for a job that women can do from home while multitasking with childcare. Second, these jobs can act as a stepping stone to less flexible work. Trying paid work from home increases take up of less flexible jobs two to three months later by 5 percentage points. "Gateway jobs" are especially important for women from more traditional households: their labor supply is more likely to be marginal to flexibility, and in turn, work experience shifts their attitudes to become less traditional. Third, from the labor demand side, remote work comes with trade-offs in terms of worker performance, causing a 4% decrease in accuracy and a 20% decrease in speed. However, these performance drawbacks may be outweighed by the increase in available workers associated with remote work.

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# 1 Introduction

There is a large latent workforce in developing countries that consists of women who would prefer to work for pay and yet are out of the labor force. Representative surveys estimate this latent labor force numbers in the hundreds of millions of women, largely concentrated in South Asia, the Middle East, and North Africa.[1] This latent workforce has high-stakes consequences for both the well-being of women and girls as well as aggregate efficiency. In addition to better respecting individual preferences, increasing the share of women in the labor force could improve outcomes such as agency, health, and educational attainment for women and girls (as shown in papers such as Duflo, 2003; Jensen, 2010; Afridi et al., 2016) and lead to a more efficient allocation of men's and women's labor that could spur economic development (Hsieh et al., 2019; Ashraf et al., 2024). This misallocation and, consequently, potential gains are particularly pronounced in countries such as India where levels of female employment are low despite advancements in women's education.

In these countries, social norms are thought to be a key barrier to women's paid work (Jayachandran, 2021). One widespread social norm in our context is that a woman's place is in the home. In India, many women spend limited time outside the home after marriage: the median working-age married woman reports spending just 0.5 hours per day outside her home, with 45% reporting not leaving the home at all on an ordinary day (Andrew and Smurra, 2024). This norm is in conflict with the types of jobs which are available: fewer than 20% of jobs in India are fully remote.[2] This mismatch between jobs that are available and jobs that women could do without violating norms of appropriate behavior for women suggests two potential strategies: (1) change *norms* so that existing jobs become more acceptable for women, or (2) change *jobs* to be more compatible with existing norms. In this project, we start by taking the second approach: change

[1] This estimate is based on a 2016 poll conducted by the International Labour Organization and Gallup in 142 countries. In India alone, the estimated number of women who prefer to work for pay but are out of the labor force is 166 million. Agte and Bernhardt (2024) estimate that more than 100 million women in India are disallowed from working.

[2] As far as we know, there is no representative survey of India's workforce that would provide a reliable estimate of the share of the labor force that works remotely. However, The Economic Times reported that 20% of new job postings in summer 2024 were for remote or hybrid work, which is likely higher than the share of all new job openings which allow for remote or hybrid arrangements. These postings are more common for openings in relatively high-skilled, high-paying employment, where remote work is more widespread (Dingel and Neiman, 2020; Zarate et al., 2024).

jobs so that women can do them while incurring lower norms-related costs. However, the two levers may be mutually reinforcing. If a woman's earned income and actual labor supply affect people's attitudes about women and work, then an approach that starts by changing jobs might also in turn change gender norms.

In a randomized field experiment in West Bengal, we test the effects of offering an emerging type of work — digital gig work — that is relatively compatible with existing norms of women's behavior due to the ability to work from home at flexible hours.[3] Our experiment is designed to speak to three main research questions. First, would offering at-home, flexible work arrangements increase female labor force participation, and if so, which dimensions of flexibility are important? Second, given that many existing jobs require in-person attendance, can women who are initially only able to work from home use flexible jobs as a stepping stone to jobs with less flexible work arrangements? Third, to understand whether employers have an incentive to introduce remote working arrangements, what are the effects of work-from-home on job performance, both for inframarginal workers, and due to any change in the composition of workers drawn to the firm?

We randomly assign women from 1,670 lower-middle-income households to a treatment group that receives a job offer of month-long digital gig work or to a control group that receives no job offer. Among those receiving job offers, we introduce variation along three dimensions of job flexibility: (1) the ability to choose one's work hours each day, (2) the ability to combine work with childcare, and (3) the ability to work from home. All jobs are part-time, last for one month, and are offered in partnership with Karya, a smartphone-based data tasks platform in India.[4] To separately estimate the effects of work-from-home on job performance versus worker composition,

[3] Jobs fitting this description have become more common, accelerated by the COVID-19 pandemic (Goldin, 2021). In the United States, remote work has increased five-fold since 2019, and online gig work opportunities are increasing in developing countries (Barrero et al., 2023; Datta et al., 2023). Recent research suggests that these gains in flexibility are here to stay (Aksoy et al., 2022) and are having large effects in high-income countries, e.g., in the US, where labor force participation reached an all-time high for women with children under five after the pandemic (Bauer and Wang, 2023). In developing countries, where commuting and childcare infrastructure are less extensive and societal norms against working outside the home are in some cases stronger, the impact could be even more substantial.

[4] Karya is a microtasks platform that was incubated at Microsoft Research India. The platform includes a wide range of tasks in domains such as data generation and data quality assurance. In this study, the randomly assigned tasks involve data generation tasks to contribute to Bangla or Hindi speech datasets that train AI language models. Karya is not the only jobs platform of its type. We use another such platform, Rani Work by Myna Mahila Foundation, for another experiment in Mumbai (Jalota and Ho, 2024).

after participants have decided to accept or reject their job offer, we randomly select half of the participants who accepted a less flexible job to be surprised with an upgrade to the most flexible job (as in Karlan and Zinman, 2009). After the jobs are completed, we estimate the effects of work experience on outcomes including women's gender attitudes and agency, as well as spillover effects on their children. Two to three months later, we measure subsequent take-up of different work opportunities (*Jobs Round 2*), including the effects of the initial at-home work experience on take up of less flexible jobs.

Most study participants are married women with little previous work experience. To focus on the extensive margin of labor force participation, women are only eligible for the study if they are not currently in the labor force or enrolled in skills training. During study recruitment, to avoid selecting the sample based on interest in finding paid work, potential participants are not told that the baseline survey could lead to a job opportunity. This leads to a sample where 69% have never worked for pay prior to the study. However, to ensure women have the skills necessary to do the job if assigned to it, they must be literate in Hindi or Bangla and have access to an Android smartphone.[5] On average, participants are thirty years old and nearly all (93%) are married. Husbands and parents-in-law play a large role in whether or not women work: only 36% of women report having the final say in their own labor supply decisions. Two-fifths of participants live with at least one of their in-laws, and 48% have a child under the age of eight.

We find three main sets of results. First, flexible work arrangements more than triple women's job take up. Compared to a job which requires working from an office, the most flexible job we offer — which includes the ability to choose work hours flexibly, combine work with childcare, and work from home — dramatically increases job take up from 15% to 48% ($p < 0.001$). This 33 pp effect on women's job take up is larger than the effects of previously-tested interventions to increase women's labor supply. For example, a promotional video shown to women's family

[5] 79.1% of women in urban and 84.7% of women in rural West Bengal are not doing paid work (Periodic Labor Force Survey (PLFS) 2021-2022). 67.7% of urban and 48.5% of rural women in West Bengal are able to read a full sentence (National Family Health Survey (NFHS) 2019-2021). Appendix Figure A1 presents rates of female participation in paid work for the states of India, including West Bengal. There is less information available on the share of women who have access to a smartphone. However, according to NFHS 2019-21, 72.4% of urban women own a mobile phone.

members in rural Uttar Pradesh increased job take up by 78% (7 pp) (McKelway, 2024), and correcting Saudi men's second-order beliefs about women working outside the home increased job sign up rates by 36% (9 pp) (Bursztyn et al., 2020). The effect is also large compared to women's wage elasticity of labor supply; in an experiment in Mumbai, we found that increasing women's wage rates by nearly five times only resulted in one-quarter of the increase achieved in women's job take up by bundled job flexibility (Jalota and Ho, 2024).

To shed light on the mechanisms driving the effects of flexible work arrangements on women's labor supply, we randomly vary dimensions of flexibility across job offers. This allows us to separately estimate the contributions of choosing work hours, combining work with childcare, and working from home to the overall effect of flexibility on job take up. The ability to work from home, even without time flexibility or the ability to combine work with childcare, doubles job take up from 14.6% to 29.2% ($p = 0.004$). The ability to multitask work with childcare is also important, increasing job take up by approximately 60% ($p < 0.001$), from 29.2% to 45.6% (without time flexibility) and from 28.6% to 47.9% (with time flexibility). The ability to choose work hours flexibly, however, does not make a significant difference to take up — it appears these out-of-labor-force women can, and choose to, set aside consistent hours for paid work. Therefore, decomposing the 33 pp effect of bundled flexibility, approximately one half (14-15 pp) comes from the ability to work from home and the other half (16-19 pp) from the ability to multitask work with childcare.

Second, we find that flexible work arrangements are not only effective at bringing out-of-labor-force women into paid work, but they can also act as a stepping stone to less flexible jobs. To test whether flexible work can act as a *gateway job* to less flexible work, we return to all study participants two to three months after the endline survey and offer them another randomly assigned job. The offers in *Jobs Round 2* vary in flexibility along the same dimensions as the initial jobs and also introduce variation in the type of work offered. While the original jobs all consisted of online gig work, jobs in the second round also include non-digital piece-rate work (sewing masks or making jewellery) to assess whether effects on interest in work operate through digital-specific mechanisms or apply to interest in paid work more broadly. Consistent with flexible paid work acting as a stepping stone from unpaid at-home production to less flexible paid work, women are 5 pp more likely to start the job randomly assigned to them during *Jobs Round 2* if they were first given the chance to experience a more flexible job during the initial intervention ($p = 0.05$). Consistent with the initial work experience offering a low-cost opportunity to learn what it is like for women to earn income, the effect is concentrated on women who had no previous work experience before the study (+7 pp, $p = 0.03$). Examining only women randomly assigned to an office job during the second round, those assigned to a *gateway job sequence* in which their first round job was more flexible are 8 pp more likely to start work compared to the control group that received no job offer in the first round ($p = 0.04$). The transition from unpaid home production as a full-time homemaker to working outside the home might be a large leap — both for a woman herself and for her family members — and our results suggest that short-term, flexible jobs can act as a bridge for women to take multiple, more manageable steps to outside-the-home work.

One mechanism consistent with this gateway effect is a mutually reinforcing relationship between pro-work gender attitudes and actual labor supply, in which work experience shifts attitudes about appropriate behavior for women, thus expanding the set of jobs within reach. Women with more traditional attitudes at baseline had a stronger labor supply response to job flexibility, and in turn were also more likely to shift their gender attitudes in response to work experience. The impact of flexibility on job take up is 50% higher in households where women's pre-intervention gender attitudes are more traditional than the median participant, even conditional on other characteristics such as education, age, religion, previous work experience, cohabitation with parents-in-law, and having a young child ($p = 0.001$). Receiving a job offer in turn shifts women's gender attitudes to become less traditional by 0.05 SDs on average ($p = 0.04$), with the effect entirely concentrated on women who held more traditional pre-intervention attitudes (0.11 SDs, $p = 0.001$).[6]

In addition to the effects on the women participants themselves, there are also spillover ef-

[6]This heterogeneity is not driven by ceiling effects (see the histograms of the gender attitudes index values for the control and treatment groups before versus after the intervention in Appendix Figure A9).

fects on other family members' gender attitudes and participation in home production.[7] When the intervention ends, our survey team asks children about their attitudes and their family members' behaviors during the last month. The gender attitudes of children over twelve shift to become 0.1 SDs less traditional after their mother had a chance to do paid work ($p = 0.03$), with no differential effects by child gender. Treatment group children are also marginally more likely to report that their fathers contribute to home production. They are 9 pp more likely to say that their fathers helped *at least once* with cooking, cleaning, or childcare during the time period of the intervention ($p = 0.05$), as compared to the 42% of children in the control group who say their fathers *never* helped with these activities.

Turning to the labor demand side, the effects of flexibility on women's job participation show that introducing work-from-home — even temporarily — might allow employers to dramatically increase their pool of potential workers. So why do firms not offer flexible work arrangements more often? Beyond fixed costs and feasibility, employers would likely want to understand how adopting home-based arrangements would affect worker performance. Introducing remote work could affect performance in two ways: (i) by changing the performance of inframarginal workers, those who are willing to work in person but can now work from home, and (ii) by drawing a different type of worker into the firm once the job is advertised as a work-from-home job. We separately identify selection into and treatment effects of flexible work arrangements using randomly assigned surprise upgrades to participants' initial job offers (as in Karlan and Zinman, 2009). The main job performance outcomes we measure are accuracy and speed.

We find that work-from-home reduces worker performance, with the effect driven by treatment rather than selection. Workers who are willing to work from the office but are randomly assigned to work from home complete tasks 20% more slowly ($p = 0.02$) and 4% less accurately ($p = 0.004$). These effects are present for both easier and more difficult tasks, but are more pronounced for tasks that require greater cognitive load. For these more difficult tasks, work-from-home causes an

[7] In the main study, the other household member who we survey is a child aged 8-18, when available. In our pilot, we also surveyed husbands, but we found that husbands who were willing to participate in our surveys were selected to have less traditional views on average, and completion rates were low (approximately 50%).

additional 1.5 times slowdown in speed ($p = 0.04$) and an additional six times decrease in accuracy ($p < 0.001$). We do not find any significant differences on the selection margin, suggesting that *work-from-home compliers* — those whose job take up is marginal to the ability to work from home — are not systematically different in their performance from those who are willing to work from both home and office. In this context of piece-rate wages, the performance cost in speed is borne by the worker.[8] However, this negative effect on performance could be exacerbated for a firm that pays according to time worked, or in any context in which productivity is more difficult to monitor and thus there are greater incentives to shirk.

Examining how work patterns differ between treatment arms, flow effects can explain the negative effect of work-from-home on productivity. Defining a work session as a period of uninterrupted work in which fewer than ten minutes passes between consecutive tasks (following Adams-Prassl et al., 2023), work-from-home causes workers to have work sessions that are 25% shorter ($p = 0.009$). These fragmented work patterns are costly to performance because of flow effects: workers complete tasks more quickly, and more accurately, when they work for a longer stretch of time without pauses.

Our paper contributes to four literatures. An extensive body of work demonstrates that female workers value flexible work arrangements more highly than men do in high-income countries (for examples, see Filer, 1985; Goldin, 2014; Goldin and Katz, 2016; Wiswall and Zafar, 2018; Mas and Pallais, 2017). These studies focus primarily on women who are in the labor force and ask to what degree compensating differentials can explain gender wage gaps.[9] In many developing countries, however —- particularly in South Asia, the Middle East, and North Africa — the first-order issue is not the gender wage gap, but rather the gender gap in labor force participation. In our setting, labor market *entry* is the origin of the gender gap: by age 30, only 14.5% of women have ever undertaken

[8] It is less clear which party would bear the cost of decreased accuracy. On the one hand, if the employer can easily detect mistakes, then the worker could bear the cost of decreased accuracy. However, in many crowdsourced micro-task settings, employers overcome worker mistakes by assigning the same task to multiple workers and then taking the modal response over the worker pool. Under this strategy, if the accuracy rate is lower, then the employer would need to assign the task to additional workers and the cost might be partially borne by the employer.

[9] One exception in studying the effects of work-from-home on the *extensive* margin of labor force participation is Tito (2024), who finds that work-from-home helps to keep more women in the United States in the labor market.

paid work, and this accounts for 90% of women who will ever enter the labor market.[10] A natural question for these countries, then, is to what extent women's preferences for flexibility explain low labor market entry of women. In this study, we focus on women who are *not* labor market participants and show that the availability of flexible work arrangements is the deciding factor in whether or not many women begin paid work.[11] In addition, although women's greater preference for flexible work arrangements is well-documented in the literature, these preferences are taken as exogenous. In West Bengal, we show that this willingness-to-pay is malleable and endogenous to women's own labor supply: work experience and the resulting shift in gender attitudes can increase women's willingness to work in less flexible jobs.

Second, we contribute to a growing literature about the effects of flexible work arrangements on job performance (e.g., Bloom et al., 2015; Choudhury et al., 2021; Bloom et al., 2022; Choudhury et al., 2022; Aksoy et al., 2023). Many recent papers examine trends in and impacts of post-pandemic increases in remote and hybrid work (e.g., Langemeier and Tito, 2022; Coskun et al., 2024; Bagga et al., 2023; Bick et al., 2024). Consistent with most of the literature examining the effects of fully remote work, we find that work-from-home lowers average performance (Emanuel and Harrington, 2024; Gibbs et al., 2023; Atkin et al., 2023; Adams-Prassl et al., 2023). The mechanisms proposed for the reduction in performance are often related to communication (e.g., in Brucks and Levav, 2022), but we show that there may be negative impacts on performance even when workers' tasks are entirely individual. Our project differs from most previous work in our focus on effects on worker performance due to changes in the extensive margin of labor force participation—who is newly brought into paid work as a result of work-from-home jobs—as well as our focus on secondary-school-educated women in a lower-middle-income country. While other studies largely focus on incumbent workers who were willing to work in person and now choose flexible work arrangements, we aim to characterize the workers who would be brought into the

[10]These statistics are calculated using the 2021-2022 Periodic Labor Force Survey (PLFS), which is conducted by India's Ministry of Statistics and Programme Implementation.

[11]In Jalota and Ho (2024), which is for now remaining a working paper, we describe the results from a related experiment in which we compare men's and women's willingness-to-pay for the ability to work from home. We find that work-from-home does not significantly affect men's job take up, while, consistent with the effects we find in West Bengal, the ability to work from home doubles women's job take up.

labor force by an increase in flexible work arrangements in India.

Third, we add to a literature studying the effects of economic behavior on gender norms. Observational studies show that different economic conditions—such as suitability to plough versus hoe agriculture—give rise to different gendered divisions of labor, and that the resulting economic practices have an effect on gender norms in the long run (e.g., Boserup, 1970; Alesina et al., 2013; Carranza, 2014; Hansen et al., 2015; Becker, 2021). However, few experimental interventions have changed gender attitudes about women and work. There is little prior evidence that norms change when women start work, with the exception of Field et al. (2021), who find that getting access to direct deposit and training increases women's labor supply and liberalizes women's own and perceived norms. In this project, we test experimentally if changing economic conditions to make them more favorable to women's employment, by increasing the flexibility of available jobs, changes gender attitudes as well as divisions of labor in the home to become more supportive of women working for pay. Our results are promising in that women's entry into paid work appears to kickstart a multiplier effect for female employment, in which women working and less traditional gender roles mutually reinforce each other.

Finally, we contribute to an active literature on barriers to and enablers of increased female labor force participation in low- and middle-income countries. This literature includes factors such as perceived and actual gender norms (e.g., Bernhardt et al. 2018; Agte and Bernhardt 2024; Bursztyn et al. 2020; Field et al. 2021; see Jayachandran 2021 for a review), internal psychological constraints (e.g., Orkin et al., 2023; McKelway, 2024), intrahousehold bargaining power (e.g., Heath and Tan, 2020; Abou Daher et al., 2023), safety and mobility (e.g., Cheema et al., 2019; Martinez et al., 2020; Field and Vyborny, 2022; Siddique, 2022), early childbearing (e.g., Miller, 2010; Herrera et al., 2019), as well as employer and client discrimination (e.g., Islam et al., 2021; Buchmann et al., 2023). In addition, a small but growing set of papers explore how digital technology might interact with these barriers (e.g., Alhorr, 2024; Jalota and Ho, 2024). The review paper Fletcher et al. (2017) shows that many women who are out of the labor force say that they are interested in working, and that there is a mismatch between the types of jobs available and women's job preferences. Our study shows that one important mismatch is the desire for flexible work arrangements, particularly the ability to work from home or multitask work with childcare.[12]

The rest of the paper proceeds as follows: Section 2 describes the study population and experimental design. Section 3 presents the first set of labor supply results, which include the effects of flexible work arrangements on the extensive margin of labor force participation. Section 4 presents the second set of labor supply results, which examines the effects of short-term work experience on take up of future work. This section also explores mechanisms for the effect on future labor supply, as well as impacts of women's work experience on the household more generally. Section 5 turns to the labor demand side and presents the impacts of introducing home-based arrangements on worker performance, separating effects that operate through treatment versus worker selection. Section 6 concludes.

## 2 Experimental design

### 2.1 Setting and Participant Characteristics

We conduct our study in eight areas in and around Kolkata, West Bengal, India, encompassing three rural locations (Canning, Noorpur, and Basanti), three peri-urban areas (Atabagan, Baruipur, and Sodepur), and two urban neighborhoods (Tiljala and New Alipore). Participant recruitment was facilitated by our local partner NGO, the Calcutta Foundation, which has operated in the region since 1994 with programs focusing on education, disaster relief, gender issues, and health.[13]

***Sample Selection.*** Recruitment occurred through a combination of door-to-door canvassing and outreach to households that had previously engaged with the Calcutta Foundation's programming. For inclusion in the study, households needed to have a female member who: (1) could read and

[12] The most related work to this study is our experiment in Mumbai, in which we also study the effects of work-from-home on female labor force participation in India using digital gig work (Jalota and Ho, 2024). Both experiments have one main result in common: work-from-home is highly effective at bringing out-of-labor-force women into paid work. From this result, the Mumbai experiment goes on to compare women's elasticity of labor supply with respect to work-from-home to their *wage* elasticity of labor supply, and then contrasts women's labor supply responses to those of men. Our paper describing the Mumbai experiment is remaining a working paper for the time being.

[13] See Appendix Figure A2 for a geographical representation of the study areas.

speak Bangla or Hindi,[14] (2) had access to an Android smartphone, and (3) was not currently in the labor force or skills training. These criteria were designed to identify women who were full-time homemakers and would likely remain so absent the intervention, but who would have the skills and resources to do the job if randomly assigned to a job offer.

Our recruitment approach does not guarantee a representative sample, but our participant demographics largely align with regional population characteristics on key dimensions. As shown in Appendix Table A3, our sample is similar to the average West Bengal household in terms of household size, religion, and the presence of parents-in-law and children within the household.[15] Given the demographic characteristics of an average adult woman in urban West Bengal, these inclusion criteria are not highly restrictive. According to large-scale demographic surveys, 67.7% of women between 18 and 60 years in urban West Bengal can read a full sentence, 72.48% own a mobile phone, and 79.12% are currently not working for pay.[16] Our sample does show slightly higher educational attainment and smartphone access than the general population, consistent with our eligibility requirements. Regarding technology access, all participants have access to Android smartphones (per our eligibility criteria), with 73% reporting exclusive ownership of their device rather than sharing with household members. This exceeds the smartphone penetration rate in the general population, where a 2021 survey found that 58% of even rural West Bengal households had smartphones, with likely higher rates in urban areas.[17]

***Participant Demographics.*** The women in our study are predominantly homemakers with limited prior labor market experience. The average participant is 30 years old and married. Half have completed education through tenth standard (students aged 15-16), while 13% have at least some

[14] Bangla (also known as Bengali) is the official language of West Bengal. Bangla is also widely spoken in Bangladesh, as well as in other Indian states such as Tripura, Assam, and Jharkhand. Hindi is the most widely-spoken language in India and is spoken by many people in West Bengal, especially in Kolkata and bordering districts.

[15] The large-scale survey data are from the National Family Health Survey (2019-21) and the Periodic Labor Force Survey (2021-2022). The large-scale survey sample is restricted to West Bengal and women between ages 18 to 60. Although the difference in means on some of these characteristics is significant, the magnitude of the difference is small across household size, presence of parents-in-law and children, and religion.

[16] These data are from NFHS 2019-21 and PLFS 2021-2022

[17] Surveys that are representative of both urban and rural households in West Bengal do not include information about *smartphone* ownership specifically rather than cellphone ownership, but as a lower bound, the Annual Status of Education Report (ASER) for 2021 found that 58% of rural households in West Bengal have a smartphone.

undergraduate education. Three-quarters of participants are Hindu, with the remainder being Muslim. Forty percent belong to a scheduled caste or other backward caste, which is somewhat lower than the regional average, with a corresponding higher representation from the Open/General caste category. Household composition reflects typical family structures in the area according to representative surveys: the average household size is 4.6 members, and 40% of participants live with at least one parent-in-law. Many women have substantial childcare responsibilities, with three-quarters of participants having children under eighteen and 48% having children under eight years of age.

***Socioeconomic Status and Agency Over Labor Supply.*** Participating households report an average monthly income of 11,791 INR (approximately 142 USD), placing them in the lower-middle-income bracket and somewhat below the West Bengal average. Most participants (69%) have never previously worked for pay, highlighting that the study sample has limited labor market experience. Notably, only 36% of participants report having the final decision-making authority over their own labor supply. The vast majority (86%) of remaining participants report that the final decision-making authority about their job take-up rests with their husbands. The other 14% say that different family members—father, mother, father-in-law, son, mother-in-law in order of response frequency—would have the final say. See Appendix Figure A3 for respondents' baseline reports of who would have the final say in their labor supply decisions.

***Gender attitudes about women and work.*** At the start of the study, many participating women hold traditional attitudes about the appropriate role for men and women in a household. For example, during the baseline survey, 67% of women agree that a woman's most important role is to be a good homemaker, and 64% agree that earning a living is the father's responsibility, while childcare is the mother's responsibility. Approximately half (52%) of women also agree that a man should have the final say about decisions in his home, which is particularly relevant to women's labor supply because on average men express lower levels of support for women working—especially outside the home—than women themselves do (Bernhardt et al., 2018; Bursztyn et al., 2024).

We aggregate women's levels of agreement with these statements, as well as ten other gender-related attitudes statements on the baseline survey, to classify women as belonging to households that are "more traditional" or "less traditional" depending on how their views compare to those of the median study participant. In our main specifications, we aggregate statements into a standardized index following Anderson (2008), but we find similar results when aggregating responses into an index in other ways (e.g. taking a within-participant average agreement level across questions). For more information on the gender attitudes index, including a list of all questions used as inputs into the index, see Appendix Section A.1.

If women themselves, their husbands, and their in-laws hold these views, it may be difficult for women to take jobs that require spending an extended period of time outside the home, away from their homemaking responsibilities. This helps to motivate the jobs offered in the experiment, which are particularly amenable to flexible, at-home work arrangements.

## 2.2 Job Description

***Jobs platform.*** We partner with a local-language smartphone-based tasks platform in India to offer the intervention jobs. The interface for the job app is presented in Figure A6. The jobs involve piece-rate paid tasks to contribute to Hindi or Bangla speech datasets, which require participants to speak into their phones and record their voices. The platform's clients build these speech datasets in low-resourced languages to train speech recognition algorithms, the idea being that there is a large and growing population of digital technology users in India who speak one of the country's many languages that do not yet have the labelled speech datasets necessary to develop voice-based applications (Abraham et al., 2020; Kumar et al., 2022).

Although the jobs platform hosts data annotation tasks across a wide variety of domains, we concentrate on speech datasets due to several advantages for our study. One advantage is that we can verify from submitted voice tasks if a woman rather than a man seems to be completing the tasks, even if the participant works remotely. Second, we can listen for children's voices in the background of the speech recordings, which allows us to implement our work arrangement variations which do not allow for multitasking work with childcare. Third, there is a good justification for why it is valuable specifically for *women* to do these tasks, which is that existing datasets underrepresent women's voices, which are important for training voice-based algorithms that work well in interpreting female voice content (as discussed in papers including Tatman, 2017; Garnerin et al., 2019; Fucci et al., 2023).

***Task structure.*** There are four types of paid tasks, which participants can choose to do in either Hindi or Bangla. The simplest tasks involve reading aloud a sentence which appears on the screen. These sentences are selected from a database of common phone or computer commands (e.g. "set an alarm for 7am tomorrow morning"), and they are that previous clients requested and paid for. The rest of the tasks require a higher cognitive load, and we classify them as "difficult" tasks for the purposes of analysis. The first difficult task involves reading a sentence backwards, which we introduce to require more concentration. The second difficult task involves finding a specific sentence within a paragraph and reading that sentence out loud. This requires more intensive concentration, as workers must read longer passages carefully. Lastly, there are open-ended questions for participants to answer. Our job platform partners are interested in collecting this type of speech data, as it is particularly scarce but useful. However, because it is difficult to assess quality for this open-ended task prompts, we exclude this last type of task from our analysis.

Participants receive 4000 tasks to complete over the course of one month, with 1000 new tasks refreshed each week that expire after seven days. Each week, workers are presented with 700 of the simplest tasks, 140 of the find-words-in-paragraph tasks, 150 read-sentence-backwards tasks, and 10 open-ended tasks. If participants are unsatisfied with their first attempt, they can re-attempt each task an unlimited number of times. The tasks are presented in a fixed order, but participants can skip any and as many tasks as they want.

***Payment.*** Participants earn up to one rupee per task they complete, with payments processed weekly according to task quality. Payments were deposited directly into women's bank accounts, except in a small number of cases in which women said they could not be paid into bank accounts, in which case they were paid in cash. Each completed task is assessed by a separate team of validators hired by our partners, who indicate whether they hear children's voices in the background of the task recordings and score each task on accuracy using a 0-2 scale. This means that participants could earn up to 4000 INR (approximately 50 USD) over the course of the intervention jobs, which is equivalent to 36% of the average household monthly income in the study. The task payment rate is set by the jobs platform and is the same as the payment rate offered to other workers on the platform for the same work. Per hour, this payment rate is also roughly equal to the wages advertised for data annotators in India on websites including Indeed.com.

***Implementing the work arrangements.*** The five work arrangements vary across three dimensions: time, multitasking with childcare, and location (see Table 1). The most flexible job we offer allows participants to work from home, at any time they choose, and while multitasking their work with childcare. In each subsequent job, we switch off one or more of these dimensions.

*Time Flexibility.* In the time-flexible groups, participants can choose to work for as many hours as they like and at any time of day to complete the tasks before they expire. In the time-inflexible groups, participants choose a 3-hour timeslot during the job offer stage, and they can only work during that timeslot for the rest of the month. Note that most workers could easily complete the tasks they were assigned in a given week if they worked for all hours during their timeslot. Even the participants in the fifth percentile for speed could complete, or at least attempt, the thousand tasks per week in approximately twelve hours. To enforce these work shifts, engineers working for the jobs platform altered the smartphone application so that it was possible to set time constraints. In the time-inflexible work arrangement, the app would not open for participants outside of their allotted work hours.

*Multitasking Work with Childcare.* In the work arrangement groups which include the ability to multitask work with childcare, participants are told that they can have their children next to them while they work. In the groups that do not include the ability to multitask with childcare, participants are told that it is not acceptable to have their children next to them while they work. If children's voices were heard in the background of their submitted tasks, they were not paid for those tasks (as determined by a separate team of validators).

*Working From the Office.* In four out of five work arrangements, participants work from home, while in the office-based group they are required to work from one of our offices. We set up an office in each of the eight recruitment areas such that workers could reach them by walking or by travelling a short distance by vehicle (a two-wheeler or three-wheeler). If workers want to travel by vehicle, we reimburse them for any associated financial commute costs. Participants are not allowed to bring their children to the office.[18] The office was open between 10am-6pm six days per week while the study was running (Monday-Saturday in most areas and Sunday-Thursday in the Muslim-majority area). Lastly, the participant's coworkers and managers are entirely female, shutting down concerns about harassment or other potential negative consequences of a workplace with men.

### 2.3 Timeline and Randomization

Study implementation was staggered across eight areas, beginning in April 2022 and ending in January 2023. See Figure 1 for a flowchart describing the experimental design.

***Recruitment, informed consent, and baseline surveys.*** During study recruitment, potential participants are not told that the study could include a job opportunity in order to avoid selection into the experiment based on interest in work. If women are eligible and consent to participate, they complete an extensive baseline survey that covers demographics, gender attitudes, agency, technology use, psychological wellbeing, and social contacts. When possible, we also survey children aged eight to eighteen about their aspirations, participation in household activities, and gender attitudes.

***Jobs round 1: initial offers.*** After the baseline survey, we randomly assign households to the control group (which receives no job offer) or to one of the five job groups for their initial job offer. Randomization is stratified by three characteristics: area, individual smartphone ownership,

[18] In Jalota and Ho (2024), we allow women to bring their children to the office to understand what the impact of requiring office-based work would be if women can multitask with childcare. Even when multitasking with childcare is allowed at the office, requiring women to come to the office cuts take up by one half.

and whether the participant has a child under age eight.[19] Participants can accept or reject the job offer, or else ask us to call back in a day or two if they need more time, for example in order to discuss the job offer with family members.

***Jobs round 1: surprise upgrades.*** After participants decide to accept or reject their initial job offers, we randomly select half of the participants who were assigned to any job other than the most flexible job to be surprised with an upgrade to the most flexible job. Women who were randomly assigned to an upgrade, but who turned down the initial less flexible job, are also offered the most flexible job. We include these randomly assigned surprise upgrades, following Karlan and Zinman (2009), in order to separately identify the characteristics of women who select into flexible work arrangements from the effects of those arrangements. To estimate selection, we compare measures of job performance between participants who initially accepted the most flexible job with participants who were upgraded to the most flexible job after initially accepting a less flexible job. This strategy to measure selection holds constant the actual work arrangement while varying worker type (i.e. *flexibility compliers*, women who will only work when the job is flexible, compared to inframargial workers). To estimate treatment effects of flexibility, we compare job performance between participants who initially accepted an inflexible job and were upgraded versus those who also accepted an inflexible job but did not get a surprise upgrade. This strategy holds constant worker type while randomly varying the work arrangement. After the upgrades, the jobs are implemented for one month as described in Section 2.2.

***Endline surveys.*** After the randomized jobs intervention, participants complete an endline survey. This survey takes place within two weeks of job completion for treated participants, and the timing of surveys for control participants is selected to balance the timing of those in the treatment group. The endline survey includes modules such as gender attitudes, agency, and psychological wellbeing. We again survey children aged eight to eighteen when possible.

[19]One region, New Alipore, was not stratified further for randomization, because of the small number of participating households (66).

***Jobs round 2.*** Two to three months after *Jobs Round 1*, each study participant is approached and offered another randomly selected job. The jobs in this second round vary across the same three dimensions of flexibility as in the first round, and they again last for one month. In addition to the five digital gig work jobs offered in the first round of jobs, *Jobs Round 2* also introduces two non-digital jobs that consist of sewing masks, making jewellery, or constructing bags. These non-digital jobs are also paid according to piece rates that vary by output quality. For the non-digital jobs, there are two possible work arrangements: one work-from-home, and one from the office. There is no way for us to enforce the ability to choose work hours or multitask work with childcare for tasks that are not completed on the jobs platform. The purpose of the non-digital jobs is to understand whether any treatment effects on interest in work are digital-specific or apply to interest in paid work more generally. Participants have an equal likelihood of being randomly assigned to any of the seven possible round two jobs (two most flexible, one time-inflexible, one multitasking-with-childcare-inflexible, one time- and multitasking-with-childcare- inflexible, and two office jobs).

### 2.4 Randomization, Balance, and Attrition

Appendix Table A1 presents means and standard deviations of participant characteristics in the control and treatment groups after randomization. Randomization produced groups which are balanced across most characteristics of interest, although households in the treatment group (i.e., those that receive any job offer) are more likely to be Hindu, a difference that is significant at the 10% level. Appendix Table A2 presents the same for participants who completed the endline survey. 1,525 households completed the endline survey with balanced attrition: the endline survey completion rate is 91.1% for the treatment group and 91.9% for the control group. The groups remain similar on average across important covariates. As with immediately after randomization, the treatment group is more likely to be Hindu ($p = 0.064$). At the endline survey stage, there is one more imbalance significant at the 10% level, which is that the control group is more likely to have parents-in-law in the household ($p = 0.119$ after randomization, and $p = 0.098$ at the time of the endline survey). To control for any imbalances, we present results from specifications that include covariates selected by double post LASSO that are predictive of both treatment assignment and outcomes of interest in regressions estimating the effects of treatment (Belloni et al., 2014).

## 3 Extensive margin labor supply response to job flexibility

### 3.1 Empirical strategy

Because job offers are randomly assigned, we can estimate the effect of a given dimension of flexibility on job take up by comparing the share of women who start work with versus without that dimension of flexibility. The sample of analysis is the 1,250 women who are randomly assigned to receive a job offer. We estimate the impact of flexibility on job take up using the following simple regression

$$StartsWork_i = \sum_{j=1}^{4} \beta_j \mathbb{1}\{WorkArrangement_i = j\} + \mu_s + \varepsilon_i \tag{1}$$

where $StartsWork_i$ is an indicator variable equal to one if participant $i$ starts the job that she is randomly assigned to. The regressor $\mathbb{1}\{WorkArrangement_i = j\}$ are indicator variables equal to one if participant $i$ is randomly assigned to a work arrangement $j$, where $j$ can take on one of four values: time-inflexible, multitasking-with-childcare-inflexible, time- and multitasking-with-childcare-inflexible, or office based. The omitted category is the most flexible job, so the $\beta_j$ coefficients represent the difference in job take up relative to this most flexible benchmark. We control for strata fixed effects $\mu_s$ and estimate Huber-White heteroskedasticity-robust standard errors. To better understand the types of women whose labor supply is marginal to job flexibility, we also estimate this model separately on different subsamples split by baseline covariates, such as whether the woman reports having the final say in her own labor supply decisions, whether she has more versus less traditional gender attitudes, or whether she has a young child.

### 3.2 Effects of flexibility on job take up

Job flexibility dramatically increases women's extensive margin labor supply by more than three times, from 15% for an office job to 48% ($p < 0.01$) for the most flexible work-from-home job (see Figure 2). To benchmark the magnitude of this effect, the bundled impact of job flexibility on take up (+33 pp or 228%) is larger than the effects of previously-studied levers that successfully increased women's labor supply. For example, correcting men's second-order beliefs about social image costs of women working increased sign-up rates for a job matching service by 9 pp or 36% (Bursztyn et al., 2020), a self-efficacy intervention increased job take up rates by 5 pp or 32% (McKelway, 2024), and giving women control over their earnings increased women's participation in paid work by 5 pp or 13% (Field et al., 2021). The effect of job flexibility is also large compared to estimates of women's wage elasticity of labor supply from another of our projects. In an experiment in Mumbai, we find that increasing women's monthly wage offers nearly five times from 5,000 INR (59 USD) to 24,000 INR (283 USD) only resulted in a 7 pp (32%) increase in job take up (Jalota and Ho, 2024). If we were to take the Mumbai labor supply elasticity estimates seriously and apply them to the West Bengal experiment participants, this would imply that women value job flexibility at 89,500 INR (1,050 USD) per month, far above the average monthly income of these households.

The effect of job flexibility on women's job take up varies—but remains large—across a range of study subsamples (see Appendix Figure A4 for sample splits by characteristics including education, religion, cohabitation with in-laws, and agency). One of the most stark patterns emerges when examining job take up separately by gender attitudes. Women with more traditional attitudes have lower levels of job take up across all work arrangements, although the differences are much smaller for less flexible jobs. That is, job flexibility makes a difference to the job take up decisions of all women, and particularly so for the women from more traditional households. This difference by gender attitudes in job take up, and the elasticity of labor supply with respect to flexibility, does not appear to be driven by the presence of young children, which could have been the case if more traditional women are also more likely to have young children. In Appendix Figure A5 we split the sample by traditional attitudes as well having a young child under age eight. Even among participants without a young child, more traditional women have lower levels of job take up and a larger labor supply response to job flexibility.

Finally, the large effecct of job flexibility on women's job take up is robust to changing the cutoff of number of tasks completed that qualifies as having started work (see Appendix Table A4 columns (2)-(4) for results using 10 tasks, 50 tasks, and 100 tasks as cutoffs).

### 3.3 Mechanisms: dimensions of flexibility

To understand the mechanisms driving the impact of this bundled job flexibility on women's labor supply, we sequentially shut down time flexibility, multitasking-with-childcare flexibility, and the ability to work from home in job offers that are randomly assigned across participants. Understanding which features of flexible work arrangements affect women's participation in paid work is important for designing jobs that can include more women into the labor market.

1. **Time flexibility.** The ability to choose hours flexibly does not make a significant difference to women's job take up. Time flexibility has no significant impact regardless of whether women are assigned to a work arrangement in which they can multitask work with childcare. The job take up rate for the time-inflexible job is 46% as compared to 48% for the most flexible job ($p$-value for the difference = 0.66). In this comparison, both work arrangements include the ability to work from home and multitask work with childcare and differ only in time flexibility. Then, to examine the impact of time flexibility when women are *not* able to multitask work with childcare, we compare the take up rate for the multitasking-with-childcare-inflexible job with the take up rate for the time- and multitasking-with-childcare-inflexible job. Both have take up rates of 29%.

   These results show that the out-of-labor-force women in our study can, and choose to, set aside consistent hours of their day to work for pay. This holds regardless of whether or not the work allows them to multitask work with childcare. This suggests that lack of time, at least for part-time work, is not the binding constraint that prevents these women from entering the labor market.

2. **Multitasking-with-childcare flexibility.** The ability to multitask work with childcare is an important determinant of women's job take up. The share of women who start work increases

from 29% for the home-based job that does not allow multitasking work with childcare to 48% for the most flexible home-based job (+19 pp or 67%, $p < 0.01$). Similarly, the share of women who start work increases from 29% for the time-inflexible home-based job that does not allow multitasking work with childcare to 46% for the job that is also time-inflexible but *does* allow multitasking with childcare (+16 pp or 56%, $p = 0.01$).

3. **Ability to work from home.** The ability to work from home — even at fixed hours and without multitasking with childcare — is also a consequential determinant of women's job take up, increasing the share of women who start work from 15% for the office job to 29% for the home-based job that is flexible neither in time nor multitasking work with childcare (+14 pp or 100%, $p < 0.01$).

In sum, the extensive margin labor supply decisions of 33% of women in our study are marginal to bundled job flexibility. A decomposition of this effect using random variation in work arrangements finds that approximately half of the effect can be attributed to the ability to multitask work with childcare (16-19 pp), and approximately half can be attributed to the ability to work from home independent of childcare concerns (14 pp).

## 4 Gateway jobs: flexible, home-based jobs as a stepping stone

### 4.1 Empirical strategy

Even if work-from-home opportunities can draw in a substantial share of out-of-labor-force women, if these women can only ever work from home, then the job opportunities available to them will remain very limited. However, experience with paid work — including in home-based jobs — might lead these women who were initially constrained to only be able to take more flexible work-from-home jobs to be able to take up less flexible outside-the-home jobs through channels such as increased bargaining power, increased self-confidence, or a shift in gender attitudes that makes it more acceptable for women to spend time away from home production tasks.

Because willingness to take up a job is monotonic in flexibility (empirically, accepting the office job during the baseline survey implies accepting the most flexible job in 99% of cases, see Appendix Table A5), we say that anyone whose round 1 job assignment was more flexible than their round 2 assignment was given a *gateway job sequence*. In this case, there are some women — *gateway job compliers* — who can take up the round 1 work arrangement, but who could not have taken up the round 2 work arrangement if it had been offered to them during round 1. That is, at baseline, the labor supply of these women is marginal to the difference in flexibility between their round 1 and round 2 work arrangements. If it is the case that work experience during round 1 expands the set of jobs that women can take up to include less flexible jobs, however, then their labor supply may no longer be marginal to the difference in flexibility by the time they receive their round 2 job offer. This would imply that a higher share of these *gateway job compliers* would be able to take up the round 2 job than their counterparts in the control group who did not receive the round 1 job offer that allowed them to gain work experience.

To test this gateway jobs prediction, we regress round 2 take up on the relative flexibility of participants' round 1 versus round 2 job assignments:

$$StartsWork_{2,i} = \beta_1 \mathbb{1}[f(j_{1,i}) - f(j_{2,i}) > 0] + \beta_2 \mathbb{1}[f(j_{1,i}) - f(j_{2,i}) \leq 0] + \gamma_{1,i} + \gamma_{2,i} + X_i + \mu_s + \varepsilon_i \tag{2}$$

where $StartsWork_{2,i}$ is an indicator variable equal to 1 if participant $i$ starts the job that was randomly assigned to her during jobs round 2. We encode information about participant $i$'s job offers in the function $f(j_{r,i})$, which takes on a discrete value that depends on the flexibility of the work arrangement $j$ randomly assigned to participant $i$ during round $r$: 1 if $j$ is the least flexible job (office), 2 for the next least flexible job that is inflexible across time and multitasking with childcare, 3 for the multitasking-with-childcare-inflexible job, 4 for the time-inflexible job, and 5 for the most flexible job. The exact values representing each work arrangement are not important to the specification; the purpose is simply to give the work arrangements a rank ordering based on the job take up rate associated with that work arrangement.

In all specifications, we include round 2 work arrangement fixed effects $\gamma_{2,i}$, which ensures that we capture the difference in take up rates *within* round 2 arrangements across participants randomly assigned to different round 1 offers. In some specifications, we also include round 1 work arrangement fixed effects $\gamma_{1,i}$. The inclusion or exclusion of round 1 work arrangement fixed effects $\gamma_{1,i}$ lead to a different interpretation of the coefficients $\beta_1$ and $\beta_2$, but we view both models as useful for delivering different insights. When $\gamma_{1,i}$ is excluded from the model, $\beta_1$ and $\beta_2$ include both the main effect of being assigned to a particular work arrangement in round 1 in addition to the effect of the relative flexibility of the round 1 versus round 2 work arrangement. Including $\gamma_{1,i}$ in our model soaks up the main effect of the round 1 job. This would be consequential if, for example, it is something about the most flexible job *other than its relative flexibility* which causes women to take up less flexible jobs in round 2 in higher numbers. To understand whether the gateway job effect that we observe is driven by (a) the main effect of more flexible jobs versus (b) their relative flexibility as compared to the round 2 jobs, we report estimated coefficients from models with and without $\gamma_{1,i}$. In our main specification, we also control for a vector of baseline covariates selected by double post lasso $X_i$ and strata fixed effects $\mu_s$, although the estimates are similar without any control variables included. We use heteroskedasticity-robust Huber-White standard errors.

Our coefficient of interest in model 2 is $\beta_1$, the effect on round 2 job take up of being offered a *gateway job sequence*. If experience with paid work expands the set of jobs that women can do to include less flexible jobs, then we will find that $\beta_1 > 0$. If instead experience with paid work shrinks the set of jobs that women are willing to do moving forward (e.g. due to income effects), then we will find that $\beta_1 < 0$.

### 4.2 Results

Consistent with flexible work arrangements acting as a stepping stone to less flexible jobs, women randomly assigned to a *gateway job sequence* are 6 pp ($p = 0.05$) more likely to start work conditional on their round 2 job offer (see Table 2, column 1). This gateway job effect is primarily driven by the relative flexibility of the round 1 versus round 2 jobs, rather than by a positive main effect of work experience in a more flexible job: the estimated effect of a gateway job sequence remains similar when controlling for round 1 work arrangement fixed effects ($\hat{\beta}_1$ = 5 pp, $p = 0.07$). In contrast, there is no effect of being assigned to a non-gateway job sequence (i.e. any other job sequence) on women's take up during round 2, as represented by $\hat{\beta}_2$ which is not significantly different from zero. Results are similar if we do not include the control variables selected by double post lasso (Appendix Table A6).

Consistent with the gateway job effect operating through learning from the experience of earning income, the estimated effect is most pronounced among women with no previous experience with paid work prior to study participation (Table 2, columns 3-6). Women who reported at baseline that they had never before worked for pay are 7 pp more likely to start the round 2 job if assigned to a gateway job sequence ($p = 0.03$), while the difference for women who had previous paid work experience before the study is also positive (+2 pp) but insignificant.

Lastly, we examine how the gateway job effect varies by round 2 work arrangement. The coefficients on the gateway job effect for all round 2 work arrangements are positive but most are noisily estimated due to smaller sample sizes (see Appendix Table A7). However, we assign a larger (randomly selected) share of workers to office jobs than other less flexible arrangements during round 2, resulting in a more precisely estimated coefficient on this interaction, which is still positive and significant ($\hat{\beta}_1 = 8$ pp, $p = 0.04$). Given that 19% of women assigned to the control group during round 1 start work if randomly assigned to the office job during round 2, this effect implies that assignment to a *gateway job sequence* increases office job take up by 42%. To put this effect size in context, increasing women's take up of office-based work by 8 pp in similar settings has proven very difficult. In Jalota and Ho (2024), we find that increasing wages by nearly five times (from 5000 INR to 24000 INR per month) achieves approximately the same magnitude increase in office job take up among married women in Mumbai. Taken literally, this means it would be more cost-effective for an employer to first offer a short-term experience in home-based work (a *gateway job*) before asking female workers to work from an office than it would be for employers to pay workers enough to immediately achieve the same attendance at an office.

### 4.3 Mechanisms

In this section, we consider six possible mechanisms through which work experience might affect future labor supply: (i) women are initially constrained by attitudes about appropriate behavior for women, and work experience shifts gender attitudes, thus expanding the set of jobs accessible to them, (ii) women learn skills or gain the ability to signal skills to a future employer, (iii) income effects from the paid work experience, (iv) increased bargaining power for the woman earning income, (v) increased confidence or self-efficacy, and (vi) greater trust in the employer. We present evidence for (i) and then assess the plausibility of the other five channels.

***(i) Attitudes about women's labor supply and women's actual labor supply reinforce each other.*** We show supporting empirical evidence for two relationships. First, traditional gender attitudes constrain women's labor supply, particularly in less flexible jobs. Figure 3 Panel A shows the round 1 take up rates for the five different work arrangements, split by whether women's baseline gender attitudes are more or less traditional than those of the median participant.[20] Job take up rates are weakly lower for traditional women across all work arrangements. In addition, note that job flexibility is important for both types of women — those who hold more traditional attitudes as well as those who hold less traditional attitudes. Lastly, and importantly for this mechanism, note that job flexibility is *differentially* more important for women with traditional gender views. This relationship is correlational but shows that labor supply elasticity with respect to flexibility is increasing in traditional views at least in the cross section.[21]

Second, work experience shifts gender attitudes to become less traditional, which expands the set of jobs that women are able to do to include less flexible jobs. Figure 3 Panel B shows that work experience in turn causes a shift in women's gender attitudes to become less traditional. Women

[20] See Appendix Section A.1 for more information on measuring gender attitudes.

[21] Appendix Figures A7 and A8 compare the additional impact of job flexibility on take up across baseline characteristics. In terms of magnitude, baseline gender attitudes emerge as the most important characteristic mediating the importance of job flexibility to take up. Other characteristics, such as not having the final say in one's own labor supply decisions, as well as having a children under the age of 8, are also associated with a greater importance of job flexibility, but to a lesser degree. Other characteristics such as 10th grade completion, household income, and co-habitation with in-laws are not associated with a differential effect of job flexibility on take up.

randomly assigned to receive a job offer become 0.05 SDs less traditional ($p = 0.03$).[22] This effect is entirely concentrated on women who held attitudes that were more traditional than the median at the start of the study; the attitudes of these more traditional women shift by 0.12 SDs ($p < 0.01$).[23] Figure A9 plots the gender attitude distributions at endline for women whose baseline attitudes were more traditional versus less traditional and shows that the lack of effect on women who were already less traditional is not due to ceiling effects.

Combined with empirical patterns in the cross section, the effect of work experience on the gender attitudes of *more traditional* women is important because this means that job flexibility and work experience are important to the same subset of participants: women from more traditional households are the ones whose labor supply is more responsive to job flexibility, and in turn, it is these same women from more traditional households whose attitudes change most in response to work experience. This means that flexible work arrangements are specifically effective at targeting the labor force participation of women from more traditional households, since their labor supply is most likely to be marginal to flexibility and their attitudes respond most to work experience.

***(ii) Women learn skills or gain the ability to signal skills to future employers.*** While this channel could strengthen the gateway job effect outside the context of this study, in our experiment we shut down this channel to focus on labor supply side factors. Job offers are decided randomly, and there is no screening on skill beyond the inclusion criteria for the study that require women to be literate in Bangla or Hindi.

***(iii) Income effects.*** The initial work experience might affect labor supply decisions during jobs round 2 if the income from jobs round 1 means that the household is less financially stressed when the second job offer arrives. However, income effects would push treatment effects in the opposite direction of what we see empirically. Random assignment to a more flexible job would increase

[22] We estimate the effects of being randomly assigned to a job offer using this participant-level intent-to-treat (ITT) regression: $y_i = \alpha_{ITT} T_i + \theta_i + \mu_s + \varepsilon_i$ where $y_i$ is the relevant outcome variable (e.g. gender attitudes or agency); $\mu_s$ stands for strata fixed effects; $T_i$ is an indicator variable for being randomly assigned to treatment (any job offer); and $\theta_i$ is a vector of covariates selected using double post LASSO (Belloni et al., 2014) to control for variables predictive of both treatment assignment and outcomes.

[23] In Appendix Figure **??**, we present effects on gender attitudes of work experience separately by different work arrangements.

income relative to the control group and make it *less* rather than more likely that women accept a less flexible job during round 2. Thus, if present, income effects are dominated by other channels.

***(iv) Increase in bargaining power.*** Women's additional income may increase their bargaining weight in household decision making, which could increase their labor supply if women are more supportive of themselves working for pay than their husbands are. However, we do not find a significant effect on agency as measured by an index aggregating responses to questions including (a) who has the final say in whether the participant accepts a job, (b) whether the participant asks for permission before purchasing clothes, (c) whether the participant asks for permission before going out of the house, (d) whether the participant asks for permission before meeting friends, (e) whether the participant's opinion is taken into account in significant purchases, and (f) whether the participant gets the final say in significant household purchases (see Appendix Figure A10). In addition, there are no heterogeneous treatment effects on agency by baseline levels of agency. This lack of effect is consistent with the effects of income on agency found in previous literature. Unlike in studies which find effects on agency of a permanent increase in women's income (e.g. Duflo, 2003; Field et al., 2021), the increase in income in this study was temporary, and we told households at the time of the job offers that this was a one-time job opportunity.[24]

***(v) Increase in self-efficacy or self-confidence.*** If the initial work experience increases women's self-efficacy (as found in McKelway, 2024), then women might be more willing to take up a job that they consider more challenging, such as an office job, after the first experience with paid work. We do not find any treatment effects on women's reported self-efficacy in their ability to use a smartphone for a variety of skills ($\hat{\beta}_1 = -1.5\%$, $p = 0.48$), although we cannot rule out treatment effects on self-efficacy in other domains.

***(vi) Increased trust in the employer.*** Work experience might affect future labor supply with the same employer if the experience helps build trust in the firm. For example, workers might learn that the employer pays workers on time, or that the managers seem reputable and safe for women

[24] There were ultimately two rounds of job offers, but in each case we told household members that this was a one-time job opportunity because we had not planned to conduct the second round of job offers at the time we made the offers for the first round of jobs.

to interact with. Applying this principle to our study, treatment group women might have learned from their interactions with the job offers team and tasks platform support staff that the employer is trustworthy. This increased trust could lead to higher subsequent take up of less flexible jobs, especially outside-the-home jobs, if women think that it is risky to their safety to go to an office run by an unknown employer. The familiarity with the employer may have made them more comfortable coming into an office. This is a plausible channel that our gateway job effect may operate through, in addition to the the treatment effect on attitudes about women and work. If this channel is important, then a policy implication is that it may not be effective for governments or third parties to provide at-home training or job opportunities that then lead to in-person job opportunities. This trust mechanism requires the same company to first offer the at-home opportunity and then the office-based opportunity.

### 4.4 Spillover effects to children

At the time of household surveys, we also attempt to speak to one child aged eight to eighteen in each household. Children are asked about their parents' labor supply and home production activities, as well as surveyed about their own gender attitudes. The children's gender attitudes module is combined into an index following the same procedure as for adult women. Questions about attitudes were similar to those asked to mothers, swapping in some statements that might be more relevant to school children (e.g. "girls are equally intelligent as boys" or "it is more important for boys to go to university than girls.").

**Do children notice their mothers are working?** Children whose mothers were assigned to the treatment group are 18 pp more likely to say that their mother had a job during the last month ($p < 0.01$, see Figure 4 Panel A and Appendix Table A8). As a placebo check, we also ask children if their fathers had a job and find no effect on this outcome, which is consistent with the responses of primary female participants about their husbands' labor supply.

**Effects on fathers' participation in home production.** We ask children to what degree their fathers help with cooking, cleaning, and laundry. In the control group, 42% of children report that their fathers *never* helped with cooking, cleaning, or laundry during the time period of the study intervention. In comparison, treatment group children are 9 pp more likely ($p = 0.06$) to report that their fathers helped at least occasionally during the treatment period (see Figure 4 Panel A and Appendix Table A8).

**Effects on children's gender attitudes.** Treatment shifts the attitudes of older children to become less traditional (see Figure 4 Panel B and Appendix Table A9). The overall effect of treatment on attitudes of children of all ages has the same point estimate as for adult women (0.05 SDs), but the sample size is smaller, and the estimate is not significantly different from zero. Splitting the sample by age, treatment has a significant impact on the attitudes of children older than the median (12 years old), who become less traditional by 0.11 SDs ($p = 0.05$). Younger children's attitudes remain unchanged. This difference in effects by age is consistent with the literature: as Dhar et al. (2022) note, adolescence is hypothesized to be a particularly important time for morality and identity formation, as adolescents are mature enough to contemplate nuanced questions about the role of gender in society, while still being young enough that their views are relatively malleable (Kohlberg, 1976; Markus and Nurius, 1986).

Treatment effects on attitudes do not differ by child gender. At baseline, boys' attitudes are 0.16 SDs less gender-equal than girls' attitudes ($p < 0.001$), suggesting that differences in gender-based preferences and beliefs start at a young age. However, treatment does not affect the attitudes of boys differentially from girls, and so the gap in gender attitudes between boys and girls persists at the same magnitude in both the treatment and control groups at endline.

**Discussion: Possible Mechanisms.** The effects on children could be driven either by seeing their mothers do paid work or by the job offers themselves. Watching their mothers earn income for the household and require non-family time might change children's perceptions of their mothers. Independently of seeing their mothers work, children could infer from a job offer — if their mother discusses the job offer at home — that their mothers' time is more valuable than they previously believed, or that their mothers are more intelligent or skilled than they previously believed. Similarly, women's husbands might infer from the job offer that their wives have a higher opportunity cost of time than they previously believed, which could cause them to do more housework.

### 4.5 Effects on wellbeing.

In addition to any effects on future labor supply, in order to assess the welfare effects of increasing the availability of flexible work arrangements, it would be useful to understand how these paid work experiences affect women's wellbeing. If women are now working for pay and continuing with most of their home production responsibilities, then the work could become a "second shift" even if the women themselves prefer to work for pay (Hochschild, 1989).

Endline survey measures show that work experience has no significant effect on an index of psychological wellbeing ($\hat{\beta}_1 = 0.02, p = 0.71$), nor on any of the individual index components (see Table A10). The index is composed of questions in which participants rate how frequently during the intervention period they report (i) sleeping peacefully, (ii) feeling overwhelmed, (iii) feeling happy, and (iv) feeling worried. Response options are never feeling this way, feeling this way a few days out of a month, feeling this way around half the days, feeling this way more than half the days, or feeling this way nearly every day.

However, paid work experience does shift women's perceptions of whether their talents and abilities are put to good use. Strikingly, in the control group, more than one in four women report that their potential and talents are put to use *not at all*. Treatment shifts the entire response distribution to the right: women randomly assigned to receive a job offer are more likely to report that their potential and talents are put to use a little bit, somewhat, or very much ($p = 0.02$, see Figure 5).

## 5 Effects of flexibility on worker performance

The results presented in Section 3 demonstrate that offering flexible work arrangements could substantially increase the pool of workers available to employers. This naturally raises the following question: Why do employers not offer home-based work options more frequently in contexts where remote work is feasible? Beyond adjustment costs associated with implementing work-from-home policies, firms would likely want to understand how remote work affects job performance. These effects could operate through two distinct channels. First, work-from-home could affect the performance of inframarginal workers who would have accepted the job even if they had to work in person. Second, advertising remote work options could alter the composition of the applicant pool, thereby changing the types of workers who join the firm.

### 5.1 Empirical Strategy

We leverage randomly assigned surprise upgrades to the most flexible work arrangement to estimate the causal effects of work-from-home on worker performance. Our research design enables us to disentangle effects operating through treatment (impacts on inframarginal workers) versus selection (changes in worker composition).

**Research Design.** Our identification strategy uses the random variation introduced by surprise upgrades to participants' job offers after their initial job acceptance decisions during jobs round 1.[25] For where this occurs in the experimental design, see the part of Figure 6 where "Initial Job Offer" splits into "No Job Offer Change" and "Job Offer Upgrade". First, by holding worker type constant—looking only at those who accepted an inflexible job—and comparing the performance of those who stayed in the inflexible job with those who were randomly selected for an upgrade to the most flexible job, we estimate the effects of flexibility on job performance for *inframarginal* workers who are willing to work inside or outside the home. Second, by holding the work arrangement constant and comparing women who were willing to accept an inflexible job with those who were not, we characterize the *flexibility compliers*, i.e. women who only take up jobs when they are flexible.

For both analyses, we examine two primary outcome measures of job performance: accuracy

[25] We use a similar strategy to Karlan and Zinman (2009), in which the authors use randomly assigned surprise changes to interest rates to separately identify adverse selection and moral hazard.

and task completion speed. Accuracy is measured on a 0-2 scale, reflecting the quality of task completion as determined by independent validators. A score of 0 indicates a completely incorrect submission, 1 indicates a partially correct submission, and 2 indicates a fully correct submission that meets all quality standards established by our implementation partner. Task completion speed is measured as the time elapsed (in seconds) between task initiation and submission.

**Identifying treatment effects of work-from-home.** To estimate the effects of work-from-home, we compare the job performance between two groups of workers who were initially assigned to the office job: (1) those who accept and stay in the less flexible job offer, and (2) those who accept the less flexible job offer but are randomly selected for an upgrade to the most flexible job. We estimate the following individual-by-task level regression to quantify the effects of office-based work on job performance:

$$y_{it} = \beta_T \, Upgrade_i + \omega_{it} + \varepsilon_{it} \tag{3}$$

where $y_{it}$ represents individual $i$'s performance on task $t$, $Upgrade_i$ is an indicator for workers who initially accepted the office-based job but were randomly selected for an upgrade to the most flexible work arrangement, and $\varepsilon_{it}$ is an idiosyncratic error term. The parameter $\beta_T$ represents the average treatment effect of flexibility on job performance for inframarginal workers who were willing to accept the office job.

The vector $\omega_{it}$ contains a rich set of controls to account for learning and attrition effects that could confound our treatment effect estimates. Specifically, we compare estimates across specifications that control for (i) the cumulative number of tasks completed before task $t$, (ii) its quadratic term, (iii) fixed effects for previously completed tasks, (iv) the cumulative number of tasks attempted, (v) its quadratic term, and (vi) fixed effects for previously attempted tasks.

We also examine heterogeneous treatment effects of flexibility by task difficulty. In this analysis, the tasks which involve reading a given sentence are classified as easy, while other tasks (reading sentences backwards, and finding sentences within paragraphs) are classified as difficult.[26] By interacting the $Upgrade_i$ indicator with task difficulty, we can identify whether the productivity effects of flexibility are more pronounced for complex tasks that require sustained concentration. This heterogeneity analysis provides insights into the mechanisms through which work-from-home affects worker performance and the conditions under which firms might benefit most from offering remote work arrangements.

**Identifying selection effects of work-from-home.** To estimate the characteristics of flexibility compliers based on their job performance, we compare two groups of participants: (1) those who are initially offered and accept the most flexible job, with (2) those who were initially offered and accepted the office job, but are then randomly selected for an upgrade to the most flexible job. To characterize the performance differences between workers who require flexibility (flexibility compliers) and those who accept work regardless of flexibility (inframarginal workers), we estimate:

$$y_{it} = \beta_S \, AcceptedOffice_i + \omega_{it} + \varepsilon_{it} \tag{4}$$

where $AcceptedOffice_i$ is an indicator variable equal to one if the worker was initially offered and accepted the office job before being randomly selected for an upgrade to the most flexible job. $y_{it}$ and $\omega_{it}$ are defined the same way as in the treatment effects analysis. The coefficient $\beta_S$ identifies productivity differences between inframarginal workers and flexibility compliers, holding constant the work arrangement (both groups ultimately work under the most flexible conditions). This allows us to isolate selection effects by comparing workers who differ only in their willingness to accept office jobs.

### 5.2 Results

**Treatment effects of Work-From-Home on Worker Performance.** Table 3 presents estimates of the causal effect of work-from-home arrangements on worker productivity for inframarginal

[26] See Section 2.2 for a more detailed description of each task type.

workers. Across a range of specifications that control flexibly for previous tasks attempted and completed, we find robust evidence that working from home significantly reduces worker productivity. Home-based work decreases task completion speed by approximately 20% ($p = 0.02$) and reduces accuracy by 4% ($p < 0.01$) relative to office-based work.

We find that the negative productivity effects of home-based work are magnified for more complex tasks. Work-from-home increases the time it takes to complete the simplest tasks by 13% ($p = 0.02$) while increasing the time it takes to complete more difficult tasks by 22% ($p = 0.03$ for difference with simple tasks). The pattern is more pronounced for accuracy outcomes: work-from-home has no statistically significant effect on accuracy for the simplest tasks, but reduces accuracy by 12% for the most difficult tasks ($p < 0.01$ for the difference relative to simple tasks).

Our findings largely align with the emerging literature on work-from-home productivity effects, which has so far mostly focused on contexts and populations very different from those in our study. In the U.S. context, Emanuel and Harrington (2024) document a 4% reduction in call center workers' productivity when working remotely, with decreases in both call quantity and quality—paralleling our findings on both speed and accuracy dimensions. In India, Atkin et al. (2023) find an 18% reduction in productivity among full-time data entry workers, comparable to our estimated effect size despite our focus on part-time female workers previously outside the labor force. Similarly, Gibbs et al. (2023) find that remote work reduced productivity among high-skilled IT professionals in India by 8-19%, though their context features collaborative work where communication costs likely play a larger role than in our setting with independent tasks.

**Selection Effects: Characterizing Work-From-Home Compliers.** Table 4 presents our analysis of selection effects by comparing the performance of inframarginal workers to flexibility compliers, both working under identical work arrangements. Contrary to concerns that offering flexibility might attract lower-productivity workers, we find no statistically significant differences in performance between these groups. The point estimates may suggest that flexibility compliers complete tasks somewhat more slowly—approximately 7% slower for simple tasks and 4% slower for difficult tasks—but these differences are not statistically distinguishable from zero.

For accuracy outcomes, selection effects are even more precisely estimated near zero, with no discernible differences between flexibility compliers and inframarginal workers across the task difficulty spectrum. These findings suggest that expanding the labor pool through flexible work arrangements does not come at the cost of reduced worker quality, at least in terms of observable task performance metrics.

Exploring heterogeneity in selection effects by task difficulty, the selection patterns remain statistically insignificant for both simple and difficult tasks, suggesting that flexibility compliers do not possess comparative advantages or disadvantages for particular task types relative to inframarginal workers.

### 5.3 Mechanisms: Workflow Interruptions and Task-Switching Costs

What is it about working from home that explains the differences in performance for workers who are situated at the office versus at home? One of the major differences between working at home and working from an office is the possibility for interruptions (Adams-Prassl et al., 2023). At home, other family members may call on participants for their immediate attention, while at the office, participants are less likely to be called away from their work. To investigate this channel, we group workers' tasks into *worksessions*, which are defined as a contiguous segment of tasks in which fewer than ten minutes passes between consecutive task submissions (following Adams-Prassl and Berg, 2017).

Figure 6 presents evidence that workflow interruptions are a key mechanism through which flexibility affects productivity. Panel A shows that working from the office causes worksessions to be 25% longer on average than when working from home ($p < 0.01$). This pattern is consistent with the hypothesis that workers face a higher arrival rate of interruptions at home from household demands, fragmenting their productive time. These interruptions are consequential for performance because of task-switching costs. We find that there is a "warm up" period associated with each worksession during which workers complete their tasks more slowly and with more mistakes (Figure 6, Panels C and D). Conditional on worksession length, initial tasks within a worksession are more likely to be completed incorrectly (meaning that participants will not receive full pay, and the output is not useful to the employer). And not only are these tasks more likely to be marked as inaccurate, tasks completed at the start of a worksession also take longer for the worker to complete.

**Discussion.** In our piece-rate setting, workers themselves bear most of the efficiency cost through lower earnings, though employers still incur quality control costs for filtering inaccurate submissions. However, if this negative effect of work-from-home on productivity exists even in this context where the *worker* pays most of the price of lower productivity, then we might expect the negative effect to be exacerbated in salaried jobs in which the *employer* bears a higher share of the cost. Furthermore, the unpredictable and fragmented work patterns we document might make home-based work particularly problematic for roles requiring synchronous collaboration or rapid response times. This might pose a challenge especially for female workers who are expected to be "on call" at home.

### 5.4 Back-of-the-envelope calculation for the employer

In this subsection, we discuss a back-of-the-envelope calculation of a firm's tradeoffs in deciding whether or not to introduce the ability to work from home. By moving work from the office to home, the firm can cut down on the fixed costs of running an office, such as renting space and materials, as well as hiring an office supervisor. However, these fixed costs of office-based work may be offset by the variable costs of introducing work-from-home (lower average speed and accuracy), which scale in the amount of work completed.

*Piece rate wages.* In a piece-rate world, introducing work-from-home is worthwhile for the employer. The firm does not have to spend on office costs, and it is the worker who bears the costs of decreased accuracy and speed, assuming that the firm can find a low-cost mechanism through which to adjust payments based on the accuracy of tasks submitted.

*Salaried workers, same wages from home and office.* In a salaried world, if the workers' wages must be kept the same between home and office, then whether or not introducing work-from-home is worthwhile will depend on the quantity of work involved. Given the fixed costs associated with the office and the variable costs associated with work-from-home, there is a breakeven amount of work above which it is worthwhile to rent an office. Intuitively, this is the point at which the fixed costs are spread out over enough tasks that they are less costly than the lower accuracy and speed associated with every task completed from home. In Table A11 we lay out assumptions about the costs of renting space and materials as well as hiring an office supervisor based on the costs in our study areas around Kolkata. Figure A11 shows how the cost per task from home versus office varies with the total number of tasks completed under these assumptions.

*Salaried workers, different wages for home versus office.* However, if it is possible for the firm to offer different wages to workers who are working in person versus at home, then there may be no breakeven point at which it is worthwhile to open an office. If the worker's willingness-to-pay to work from home exceeds the productivity loss of them working from home, then introducing work-from-home will be worthwhile for the employer. The results on women's wage elasticity of labor supply from our experiment in Mumbai (Jalota and Ho, 2024) suggest that in this and similar contexts, women's willingness-to-pay to work from home is more than high enough to compensate for the loss of productivity.

In addition, all of the scenarios considered above are employment decisions during one time period. Given the dynamic effects of work experience in one time period on the next (as shown through the analysis of gateway job sequences in Section 4), it may be possible for the firm to reduce workers' willingness-to-pay to work from home by first offering a short-term experience with the company while working from home before asking workers to transition to in-person work.

## 6 Conclusion

Many women who would like to work for pay cannot do so because available jobs are incompatible with their household roles. In a field experiment with 1,670 households in West Bengal, we study the consequences of shaping work arrangements to accomodate expectations of women's domestic responsibilities. We randomly assign women to receive one of five jobs that vary along the ability to (i) flexibly choose work hours, (ii) multitask work with childcare, and (iii) work from home, and we estimate the effects of these attributes on job take up. To separately identify the effects of flexibility on worker composition and job performance, we use randomly assigned surprise job offer upgrades. Jobs are implemented over the course of one month, and a post-job survey measures effects on the gender attitudes of women and their children. Two to three months after the initial randomized controlled trial, we offer another set of jobs to participants to assess whether work experience increased future interest in work, particularly in less flexible jobs.

We find three sets of results. First, flexible work arrangements are very effective at bringing women who were previously out of the labor force into paid work. Varying different dimensions of job flexibility, we document that the the effect of flexibility on labor supply is driven by the ability to multitask (combining work with childcare) and to work from home. These are the deciding factors in whether or not to work for many women. Second, flexible work arrangements act as a stepping stone to less flexible jobs, including outside-the-home work. Job flexibility makes a large difference to women's labor supply—especially the labor supply of those from more traditional households—and experience with flexible jobs in turn shifts the gender attitudes of these women to be less supportive of traditional household roles. Our results highlight that there is a mutually reinforcing relationship between women's actual employment and gender attitudes that support women's work. Last, we examine the incentives of employers to introduce flexible work arrangements, and we find that work-from-home decreases worker performance in terms of both accuracy and speed. In both cases, a higher arrival rate of interruptions combined with flow effects leads to lower performance when working from home.

One implication of the gateway jobs finding is that a gradual approach to transitioning women from unpaid home production to market labor, through intermediate "stepping stone" jobs, could be effective. However, in order for this approach to not trap women in lower-paying, at-home jobs, the intermediate jobs may need to be temporary so that they do not become an absorbing equilibrium, although the job cannot be so temporary that households do not adjust their attitudes to align with a new reference point (Gulesci et al., 2023). In our study, we only offer women one job at a time: their choice is always to take the job or not have any job at all. This raises a question for future research: if women are given the option to continue working from home indefinitely, is it possible that flexible work arrangements could result in a more gender-segregated labor market? If so, what would help to ensure that women do not get "trapped" in jobs that are more flexible but also more precarious or less well paid?

Another policy implication of these findings is that offering flexible work arrangements would likely be an effective strategy for the recruitment and retention of female workers. If firms have work that can be completed from home, then it could be in the firm's best interest to allow workers to work from home, as this increases the number of potential workers they could access. However, the negative effect of work-from-home on productivity might discourage firms from offering greater job flexibility. That said, as we document in Jalota and Ho (2024), women's labor supply is very inelastic, and according to our estimates it may be possible to offset the negative effect on productivity with lower wages.

Even taking the negative effect on productivity into account, however, there are at least two reasons that work-from-home arrangements may be *underprovided*. One is that employers may lack the information or skills to offer flexible work arrangements at the efficient level. Employers may underestimate the pool of workers that they miss out on by not offering flexible work arrangements, either in terms of the size or skill of the worker population whose labor supply is marginal to flexibility. Employers may also be interested in introducing greater job flexibility, but may not know how to introduce flexible work arrangements without sacrificing worker performance. In addition, employers may not internalize the positive effect of home-based work experience on women's future labor supply in outside-the-home work. Better understanding firms' decisions to offer flexible work arrangements, as well as their willingness to experiment with work-from-home, may be a fruitful area for future research especially in countries with low rates of women's participation in paid work.

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## Tables

Table 1: Description of Work Arrangements

| Work Arrangement | Time Flexibility | Multitasking with Childcare | Work From Home |
|---|---|---|---|
| 1. Most Flexible | Yes | Yes | Yes |
| 2. Time-Inflexible | No | Yes | Yes |
| 3. Child-Inflexible | Yes | No | Yes |
| 4. Time- & Child-Inflexible | No | No | Yes |
| 5. Office | (Yes) | No | No |

*Notes:* This table presents the five work arrangements included in the experiment. The work arrangements varied in three characteristics: flexibility in (i) time, (ii) multitasking work with childcare, and (iii) work-from-home. In work arrangements that were time-flexible, workers could work at any hours they wished throughout the job, while in time-inflexible arrangements they had to commit to a particular timeslot and then could only work during that shift for the rest of the job. In work arrangements that included the ability to multitask work with childcare, workers were told that it would be completely all right for them to have children next to them while they work, while in jobs without this feature, workers were told that they could not have children next to them while working. In the office work arrangement, workers had to come to a nearby office to complete their tasks. There were eight offices set up (one in each area or neighborhood of recruitment). The offices were open from 10am-6pm six days per week.

Table 2: Gateway Jobs—Effect of Round 1 Job Assignment on Starting Work in Round 2

| | Started Work During Jobs Round 2 | | | | | |
|---|---|---|---|---|---|---|
| | All Participants | | Previous Work Experience | | | |
| | | | No | | Yes | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| R1 More Flexible Than R2 | 0.06* | 0.05* | 0.07** | 0.07** | 0.02 | 0.03 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.06) | (0.06) |
| R1 Less or Equally Flexible Than R2 | -0.02 | -0.02 | -0.01 | -0.02 | -0.03 | -0.04 |
| | (0.04) | (0.05) | (0.04) | (0.06) | (0.07) | (0.10) |
| Observations | 1,524 | 1,524 | 1,049 | 1,049 | 475 | 475 |
| R2 work arrangement FE | Yes | Yes | Yes | Yes | Yes | Yes |
| R1 work arrangement FE | No | Yes | No | Yes | No | Yes |
| Strata FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Lasso selected controls | Yes | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table presents results on effects of the initial jobs RCT on starting work during the second round of jobs.

- The outcome variable in all columns is an indicator variable equal to 1 if the participant starts the job that she was randomly assigned to during round 2 and is otherwise equal to 0.
- The omitted group is the control group. Treatment group participants are categorized by the relative flexibility of their randomly-assigned round 1 (R1) versus round 2 (R2) job assignments.
- Columns (1) and (2) report the effects of round 1 job assignment on round 2 job take up for all participants. Columns (3)-(6) report effects separately by whether or not a participant had previous paid work experience before the study.
- Odd-numbered columns report results when controlling for round 2 work arrangement fixed effects. Even-numbered columns report results controlling for both round 2 and round 1 work arrangement fixed effects. All columns include strata fixed effects and controls selected by double post lasso.
- Huber-White standard errors in parentheses $(\cdot)$ are robust to heteroskedasticity. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Table 3: Treatment Effect of Work-From-Home on Performance

| | Effects of work-from-home on performance | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | *Panel A: Speed* | | | |
| Upgraded to work-from-home | 5.09** | 5.31*** | 5.32** | 5.10** | 5.14** | 5.07** |
| | (2.11) | (2.01) | (2.04) | (2.04) | (2.04) | (2.06) |
| Upgraded to work-from-home | 3.35** | 3.60** | 3.67** | 3.36** | 3.42** | 3.65** |
| | (1.59) | (1.48) | (1.51) | (1.52) | (1.51) | (1.51) |
| Difficult task | 17.3*** | 17.5*** | 17.2*** | 17.3*** | 17.5*** | 18.1*** |
| | (1.76) | (1.77) | (1.79) | (1.76) | (1.76) | (2.15) |
| Upgraded to work-from-home × difficult task | 5.84** | 5.85** | 5.67** | 5.85** | 5.86** | 5.05** |
| | (2.34) | (2.35) | (2.34) | (2.34) | (2.34) | (2.35) |
| Observations | 227,994 | 227,994 | 227,994 | 227,994 | 227,994 | 227,994 |
| Office Seconds Per Task | 25.1 | 25.1 | 25.1 | 25.1 | 25.1 | 25.1 |
| | | | *Panel B: Accuracy* | | | |
| Upgraded to work-from-home | -0.081*** | -0.082*** | -0.082*** | -0.082*** | -0.082*** | -0.082*** |
| | (0.027) | (0.027) | (0.027) | (0.027) | (0.027) | (0.027) |
| Upgraded to work-from-home | -0.022 | -0.023 | -0.025 | -0.023 | -0.024 | -0.028 |
| | (0.024) | (0.024) | (0.024) | (0.024) | (0.024) | (0.024 |
| Difficult task | -0.083** | -0.084** | -0.081** | -0.083** | -0.084** | -0.12*** |
| | (0.033) | (0.033) | (0.036) | (0.033) | (0.033) | (0.043 |
| Upgraded to work-from-home × difficult task | -0.20*** | -0.20*** | -0.19*** | -0.20*** | -0.20*** | -0.18*** |
| | (0.054) | (0.054) | (0.055) | (0.054) | (0.054) | (0.053) |
| Observations | 231,017 | 231,017 | 231,017 | 231,017 | 231,017 | 231,017 |
| Office Average Accuracy | 1.82 | 1.82 | 1.82 | 1.82 | 1.82 | 1.82 |
| Number Previous Tasks Completed | ✓ | ✓ | | | | |
| Number Previous Tasks Completed Squared | | ✓ | | | | |
| Previous Tasks Completed Fixed Effects | | | ✓ | | | |
| Number Previous Tasks Attempted | | | | ✓ | ✓ | |
| Number Previous Tasks Attempted Squared | | | | | ✓ | |
| Previous Tasks Attempted Fixed Effects | | | | | | ✓ |

*Notes:* This table presents effects of work from home on job performance, and then differentially by task difficulty. It compares individuals who were randomly chosen for an upgrade from an inflexible, office-based job to the most flexible, work-from-home job after they accepted the less flexible job to those who were not randomly chosen. This holds worker type constant, and compares performance across flexible and inflexible work arrangements.

- In Panel A, the outcome variable is the seconds taken to complete a task. In Panel B, the outcome is the accuracy score assigned to a task. In both Panels A and B, the omitted group is individuals who accepted a less flexible job but were not randomly upgraded to the flexible, work-from-home job.
- Upgraded to work-from-home takes value 1 if the participant was randomly chosen for an upgrade to the most flexible work-from-home job and 0 otherwise. Difficult job takes value 1 if the task is one of the three difficult task types and 0 otherwise.
- Columns (1) and (2) include controls for the number of tasks completed before the task, Column (2) also controls for non-linear effects of prior task completion. Column (3) includes fixed effects for all tasks that were previously completed. Column (4) and (5) control for number of tasks previously attempted, Column (5) also controls for non-linear effects. Column (6) includes fixed effects for previously attempted tasks.
- Standard errors in parentheses $(\cdot)$ are clustered at the worker level. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Table 4: Characterizing work-from-home compliers

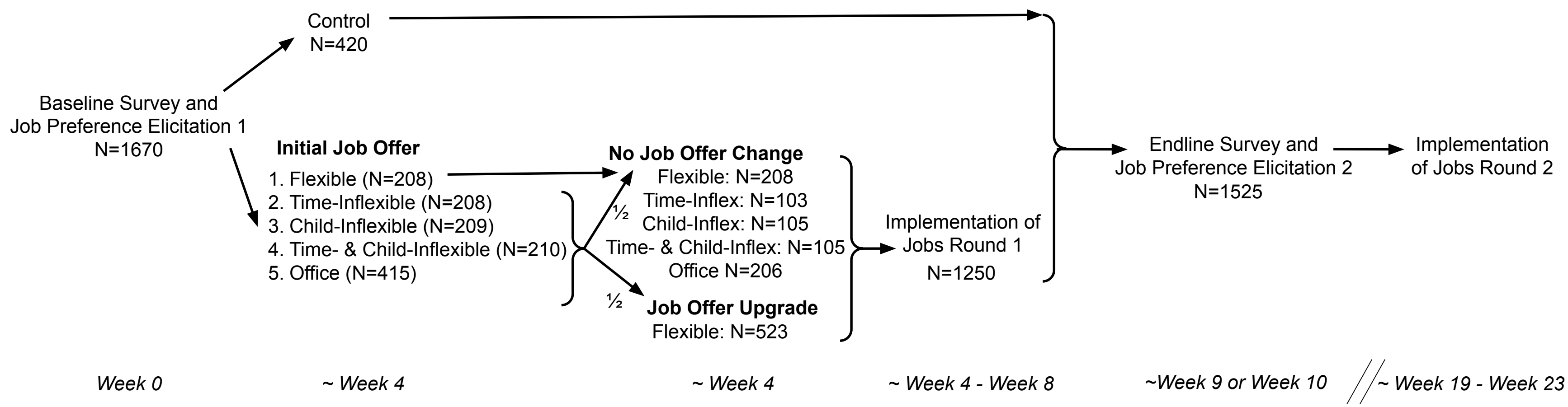
| | Characterizing work-from-home compliers | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | | | *Panel A: Speed* | | | |
| Work-from-home compliers | 2.04 | 1.59 | 1.49 | 1.92 | 2.04 | 2.07 |
| | (1.98) | (1.79) | (1.77) | (1.95) | (1.95) | (1.97) |
| Work-from-home compliers | 2.07 | 1.59 | 1.56 | 1.94 | 2.08 | 1.99 |
| | (1.58) | (1.40) | (1.40) | (1.55) | (1.55) | (1.55) |
| Difficult Task | 23.1*** | 23.3*** | 22.7*** | 23.2*** | 23.4*** | 22.9*** |
| | (1.53) | (1.53) | (1.54) | (1.53) | (1.53) | (1.63) |
| Work-from-home compliers × Difficult Task | 0.057 | 0.094 | -0.077 | 0.069 | 0.092 | 0.273 |
| | (2.10) | (2.11) | (2.05) | (2.10) | (2.10) | (2.09) |
| Observations | 409,300 | 409,300 | 409,300 | 409,300 | 409,300 | 409,300 |
| Office Accepters Average Seconds | 27.9 | 27.9 | 27.9 | 27.9 | 27.9 | 27.9 |
| | | | *Panel B: Accuracy* | | | |
| Work-from-home compliers | -0.000 | -0.001 | 0.001 | 0.000 | 0.001 | 0.001 |
| | (0.029) | (0.029) | (0.030) | (0.029) | (0.029) | (0.030) |
| Work-from-home compliers | -0.007 | -0.005 | -0.005 | -0.005 | -0.006 | -0.006 |
| | (0.023) | (0.023) | (0.024) | (0.024) | (0.023) | (0.024) |
| Difficult task | -0.28*** | -0.28*** | -0.27*** | -0.28*** | -0.28*** | -0.29*** |
| | (0.043) | (0.043) | (0.044) | (0.043) | (0.043) | (0.047) |
| Work-from-home compliers × difficult task | 0.020 | 0.020 | 0.016 | 0.020 | 0.020 | 0.021 |
| | (0.056) | (0.056) | (0.057) | (0.056) | (0.056) | (0.056) |
| Observations | 416,562 | 416,562 | 416,562 | 416,562 | 416,562 | 416,562 |
| Office Accepters Average Accuracy | 1.71 | 1.71 | 1.71 | 1.71 | 1.71 | 1.71 |
| Number Previous Tasks Completed | ✓ | ✓ | | | | |
| Number Previous Tasks Completed Squared | | ✓ | | | | |
| Previous Tasks Completed Fixed Effects | | | ✓ | | | |
| Number Previous Tasks Attempted | | | | ✓ | ✓ | |
| Number Previous Tasks Attempted Squared | | | | | ✓ | |
| Previous Tasks Attempted Fixed Effects | | | | | | ✓ |

*Notes:* This table characterizes the job performance of flexibility compliers. It compares individuals who were randomly offered a flexible, work-from-home job to those who were offered and accepted inflexible, office-based-work but then randomly upgraded to the flexible, home-based job. This holds work type constant, and compares individuals who are marginal to flexibility to those who are not.

- In Panel A, the outcome variable is the seconds taken to complete a task. In Panel B, the outcome is the accuracy score assigned to a task. In both Panels A and B, the omitted group is individuals who accepted office-based work before being randomly upgraded.
- Work-from-home compliers takes value 1 if an individual accepted flexible, home based work and 0 otherwise. Difficult job takes value 1 if the task is one of the three difficult task types and 0 otherwise.
- Columns (1) and (2) include controls for the number of tasks completed before the task, Column (2) also controls for non-linear effects of prior task completion. Column (3) includes fixed effects for all tasks that were previously completed. Column (4) and (5) control for number of tasks previously attempted, Column (5) also controls for non-linear effects. Column (6) includes fixed effects for previously attempted tasks.
- Standard errors in parentheses $(\cdot)$ are clustered at the worker level. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

## Figures

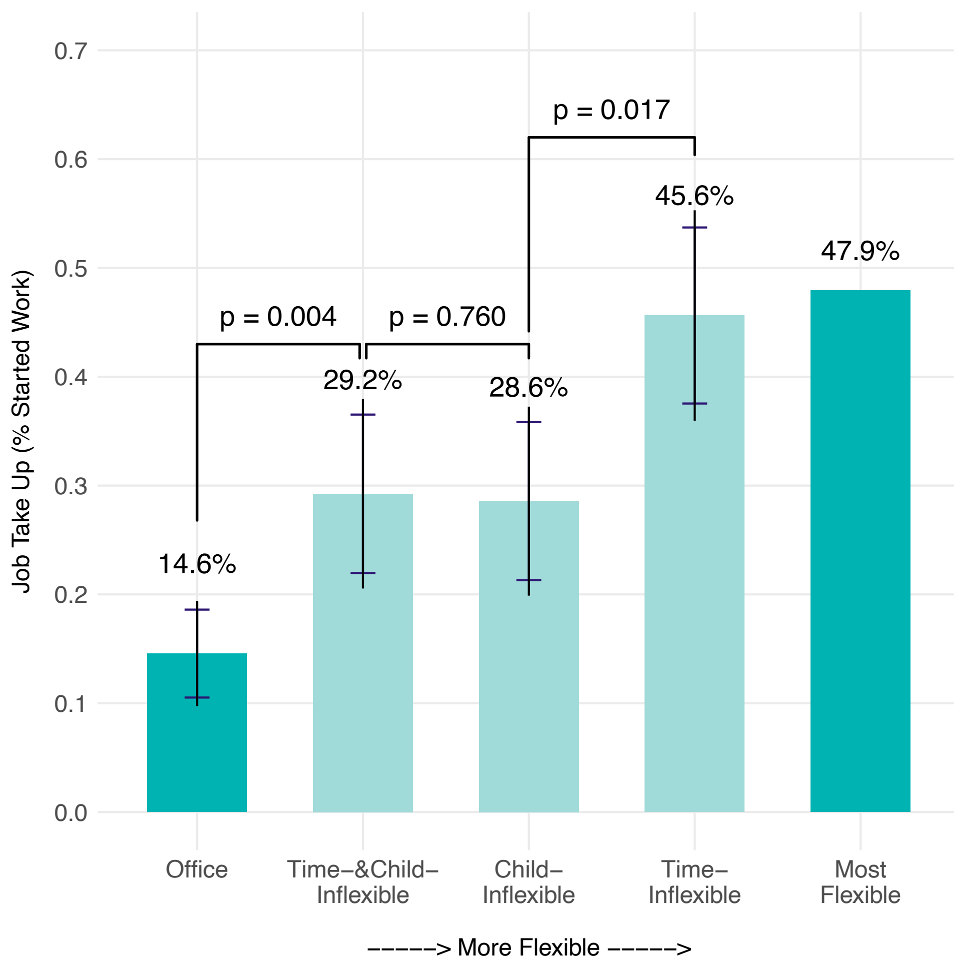
Figure 1: Experimental Design




*Notes:* This figure visualizes the experimental design and timeline.

- Eligible households complete a baseline survey, with one part for women and one optional part for children aged 8-18. The baseline survey for women includes modules about (i) demographics, household composition, and previous work experience, (ii) technology use, (iii) time use, (iv) gender attitudes, (v) agency, (vi) psychological wellbeing, (vii) bank use, and (viii) social contacts. As part of the baseline survey, women also complete a job preferences elicitation which involves stating whether or not they would accept each of the five work arrangements offered as part of the study. The baseline survey for children includes modules about (i) aspirations, (ii) gender attitudes, and (iii) their role in home production.
- 1,670 households are randomized into receiving a job offer or to the control group. The jobs vary along three dimensions: (1) the ability to flexibly choose work hours, (2) the ability to multitask work with childcare, and (3) the ability to work from home. Time-flexibility and childcare-flexibility are cross-randomized, resulting in five job groups.
- After deciding whether or not to accept the job offer, half of the participants who initially received an inflexible job are randomly selected for an upgrade to the most flexible job. This surprise upgrade allows us to separately measure selection into flexible work arrangements (characterizing the "flexibility compliers") and to estimate the treatment effects of the flexible work arrangements on job performance, mirroring the design in Karlan and Zinman (2009). After this final job offer, participants start their part-time, month-long job that consists of microtasks that can be done on a smartphone. The purpose of the microtasks is to build datasets to train speech recognition algorithms.
- Within two weeks of job completion, participating women and children complete an endline survey. The children's survey includes the same modules as the baseline survey, with some questions modified. The endline survey for women includes modules on (i) household members' labor supply, (ii) gender attitudes, (iii) agency, (iv) psychological wellbeing, and, if the woman participated in the intervention, (v) her experience with the job.
- As part of the endline survey, women also complete another job preferences elicitation that involves making 7 incentivized choices between jobs and gifts. We use the strategy method to incentivize the choices, randomly selecting one of the decisions to be implemented as "Jobs Round 2." This second round of jobs includes digital and non-digital job options, and varies in flexibility along the same dimensions as the initial intervention (work hours, multitasking work with childcare, and working from home). The digital jobs are the same as in the initial intervention (contributing to speech datasets), and the non-digital jobs involve sewing masks and making jewellery. In order to estimate a real-stakes treatment effect on interest in future work, jobs in the second round are fully implemented for the same duration as the initial intervention jobs.

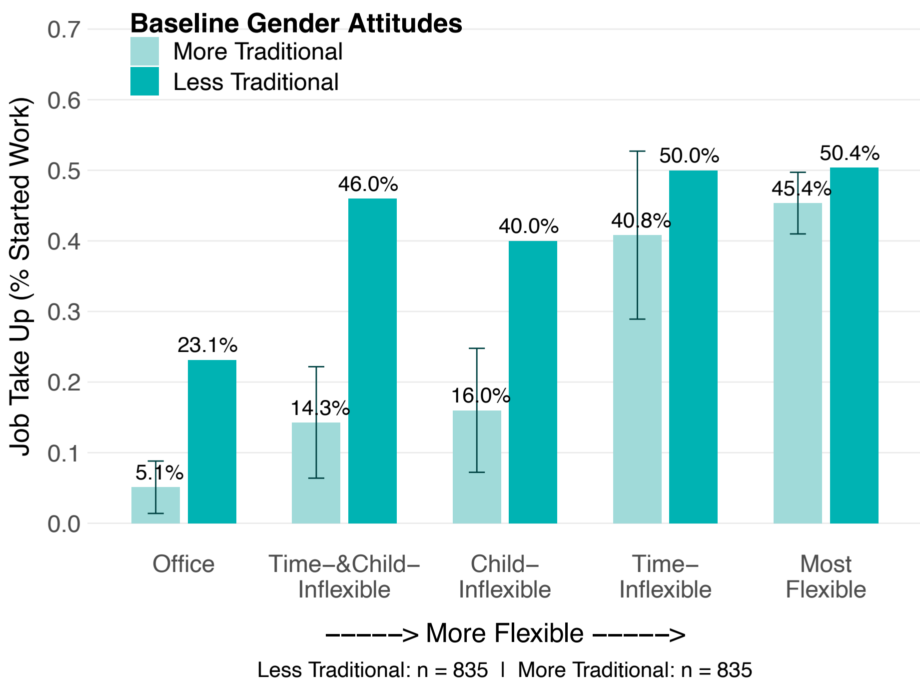
Figure 2: Impact of flexible work arrangements on take up of jobs



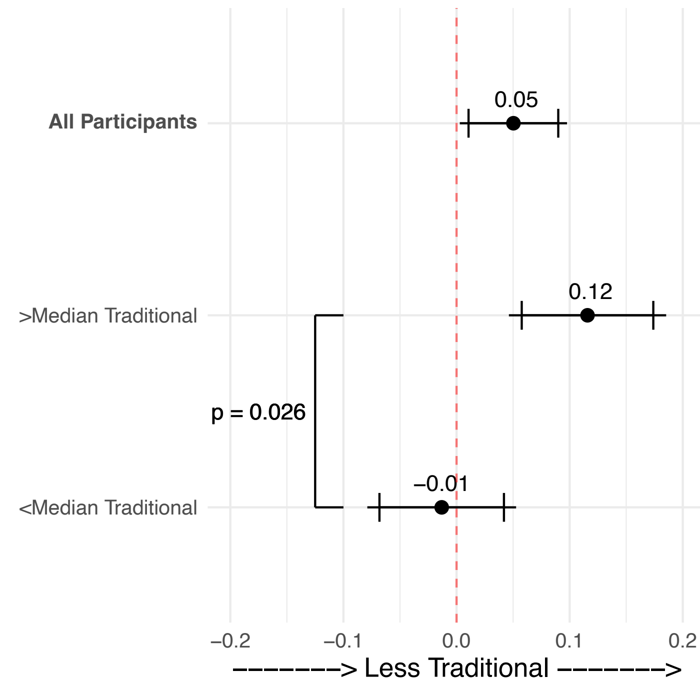

*Notes:* This figure plots the take up rate for each of the five jobs during the initial intervention (Jobs Round 1). The sample consists of the 1,250 women in treatment households, each of whom receives one randomly assigned job offer. Take up is measured as a binary variable equal to one if the participant starts work (i.e. submits any completed tasks to the employer). The whiskers indicate 90% and 95% confidence intervals from a regression of job take up on dummy variables for each of the four jobs other than "Flex," which is the most flexible work arrangement. This figure plots the raw take-up rates for each of the jobs, while the standard errors for these regressions, along with pairwise tests of equality between job take up rates, are presented in column 1 of Appendix Table A4. In these regressions, we control for strata fixed effects and estimate Huber-White heteroskedasticity-robust standard errors. The brackets above the bars show $p$-values for pairwise tests of equality between job take-up rates between work arrangements.

Figure 3: Relationship Between Job Flexibility, Gender Attitudes, and Women's Labor Supply

(a) Heterogeneity in Job Take Up by Baseline Gender Attitudes




(b) Treatment Effect of Labor Supply on Gender Attitudes



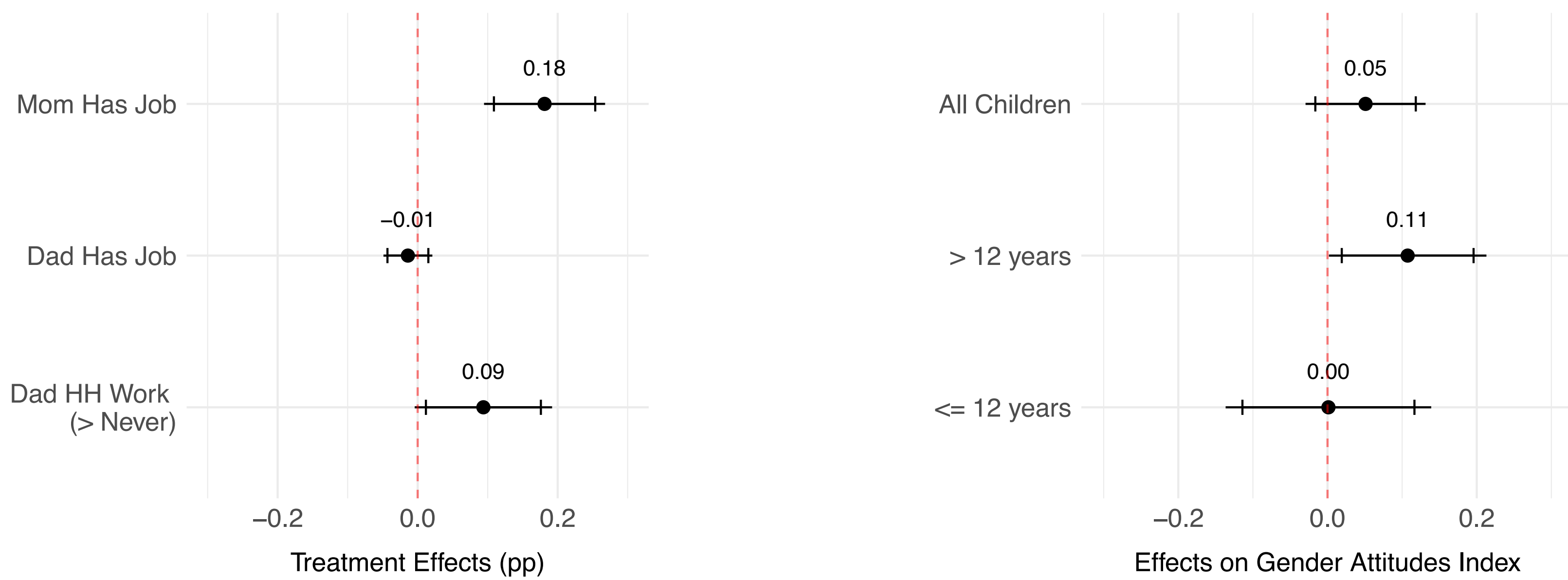

*Notes:* This figure shows empirical evidence for two relationships: (i) that traditional gender attitudes are associated with lower labor supply for women and mediate the importance of job flexibility (Panel A), and (ii) that work experience in turn shifts gender attitudes to become less traditional (Panel B).

- Panel A displays round 1 job take up rates across the five work arrangements, splitting the sample by baseline gender attitudes. The analysis sample consists of the 1,250 participants assigned to a round 1 job offer. The $x$-axis consists of the five different work arrangements, in order of increasing flexibility from left to right. Participants are classified as "less than median traditional" or "more than median traditional" according to their score on the baseline gender attitudes index, which is a standardized, weighted average of thirteen questions from the baseline survey (as in Anderson, 2008). See Appendix Section A.1 for more information on constructing the gender attitudes index. Confidence intervals at the 90% level are shown.
- Panel B plots treatment effects of the jobs intervention on women's gender attitudes. In this analysis, all treatment groups are pooled and compared to the control group ($N = 1,524$). Point estimates show the impact on the gender attitudes index for all participants (top), participants with more traditional attitudes at baseline (middle), and participants with less traditional attitudes at baseline (bottom). The $x$-axis measures the standardized effect size, with positive values indicating less traditional attitudes. The gender attitudes index is a standardized, weighted average of fifteen questions on the endline survey (as in Anderson, 2008). The index is standardized to have mean zero and standard deviation one in the control group. All regressions include lasso-selected controls and strata fixed effects. The whiskers denote 90% and 95% confidence intervals estimated using Huber-White heteroskedasticity-robust standard errors.

Figure 4: Treatment Effects on Children

(a) Effect on Children's Report of Parents' Work

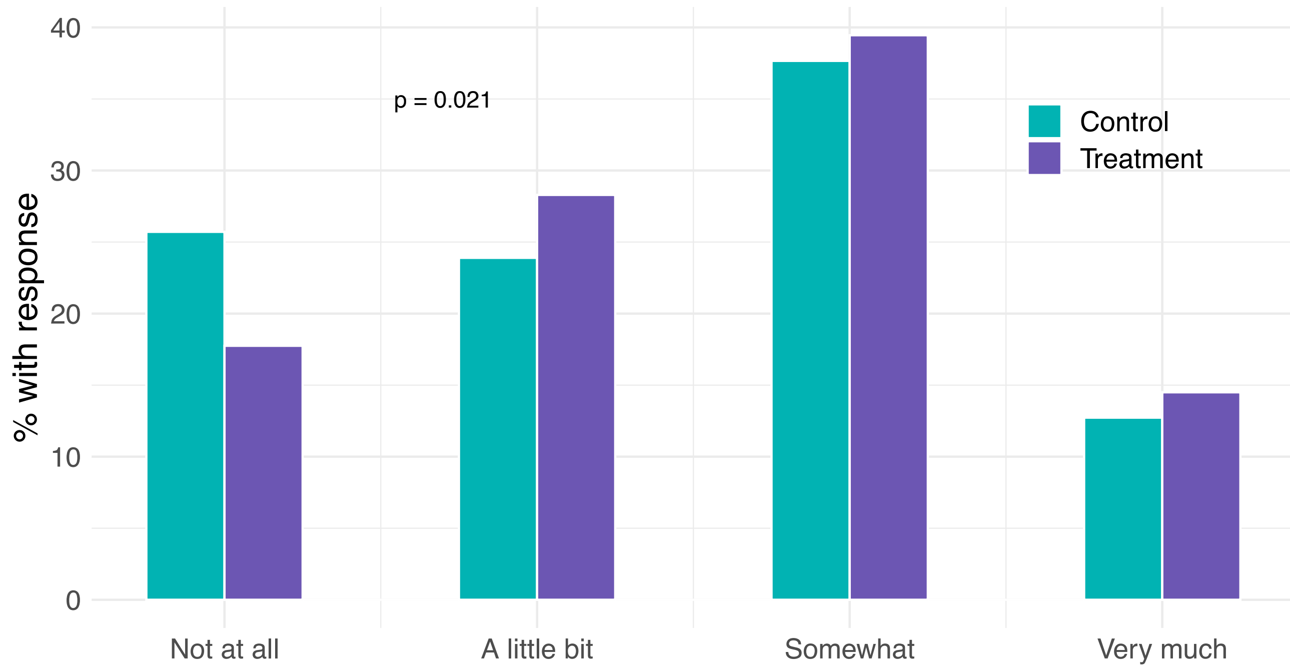
(b) Effect on Children's Attitudes




*Notes:* This figure reports treatment effects on children. All treatment groups are pooled and compared to the control group. Households are only included in the analysis sample if children participated in the survey at both baseline and endline ($N = 515$).

- Panel A reports effects on whether or not children say that, during the timeframe of the intervention, their mother had a job (top), their father had a job (middle), and their father helped more than never with cooking, cleaning, or laundry (bottom).
- Panel B reports effects on children's gender attitudes for all children pooled together (top), for children older than the median age of 12 (middle), and for younger children (bottom). The gender attitudes index is computed as a weighted average of questions from the endline survey, in which the weights take into account the covariance structure of the components as in Anderson (2008).
- All regressions include strata fixed effects and the baseline measure of the outcome variable. Standard errors are Huber-White heteroskedasticity robust. For the same results in table form, see Appendix Table A8 (Panel A) and Appendix Table A9 (Panel B).
- Estimates are plotted with corresponding 90% and 95% confidence intervals.

Figure 5: Perception of Whether Talents and Abilities Are Put to Good Use

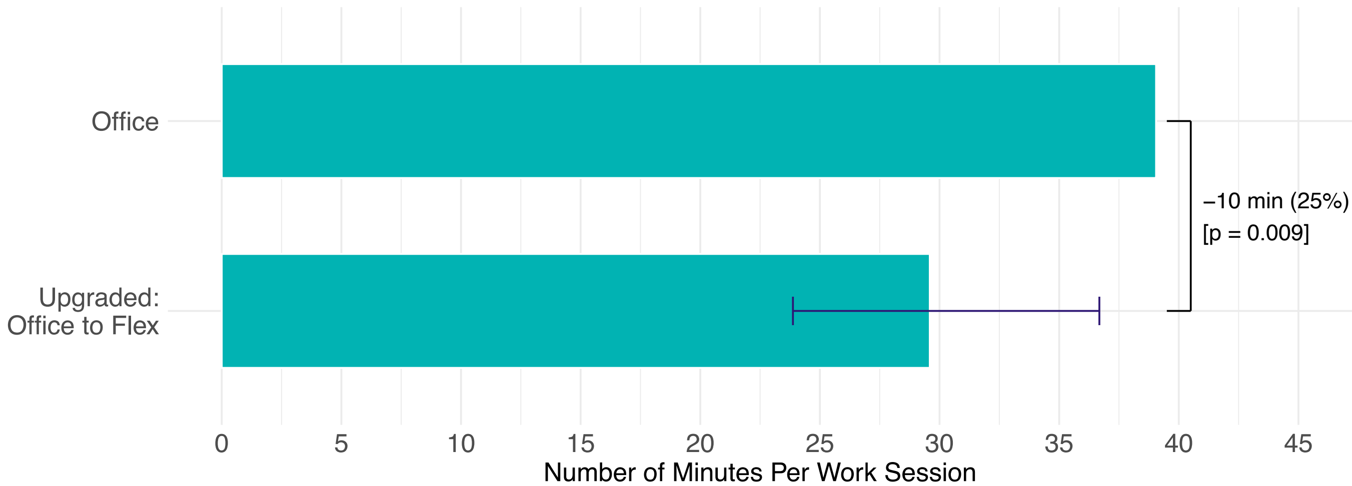



*Notes:* This figure plots participants' endline survey responses to the question, "Do you feel that your full potential and talents are put to good use?"

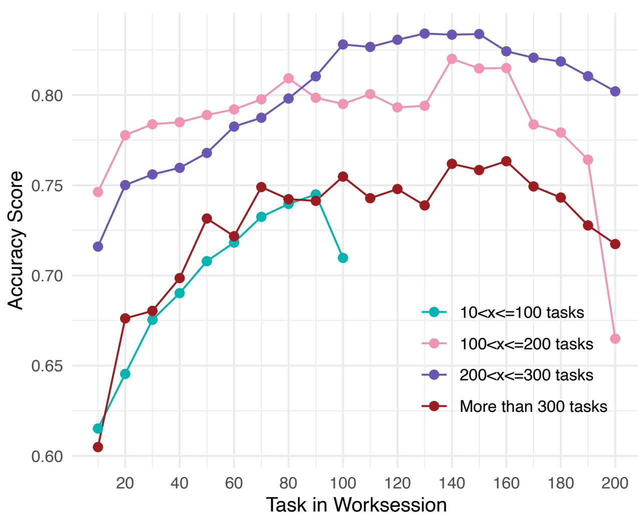
- Response distributions are plotted separately according to treatment assignment. The turquoise bars represent the share of control group participants with each response, while the purple bars represent the share of treatment group participants with each response. All treatment groups are pooled in this analysis.
- The reported *p*-value comes from a regression which codes participants' responses numerically, with "not at all" as 0, "a little bit" as 1, "somewhat" as 2, and "very much" as 3. The treatment group has a mean score of 1.50, while the control group has a mean score of 1.37. This outcome is regressed on treatment assignment, lasso-selected controls, and strata fixed effects. Standard errors are heteroskedasticity-robust.

Figure 6: Flow Effects

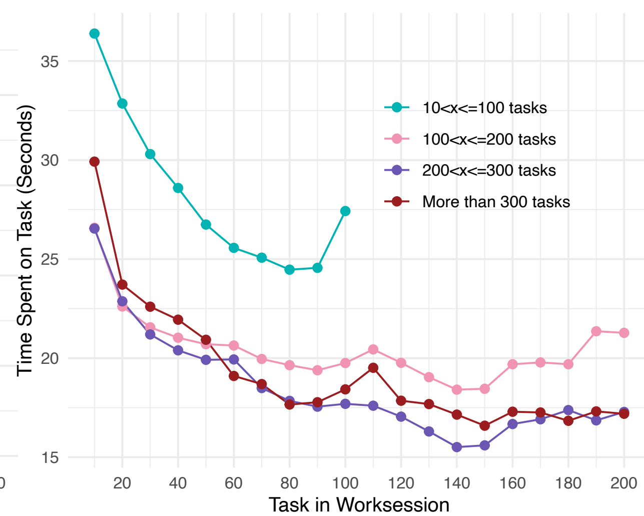
(a) Treatment Effect of Office on Worksession Length




(b) Task accuracy, by task within worksesion




(c) Time spent on task, by task within worksession

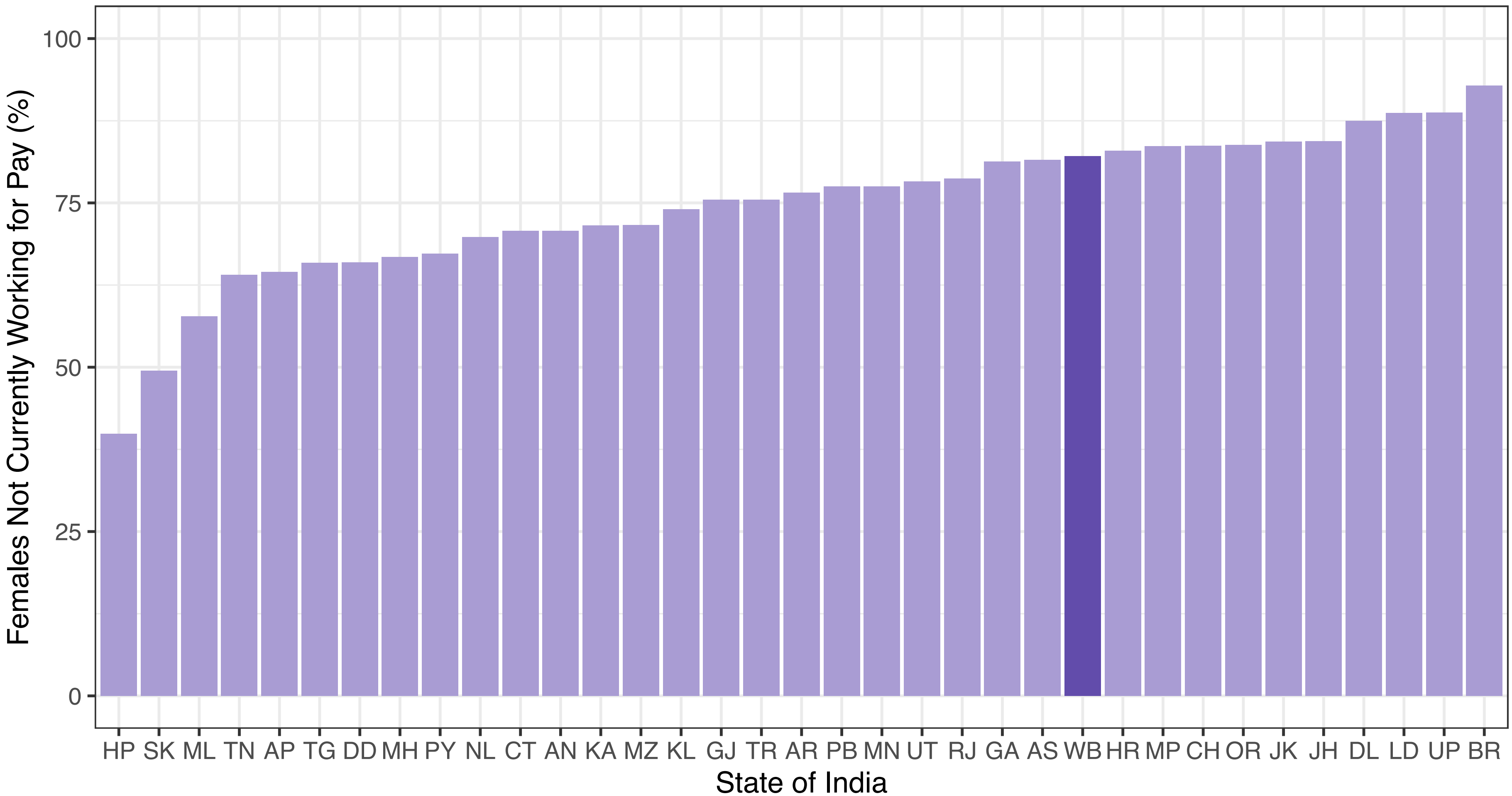



*Notes:* This figure illustrates how worksession interruptions lead to a decrease in worker performance.

- Panel A reports the treatment effects of work-from-home on worksession length. A worksession is defined as a continuous segment of time during which a worker submits tasks without taking a break. This subfigure compares the worksessions of workers who accepted an office job and are randomly selected for an upgrade to the most flexible job with those of workers who accept an office job and are not randomly selected for an upgrade. The bracket reports the difference in length between worksessions from home versus office, along with the *p*-value from a worksession-by-individual level regression estimating the effect of work-from-home on worksession length (standard errors clustered at the worker level).
- Panels C and D describe how two key inputs into productivity change over the course of a worksession. In order to capture "flow effects" rather than selection into longer versus shorter worksessions, worksessions are grouped according to their number of tasks. The dependent variable is task-level accuracy in Panel C and time spent per task in Panel D. Accuracy is assessed on a scale from 0-2 by an independent team of validators, and time spent per task is measured at the number of seconds that elapse between task initiation and submission.

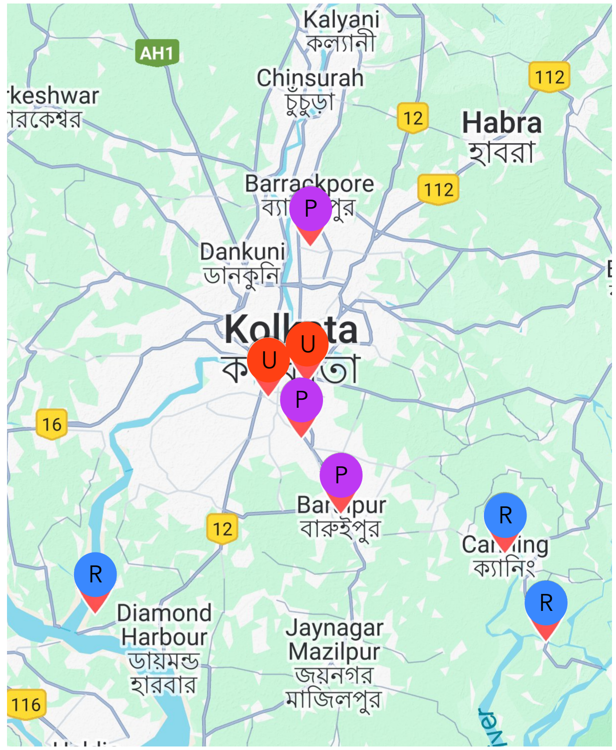
**Appendix Figures**

Figure A1: % Women Not Currently Working For Pay: States of India



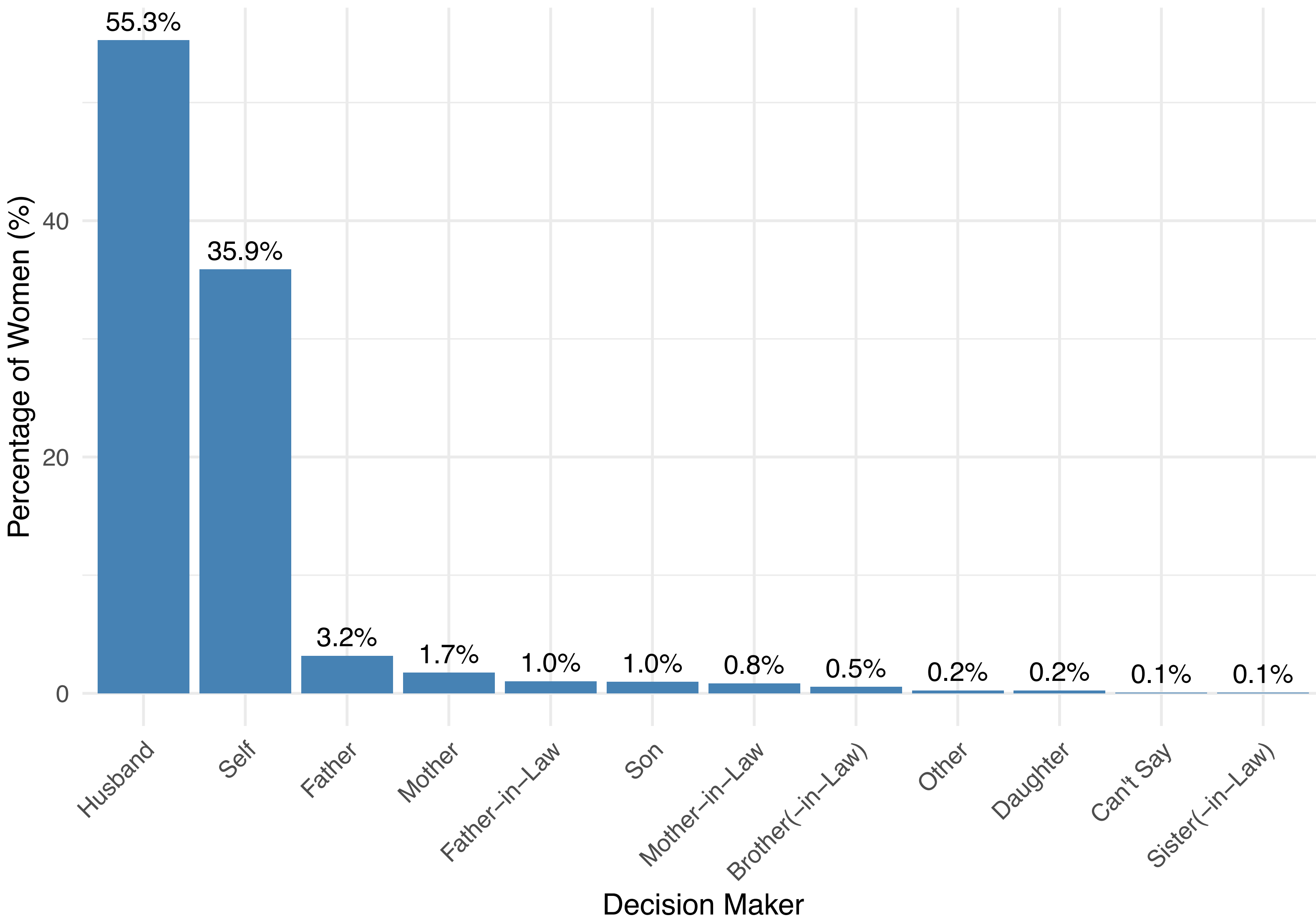

*Notes:* This figure plots the share of women between 18 and 60 years who currently work for pay. The sample excludes women in unpaid work. Data are from the Periodic Labour Force Survey (2021-2022)

Figure A2: Map of study areas



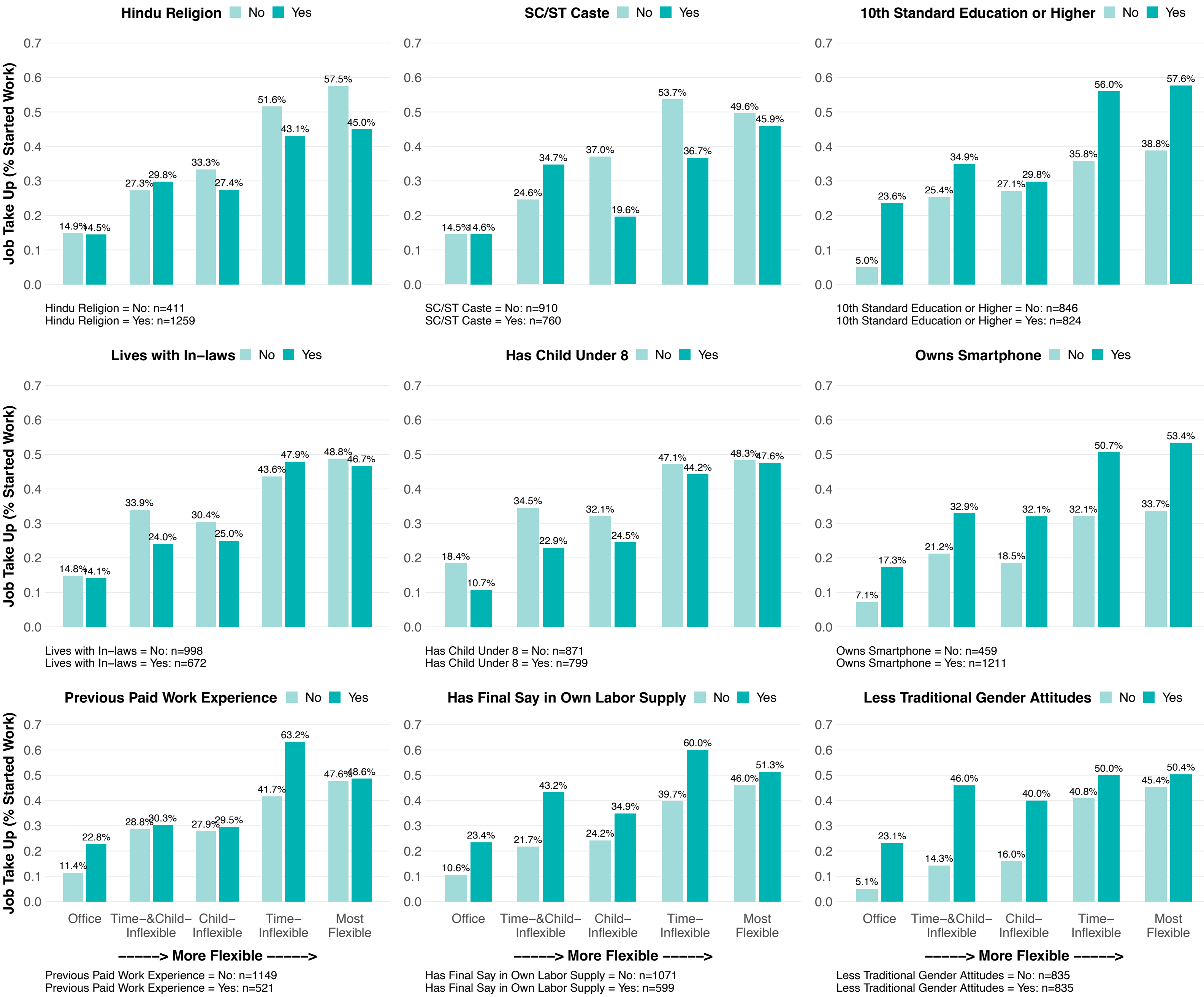

*Notes:* Figure presents locations of study areas. The red "U" bubbles indicate urban areas (Tiljala and New Alipore). The purple "P" bubbles indicate peri-urban areas (Sodepur, Atabagan, and Baruipur). The blue "R" bubbles indicate rural areas (Noorpur, Canning, and Basanti).

Figure A3: Who Has the Final Say in Women's Labor Supply Decisions?



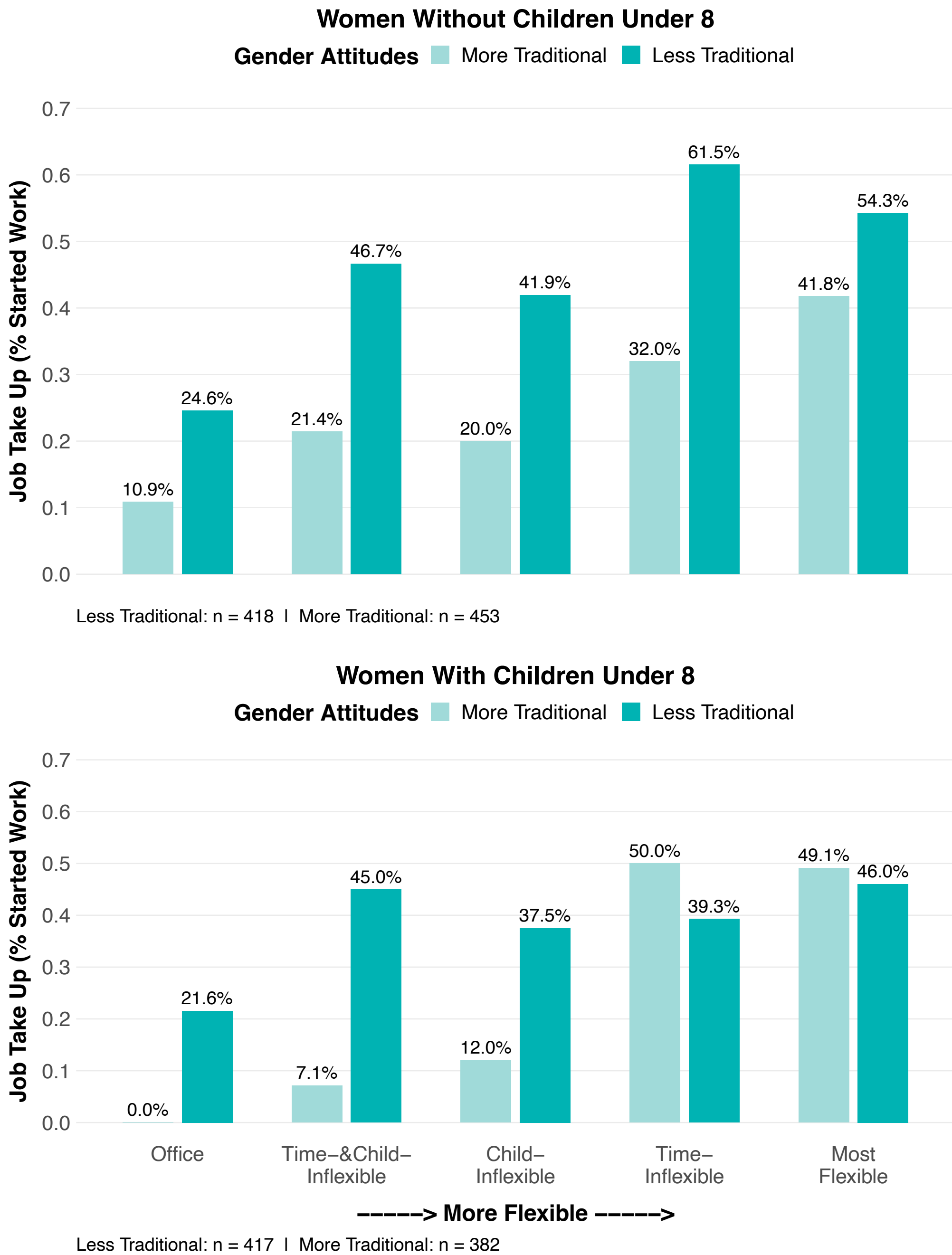

*Notes:* This figure displays the percentage distribution of responses to the question, "If you were offered a new job, who is the one person who would have the final say in whether or not you take it?" This question is asked during the baseline survey (N=1,670).

Figure A4: Heterogeneity in Job Take-up Rates by Flexibility and Respondent Characteristics




*Notes:* This figure displays job take-up rates (defined as the percentage of respondents who started work) across five job types with varying degrees of flexibility, split by nine binary respondent characteristics. Each panel represents the heterogeneous effects for a different characteristic: Hindu religion, SC/ST caste status, 10th grade education or higher, living with in-laws, having a child under age 8, smartphone ownership, never being previously employed, having the final say in one's own labor supply, and holding traditional gender attitudes. Job types are ordered from least flexible (Office) to most flexible (Flex), with three intermediate categories representing jobs with specific inflexibilities. Numbers below each panel indicate subsample sizes for respondents with and without each characteristic. Percentages shown above each bar represent the raw mean take-up rates. Data on characteristics are from the baseline survey. The sample in this figure consists of the women who were randomly selected to receive a job offer (N=1,250).

Figure A5: Job Take-Up Rates by Flexibility Type, Gender Attitudes, and Child Status




*Notes:* This figure presents job take-up rates (defined as percentage of respondents who started work) across five job types with varying degrees of flexibility, comparing women with more versus less traditional gender attitudes, separately for women with and without children under age 8. The top panel displays results for women without young children; the bottom panel displays results for women with children under age 8. Job flexibility increases from left to right: "Office" (fixed location, fixed hours, no childcare); "Time-&Child-Inflexible;; (fixed hours and no childcare); "Child-Inflexible" (flexible hours but no childcare); "Time-Inflexible" (fixed hours but childcare accommodated); and "Most Flexible" (flexible hours and childcare accommodated). Gender attitudes are measured as an index composed of thirteen questions on the baseline survey with weights based on the inverse-covariance matrix (Anderson, 2008). Respondents with an attitudes index value higher than the median are classified as holding "More Traditional" attitudes. The sample includes 1,250 women randomized into treatment arms, with observation counts for each subgroup displayed at the bottom of each panel.

Figure A6: Job Task Platform App Screenshots

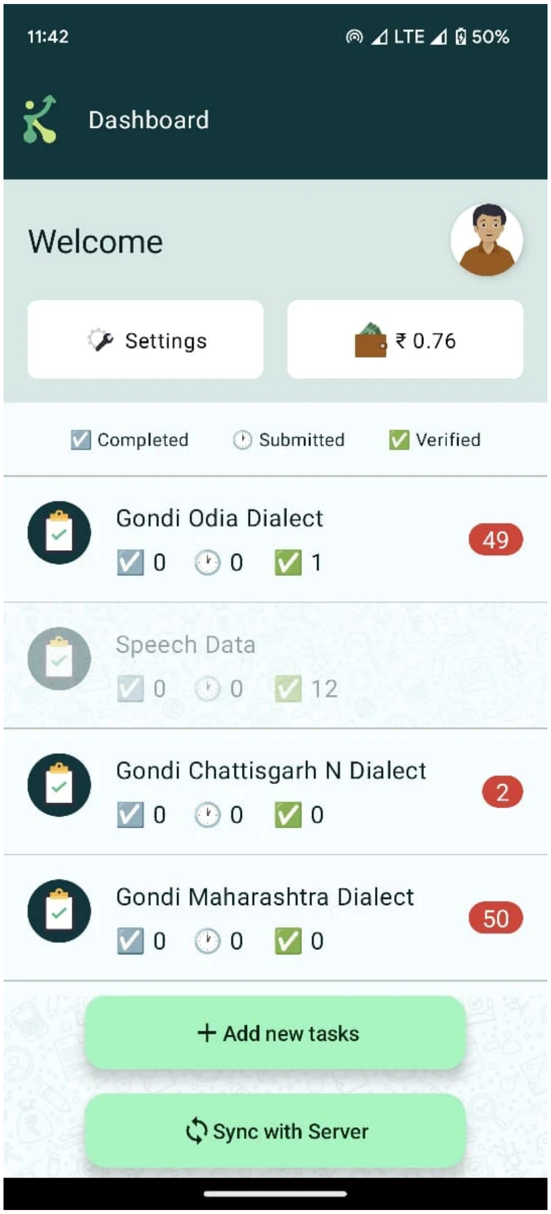
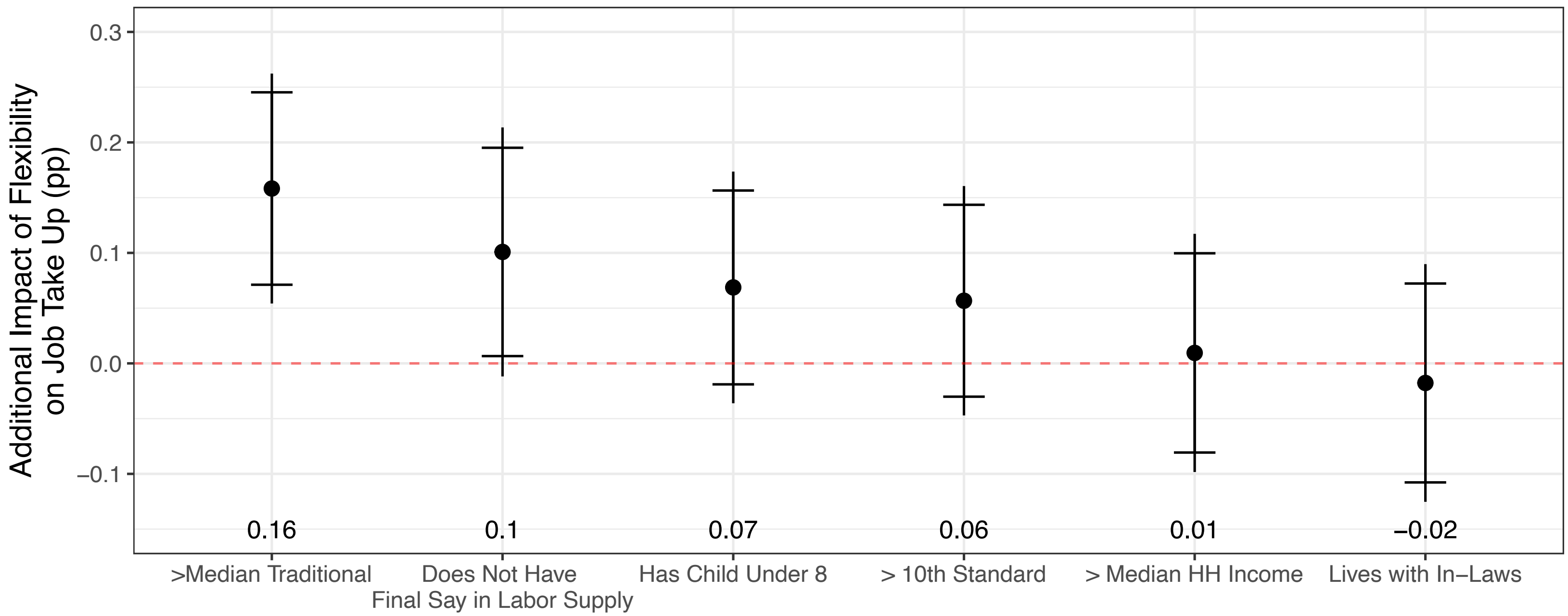


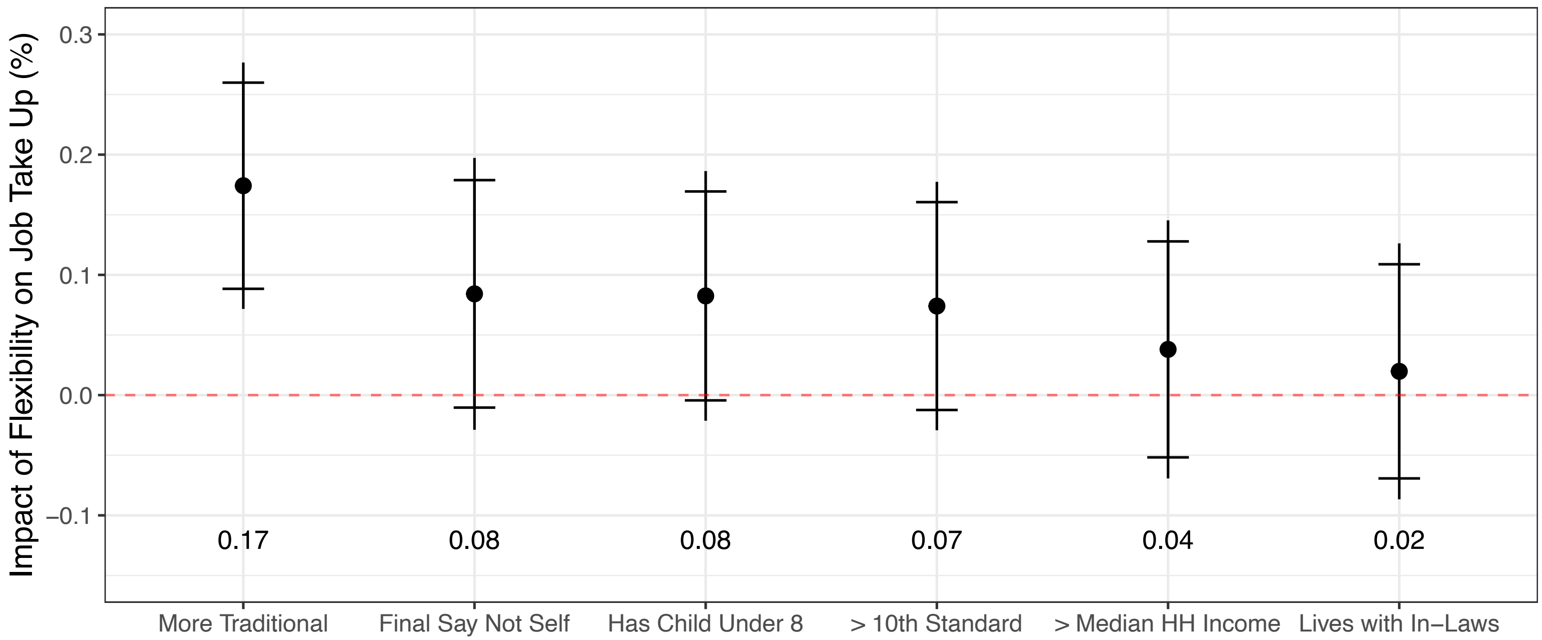

Figure A7: Heterogeneous Importance of Flexibility for Take Up of Work (Flexible = Most Flexible Only)




*Notes:* This figure shows how job flexibility affects take up differentially by worker characteristics.

- The plotted coefficients are the $\beta_3$ estimates from regressions that take the following form: $y_i = \beta_1 \mathbb{1}\{job_i = flexible\} + \beta_2 x_i + \beta_3 \mathbb{1}\{job_i = flexible\} \times x_i + \varepsilon_i$, where $y_i$ is a binary variable equal to one if participant $i$ starts the job that she was randomly assigned to $(job_i)$. $\mathbb{1}\{job_i = flexible\}$ is an indicator variable equal to one if $job_i$ is the most flexible job and equal to zero for all other jobs.
- The $\beta_3$ coefficient represents the additional importance of flexibility to job take up for women satisfying a particular characteristic (from left to right: having gender attitudes more traditional than the median participant; not having the final say in one's own labor supply; having a child younger than eight; being educated to at least the 10th grade (standard); having household income greater than the median participating household; living with at least one parent-in-law). All characteristics are binary and are defined to be associated with a greater importance of job flexibility to take up. Characteristics are ordered according to the magnitude of the labor supply response to job flexibility associated with them.
- Standard errors are heteroskedasticity-robust. Confidence intervals at the 90% and 95% levels are reported.

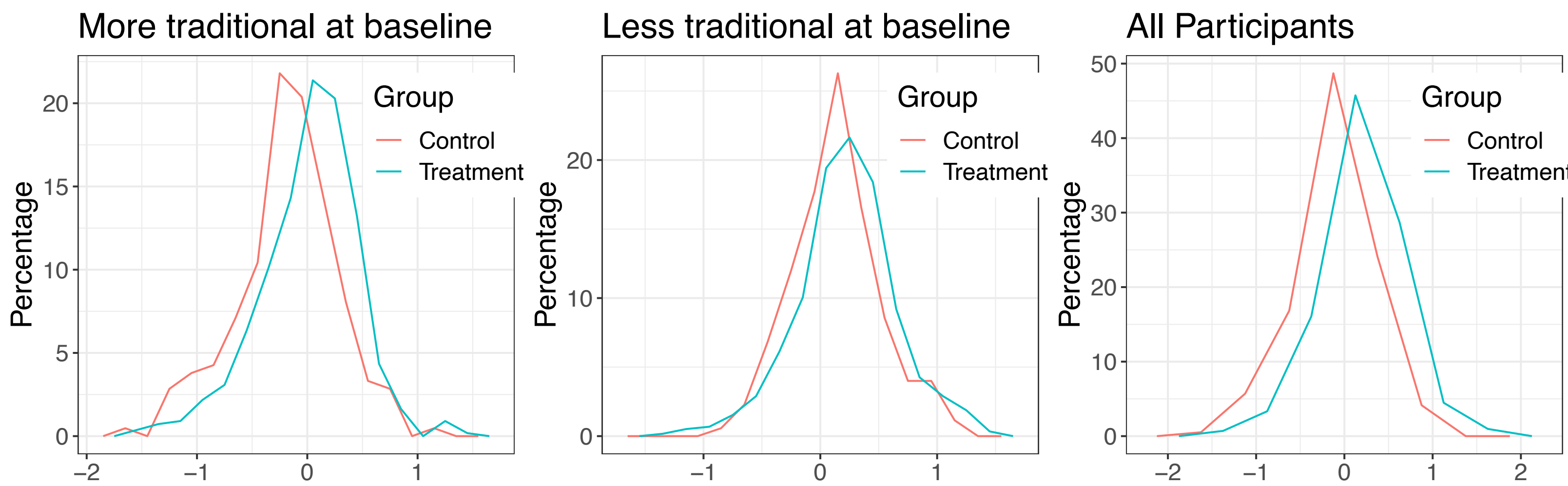
Figure A8: Heterogeneous Importance of Flexibility for Take Up of Work (Flexible = Most Flexible or Time-Inflexible)




*Notes:* This figure shows how job flexibility affects take up differentially by worker characteristics. This figure presents similar results to those of Figure A7 except, because the take-up rates are indistinguishable, classifies both the time-inflexible and most flexible jobs as "flexible."

- The plotted coefficients are the $\beta_3$ estimates from regressions that take the following form: $y_i = \beta_1 \mathbb{1}\{job_i = flexible\} + \beta_2 x_i + \beta_3 \mathbb{1}\{job_i = flexible\} \times x_i + \varepsilon_i$, where $y_i$ is a binary variable equal to one if participant $i$ starts the job that she was randomly assigned to $(job_i)$. $\mathbb{1}\{job_i = flexible\}$ is an indicator variable equal to one if $job_i$ is the most flexible job or the time-inflexible job, and $job_i$ is equal to zero for all other jobs.
- The $\beta_3$ coefficient represents the additional importance of flexibility to job take up for women satisfying a particular characteristic (from left to right: having gender attitudes more traditional than the median participant; not having the final say in one's own labor supply; having a child younger than eight; being educated to at least the 10th grade (standard); having household income greater than the median participating household; living with at least one parent-in-law). All characteristics are binary and are defined to be associated with a greater importance of job flexibility to take up. Characteristics are ordered according to the magnitude of the labor supply response to job flexibility associated with them.
- Standard errors are heteroskedasticity-robust. Confidence intervals at the 90% and 95% levels are reported.

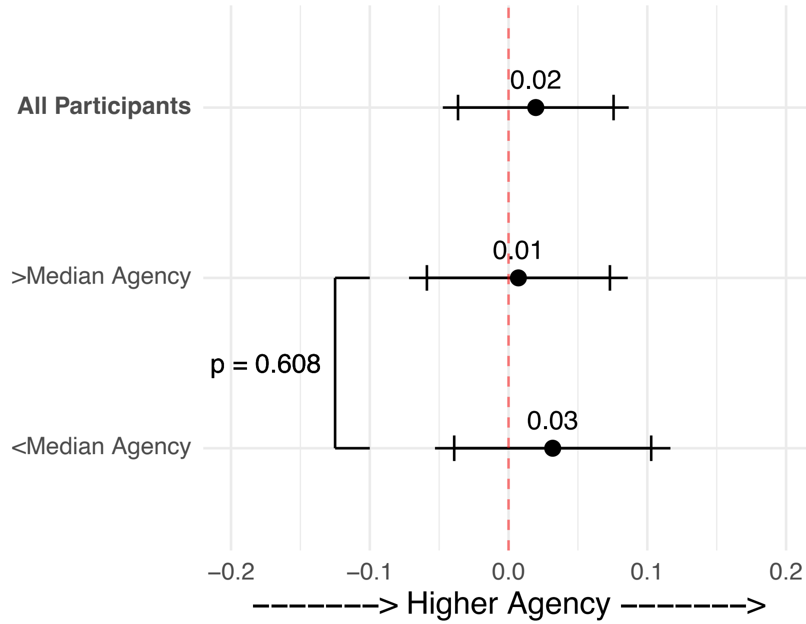
Figure A9: Gender Attitudes at Endline, Split by Baseline Attitudes (Histograms)




*Notes:* This figure plots the distribution of gender attitudes at endline separately for control group (red) and treatment group (blue) participants.

- The left panel plots endline gender attitudes for participants who were more traditional than the median participant at baseline.
- The middle panel plots endline gender attitudes for participants who were less traditional than the median participant at baseline.
- The right panel plots endline gender attitudes for all participants combined.

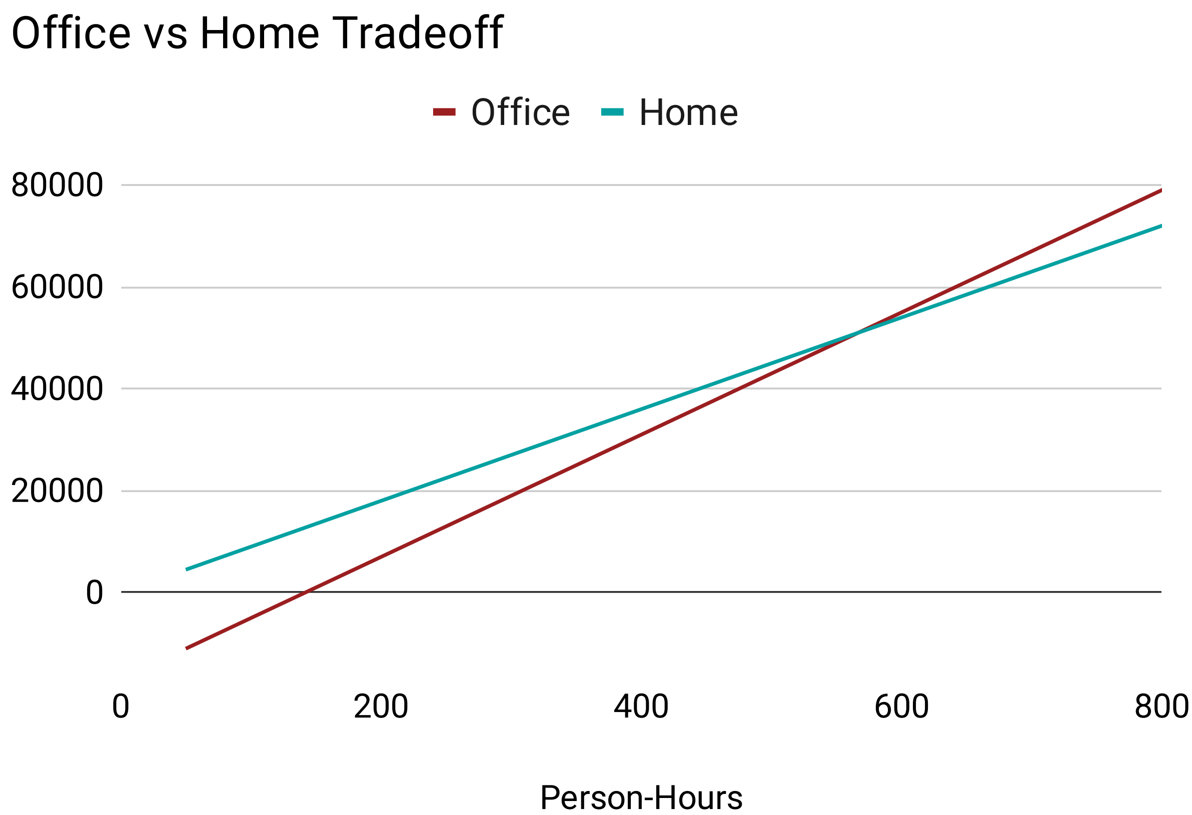
Figure A10: Treatment Effect of Jobs on Women's Agency




*Notes:* This figure presents treatment effects of the jobs intervention on an index of women's agency.

- In this analysis, all treatment groups are pooled and compared to the control group ($N = 1,524$). Point estimates show the impact on the agency index for all participants (top), participants with above-median baseline agency (middle), and participants with below-median baseline agency (bottom).
- The $x$-axis measures the standardized effect size, with positive values indicating higher agency. The agency index is a standardized, weighted average of seven questions on the endline survey, with weights on the different index components in proportion to their covariance (as in Anderson, 2008). The index is standardized to have mean zero and standard deviation one in the control group. The component questions are (i) whether the participant says that she herself would have the final say in her own labor supply decisions, (ii) how often the participant leave home alone, (iii) the participant's ability to leave home without asking permission, (iv) the participant's ability to meet friends without permission, (v) to what extent the participant can purchase clothes without asking permission, (vi) to what extent she can buy items from the market without permission, and (vii) whether the participant has a say in significant household purchases.
- All regressions include lasso-selected controls and strata fixed effects. The whiskers denote 90% and 95% confidence intervals estimated using Huber-White heteroskedasticity-robust standard errors.

Figure A11: Home-Office Tradeoff (Firm's Perspective)




The figure presents how the cost per task for work from home versus office varies with the total number of tasks.

## Appendix Tables

Table A1: Characteristics of Study Participants

| | Round 1 Work Arrangement (Initial) | | | | | | |
|---|---|---|---|---|---|---|---|
| | Control | Most Flexible | Time-Inflexible | Child-Inflexible | Time- and Child-Inflexible | Office | p-value |
| Age (Years) | 29.76 | 29.26 [-0.46] | 28.88 [-0.96] | 30.01 [0.38] | 30.62 [0.61] | 30.50 [0.86] | 0.592 |
| | (7.94) | (7.72) [0.420] | (8.36) [0.125] | (7.90) [0.544] | (8.61) [0.316] | (8.33) [0.087*] | |
| Educated to 10th Standard | 0.51 | 0.49 [-0.02] | 0.52 [0.02] | 0.50 [-0.01] | 0.43 [-0.07] | 0.50 [-0.01] | 0.617 |
| | (0.50) | (0.50) [0.611] | (0.50) [0.555] | (0.50) [0.853] | (0.50) [0.102] | (0.50) [0.849] | |
| Native Language = Bangla | 0.89 | 0.91 [0.02] | 0.91 [0.02] | 0.89 [-0.01] | 0.91 [0.02] | 0.89 [-0.00] | 0.542 |
| | (0.31) | (0.29) [0.261] | (0.29) [0.361] | (0.32) [0.702] | (0.28) [0.137] | (0.31) [0.753] | |
| Scheduled Caste/Scheduled Tribe | 0.38 | 0.36 [-0.02] | 0.38 [-0.02] | 0.42 [0.04] | 0.39 [0.01] | 0.39 [0.00] | 0.968 |
| | (0.49) | (0.48) [0.547] | (0.49) [0.609] | (0.49) [0.330] | (0.49) [0.845] | (0.49) [0.959] | |
| Other Backward Caste | 0.10 | 0.11 [0.01] | 0.09 [-0.01] | 0.05 [-0.04] | 0.11 [0.01] | 0.11 [0.01] | 0.936 |
| | (0.30) | (0.31) [0.568] | (0.28) [0.793] | (0.22) [0.059*] | (0.31) [0.622] | (0.31) [0.503] | |
| Religion = Hindu | 0.72 | 0.78 [0.05] | 0.74 [-0.00] | 0.78 [0.05] | 0.78 [0.04] | 0.76 [0.02] | 0.094 |
| | (0.45) | (0.41) [0.035**] | (0.44) [0.905] | (0.42) [0.075*] | (0.42) [0.092*] | (0.43) [0.312] | |
| Rural Location | 0.48 | 0.48 [-0.00] | 0.48 [-0.01] | 0.48 [-0.01] | 0.48 [-0.01] | 0.48 [-0.01] | 0.469 |
| | (0.50) | (0.50) [0.785] | (0.50) [0.397] | (0.50) [0.553] | (0.50) [0.754] | (0.50) [0.577] | |
| Married or Previously Married | 0.94 | 0.92 [-0.02] | 0.91 [-0.03] | 0.92 [-0.02] | 0.95 [0.01] | 0.93 [-0.01] | 0.298 |
| | (0.24) | (0.27) [0.306] | (0.28) [0.121] | (0.27) [0.396] | (0.21) [0.610] | (0.26) [0.529] | |
| Lives with In-Laws | 0.43 | 0.37 [-0.06] | 0.40 [-0.03] | 0.36 [-0.07] | 0.44 [0.01] | 0.39 [-0.05] | 0.119 |
| | (0.50) | (0.48) [0.117] | (0.49) [0.405] | (0.48) [0.068*] | (0.50) [0.791] | (0.49) [0.156] | |
| Has Children under 8 | 0.47 | 0.49 [0.01] | 0.48 [0.01] | 0.47 [-0.00] | 0.47 [0.01] | 0.49 [0.01] | 0.185 |
| | (0.50) | (0.50) [0.155] | (0.50) [0.260] | (0.50) [0.763] | (0.50) [0.557] | (0.50) [0.158] | |
| Individual (Not Shared) Smartphone | 0.73 | 0.74 [0.00] | 0.72 [0.01] | 0.74 [0.01] | 0.70 [0.01] | 0.72 [-0.00] | 0.366 |
| | (0.44) | (0.44) [0.443] | (0.45) [0.037**] | (0.44) [0.046**] | (0.46) [0.376] | (0.45) [0.701] | |
| Labor Supply Final Say = Self | 0.38 | 0.36 [-0.02] | 0.35 [-0.03] | 0.33 [-0.04] | 0.38 [0.01] | 0.35 [-0.03] | 0.382 |
| | (0.49) | (0.48) [0.648] | (0.48) [0.498] | (0.47) [0.290] | (0.49) [0.900] | (0.48) [0.380] | |
| F-test stat: | | 0.58 | 0.58 | 0.49 | 0.50 | 0.51 | |
| F-test $p$-value: | | 0.977 | 0.976 | 0.995 | 0.994 | 0.993 | |
| Number of observations | 420 | 208 | 208 | 209 | 211 | 414 | |

*Notes:* This table presents baseline characteristics of study participants randomized across treatment conditions. Each cell in the treatment columns reports the mean of the characteristic, with standard deviations in parentheses below. Sample sizes for each cell are reported in parentheses next to the means. The numbers in square brackets next to the group means represent coefficients from regressions of each characteristic on treatment indicators (with Control as the omitted category), controlling for strata fixed effects. $p$-values for these coefficients appear in square brackets below the coefficients. The final column reports $p$-values from tests comparing the Control group to all job groups pooled together. The bottom row presents F-statistics with their corresponding $p$-values. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Binary indicators (Bangla as native language, SC/ST, Hindu, rural, married/previously married, lives with in-laws, has children under 8, and smartphone ownership) take values of 0 or 1.

Table A2: Characteristics of Endline Survey Respondents

| | Round 1 Work Arrangement (Final) | | | | | | |
|---|---|---|---|---|---|---|---|
| | Control | Most Flexible | Time-Inflexible | Child-Inflexible | Time- and Child-Inflexible | Office | p-value |
| Age (Years) | 29.64 | 29.78 [0.18] | 29.36 [-0.34] | 30.82 [1.43] | 30.95 [1.27] | 30.62 [1.18] | 0.232 |
| | (7.81) | (7.95) [0.679] | (7.96) [0.677] | (8.71) [0.135] | (8.99) [0.124] | (8.23) [0.066*] | |
| Educated to 10th Standard | 0.53 | 0.48 [-0.04] | 0.49 [-0.02] | 0.53 [0.00] | 0.40 [-0.12] | 0.52 [-0.00] | 0.230 |
| | (0.50) | (0.50) [0.208] | (0.50) [0.745] | (0.50) [0.965] | (0.49) [0.023**] | (0.50) [0.934] | |
| Native Language = Bangla | 0.90 | 0.90 [0.02] | 0.95 [0.06] | 0.91 [0.00] | 0.90 [0.01] | 0.90 [0.00] | 0.153 |
| | (0.30) | (0.29) [0.182] | (0.22) [0.005***] | (0.28) [0.860] | (0.31) [0.654] | (0.30) [0.908] | |
| Scheduled Caste/Scheduled Tribe | 0.39 | 0.39 [-0.01] | 0.40 [0.01] | 0.47 [0.06] | 0.42 [0.03] | 0.37 [-0.03] | 0.995 |
| | (0.49) | (0.49) [0.835] | (0.49) [0.782] | (0.50) [0.205] | (0.50) [0.578] | (0.49) [0.409] | |
| Other Backward Caste | 0.10 | 0.09 [-0.00] | 0.08 [-0.01] | 0.04 [-0.05] | 0.14 [0.04] | 0.13 [0.04] | 0.946 |
| | (0.30) | (0.29) [0.833] | (0.28) [0.641] | (0.21) [0.054*] | (0.34) [0.325] | (0.34) [0.217] | |
| Religion = Hindu | 0.74 | 0.76 [0.03] | 0.70 [-0.04] | 0.84 [0.09] | 0.80 [0.07] | 0.79 [0.04] | 0.064 |
| | (0.44) | (0.43) [0.106] | (0.46) [0.422] | (0.37) [0.010**] | (0.40) [0.048**] | (0.41) [0.153] | |
| Rural Location | 0.50 | 0.49 [-0.01] | 0.48 [-0.00] | 0.49 [-0.03] | 0.49 [0.01] | 0.49 [-0.02] | 0.381 |
| | (0.50) | (0.50) [0.491] | (0.50) [0.924] | (0.50) [0.258] | (0.50) [0.744] | (0.50) [0.267] | |
| Married or Previously Married | 0.94 | 0.92 [-0.02] | 0.96 [0.02] | 0.92 [-0.03] | 0.95 [0.01] | 0.93 [-0.02] | 0.332 |
| | (0.24) | (0.27) [0.218] | (0.20) [0.397] | (0.27) [0.345] | (0.22) [0.484] | (0.26) [0.405] | |
| Lives with In-Laws | 0.44 | 0.39 [-0.05] | 0.47 [0.04] | 0.32 [-0.13] | 0.47 [0.03] | 0.35 [-0.09] | 0.098 |
| | (0.50) | (0.49) [0.123] | (0.50) [0.500] | (0.47) [0.015**] | (0.50) [0.554] | (0.48) [0.036**] | |
| Has Children under 8 | 0.47 | 0.48 [0.01] | 0.49 [0.02] | 0.48 [-0.01] | 0.45 [-0.01] | 0.51 [0.01] | 0.204 |
| | (0.50) | (0.50) [0.168] | (0.50) [0.083*] | (0.50) [0.321] | (0.50) [0.479] | (0.50) [0.195] | |
| Individual (Not Shared) Smartphone | 0.73 | 0.72 [0.00] | 0.73 [0.01] | 0.74 [0.01] | 0.72 [-0.00] | 0.73 [0.00] | 0.642 |
| | (0.44) | (0.45) [0.915] | (0.45) [0.085*] | (0.44) [0.169] | (0.45) [0.959] | (0.45) [0.641] | |
| Labor Supply Final Say = Self | 0.37 | 0.37 [-0.00] | 0.29 [-0.08] | 0.43 [0.06] | 0.35 [-0.01] | 0.33 [-0.04] | 0.729 |
| | (0.48) | (0.48) [0.978] | (0.46) [0.114] | (0.50) [0.298] | (0.48) [0.813] | (0.47) [0.357] | |
| F-test: statistic | | 0.58 | 0.60 | 0.77 | 0.62 | 0.86 | |
| F-test: *p*-value | | 0.977 | 0.967 | 0.833 | 0.960 | 0.704 | |
| Number of observations | 386 | 668 | 97 | 91 | 96 | 187 | |

*Notes:* This table presents baseline characteristics of endline survey participants across treatment conditions. Each cell in the treatment columns reports the mean of the characteristic, with standard deviations in parentheses below. Sample sizes for each cell are reported in parentheses next to the means. The numbers in square brackets next to the group means represent coefficients from regressions of each characteristic on treatment indicators (with Control as the omitted category), controlling for strata fixed effects. *p*-values for these coefficients appear in square brackets below the coefficients. The final column reports *p*-values from tests comparing the Control group to all job groups pooled together. The bottom row presents F-statistics with their corresponding *p*-values. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Binary indicators (Bangla as native language, SC/ST, Hindu, rural, married/previously married, lives with in-laws, has children under 8, and smartphone ownership) take values of 0 or 1.

Table A3: Demographics Comparison Table: Study Sample vs Representative Household Survey

| | (1) Study Sample | | (2) Large-scale Survey Data | | Difference in Means | |
|---|---|---|---|---|---|---|
| Variable | N | Mean/SE | N | Mean/SE | (1)-(2) | |
| Scheduled Caste/Tribe (=1) | 1553 | 0.414 | 18658 | 0.466 | -0.052 | |
| | | (0.013) | | (0.004) | | |
| Other Backward Classes (=1) | 1553 | 0.104 | 18658 | 0.164 | -0.060 | |
| | | (0.008) | | (0.003) | | |
| Open/General (=1) | 1553 | 0.482 | 18658 | 0.340 | 0.142 | |
| | | (0.013) | | (0.003) | | |
| Hindu (=1) | 1670 | 0.754 | 24200 | 0.736 | 0.018 | |
| | | (0.011) | | (0.003) | | |
| Muslim (=1) | 1670 | 0.244 | 24200 | 0.246 | -0.002 | |
| | | (0.011) | | (0.003) | | |
| Number HH Members | 1670 | 4.626 | 24200 | 4.724 | -0.098 | |
| | | (0.042) | | (0.013) | | |
| Parent-in-Law in HH (=1) | 1649 | 0.400 | 24200 | 0.370 | 0.031 | |
| | | (0.012) | | (0.003) | | |
| Has Child Under 8 (=1) | 1670 | 0.516 | 24200 | 0.478 | 0.038 | rft |
| | | (0.012) | | (0.003) | | |
| Monthly Household Income (Rs.) | 1583 | 11791.437 | 7831 | 14867.574 | -3076.137 | |
| | | (299.440) | | (208.776) | | |
| Age | 1670 | 29.911 | 19359 | 32.432 | -2.521 | |
| | | (0.199) | | (0.067) | | |
| Currently Married (=1) | 1670 | 0.902 | 24200 | 0.818 | 0.084 | |
| | | (0.007) | | (0.002) | | |
| Never Attended School (=1) | 1670 | 0.004 | 19359 | 0.218 | -0.214 | |
| | | (0.002) | | (0.003) | | |
| Completed Secondary School (=1) | 1670 | 0.493 | 19359 | 0.598 | -0.104 | |
| | | (0.012) | | (0.004) | | |
| Has Own Cellphone (=1) | 1670 | 0.881 | 2919 | 0.512 | 0.369 | |
| | | (0.008) | | (0.009) | | |
| Has Never Worked for Pay (=1) | 1670 | 0.688 | 6847 | 0.771 | -0.083 | |
| | | (0.011) | | (0.005) | | |
| Currently Works for Pay (=1) | 1644 | 0.041 | 7603 | 0.179 | -0.137 | |
| | | (0.005) | | (0.004) | | |

*Notes:* This table presents demographic data for study participants and households from large-scale demographic surveys in India. The data for the study sample includes participants who were randomized into a treatment or control group. A ‘don’t know’ response is coded as missing, reducing the number of observations in the case of variables like caste and household income. The large-scale survey data are from the National Family Health Survey (2019-21) and the Periodic Labor Force Survey (2021-2022). The large-scale survey sample is restricted to West Bengal and women between 18 and 60 years.

Table A4: Effect of Flexible Job Attributes on Take Up of Work

| | Number Tasks Completed | | | |
|---|---|---|---|---|
| | ≥1 task | ≥10 tasks | ≥50 tasks | ≥100 tasks |
| | (1) | (2) | (3) | (4) |
| Time-inflexible | -0.02 | -0.01 | -0.01 | -0.01 |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| Multitasking-with-childcare inflexible | -0.19*** | -0.19*** | -0.19*** | -0.19*** |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| Time- & multitasking-with-childcare inflexible | -0.18*** | -0.18*** | -0.17*** | -0.17*** |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| Office | -0.34*** | -0.34*** | -0.34*** | -0.33*** |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| *Tests of equality (p-values):* | | | | |
| Time-inflexible = Multitasking-with-childcare | 0.007*** | 0.007*** | 0.006*** | 0.006*** |
| Time-inflexible = Time-& multitasking | 0.017** | 0.011** | 0.017** | 0.017** |
| Time-inflexible = Office | 0.000*** | 0.000*** | 0.000*** | 0.000*** |
| Multitasking-with-childcare = Office | 0.004*** | 0.001*** | 0.002*** | 0.002*** |
| Multitasking-with-childcare = Time-& multitasking | 0.760 | 0.866 | 0.742 | 0.744 |
| Time-& multitasking = Office | 0.001*** | 0.001*** | 0.001*** | 0.001*** |
| Observations | 1,250 | 1,250 | 1,250 | 1,250 |
| Most flexible take up rate | 0.48 | 0.47 | 0.46 | 0.46 |

*Notes:* This table presents the impacts of flexible work arrangements on job take up. The estimates come from regressions where the outcome variable is an indicator for whether or not the participant started work, and the regressors are indicator variables for each of the four work arrangements that are not the most flexible job (“Flex”). Columns (2)-(4) show alternative definitions of job take up with higher thresholds for the number of tasks completed to qualify as having started work. The regressions control for strata fixed effects and are estimated using Huber-White heteroskedasticity-robust standard errors. Stars next to coefficients represent significance (* at 10%; ** at 5%; *** at 1%).

Table A5: Monotonicity of Job Take-up in Flexibility

| | Did not accept job on left, but would accept... | | |
|---|---|---|---|
| | 1 less flexible job | 2 less flexible jobs | 3 less flexible jobs |
| Most Flexible | $<$1% | $<$1% | $<$1% |
| Time-Inflexible | 4.4% | 1.7% | $<$1% |
| Multitasking-with-Childcare-Inflexible | 4.8% | $<$1% | - |
| Time- and Multitasking-with-Childcare-Inflexible | 6.0% | - | - |

*Notes:* This table presents results about the monotonicity of job take up with respect to flexibility. Each cell shows the share of individuals that refused a job offer of the type indicated in the left column, but who state on the baseline survey that they would accept a less flexible job if offered it. The three columns indicate whether they state they would accept one, two, or three less flexible jobs than the job they refused. "-" indicates that this case is not relevant, which could occur if there are not enough jobs which are less flexible (for example, because there is only one job that is less flexible than the time- and multitasking-with-childcare-inflexible job).

Table A6: Effect of Round 1 Job Assignment on Round 2 Job Take Up (No Lasso Controls)

| | Started Work During Jobs Round 2 | | | | | |
|---|---|---|---|---|---|---|
| | All Participants | | Previous Work Experience | | | |
| | | | No | | Yes | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| R1 More Flexible Than R2 | 0.06* | 0.05* | 0.08** | 0.07** | 0.03 | 0.03 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.05) | (0.06) |
| R1 Less or Equally Flexible Than R2 | -0.03 | -0.04 | -0.03 | -0.02 | -0.05 | -0.09 |
| | (0.04) | (0.05) | (0.05) | (0.04) | (0.07) | (0.10) |
| Observations | 1,524 | 1,524 | 1,049 | 1,049 | 475 | 475 |
| R2 work arrangement FE | Yes | Yes | Yes | Yes | Yes | Yes |
| R1 work arrangement FE | No | Yes | No | Yes | No | Yes |
| Strata FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Lasso selected controls | No | No | No | No | No | No |

*Notes:* This table presents results on effects of the initial jobs RCT on starting work during the second round of jobs.

– The outcome variable in all columns is an indicator variable equal to 1 if the participant starts the job that she was randomly assigned to during round 2 and is otherwise equal to 0.

– The omitted group is the control group. Treatment group participants are categorized by the relative flexibility of their randomly-assigned round 1 (R1) versus round 2 (R2) job assignments.

– Columns (1) and (2) report the effects of round 1 job assignment on round 2 job take up for all participants. Columns (3)-(6) report effects separately by whether or not a participant had previous paid work experience before the study.

– Odd-numbered columns report results when controlling for round 2 work arrangement fixed effects. Even-numbered columns report results when controlling for both round 2 and round 1 work arrangement fixed effects. All columns include strata fixed effects. No other control variables are included.

– Huber-White standard errors in parentheses $(\cdot)$ are robust to heteroskedasticity. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Table A7: Gateway Jobs—Heterogeneity by Round 2 Work Arrangement

| | Started Work During Jobs Round 2 | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| R1 Less or Equally Flexible Than R2 | -0.04 | -0.04 | -0.03 | -0.03 |
| | (0.04) | (0.06) | (0.04) | (0.06) |
| R2: Time-Inflexible × R1 More Flexible Than R2 | 0.01 | 0.01 | 0.01 | 0.01 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| R2: Child-Inflexible × R1 More Flexible Than R2 | 0.09 | 0.09 | 0.10 | 0.09 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| R2: Time- & Child-Inflexible × R1 More Flexible Than R2 | 0.01 | 0.01 | 0.01 | 0.01 |
| | (0.07) | (0.07) | (0.07) | (0.07) |
| R2: Office × R1 More Flexible Than R2 | 0.08** | 0.08* | 0.08** | 0.08* |
| | (0.04) | (0.04) | (0.04) | (0.04) |
| Observations | 1,524 | 1,524 | 1,524 | 1,524 |
| R2 work arrangement FE | Yes | Yes | Yes | Yes |
| R1 work arrangement FE | No | Yes | No | Yes |
| Strata FE | Yes | Yes | Yes | Yes |
| Lasso selected controls | No | No | Yes | Yes |

*Notes:* This table presents results on the heterogeneous effects of the initial jobs RCT on starting work during the second round by different round 2 work arrangements.

- The outcome variable in all columns is an indicator variable equal to 1 if the participant starts the job that she was randomly assigned to during round 2 and is otherwise equal to 0.
- The omitted group is the control group, who received no job offer during round 1. Treatment group participants are categorized by the relative flexibility of their randomly-assigned round 1 (R1) versus round 2 (R2) job assignments. The indicator for being assigned a gateway job sequence (i.e. R1 More Flexible Than R2) is in turn interacted with indicators for different round 2 work arrangements.
- The analysis sample consists of all 1,524 participants who were randomly assigned a round 2 job offer.
- Odd-numbered columns report results when controlling for round 2 work arrangement fixed effects. Even-numbered columns report results when controlling for both round 2 and round 1 work arrangement fixed effects. All columns include strata fixed effects. Columns (3) and (4) also control for lasso-selected covariates.
- Standard errors in parentheses (·) are Huber-White (robust to heteroskedasticity). Stars next to coefficients represent *p*-values (* significant at 10%; ** at 5%; *** at 1%).

Table A8: Treatment Effects on Children's Reports of Parents' Activities

| | Mom has Job<br>(1) | Dad has Job<br>(2) | Dad Helps (> Never)<br>(3) |
|---|---|---|---|
| Treatment | 0.18*** | -0.01 | 0.09* |
| | (0.04) | (0.02) | (0.05) |
| Observations | 510 | 508 | 512 |
| Control Mean | 0.20 | 0.97 | 0.58 |
| Strata fixed effects | Yes | Yes | Yes |
| Baseline measure | Yes | Yes | Yes |

*Notes:* This table presents effects of the job treatment on children's reports of whether their parents work for pay and whether their parents contribute to home production activities.

- All treatment groups are pooled and compared to the control group. Households are only included in the analysis sample if children participated in the survey at both baseline and endline ($N = 515$). Differences in the number of observation across columns are due to "don't know" responses.
- The outcome variables in columns (1) and (2) are indicators that take value 1 if the child reports that their mother and father work for pay respectively, and 0 otherwise. The outcome variable in column (3) is based on the child's report about the father's participation in cleaning, cooking, or laundry. The indicator takes value 1 if the child responds that their father helped more than "never" with these activities during the time frame of the intervention.
- Standard errors in parentheses $(\cdot)$ are Huber-White (robust to heteroskedasticity). Stars next to coefficients represent p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A9: Treatment Effects on Children's Gender Attitudes

| | Gender Attitudes Index | | |
|---|---|---|---|
| | All Children | By Child Age | |
| | | >12 | ≤12 |
| | (1) | (2) | (3) |
| Treatment | 0.05 | $0.11^{**}$ | 0.00 |
| | (0.04) | (0.05) | (0.07) |
| Observations | 514 | 278 | 236 |
| Strata fixed effects | Yes | Yes | Yes |
| Baseline measure | Yes | Yes | Yes |

*Notes:* This table presents effects of the job treatment on children's gender attitudes.

- All treatment groups are pooled and compared to the control group. Households are only included in the analysis sample if children responded to the gender attitudes module at both baseline and endline ($N = 514$).
- The outcome variables across all columns is an index of children's gender attitudes from the endline survey. The index is computed as a weighted average of questions in which the weights take into account the covariance structure of the components as in Anderson (2008). See Appendix Section A.1 for more information on the questions that are used in constructing this index.
- Column (1) includes all children, while columns (2) and (3) report heterogeneous treatment effects by child age. Children are split by the median age of twelve, with treatment effects on older children reported in column (2) and treatment effects on younger children reported in column (3).
- Standard errors in parentheses $(\cdot)$ are Huber-White (robust to heteroskedasticity). Stars next to coefficients represent p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A10: Effect of Job Treatments on Women's Psychological Well-Being

| | Psych Index<br>(1) | Sleep Peacefully<br>(2) | (Not) Overwhelmed<br>(3) | Happy<br>(4) | (Not) Worried<br>(5) |
|---|---|---|---|---|---|
| Treatment | 0.02 | -0.02 | 0.07 | -0.02 | -0.01 |
| | (0.04) | (0.06) | (0.06) | (0.06) | (0.06) |
| Observations | 1,524 | 1,524 | 1,524 | 1,524 | 1,524 |
| Control Mean | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Strata FE | Yes | Yes | Yes | Yes | Yes |
| Lasso Selected Controls | Yes | Yes | Yes | Yes | Yes |

*Notes:* This table presents the treatment effect of the jobs intervention on women's psychological wellbeing.

- In this analysis, all treatment groups are pooled and compared to the control group.
- The dependent variable in column (1) is a standardized, weighted average of four questions on the endline survey. The weights on the different index components in column (1) are informed by their covariance, as in Anderson (2008).
- The dependent variables in columns (2)-(5) are components making up the column (1) index. For questions corresponding to columns (2)-(5), participants were asked about how often they felt a certain way in the last month, and the outcome is standardized to have mean 0 and standard deviation 1 in the control group. Column (2) is how often they slept peacefully, column (3) is how often they felt overwhelmed, column (4) is how often they were feeling generally happy, and column (5) is how often they felt worried. Outcomes in columns (3) and (5) are negated so that positive values correspond to better well-being. In response to these questions, participants could answer that in the last month they felt this way (i) Never, (ii) A few days, (iii) Around half the days, (iv) More than half the days, and (v) Nearly every day.
- All regressions include lasso-selected controls and strata fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. Stars next to coefficients represent p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A11: Assumptions for back-of-the-envelope firm tradeoffs

| **Item** | **Value** | **Units** |
|---|---|---|
| Rent | 8000 | Rs/month |
| Office Supervisor | 1500 | Rs/month |
| Other office materials | 500 | Rs/month |
| Market wage | 120 | Rs/hour |

*Notes:* This table presents assumptions for back-of-the-envelope calculations of the firm's tradeoff.

### A.1 Gender Attitudes Index

***Questions.*** The baseline gender attitudes index is composed of responses to the thirteen questions below:

1. People have different opinions about women having a job. Some people feel that women in your religion, caste and area should not have a job outside the home and they should only look after their families, while others say that there is nothing wrong if women have a job and earn money outside the home. What is your opinion?
2. What about if the woman can do the job from home?
3. It is the father's responsibility to earn a living in a family, and it is the mother's responsibility to take care of the children.
4. A woman's most important role is being a good homemaker.
5. Husband and wife should have equal say in decisions about significant household expenditures.
6. It is better for women to quit their jobs when they get married or have children.
7. A man's neighbors or friends will think poorly of him if his wife goes out to work.
8. It is possible for women to be good wives as well as to have a job.
9. A man should have the final word about decisions in his home.
10. Women who use social media risk their family's reputation.
11. A husband should know what his wife is doing while using a mobile phone at all times.
12. It is not wise for a family to allow their unmarried daughters to use their mobile phones or social media without supervision.
13. Husbands should help with household responsibilities such as cooking, cleaning, and childcare.

***Responses.*** For questions (1) and (2), respondents had four options to respond with:

0. Strongly believe women should not work for pay
1. Believe women should not work for pay
2. Believe there is nothing wrong if women work for pay
3. Strongly believe there is nothing wrong if women work for pay

For questions (3)-(13), respondents could respond that they strongly disagree (0), disagree (1), agree (2), or strongly agree (3).

***Aggregation into Index.*** First, the likert scale above was used to code participants' responses into numeric values. Second, responses were standardized to have mean zero and standard deviation one. Third, questions that were supportive of traditional gender roles were negated so that higher numeric values would always indicate holding less traditional views. This corresponds to negating the standardized numeric responses to questions (1), (2), (5), (8), and (13). In our main analysis, the responses are then aggregated following Anderson (2008), in which weights are the inverse of the covariance matrix of the residualized, standardized responses. This process is designed to put more weight on questions that capture independent information and less weight per response for questions with responses that are highly correlated. We also show our analysis is robust to aggregating responses into an index that consists of calculating the within-respondent average of their responses, although this method results in less precise estimates.

***Children's Gender Attitudes Index.*** The children's gender attitudes index is constructed and analyzed in the same way as the gender attitudes index for adults. However, the questions are adjusted somewhat in order to (a) have some questions that are appropriate specifically to children and adolescents and (b) to create some differences from the gender attitudes index questions asked to adults.

1. Boys are better than girls at using technology like phones or computers.
2. A woman's most important role is being a good homemaker.
3. A man should have the final word about decisions in his home.
4. Men are better suited than women to have a job or career.
5. Men and women should contribute to household work such as cooking, cleaning, and childcare equally.
6. Women should be allowed to work outside home.
7. Parents should supervise girls' phone use more closely than boys'.
8. Men cannot cook and clean well even if they try hard to learn it.
9. Women cannot play football well even if they try hard to learn it.
10. Men are better at math than women.

11. Being a politician is not a suitable profession for a woman.
12. Girls are equally as intelligent as boys.
13. It is more appropriate for girls to help with housework than boys.
14. Men generally understand money-related issues better than women.
15. It is more important for boys to go to university than girls.
16. Men have better judgment compared to women, hence they are better leaders.
17. When women have children, it’s best if they quit their jobs and become housewives.