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 field 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 job 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 drawbacks are likely outweighed by the high value women place on the ability to work from home. (JEL Codes: J16, J22, J32, O15)

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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 equity and efficiency. In addition to better respecting individual preferences, increasing the share of women who have access to independent income could also 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), as well as lead to a more efficient allocation of men's and women's labor that could contribute to 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: over half of 25-year-old women report that they do not step out of their homes *even once* on an average 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 take the second approach: change jobs so that women can do them while incurring lower norms-related

[1] This estimate is based on a 2016 poll conducted by the International Labour Organization and Gallup in 142 countries. Agte and Bernhardt (2023) estimate that more than 100 million women in India alone 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 will be an overestimate for the average worker if remote jobs are more common for formal high-skilled workers.

costs.[3] However, if a woman's earned income and actual labor supply affect people's attitudes about women and work, then our 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.[4] Our experiment is designed to speak to three main research questions. First, does offering 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 flexible jobs act as a stepping stone to less flexible jobs for women initially only able to work from home? Third, to assess the viability of employers introducing flexible work arrangements, what are the effects of flexibility 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. The jobs involve tasks contributing to Bangla or Hindi speech datasets for training language models. To separately estimate the effects of flexible work arrangements on job performance versus worker composition, after participants have decided to accept or reject their job offer, we randomly select

[3]Previous work shows that directly changing people's attitudes towards women and work is very difficult (e.g. Dean and Jayachandran, 2019).

[4]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 is at an all-time high for women with children under five (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.

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 jobs outside the home.

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% ($p < 0.001$). To contextualize this 33 pp effect size, this is a larger effect on women's job take up than those found in evaluations of previous effective interventions designed to increase women's labor supply. For example, a promotional video shown to women's family members in rural Uttar Pradesh increased job take up by 78% (7 pp) (McKelway, 2024), and correcting Saudi men's second-order beliefs

[5] 75.8% of women in urban West Bengal are out of the labor force and 67.7% are literate in Hindi or Bangla. There is less information available on the share of women who have access to a smartphone. However, according to the NFHS (2019-21), 72.4% of women own a mobile phone.

about women working outside the home increased job sign up rates by 36% (9 pp) (Bursztyn et al., 2020).

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. Work from home, even without multitasking work with childcare or time flexibility, 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 inflexibility 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 offered during *Jobs Round 2* if they were first given the chance to experience a more flexible job during the initial intervention. The effect is concentrated on women who had no previous work experience before the study (+7 pp, $p = 0.03$). Examining only the women who are 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 can 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, expanding the set of jobs within reach for women. Women who held more traditional attitudes at baseline had a more responsive labor supply 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 ($p = 0.038$), 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 effects 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 ($p = 0.034$). Treatment group

[6] This heterogeneity is not driven by ceiling effects, as can be seen from the histograms of the gender attitudes index before and after the intervention (see Figure A.8).

[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%).

children also report that their fathers contribute more to home production. They are 9 pp more likely to say that their fathers helped at least a bit with cooking, cleaning, or childcare during the time period of the intervention ($p = 0.045$), as compared to the 42% of children in the control group who say their fathers *never* help with these activities.

These effects on take up show that introducing greater job flexibility — even temporarily — can dramatically increase the pool of workers that employers could hire from. So why do firms not offer flexible work arrangements more often? Beyond fixed costs and feasibility, employers would likely want to understand how adopting flexible work arrangements would affect worker performance. Introducing job flexibility could affect worker performance in two ways: (i) by changing the performance of inframarginal workers, those who are willing to work from the office but can now work from home, and (ii) by drawing in a different type of worker to the firm, once the employer advertises that it is possible to work from home. 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). We measure job performance using three main outcomes: (i) reliability—how likely the worker is to show up to work after accepting the job, (ii) quality—the average accuracy with which workers complete tasks, and (iii) efficiency—the speed with which workers complete their assigned tasks.

Flexible work arrangements improve the reliability of inframarginal workers but at the cost of quality and efficiency. Worker reliability, measured by the likelihood of showing up after accepting a job, improves with work-from-home arrangements, reducing the no-show rate by 25 percentage points ($p = 0.004$). Consistent with Aksoy et al. (2023), workers in our study who were willing to go to the office but were upgraded to work from home also spend more time in total working. However, there is a negative effect of work-from-home on speed and quality: workers who are willing to work from the office but are randomly assigned to work from home complete tasks 20% more slowly ($p = 0.016$) and 4% less accurately ($p = 0.004$), when working from home. These effects are present for both easier and more difficult tasks, but are more pronounced for tasks that require greater cognitive load—work-from-home causes difficult tasks to be completed an additional 1.5 times more slowly ($p = 0.035$) and an additional 6 times less accurately ($p < 0.001$). We do not find any significant differences on the selection margin, suggesting that *flexibility compliers* — those whose job take up is marginal to flexibility — 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 is borne by the worker. 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 there are greater incentives to shirk.

Examining work patterns between different treatment arms, we find that 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, work-from-home causes workers to have work sessions that are 25% shorter ($p = 0.009$). These fragmented work patterns result in lower efficiency 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.[8] 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 29% of women have ever undertaken paid work, and this accounts for 90% of women who will ever enter the labor market.[9] 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

[8] 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.

[9] 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.

whether or not many women begin paid work.[10] 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 changes in worker performance due to 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 developing countries. While other studies characterize the incumbent workers who choose flexible work arrangements, we aim to characterize the workers who would be brought into the 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 (Boserup, 1970; Alesina et al., 2013; Carranza, 2014; Hansen et al., 2015; Becker, 2021). However, few experimental interventions

[10] 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.

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.

Lastly, 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, 2023; Bursztyn et al., 2020; Field et al., 2021; 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; Field and Vyborny, 2022; Siddique, 2022; Martinez et al., 2020), 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) and a small but growing set of papers explore how digital technology might interact with these barriers (e.g. Alhorr, 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.[11]

The rest of the paper proceeds as follows: Section 2 describes the study population and ex-

[11]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.

perimental design. Section 3 presents results on the effects of flexible work arrangements on take up of work. Section 4 presents the effects of work experience on the household, including the gender attitudes of women and their children, as well as women's take up of future jobs. Section 5 presents the impacts of flexible work arrangements on job performance, separating effects that operate through treatment versus worker selection.

2 Experimental design

2.1 Setting and Participant Characteristics

Households are recruited for the study from eight areas in and near Kolkata, West Bengal (see Figure A.12 for a map) with the support of our local partner NGO, the Calcutta Foundation.[12] Three of these areas are rural (Canning, Noorpur, and Taldhi), three are peri-urban (Atabagan, Baruipur, and Sodepur), and two are urban (Tiljala and New Alipore). Households are eligible for the study if there is a woman household member who consents to participate and fulfills the following criteria: (1) she can read and speak Bangla or Hindi, (2) she has access to an Android smartphone, and (3) she is not in the labor force.[13] Given the demographic characteristics of an average adult woman in West Bengal, these inclusion criteria are not highly restrictive. Data from large-scale demographic surveys in India show that 54.3% of women in West Bengal are literate, 94% belong to households with a mobile phone, 51% have a phone for personal use, and 78.8% are not in the labor force.[14]

The average participant is 30 years old and married. Half have completed education through to 10th standard (students aged 15-16), and 13% have at least started an undergraduate degree. Three quarters of the participants are Hindu, with the remaining coming from Muslim households.

[12]The Calcutta Foundation was founded in 1994 and has worked on projects related to education, disaster relief, health, and more recently gender.

[13]We define this last criterion as (i) has not worked for pay in the last month (or spent less than 5 hours per week on any paid work) and (ii) is not in skills training. The purpose is to identify women who are and would continue to be housewives in the absence of the experiment.

[14]These statistics are computed using data from the Periodic Labor Force Survey (2021-2022) and the National Family Health Survey (2019-2021). These surveys do not include information about smartphone ownership specifically, 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. In urban areas, this number is likely considerably higher.

40% of participants belong to a scheduled caste or scheduled tribe. The average household size is 4.6 people, and 40% live with at least one parent-in-law. Three quarters of participants have a child under eighteen living in the household, and 48% of participants have a child under eight. Access to an Android smartphone is part of the eligibility criteria for the study, and 73% of women report having their own smartphone, i.e. not sharing their phone with any other household member. Average household income is 11,791 INR (approximately 142 USD) per month, placing participing households in the lower-middle-income level. Approximately two-thirds (69%) of participants have never previously worked for pay before starting the study. If participants were to get a job offer, only 36% report that they would have the final say in whether or not to take the job.

Households in our study sample are comparable to the average household in West Bengal. Table A.6 presents means and standard deviations for important characteristics for the study sample and households in West Bengal from publicly available large-scale demographic survey data.[15] Households in our study sample are representative of the average household in West Bengal in terms of household size, religion and the presence of parents-in-law and children under eight within the household. Our sample has a slightly lower representation of Scheduled Castes/Scheduled Tribes and Other Backward Classes in favor of households from the Open/General caste category, and the monthly household income in our study sample is lower than that for an average household in West Bengal. Women in our study sample are considerably more likely to have ever attended school and are far more likely to have their own phone than the average adult woman between 18 and 60 years in West Bengal. This is because we impose being able to read either Hindi or Bangla and having access to a smartphone as an inclusion criteria so that participants could do the job if offered it.

2.2 Job Description

Jobs platform. We partner with Karya, a local-language smartphone-based tasks platform in India, to offer the intervention jobs. The interface for the job app is presented in Figure A.7. The

[15]These data are from the Periodic Labor Force Survey (2021-2022) and the National Family Health Survey (2019-2021).

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. Karya'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).[16]

Although Karya 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. Participants receive 4000 tasks to complete over the course of one month, with 1000 new tasks refreshed each week that expire after seven days. If participants are unsatisfied with their first attempt, they can re-attempt any task as many times as they wish. The tasks are presented in a fixed order, but participants can skip any and as many tasks as they want.

There are four types of paid tasks, which participants can choose to do in either Hindi or Bengali. 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 selected from tasks that previous Karya clients

[16]From 1898 to 1928, George Grierson developed a linguistic survey of India which identified 179 languages, defined by mutual unintelligibility, along with 544 dialects (Grierson, 1927). More recently, a survey from the 1980s reported that there are over 50 Indian languages with substantial written traditions and corpuses (Mahapatra et al., 1989), and the 1991 census reported 184 distinct mother tongues.

requested and paid for. The second type of task involves reading a sentence backwards, which we introduce to require more concentration. The third type of task involves finding a specific sentence within a paragraph, and reading that sentence out loud. The target sentence includes a specific word prompted by the task, and requires participants to locate the correct sentence within the paragraph. Lastly, there are open-ended questions for participants to answer. Our partners at Karya are interested in collecting this type of speech data, as it is particularly scarce but useful.

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. For the purposes of analysis, we consider the simplest tasks *easy* tasks, and we consider all other tasks *difficult* tasks because they require a greater cognitive load.

Payment. Participants earn up to one rupee per task they complete, with payments processed weekly according to task quality. 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. In the broader population, 4000 INR is roughly equal to the mean monthly wages in urban West Bengal for individuals who have completed at least secondary school and is about 1000 INR (approximately 10 USD) above the wages in rural West Bengal for this demographic.[17] The task payment rate is set by Karya 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 below). The most flexible job we offer allows participants to work from home, at any time they choose, and while multitasking their

[17] As per the Periodic Labor Force Survey (2021-22).

work with childcare. In each subsequent job, we switch off one or more of these dimensions.

Work Arrangement	Time Flexibility	Multitasking with Childcare	Work From Home
1. Most Flexible	✓	✓	✓
2. Time-Inflexible	✗	✓	✓
3. Child-Inflexible	✓	✗	✓
4. Time- & Child-Inflexible	✗	✗	✓
5. Office	(✓)	✗	✗

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 time-inflexibility does not create pressure in terms of whether or not the worker will be able to complete the tasks in a given week if she works for all 21 hours per week that she is eligible to during her timeslot. The average participant could complete, or at least attempt, all 1000 tasks in approximately eight hours. To enforce these work shifts, Karya's engineers altered the app 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 groups which allow participants to multitask work with childcare, participants are told that they can have their children next to them while they work. In the groups without multitasking with childcare, participants are told that it is *not* acceptable to have their children next to them while they work, and participants do not get paid for tasks they submit which have children's voices in the background of the recordings, as determined by a separate team of validators.

Working From the Office. In the location-flexible jobs, participants work from home, while in the office-based group they are required to work from one of our offices. We set up 1-2 offices per treatment area such that they are a short distance by vehicle (a two-wheeler or three-wheeler) and a moderate walking distance for study participants. Participants are not allowed to bring their children to the office.[18] For logistical and safety reasons, we do not keep the offices open at all times of day, but we keep the offices open at hours that participants would likely want to come to the office. 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. Participant recruitment took place both over the phone, using contact details provided by the Calcutta Foundation, as well as through in-person door-to-door conversations. 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 8-18 about their aspirations, participation in household activities, and gender attitudes.

Jobs Round 1: Initial Job Offers. After the baseline survey, we randomly assign households to the control group or to one of the five job groups for their initial job offers. Randomization is stratified by three characteristics: area, individual smartphone ownership, 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

[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.

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

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 when possible.

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

households (66).

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

Table A.5 presents means and standard deviations of important characteristics in the control and treatment groups after randomization, and Table A.4 presents the same for participants who completed the endline survey. As seen in Table A.5, randomization produced groups which are balanced across most major characteristics of interest, although the treatment group is more likely to be Hindu (significant at the 10% level).

As seen in Table A.4, 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 ($p = 0.098$). 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 the job offers are randomly assigned, estimating the effects of different work arrangements on job take up is straightforward. To estimate the effect of a given dimension of flexibility on job take up, we compare the share of women who start work with versus without that dimension of flexibility.

We also estimate an equation of the following form to understand how the effects of flexibility vary by baseline covariates:

$$StartsWork_{ij} = \alpha + \sum_{j \in J} \beta_j WorkArrangement_{ij} + x_i + \sum_{j \in J} \gamma_j (WorkArrangement_{ij} \times x_i) + \mu_s + \varepsilon_i$$

where $StartsWork_{ij}$ is a dummy variable equal to 1 if participant i starts the job j that she is randomly assigned to.[20] The regressors $WorkArrangement_{ij}$ are indicator variables equal to 1 if i is randomly assigned to work arrangement j, where j can take on one of five values (Most Flexible, Time-Inflexible, Multitasking-with-Childcare-Inflexible, Time- and Multitasking-with-Childcare-Inflexible, and Office). The covariate x_i is measured from the baseline survey, and takes on values such as whether the woman reports having the final say in her own labor supply decisions, or whether she has more versus less traditional gender attitudes. We control for strata fixed effects μ_s and estimate Huber-White heteroskedasticity-robust standard errors.

3.2 Results

Job flexibility dramatically increases women's extensive margin labor supply by more than three times, from 15% for an office job to 48% for the most flexible work-from-home job ($p < 0.01$).

[20] In our main figures, we say that a participant starts work if she downloads the tasks platform app and submits at least one completed task. We conduct similar specifications with a higher cutoff for number of tasks that qualify as having started work and find similar results. See Appendix Table A.8 for results using 10 tasks, 50 tasks, and 100 tasks as cutoffs.

To provide a benchmark for the magnitude of this effect, the impact of job flexibility on take up (+33 pp or +228%) is larger than the impact that has been estimated for other interventions that successfully increased women's labor supply, for example correcting men's second-order beliefs about social image costs of women working that resulted in a 9 pp or 36% increase in sign-up rates for a job matching service (Bursztyn et al., 2020), a self-efficacy intervention that increased job take up rates by 5 pp or 32% (McKelway, 2024), or giving women control over their earnings, which 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 our estimates of women's wage elasticity of labor supply from another project. In an experiment in Mumbai, we find that increasing wage offers nearly five times from 5000 INR (59 USD) to 24000 INR (283 USD) only results in a 7 pp (32%) increase in job take up (Jalota and Ho, 2024). If we were to take these labor supply elasticity estimates seriously, this would imply that women value job flexibility at 89500 INR (1050 USD) per month, far above the average monthly income of these households.

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 job that is flexible neither in time nor multitasking work with childcare (+14 pp or 100%, $p < 0.01$).

In sum, 33% of women in our study have extensive margin labor supply decisions that are marginal to bundled job flexibility. A decomposition of this effect that uses 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 A.11), 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:

$$
\begin{aligned}
StartsWork_{i2} = &\beta_1 \sum_{j_1 \in J} \sum_{j_2 \in J} WorkArrangement_{ij_1 1} WorkArrangement_{ij_2 2} \mathbb{1}[f(j_1) - f(j_2) > 0] + \\
&\beta_2 \sum_{j_1 \in J} \sum_{j_2 \in J} WorkArrangement_{ij_1 1} WorkArrangement_{ij_2 2} \mathbb{1}[f(j_1) - f(j_2) \leq 0] + \\
&\gamma_{i1} + \gamma_{i2} + X_i + \mu_s + \varepsilon_i
\end{aligned} \tag{1}
$$

where $StartsWork_{i2}$ is an indicator variable equal to 1 if participant i started the job that was randomly assigned to her during jobs round 2. We control for a vector of covariates selected by double post lasso X_i and strata fixed effects μ_s. Standard errors are Huber-White heteroskedasticity-robust.

We encode information about participant's job offers in the function $f(j)$, which takes on a discrete value that depends on the flexibility of the work arrangement $j \in J$: 1 if offered the least flexible job (office), 2 if offered the next least flexible job (inflexible across time and multitasking with childcare), 3 if offered the multitasking-with-childcare-inflexible job, 4 if offered the time-inflexible job, and 5 if offered 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 γ_{i2}, 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 γ_{i1}. The inclusion or exclusion of round 1 work arrangement fixed effects γ_{i1} leads to a different interpretation of the coefficients β_1 and β_2, but we view both models as useful for delivering different insights. When γ_{i1} is excluded from the model, β_1 and β_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 γ_{i1} 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 γ_{i1}.

In model 1, β_1 is 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 $\beta_1 < 0$ and $\beta_2 < 0$. If the coefficient $\beta_1 < 0$, then there are some women who would have been willing to do a less flexible job in round 1 had they been offered it, but now that they have done a more flexible job, they are no longer willing to do this less flexible job during round 2. If $\beta_2 < 0$, then there are some women who were willing to do a less flexible job when offered it during round 1, but now that they have done that less flexible job, they are no longer willing to do the more flexible job in round 2.

4.2 Results

Consistent with more flexible work arrangements acting as a stepping stone, 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 1, 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: when the regression controls for round 1 work arrangement fixed effects, the estimated effect of a gateway job sequence remains similar ($\hat{\beta}_1$ = 5 pp, $p = 0.07$). In contrast, there is no significant effect of being assigned to a non-gateway job sequence (i.e. any other job sequence) on women's take up during round 2.

If the gateway job effect operates through learning from the experience of earning income, then we would expect larger effects for participants who have less previous work experience. Consistent with this prediction, the estimated effect is most pronounced among women with no previous experience with paid work prior to study participation (Table 1, 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 ($p = 0.03$), while women who had previous work experience before the study are only marginally more likely to start work if assigned to a gateway job sequence (+2 pp, $p = 0.11$).

Lastly, we examine heterogeneous effects by round 2 work arrangement among first-time workers. The coefficients on all these interactions are positive but most are noisily estimated due to smaller sample sizes (see Appendix Table A.24). However, a larger share of workers were randomly assigned to office jobs than other less flexible arrangements during round 2, and the coefficient on the interaction with a round 2 office-based job 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, assignment to a gateway job 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 500% (from 5000 INR to 24000 INR per month) achieves approximately the same 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

There are several mechanisms through which work experience might affect future labor supply. In this section, we consider six different possible mechanisms: (i) women are initially constrained by attitudes about appropriate behavior for women, and work experience shifts gender attitudes, thus expanding the set of jobs available to them, (ii) women learn skills or gain the ability to signal skills to a future employer, (iii) income effects from the 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 *actual* labor supply mutually 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. 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. The gender attitudes index is composed of questions in which respondents are asked to rate whether they strongly disagree, disagree, agree, or strongly agree with each of fifteen statements across four domains (household roles, employment, technology use, and ability). In the main specifications, the gender attitudes index is constructed using the correlation adjustment strategy proposed by Anderson (2008). The effects of work experience on attitudes are similar when we use other methods of aggregating the gender attitudes questions, such as taking a simple average (see Table A.17).

First, 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. Note that job take up rates are lower for more 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 more traditional gender views. This relationship is a correlational, but shows that labor supply elasticity with respect to flexibility is increasing in traditional views at least in the cross section.

Second, Figure 3 Panel B shows that work experience in turn causes a shift in women's gender attitudes to become less traditional. Women randomly assigned to receive a job offer become 0.05 SDs less traditional ($p = 0.03$).[21] This effect is entirely concentrated on women who held attitudes

[21] We estimate the effects of being randomly assigned to a job offer using this participant-level intent-to-treat (ITT)

that were more traditional than the median at the start of the study, whose attitudes shift by 0.12 SDs ($p < 0.01$). Figure A.8 plots the gender attitude distributions before and after the intervention 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 cross section, the effect of work experience on *more traditional* women's attitudes 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 effective at targeting the labor force participation of women from more traditional households, since they are both the flexibility compliers and also the ones whose attitudes are most affected by 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 could affect labor supply during jobs round 2 because 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 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 income effects are in operation, they are dominated by other channels.

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); μ_s stands for strata fixed effects; T_i is an indicator variable for being randomly assigned to treatment; and θ_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.

(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 any significant effect on agency as measured by an index including (a) who has the final say in whether the participant could take 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 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 Table A.21). 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.[22]

The lack of change in agency combined with women's increased willingness to work outside the home provides indirect evidence that *men* changed their beliefs and attitudes. Men are still most likely to have the final say in their wife's labor supply at the end of the study (in more than 60% of households), which is not significantly different from the share of men who have the final say in their wives' labor supply at baseline. Given that men's preferences are still binding on women's labor supply in most households, and that treated women are more likely to work in less flexible jobs in round 2 than in round 1, the null effect on women's agency suggests that treatment did affect men's beliefs or attitudes to become more supportive of women working outside the home.

(v) increase in self-efficacy or self-confidence. If the initial work experience increases women's self-efficacy as in (McKelway, 2024), then they might be more likely to take up a job they consider more challenging, such as an office job. We do not find any treatment effects on women's reported

[22] Ultimately, there ended up being two rounds of job offers, and in each case we told household members that this was a one-time job opportunity. 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.

self-efficacy in their ability to use a smartphone ($\hat{\alpha} = -2\%$, $p = 0.32$), although we cannot rule out treatment effects on self-efficacy in other domains.

(vi) increased trust in the employer. The initial work experience might have increased women's trust in the employer. From the first work experience, they could learn that the employer pays workers on time. More generally, their interactions with our staff providing the job offers and tasks platform application support may have convinced workers that the employer is trustworthy. This could increase 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 offer first 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 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 16 pp more likely to say that their mother had a job during the last month ($p < 0.01$, see Appendix Table **??**). 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.

Effects on children's gender attitudes. We find evidence that treatment shifts the attitudes of older children to become less traditional. 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, however, 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 ($p = 0.03$). 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).

There are no significant heterogeneous treatment effects on attitudes by child gender. At baseline, boy children have attitudes that are 0.13 SDs less gender-equal than girl children ($p = 0.06$), showing 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 is the same in the treatment and control group at endline.

Effects on fathers' home production. We ask children to what degree their fathers help with cooking, cleaning, and childcare. In the control group, 42% of children report that their fathers *never* helped with cooking, cleaning, or childcare during the time period of the study intervention. In comparison, treatment group children are 9 pp more likely to report that their fathers helped at least occasionally (i.e. more than never) during the treatment period (see Table A.28).

Discussion. The effects on children could be driven either by the job offers themselves or by seeing their mothers do paid work. 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 nor on any of the individual index components (see Table A.22). 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. The options are to report 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 her 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 ($p = 0.02$, Figure 5).

5 Effects of flexibility on worker performance

The results presented in Section 3 show that introducing flexible work arrangements would likely greatly increase the pool of workers available to employers. So why do employers not offer home-based work options more frequently? In addition to any adjustment costs associated with introducing work-from-home, firms would likely want to understand the effects of flexible work arrangements on job performance. There are two ways that introducing the option to work from home could affect job performance. First, work-from-home could affect the performance of the inframarginal workers who would have accepted the job even if they had to work from an office. Second, advertising a job as having a remote work option could change the types of workers that are drawn to starting work at the firm.

5.1 Empirical Strategy

Using the randomly assigned surprise upgrades to the most flexible job, we estimate the effects of work-from-home on worker performance, separating effects that operate through treatment effects of flexibility versus changes in worker selection. By holding the work arrangement constant and comparing women who were willing to accept an inflexible job with those who were not, we can characterize the *flexibility compliers*, i.e. women who will only accept jobs when they are flexible. 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* women workers who are willing to work inside or outside the home.

Treatment effects of work-from-home. To estimate the effects of work-from-home, we compare the job performance between two groups who were initially assigned to the less flexible 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 task-level regression to quantify the effects of office-based work on job performance:

$$y_{it} = \beta_T \, Upgrade_i + \omega_{it} + \varepsilon_{it} \tag{2}$$

where y_{it} is individual i's performance on task t and ω_{it} is a vector of covariates at the task-by-individual level. $Upgrade_i$ is an indicator variable equal to 1 if worker i was initially assigned to and accepted the office job, but then was randomly selected for an upgrade to the most flexible job.

The two measures of performance we use are task accuracy and speed. Accuracy is measured on a scale of 0-2 and reflects how correct the worker's submission is for a given task. A task receives a score of zero if the task submission is completely wrong, i.e. it will not be useful to our tasks platform partner Karya's dataset creation, a score of one if the task submission is partially correct, and a score of two if the task submission is entirely correct. The accuracy score is determined by a team of validators hired by Karya to listen to and score every submitted task. Speed is a task-level measure defined by the amount of time to complete a given data task, measured in the number of seconds taken between starting and finishing a task. This is measured as the time difference between the timestamp when the worker presses the "start task" button on the app platform and the timestamp when the worker submits the completed task for evaluation.

To control for differences in experience with attempting tasks similar to task t before reaching task t across the different work arrangements, our task-level regressions also control flexibly for previously attempted and completed tasks in a vector of covariates ω_{it}. In different specifications, we control for (i) the number of tasks completed before task t, (ii) the number of tasks completed before task t, squared, (iii) fixed effects corresponding to which tasks were previously completed, (iv) the number of tasks previously attempted, (v) the number of tasks attempted before task t, squared, and (vi) fixed effects corresponding to which tasks were previously attempted.

Selection into work-from-home. We call women who take up jobs only when they are flexible *flexibility compliers*. 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. We use the following regression to characterize the differences between these two groups:

$$y_{it} = \beta_S \, AcceptedOffice_i + \omega_{it} + \varepsilon_{it} \tag{3}$$

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 ω_{it} are defined the same way as in the treatment effects analysis. The difference in job performance between these two groups $\hat{\beta}_S$ allows us to assess whether participants who are willing to work in an office job perform systematically differently from work-from-home compliers.

5.2 Results

Treatment effects on inframarginal workers. Across a range of specifications that control flexibly for previous tasks attempted and completed, working from home decreases speed by 20% ($p = 0.02$) and decreases accuracy by 4% ($p < 0.01$). These negative treatment effects increase with task difficulty. Work-from-home increases the time it takes to complete the simplest tasks by 21% ($p = 0.02$) while increasing the time it takes to complete more difficult tasks by 22% ($p = 0.03$ for difference with simple tasks).[23] Similarly, work-from-home has negative effects on accuracy that are more pronounced for difficult tasks. There is no significant effect of work-from-home on accuracy for the simplest tasks (although there is an estimated coefficient corresponding to a negative 2% effect), while work-from-home decreases accuracy for more difficult tasks by 11% ($p < 0.01$ for difference with simple tasks).

This negative treatment effect of work-from-home on performance is largely consistent with the results found in other contexts and with other populations. In the United States, Emanuel and Harrington (2024) find that work-from-home reduced the performance of call center workers by decreasing both the quantity of calls they answer by 4% as well as the quality of their conversations—our results our similar in that we find negative treatment effects on both speed and

[23] For a recap of the differences between simple and difficult tasks, refer to Section 2.2.

quality. Also in India, but with full-time data entry workers, Atkin et al. (2023) find that work-from-home reduced productivity by 18%, which is similar to the magnitude of the treatment effect we estimate among out-of-labor-force women for part-time data entry work. Gibbs et al. (2023) find that work-from-home reduced productivity among high-skilled IT professionals in India by 8-19%, likely because of increased communication costs. This is of a comparable magnitude to the treatment effect we find in our study, although our population is less educated, and the mechanism is likely different because our workers perform simpler tasks that do not require communicating with other workers.

Characterizing work-from-home compliers. Workers whose labor supply is marginal to the ability to work from home are not significantly different from job always takers in terms of their performance (see Table 3). In terms of speed, the point estimates correspond to work-from-home compliers being slower by 7% for simple tasks and 4% for more difficult tasks, but the estimates are not significantly different from zero. For accuracy, the point estimates characterizing selection are close to zero and insignificant for both simpler and more difficult tasks. The lack of negative selection in the performance of work-from-home compliers suggests that concerns about drawing "worse" quality workers into the firm when offering remote work options is unfounded in this context.

5.3 Mechanisms: workflow interruptions

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).

Consistent with the hypothesis that work-from-home has a negative effect on performance because there is a higher arrival rate of interruptions at home, our results show that working from home causes a decrease in the length of worksessions. As reported in Figure 6 (Panel B), the average office-based worksession is 25% longer than a worksession completed from home ($p < 0.01$). 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.

In this piece-rate context, the worse performance of home-based workers is largely unimportant to an employer, with the caveat that there may costs to the employer filtering out more inaccurate submitted tasks. However, if this negative effect of work-from-home on productivity exists even in this context where the *worker* pays the price of lower efficiency, then we might expect the negative effect to be exacerbated in salaried jobs in which the *employer* bears the cost of lower efficiency. In addition, if many of the home-based workers need to be able to switch from their job to household responsibilities many times throughout the day, and potentially unpredictably, the way in which female workers are "on call" at home may be a barrier for jobs that require teamwork or quick assignment turnarounds. The fragmented work patterns caused by home-based working arrangements might discourage employers from offering greater job flexibility.

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 adjust payments based on the accuracy of tasks submitted.

Salaried workers, same wages from home and office. In a salaried world, if the worker's 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 A.34 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 A.14 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.

In addition, all of the scenarios considered above are employment decisions during one time period. Given the dynamic effects of work experience in time period one on labor supply during time period two (as shown 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 a surprise job offer upgrade design similar to Karlan and Zinman (2009). 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.

We find three sets of results. First, flexible work arrangements are very effective at increasing labor supply for women, particularly those from traditional households. 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 factor in whether or not to work for many women. Second, we ask why employers do not offer flexible work arrangements more often, and we find that job flexibility decreases worker performance. Working from home causes women to work more slowly and to make more mistakes, and the women drawn into the firm by flexible work arrangements are also slower workers. In both cases, it appears that differences in work performance are driven by interruptions to workflow. Third, flexible work arrangements act as a stepping stone to less flexible jobs, including outside-the-home work. Job flexibility makes the biggest difference to the labor supply for women from more traditional households, and experience with flexible jobs in turn shifts the gender attitudes of these women and children from traditional households 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.

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 (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?

One 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 should be possible to offset the negative effect on productivity with lower wages.

Even taking this 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 not know the benefits of remote work, for example not realizing how large the effects would be on their pool of potential workers. 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 in developing countries — the labor demand side — may be a fruitful area for future research.

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Tables

Table 1: 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 controls	✓	✓	✓	✓	✓	✓
R1 work arrangement contols		✓		✓		✓
Strata fixed effects	✓	✓	✓	✓	✓	✓
Lasso selected controls	✓	✓	✓	✓	✓	✓

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 a dummy 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 Round 1 versus Round 2 job assignment.
- Columns (1) and (2) report estimated 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 but not for Round 1 work arrangement. Even-numbered columns report results when controlling for both Round 2 and Round 1 work arrangements. All columns include strata fixed effects and controls selected by double post lasso.
- Standard errors in parentheses $(\cdot)$ are Huber-White (robust to heteroskedasticity). Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Table 2: 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:

– Standard errors in parentheses $(\cdot)$ are clustered at the worker level. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Table 3: Characterizing work-from-home compliers

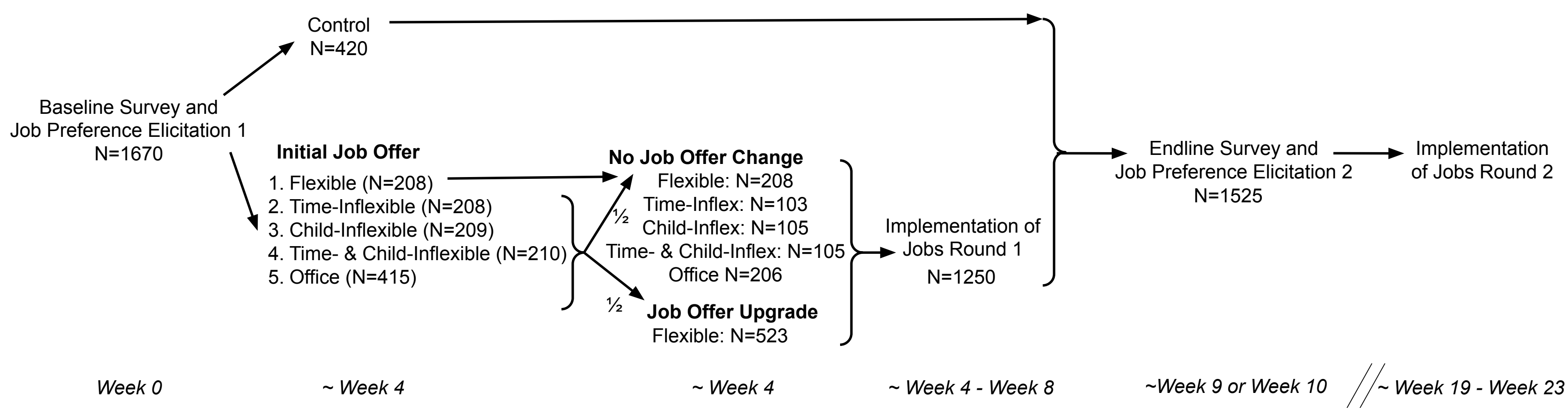
	Characterizing work-from-home compliers					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: Speed			
Work-from-home compliers	2.04	1.59	1.49	1.92	2.04	2.07
	(1.98)	(1.79)	(1.77)	(1.95)	(1.95)	(1.97)
Work-from-home compliers	2.07	1.59	1.56	1.94	2.08	1.99
	(1.58)	(1.40)	(1.40)	(1.55)	(1.55)	(1.55)
Difficult Task	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:

- Standard errors in parentheses $(\cdot)$ are clustered at the worker level. Stars next to coefficients denote significance (* at 10%; ** at 5%; *** at 1%).

Figures

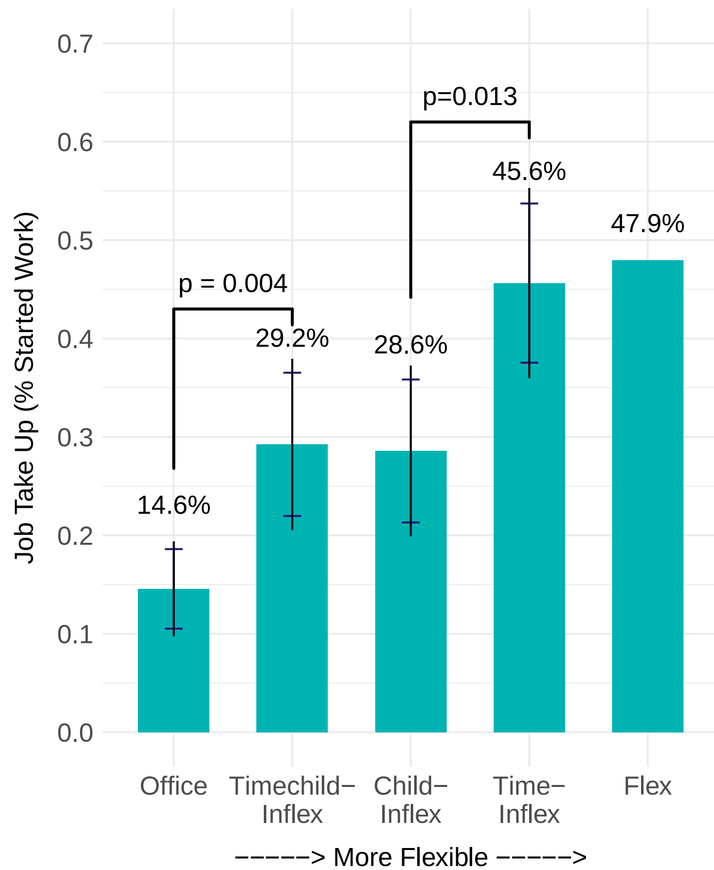
Figure 1: Experimental Design



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



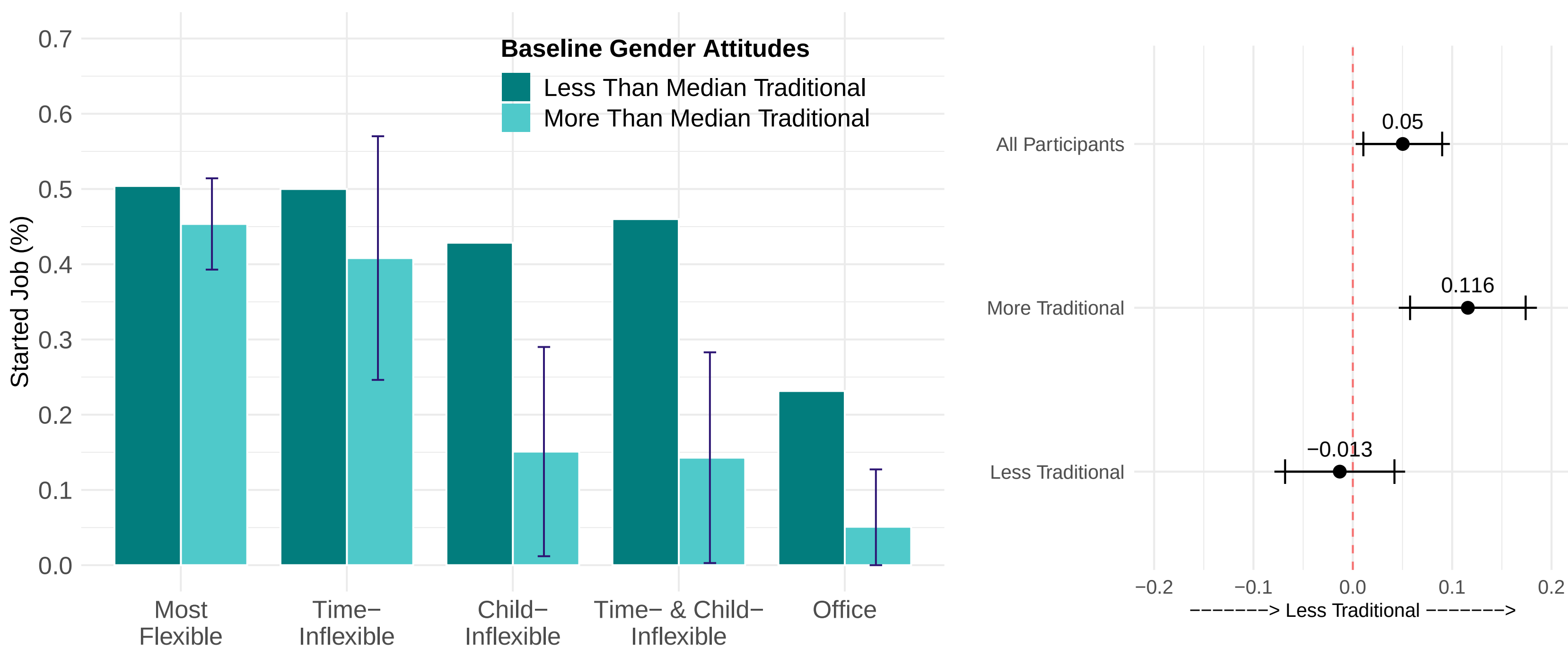
Notes: This figure plots the take up rate for each of the five jobs during the initial intervention (Jobs Round 1).

- Panel A plots job take up for the 1,250 treatment group participants, each of whom receives one job offer. Take up is measured as a binary variable equal to one if the participant starts work (i.e. submits 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. The estimates and standard errors for these regressions, along with pairwise tests of equality between job take up rates, are presented in column 3 of Table A.7.
- The table in Panel B describes how the five jobs sequentially turn on the ability to (1) choose work hours flexibly, (2) multitask work with childcare, and (3) work from home.

Figure 3: Gender attitudes and labor supply response to job flexibility

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

(b) Treatment Effect on Women's Gender Attitudes



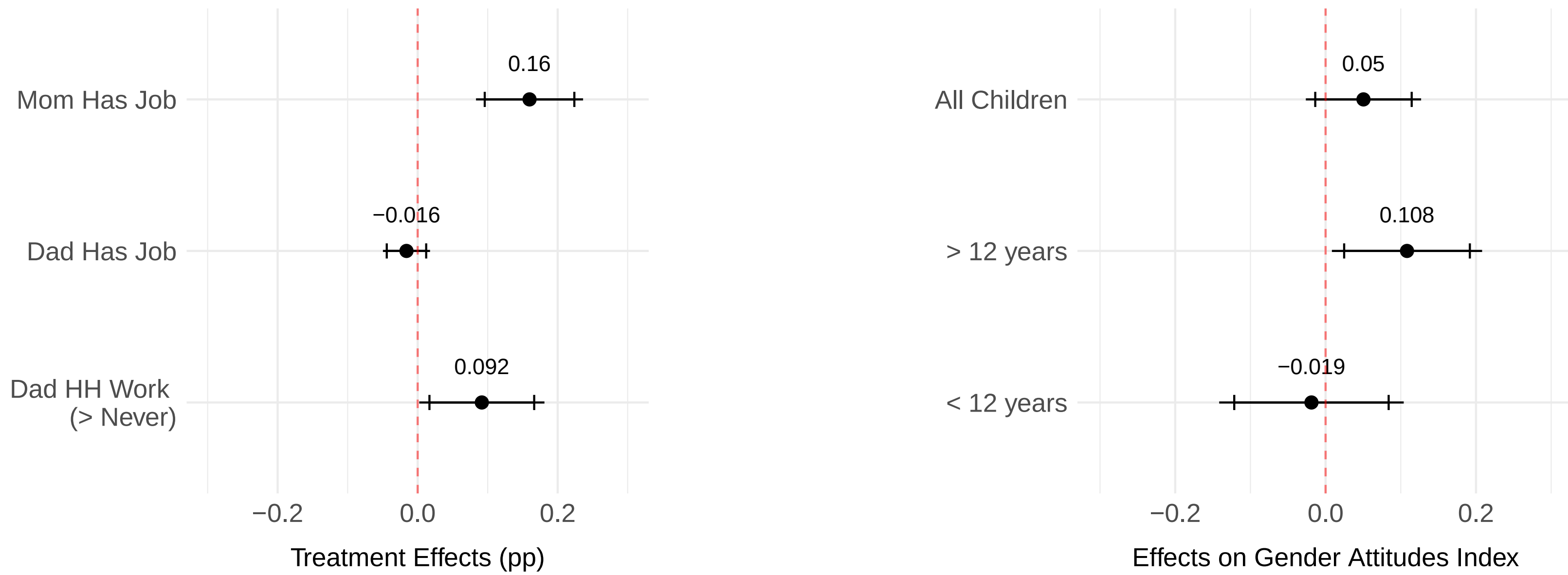
Notes: This figure shows the differential job take up rates (starting work) by baseline household gender attitudes for the 1,250 treatment group participants.

- Take up is measured as a binary variable equal to one if the participant started work (i.e. submitted completed tasks) after the job offer.
- To compare job take up in each category between more traditional and less traditional participants, we regress take up on a binary variable equal to one if the participant's baseline gender attitudes were more traditional than the median participant. Gender attitudes are computed as a weighted average of 16 questions, in which the weights take into account the covariance structure of the components (Anderson, 2008).
- To see results of regressions that estimate the differential importance of job flexibility by baseline gender attitudes, see Table A.10. The results presented in the table come from regressions that also control for household characteristics such as income, age, household composition, education, and religion.
- Confidence intervals at the 90% level are shown.
- Participants' scores on the endline gender attitudes index are regressed on treatment assignment. The regression is first on all participants (top), and then separately on participants with pre-intervention attitudes more traditional (middle) and less traditional (bottom) than those of the median participant.
- The gender attitudes index is computed as a weighted average of questions from the baseline survey or the endline survey, in which the weights take into account the covariance structure of the components (as in Anderson, 2008).
- All specifications include lasso-selected controls and strata fixed effects. Standard errors are heteroskedasticity-robust Huber-White standard errors. Estimates are plotted along with corresponding 90% and 95% confidence intervals.

Figure 4: Treatment Effects on Children

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

(b) Effect on Children's Attitudes



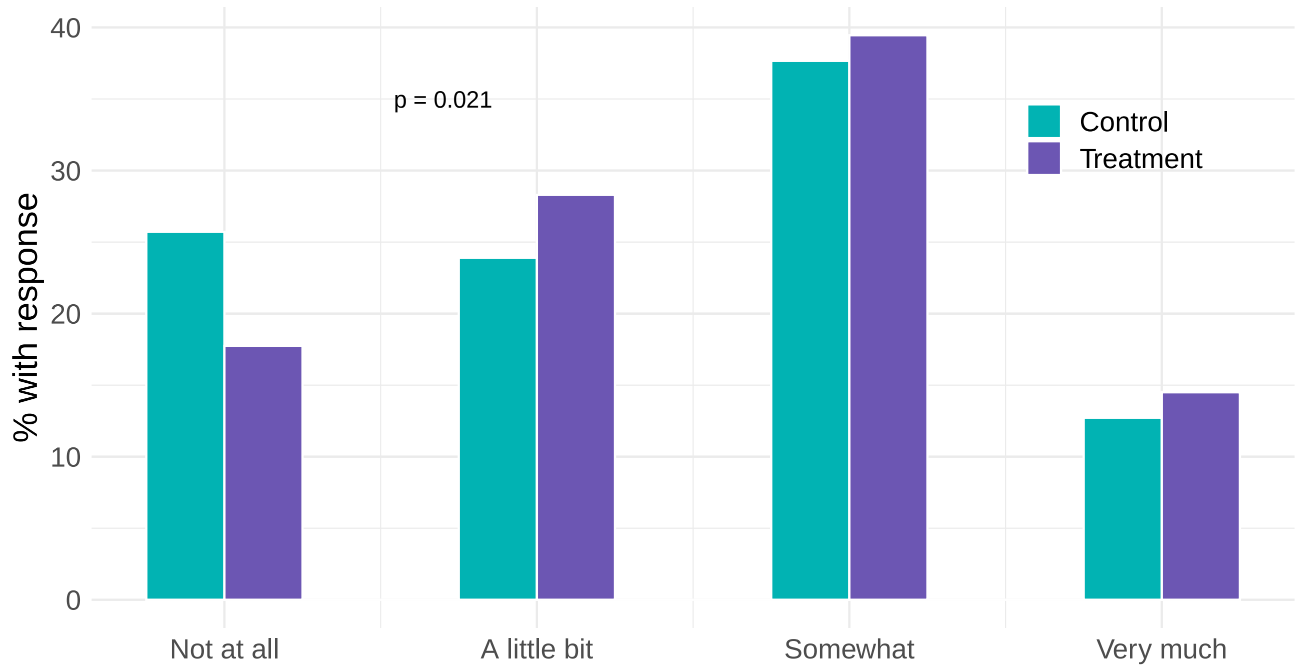
Notes: This figure reports intent-to-treat effects on children, including controls for the baseline survey measure of the outcome when possible.

– Panel A reports effects on whether or not children say that their mother had a job in the last month during the intervention (top), whether or not children say that their father had a job in the last month during the intervention (middle), and a binary variable for whether their father ever helped with childcare, cooking, or cleaning in the last month (bottom).

– Panel B reports effects on children's gender attitudes, first for all children pooled together (top), and then for children older than the median age of 12 (middle), and then 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).

– Standard errors are heteroskedasticity robust. Estimates are plotted along with corresponding 90% and 95% confidence intervals.

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

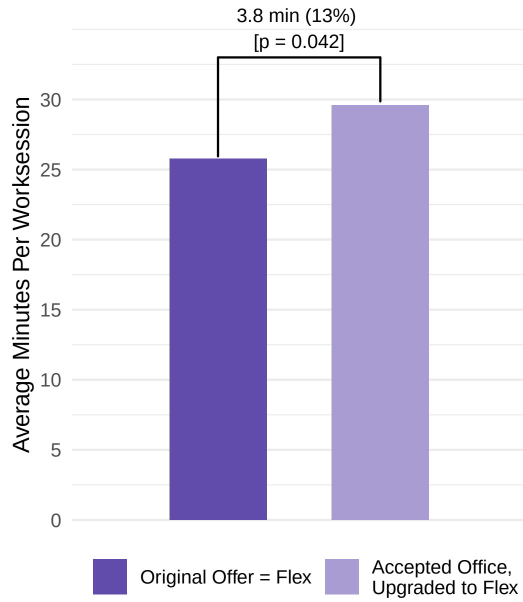


Notes: This figure plots participants' responses to the question, "Do you feel that your full potential and talents are put to good use?" during the endline survey, separating treatment and control participants.

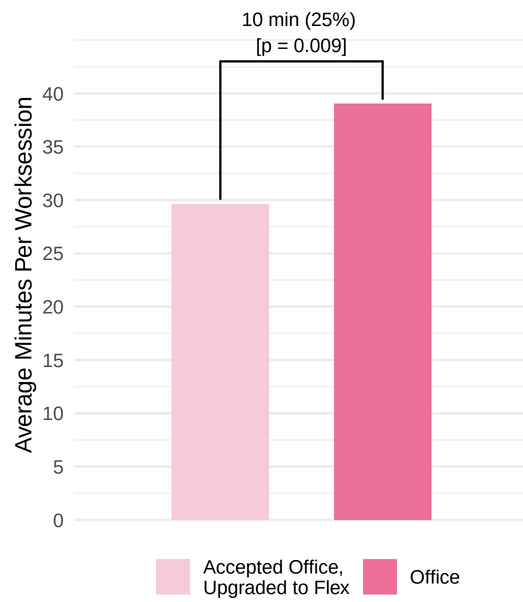
- The green bars represent the fraction of control group participants with each response, while the blue bars represent the fraction of treatment group participants with each response. All treatment groups are pooled in this analysis.
- The reported *p*-value comes from a regression which codes participants' 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

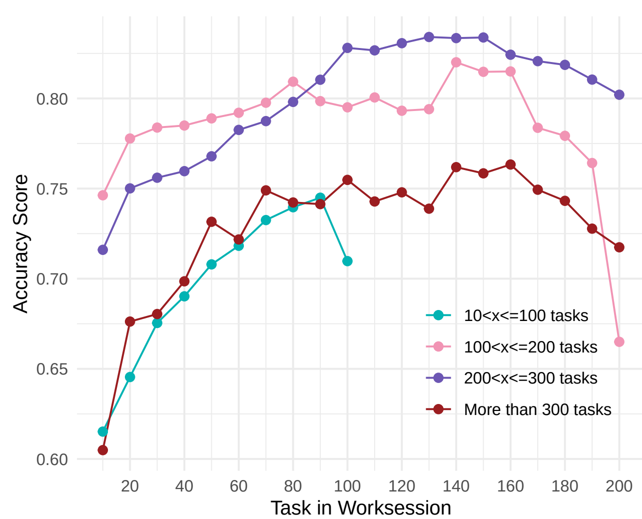
(a) Selection: Worksession Length



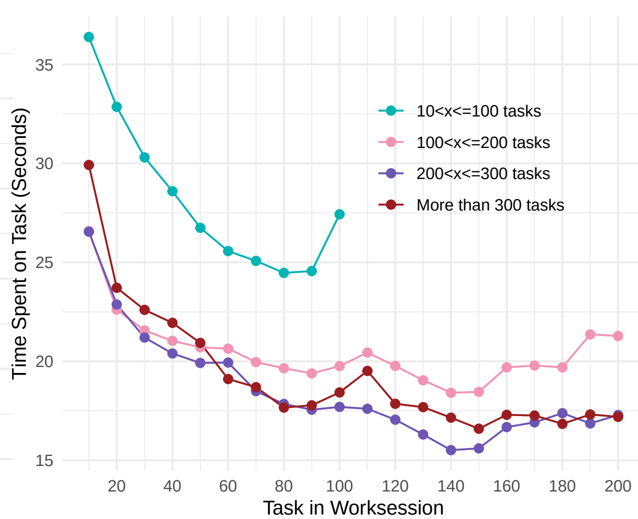
(b) Treatment Effect: Worksession Length



(c) Task accuracy, by task within worksesion



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



Notes: This figure offers evidence for worksession interruptions as a mechanism driving effects on job performance.

– Panel A reports the selection effects of work-from-home on worksession length (minutes elapsed). A worksession is defined as a continuous stretch of work time during which no more than 10 minutes elapse between consecutive tasks. Panel A compares workers who accept the office job and are randomly selected for an upgrade to the most flexible job (lighter bar) with workers who were initially assigned to the most flexible job (darker bar).

– Panel B reports the treatment effects of work-from-home on worksession length (minutes elapsed). The subfigure compares workers who accept an office job and are randomly selected for an upgrade to the most flexible job (lighter bar) with workers who accept an office job and are not randomly selected for an upgrade (darker bar).

– Panels C and D describe how the two key inputs into productivity (average time spent on a task, and task accuracy) change over the course of a worksession. In order to capture "flow effects" rather than selection into longer versus shorter worksessions, worksessions are first grouped according to their number of tasks.

A Appendix

Appendix Figures

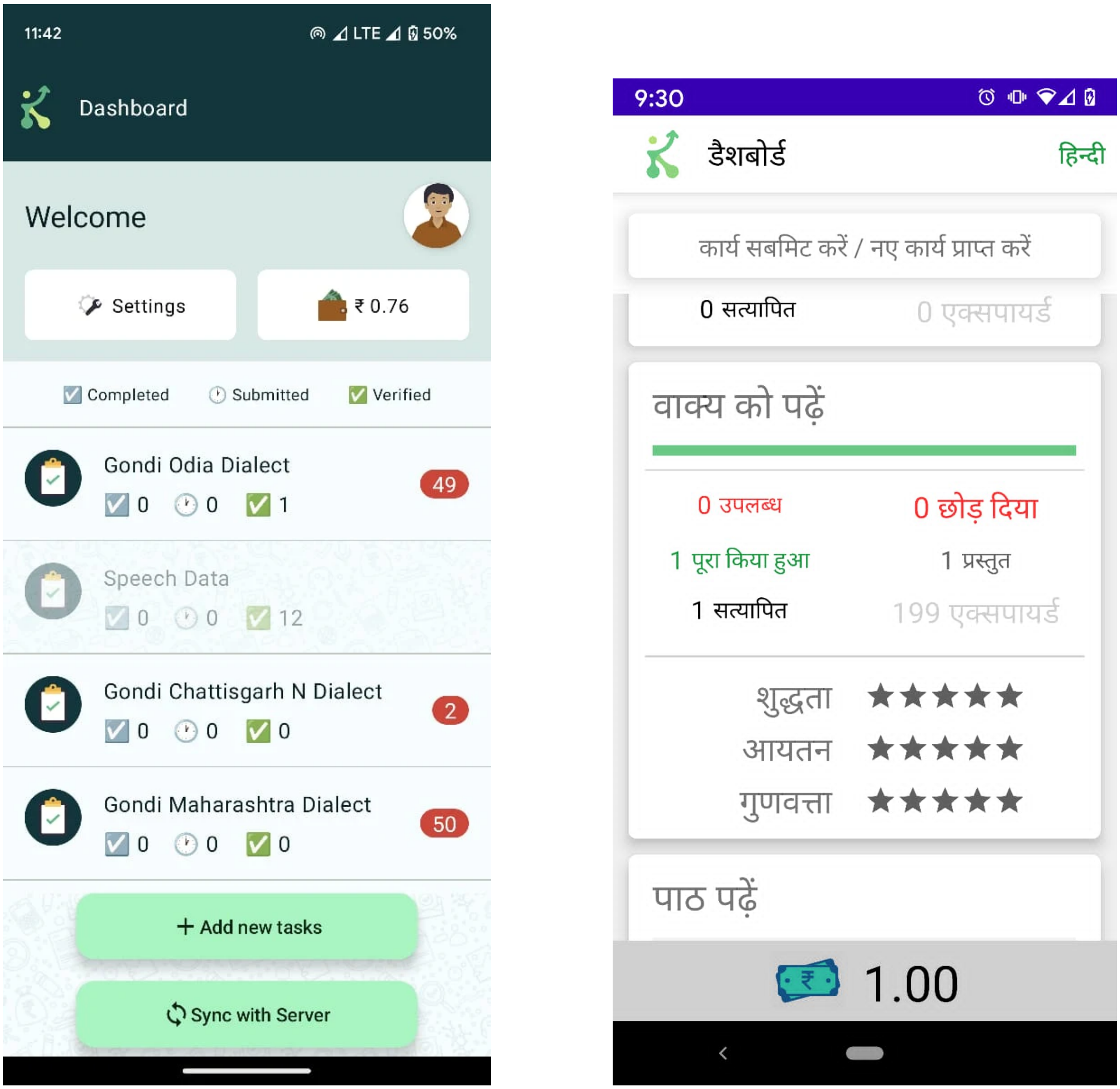
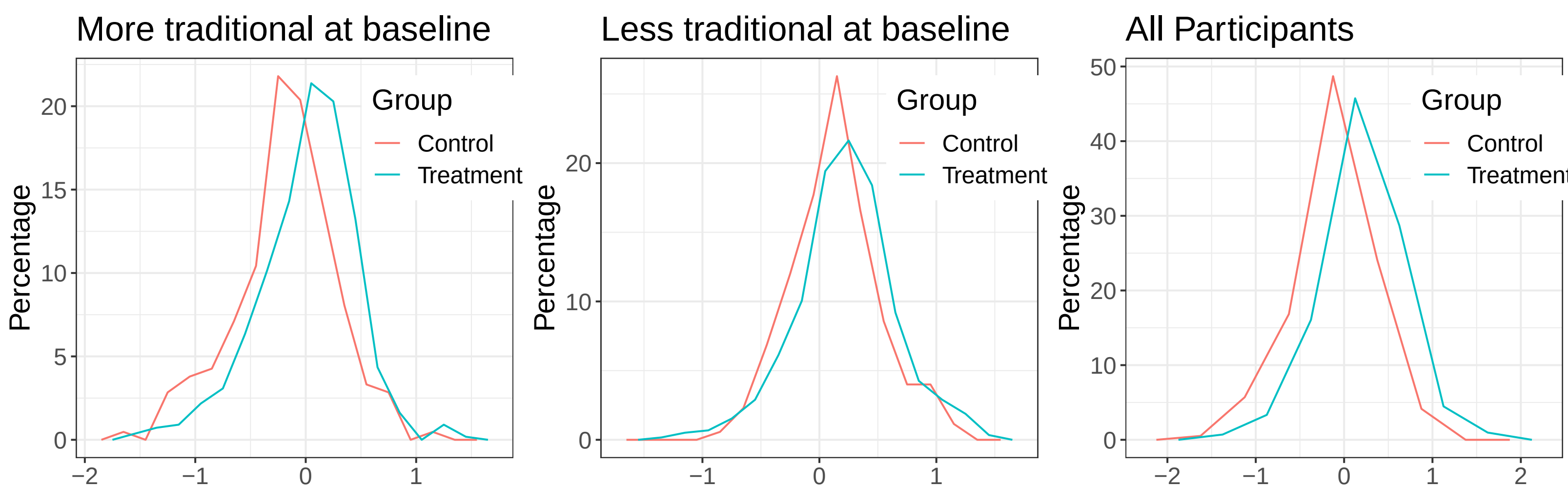
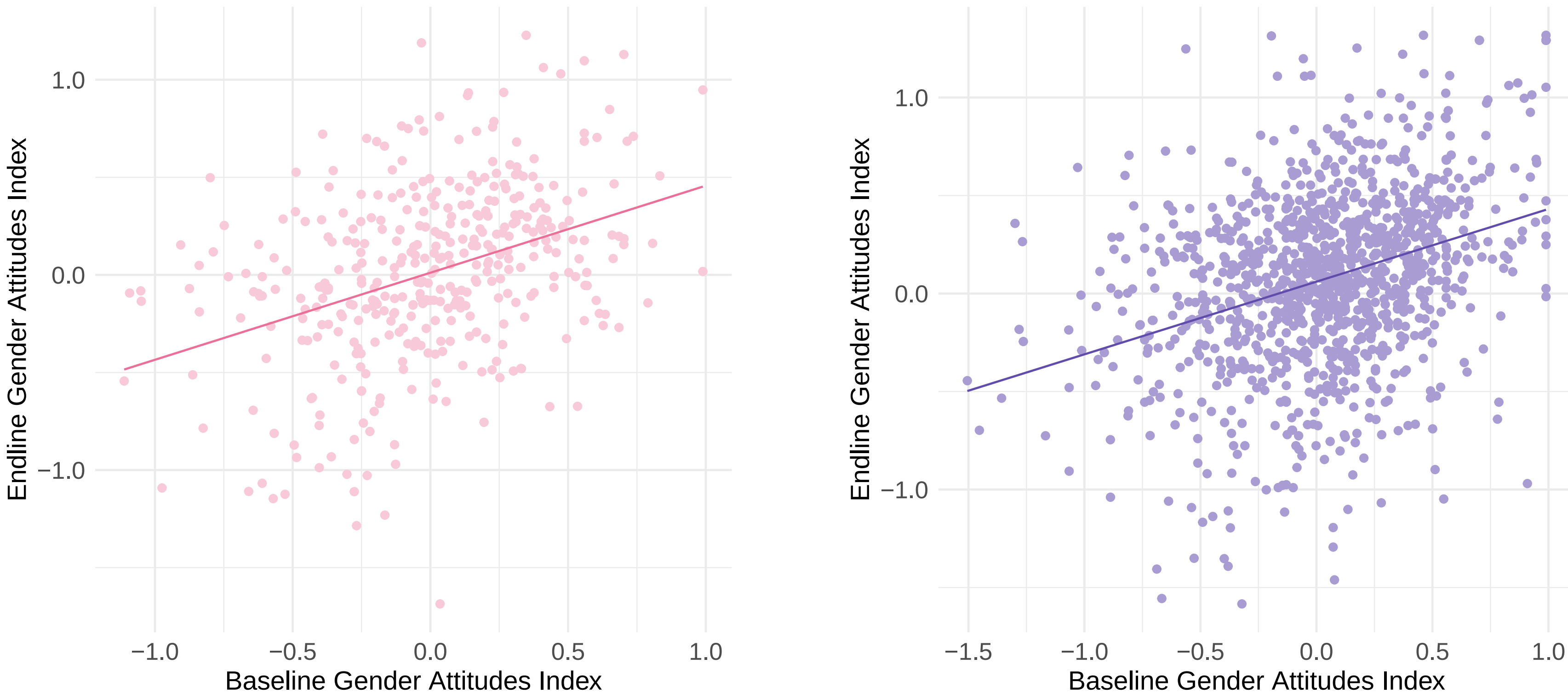


Figure A.7: Karya Job Task Platform

Figure A.8: Gender Attitudes at Baseline and Endline (Histogram)



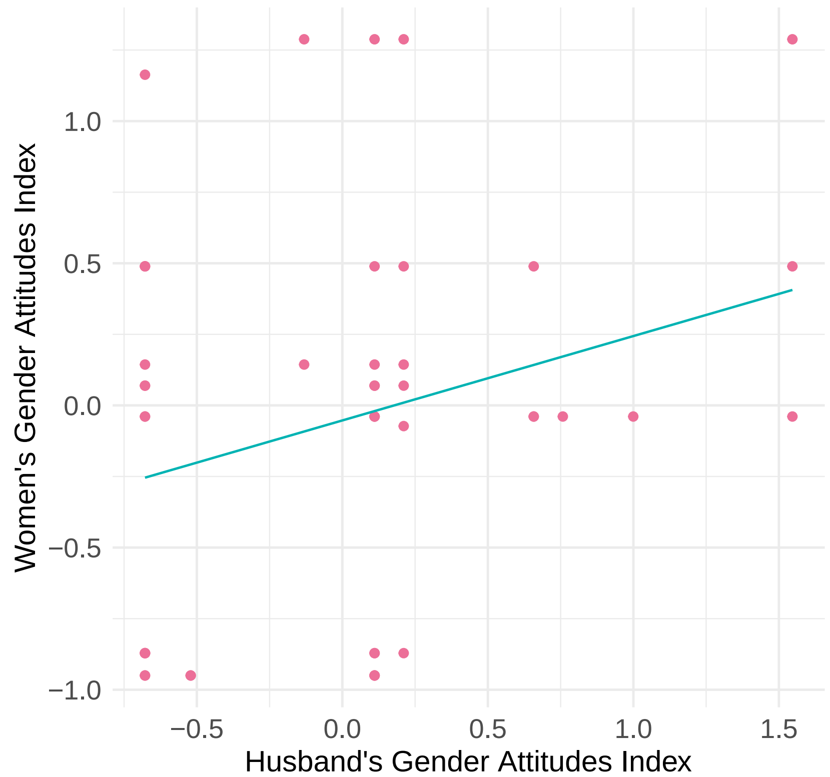


(a) Control Group

(b) Treatment Group

Figure A.9: Gender Attitudes at Baseline and Endline

Figure A.10: Correlation between Gender Attitudes within the Household

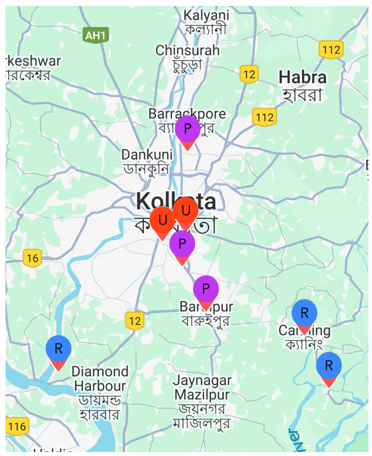


Notes: Data from the study pilot.

Figure A.11: Monotonicity of job take up in flexibility

Did not accept job below, but accepted...	**1 less flexible job**	**2 less flexible jobs**	**3 less flexible jobs**
Most Flexible	<1%	<1%	<1%
Time-Inflexible	4.4%	1.7%	<1%
Child-Inflexible	4.8%	<1%	-
Timechild-Inflexible	6.0%	-	-

Figure A.12: Map of study areas



Appendix Tables

Table A.4: Balance Table (Participants Who Completed the Endline Survey)

	All Participants	Control	Treatment	Pairwise t-test
Endline Complete (=1)	0.913	0.919	0.911	0.585
Age	29.955	29.640	30.062	0.232
Completed 10th Standard (=1)	0.494	0.526	0.483	0.230
Scheduled Caste/Tribe (=1)	0.395	0.394	0.396	0.995
Hindu (=1)	0.762	0.736	0.771	0.064*
Never Married (=1)	0.069	0.060	0.072	0.332
Number HH Members	4.603	4.687	4.575	0.254
Parent-in-Law in HH (=1)	0.403	0.438	0.391	0.098*
Has Child Under 8 (=1)	0.483	0.474	0.486	0.204
Job Decision Final Say (=Self)	0.362	0.370	0.359	0.729
Has Own Smartphone (=1)	0.725	0.731	0.723	0.642
Gender Attitudes Index	0.029	0.001	0.038	0.102
Agency Index	0.017	-0.005	0.025	0.441
Number of participants	1525	386	1139	
F-test of joint significance (F-stat)				1.251
F-test, number of observations				1525

Notes: This table presents summary statistics and balance checks on participant characteristics. Each row shows the mean for that variable for the entire study population, the control group, and the treatment group. The top row of this table records the endline completion rates for all 1,670 participants who were randomized (420 for the control group and 1,250 for the treatment group). Regressions include strata fixed effects. Significance at the 0.10, 0.05, and 0.01 levels are indicated by *, **, and ***, respectively.

Table A.5: Balance Table for Participants Who Were Randomized

	(1) All Participants	(2) Control	(3) Treatment	(2)-(3) Pairwise t-test
Age	29.911	29.760	29.962	0.592
Completed 10th Standard (=1)	0.493	0.510	0.488	0.617
Scheduled Caste/Tribe (=1)	0.385	0.376	0.388	0.968
Hindu (=1)	0.754	0.724	0.764	0.094*
Never Married (=1)	0.069	0.062	0.071	0.298
Number HH Members	4.626	4.702	4.601	0.332
Parent-in-Law in HH (=1)	0.402	0.433	0.392	0.119
Has Child Under 8 (=1)	0.478	0.471	0.481	0.185
Job Decision Final Say (=Self)	0.359	0.376	0.353	0.382
Has Own Smartphone (=1)	0.725	0.731	0.723	0.366
Gender Attitudes Index	0.025	-0.000	0.034	0.112
Agency Index	0.013	-0.000	0.018	0.674
Number of participants	1670	420	1250	
F-test of joint significance (F-stat)				1.152
F-test, number of observations				1670

Notes: The data in this table are from women's baseline surveys and compares participants who were randomized into the control group versus one of the treatment groups. All job treatment groups are pooled in this table. The regressions include strata fixed effects.

Table A.6: Demographics Comparison Table for Study Sample and Large-scale Household Survey Data

	(1) Study Sample		(2) Large-scale Survey Data	
Variable	N	Mean/SE	N	Mean/SE
Scheduled Caste/Tribe (=1)	1553	0.414	18658	0.466
		(0.013)		(0.004)
Other Backward Classes (=1)	1553	0.104	18658	0.164
		(0.008)		(0.003)
Open/General (=1)	1553	0.482	18658	0.340
		(0.013)		(0.003)
Hindu (=1)	1670	0.754	24200	0.736
		(0.011)		(0.003)
Muslim (=1)	1670	0.244	24200	0.246
		(0.011)		(0.003)
Number HH Members	1670	4.626	24200	4.724
		(0.042)		(0.013)
Parent-in-Law in HH (=1)	1649	0.400	24200	0.370
		(0.012)		(0.003)
Has Child Under 8 (=1)	1670	0.516	24200	0.478
		(0.012)		(0.003)
Monthly Household Income (Rs.)	1583	11791.437	5710	13497.015
		(299.440)		(221.514)
Age	1670	29.911	19359	32.432
		(0.199)		(0.067)
Currently Married (=1)	1670	0.902	24200	0.818
		(0.007)		(0.002)
Never Attended School (=1)	1670	0.004	19359	0.218
		(0.002)		(0.003)
Completed High School (=1)	1670	0.142	19359	0.490
		(0.009)		(0.004)
Has Own Cellphone (=1)	1670	0.881	2919	0.512
		(0.008)		(0.009)
Has Never Worked for Pay (=1)	1670	0.688	5127	0.737
		(0.011)		(0.006)

Notes: This table compares study participants to 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. 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 for women-level characteristics to women between 18 and 60 years.

Table A.7: Effect of Flexible Job Attributes on Take Up of Work

	Job Take Up		
	Baseline	Job Offer	Start Work
	(1)	(2)	(3)
Time-inflexible	-0.04***	-0.04	-0.02
	(0.01)	(0.04)	(0.05)
	[0.00]	[0.38]	[0.66]
Child-inflexible	-0.27***	-0.17***	-0.19***
	(0.01)	(0.05)	(0.05)
	[0.00]	[0.00]	[0.00]
Time- & Child-inflexible	-0.32***	-0.15***	-0.19***
	(0.01)	(0.05)	(0.05)
	[0.00]	[0.00]	[0.00]
Office	-0.42***	-0.25***	-0.33***
	(0.01)	(0.04)	(0.03)
	[0.00]	[0.00]	[0.00]
Observations	8,290	1,250	1,250
Most Flexible Job Take Up Rate	0.98	0.75	0.48
P-val: equality of coefficients			
Time-inflex == Child-inflex	0.000	0.006	0.010
Time-inflex == Time & Child-inflex	0.000	0.018	0.013
Time-inflex == Office	0.000	0.000	0.000
Child-inflex == Time & Child-inflex	0.000	0.705	0.914
Child-inflex == Office	0.000	0.061	0.006
Time & Child-inflex == Office	0.000	0.020	0.004

Notes: This table presents the impacts of flexible work arrangements on job take up.

– The estimates come from regressions where the outcome variable is take up, and the regressors are dummy variables for each of the four work arrangements that are not the most flexible job ("Flex"). No control variables are included. Take up is measured as a dummy variable equal to 1 if the participant took up the job.

– Each column shows a different definition of job take up. Column (1) measures take up according to whether, on the baseline survey, the participant says she would accept the job if offered it. Column (2) measures take up according to whether the participant says yes when called with a job offer by the jobs team. Column (3) measures take up according to whether the participant actually begins work (i.e. submitted job tasks to the employer for review).

– At baseline, each participant was asked about each of the five work arrangements (in randomized order). At the job offer and starting work stage, each

Table A.8: Effect of job attributes on take up: vary number of tasks

	Number Tasks Completed		
	>10 tasks	> 50 tasks	> 100 tasks
	(1)	(2)	(3)
Time-inflexible	-0.02	-0.02	-0.01
	(0.05)	(0.05)	(0.05)
Multitasking-with-childcare inflexible	-0.19***	-0.19***	-0.18***
	(0.05)	(0.05)	(0.05)
Time- & multitasking-with-childcare inflexible	-0.19***	-0.18***	-0.18***
	(0.05)	(0.05)	(0.05)
Office	-0.34***	-0.33***	-0.33***
	(0.03)	(0.03)	(0.03)
Observations	1,250	1,250	1,250
Most flexible	0.47	0.46	0.46

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table A.9: Effect of job attributes on take up (with controls)

Dependent Variables:	Baseline	Job Offer	Start Work
Model:	(1)	(2)	(3)
Variables			
Time inflexible	-0.04***	-0.04	-0.01
	(0.01)	(0.04)	(0.05)
Child inflexible	-0.27***	-0.17***	-0.20***
	(0.01)	(0.04)	(0.05)
Time & child inflexible	-0.32***	-0.13***	-0.17***
	(0.01)	(0.04)	(0.05)
Location inflexible	-0.42***	-0.25***	-0.34***
	(0.01)	(0.04)	(0.03)
Fixed-effects			
strata_control	Yes	Yes	Yes
Fit statistics			
Observations	8,290	1,250	1,250
Flex Take Up	0.98	0.75	0.48

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work (i.e. submitted job tasks to the employer for review). At baseline, each participant was asked about each of the five work arrangements (in randomized order). At the job offer and starting work stage, each participant had been randomized to one work arrangement. The regressors are four dummy variables, one for each inflexible work arrangement. The omitted group represents the percentage of participants who took up the most flexible job. These regressions control for strata fixed effects and the following participant covariates: age, marital status, employment history, educational attainment, cohabitation with parents-in-law, religion, and household members.

Table A.10: Differential Importance of Flexibility by Gender Attitudes and Young Children

	Baseline (1)	Job Offer (2)	Start Work (3)
Panel A: All Participants			
Flexible Job	0.34***	0.19***	0.34***
	(0.01)	(0.04)	(0.03)
>Median Traditional Gender Atts	-0.06***	-0.12***	-0.23***
	(0.02)	(0.03)	(0.04)
Flexible Job × >Median Traditional Gender Atts	0.04**	0.02	0.17***
	(0.02)	(0.06)	(0.05)
Take Up Rate for < Median Traditional, Inflexible Jobs	0.67	0.60	0.33
P-val: equality of coefficients			
Flexible Job: More Traditional == Less Traditional	0.034	0.026	0.109
Observations	8,290	1,250	1,250
Panel B: Participants without children under 8			
Flexible Job	0.25***	0.17***	0.24***
	(0.01)	(0.06)	(0.05)
>Median Traditional Gender Atts	-0.07***	-0.10**	-0.19***
	(0.02)	(0.05)	(0.06)
Flexible Job × >Median Traditional Gender Atts	0.05***	0.03	0.04
	(0.02)	(0.08)	(0.07)
Take Up Rate for < Median Traditional, Inflexible Jobs	0.77	0.62	0.35
P-val: equality of coefficients			
Flexible Job: More Traditional == Less Traditional	0.238	0.196	0.002
Observations	4,320	649	649
Panel C: Participants with children under 8			
Flexible Job	0.42***	0.21***	0.44***
	(0.02)	(0.06)	(0.04)
>Median Traditional Gender Atts	-0.02	-0.13***	-0.26***
	(0.03)	(0.05)	(0.05)
Flexible Job × >Median Traditional Gender Atts	0.00	0.02	0.30***
	(0.03)	(0.08)	(0.07)
Take Up Rate for < Median Traditional, Inflexible Jobs	0.55	0.58	0.31
P-val: equality of coefficients			
Flexible Job: More Traditional == Less Traditional	0.075	0.065	0.408
Observations	3,970	601	601

Notes: This table presents the heterogeneous importance of job flexibility for take up by baseline gender attitudes and having a young child. Panel A shows heterogeneity by baseline gender attitudes among all participants, while Panels B and C show heterogeneity by baseline gender attitudes among different subgroups (without children under age 8 and with children under 8, respectively).

- Job take up is measured in three ways: in column (1), whether the participant says on the baseline survey that she would accept the job if offered it, in column (2), whether she accepts the job when called by the jobs team, and in column (3), whether she starts work.
- Gender attitudes are measured by a weighted index of 16 questions from the baseline survey, with weights accounting for the covariance structure of the index components (as in Anderson, 2008).
- "Flexible Job" is a dummy variable equal to one for the flexible and time-inflexible jobs, and is equal to zero for all other work arrangements.
- These regressions control for age, marital status, previous employment, completion of 10th standard, living with parents-in-law, religion, number of household members, whether the participant has a child under age eight (in Panel A), smartphone ownership, and region of Kolkata.
- Standard errors in parentheses are robust to heteroskedasticity and clustered at the participant level for column (1). Stars next to coefficients represent unadjusted p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A.11: Heterogeneous Impact of Flexibility on Take-Up by Who Has Final Say in Labor Supply

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.36***	0.18***	0.29***
	(0.01)	(0.04)	(0.03)
Final Say in Job Decision = Self	0.14***	0.09**	0.15***
	(0.02)	(0.04)	(0.04)
Flexible Job × Final Say in Job Decision = Self	-0.12***	0.01	-0.08
	(0.02)	(0.06)	(0.06)
Inflexible Take-Up, Final Say Not Self	0.59	0.51	0.16
P-val: equality of coefficients			
Flexible Job: Self Has Final Say = Other Has Final Say	0.013	0.026	0.051
Observations	8,290	1,250	1,250

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The third row of coefficients is the (negative) differential importance of flexibility for participants who say on the baseline survey that they have the final say in their own labor supply.

Table A.12: Heterogeneous Impact of Flexibility on Take-Up by Having Young Child

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.22***	0.16***	0.22***
	(0.01)	(0.04)	(0.04)
Has Child Under 8	-0.20***	-0.06*	-0.09**
	(0.02)	(0.03)	(0.04)
Flexible Job × Has Child Under 8	0.19***	0.05	0.08
	(0.02)	(0.06)	(0.05)
Inflexible Take-Up, No Child Under 8	0.74	0.57	0.26
P-val: equality of coefficients			
Flexible Job: Has Child = No Child	0.394	0.793	0.770
Observations	8,290	1,250	1,250

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The third row of coefficients is the differential importance of flexibility for participants who have a child under eight years old.

Table A.13: Heterogeneous Impact of Flexibility on Take-Up by Educational Attainment

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.33***	0.19***	0.22***
	(0.01)	(0.04)	(0.03)
>10th Standard	0.03*	0.11***	0.12***
	(0.02)	(0.03)	(0.04)
Flexible Job × >10th Standard	-0.03*	-0.02	0.07
	(0.02)	(0.06)	(0.05)
Inflexible Take-Up, <10th Standard	0.70	0.52	0.20
P-val: equality of coefficients			
Flexible Job: <10th == >10th	0.782	0.023	0.000
Observations	8,290	1,250	1,250

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The third row of coefficients is the additional importance of flexibility for participants who finished at least 10th standard.

Table A.14: Heterogeneous Impact of Flexibility on Take-Up by Household Income

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.33***	0.20***	0.24***
	(0.01)	(0.04)	(0.04)
High Income	0.05***	0.05	0.03
	(0.02)	(0.04)	(0.04)
Flexible Job × High Income	-0.03	-0.02	0.04
	(0.02)	(0.06)	(0.05)
Inflexible Take-Up, Low Income	0.62	0.52	0.21
P-val: equality of coefficients			
Flexible Job: High Income == Low Income	0.006	0.585	0.067
Observations	7,860	1,185	1,185

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The third row of coefficients is the differential importance of flexibility for participants who with household incomes higher than the median in the study.

Table A.15: Heterogeneous Impact of Flexibility on Take-Up by Cohabitation with Parent(s)-in-Law

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.31***	0.22***	0.25***
	(0.01)	(0.03)	(0.03)
Lives with In-Laws	-0.03	0.02	-0.03
	(0.02)	(0.04)	(0.04)
Flexible Job × Lives with In-Laws	0.02	-0.11*	0.02
	(0.02)	(0.06)	(0.05)
Inflexible Take-Up, Not Living with In-Laws	0.65	0.54	0.23
P-val: equality of coefficients			
Flexible Job: With In-Laws == Without In-Laws	0.354	0.042	0.702
Observations	8,290	1,250	1,250

Note: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The third row of coefficients is the additional importance of flexibility for participants who live with at least one parent-in-law.

Table A.16: Heterogeneous Impact of Flexibility on Take-Up by Baseline Gender Attitudes (Continuous)

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.32***	0.18***	0.25***
	(0.01)	(0.03)	(0.03)
Baseline Gender Attitudes (Continuous)	0.11***	0.19***	0.22***
	(0.02)	(0.04)	(0.05)
Flexible Job × Baseline Gender Attitudes (Continuous)	-0.08***	-0.04	-0.11*
	(0.02)	(0.07)	(0.06)
Standard-Errors	hid	Heteroskedasticity-robust	
Observations	8,290	1,250	1,250
Inflexible Take-Up	0.70	0.52	0.20

Notes: This table presents the results of regressions where the outcome variable is a dummy variable equal to 1 if the participant took up the job offer. Each column shows a different way of measuring job takeup. Column (1) measures take up according to whether or not the participant said they would accept the job if offered it during the baseline survey. Column (2) measures take up according to whether the participant said yes when actually called with a job offer by the jobs team. Column (3) measures take up according to whether or not the participant actually began work. The second row of coefficients is the increased job take up rate for participants who are 1 SD less traditional than average. The third row of coefficients is the (negative) additional importance of flexibility for participants who are 1 SD less traditional.

Table A.17: Effect of Job Treatments (Pooled) on Gender-Related Attitudes

Dependent Variables:	All Gender Attitudes	HH Roles	Women & Work	Technology
Model:	(1)	(2)	(3)	(4)
Variables				
Treatment	0.10*	0.05	0.08	0.09*
	(0.05)	(0.05)	(0.05)	(0.06)
Baseline Gender Attitudes	0.91***	0.79***	0.48***	0.66***
	(0.06)	(0.05)	(0.06)	(0.06)
Fixed-effects				
Strata FE	Yes	Yes	Yes	Yes
Fit statistics				
Observations	1,522	1,522	1,522	1,522
Control Mean	0.00	0.00	0.00	0.00

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Column (1) is an index which includes all 15 gender-related attitudes questions, computed as sum of responses divided by number of questions answered by the respondent. The responses for each question range from 0-3 (strongly disagree, disagree, agree, strongly agree). The regressions include controls for age, whether currently married, whether ever employed before, whether completed 10th standard, whether living with in-laws, whether Hindu, number of household members, and baseline gender-related attitudes. Regressions also includes strata fixed effects, where strata are determined by region, whether participants own their own smartphone, and whether the participant has a child under the age of eight.

Table A.18: Effect of Job Treatments (By Work Arrangement) on Gender Attitudes

	All Gender Attitudes							
	ITT Estimates				2SLS Estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
arrangement = Flex	0.08***	0.08***	0.06**	0.06**				
	(0.03)	(0.03)	(0.03)	(0.03)				
arrangement = Time-Inflex	0.11**	0.11**	0.10**	0.10**				
	(0.05)	(0.05)	(0.04)	(0.04)				
arrangement = Child-Inflex	0.08	0.08*	0.06	0.06				
	(0.05)	(0.05)	(0.05)	(0.05)				
arrangement = Time- & Child-Inflex	-0.07	-0.08	-0.06	-0.06				
	(0.06)	(0.06)	(0.05)	(0.05)				
arrangement = Office	0.05	0.05	0.03	0.03				
	(0.04)	(0.04)	(0.04)	(0.04)				
Started Flex					0.15***	0.15***	0.12**	0.12**
					(0.05)	(0.05)	(0.05)	(0.05)
Started Time					0.23**	0.23**	0.22***	0.21**
					(0.10)	(0.10)	(0.08)	(0.09)
Started Child					0.26*	0.26*	0.19	0.18
					(0.15)	(0.15)	(0.14)	(0.14)
Started TimeChild					-0.23	-0.25	-0.15	-0.18
					(0.19)	(0.19)	(0.17)	(0.17)
Started Office					0.34	0.30	0.21	0.18
					(0.25)	(0.25)	(0.23)	(0.23)
Strata Fixed Effects		×		×		×		×
Lasso Selected Controls			×	×			×	×
Observations	1,525	1,525	1,525	1,525	1,525	1,525	1,525	1,525
Control Mean	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Table A.19: Effect of Job Treatments (Pooled) on Gender Attitudes: Heterogeneity by Baseline Gender Attitudes

Dependent Variables:	All Gender Attitudes Index	HH Roles	Women & Work	Tech	Ability
Model:	(1)	(2)	(3)	(4)	(5)
Treatment	0.70*	0.19	0.27*	0.18	0.06
	(0.37)	(0.16)	(0.14)	(0.16)	(0.09)
Less Traditional × Treatment	-0.02	0.03	-0.13	0.07	0.02
	(0.43)	(0.19)	(0.15)	(0.18)	(0.10)
Baseline Gender Attitudes	0.44***	0.18***	0.09***	0.11***	0.05***
	(0.04)	(0.01)	(0.01)	(0.02)	(0.01)
Strata FE	Yes	Yes	Yes	Yes	Yes
Observations	1,524	1,524	1,524	1,524	1,524
Control Mean	6.43	2.55	3.87	0.00	0.00

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Note: Same as previous table, looking at heterogeneous effects by baseline gender attitudes index.

Table A.20: Treatment Effect of Jobs Intervention on Gender Attitudes - Heterogeneity by Baseline Attitudes (Continuous Version)

	Endline Gender Attitudes							
	ITT Estimates				2SLS Estimates			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	0.05**	0.05**	0.05**	0.05**				
	(0.02)	(0.02)	(0.02)	(0.02)				
Treatment × Baseline Gender Attitudes	-0.08	-0.09	-0.10	-0.12*				
	(0.07)	(0.07)	(0.06)	(0.07)				
Started Work					0.14**	0.14**	0.15**	0.15**
					(0.06)	(0.06)	(0.06)	(0.06)
Started Work × Baseline Gender Attitudes					-0.25	-0.28	-0.33*	-0.36**
					(0.18)	(0.18)	(0.18)	(0.18)
Baseline Gender Attitudes	0.45***	0.43***	0.26***	0.26***	0.45***	0.43***	0.26***	0.26***
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Strata Fixed Effects		×		×		×		×
Lasso Selected Controls			×	×			×	×
Observations	1,525	1,525	1,525	1,525	1,525	1,525	1,525	1,525
Control Mean (Endline)	0.01	0.01	0.01	0.01	0.01	0.01	0.01	0.01

Notes: This table tests for the impact of the job treatments on gender attitudes, examining heterogeneity by baseline attitudes. In this analysis, all treatment groups (work arrangements) are pooled together. Gender attitudes are computed as a standardized, weighted average of questions from the baseline survey or the endline survey, in which the weights take into account the covariance structure of the components (Anderson, 2008). Both baseline and endline gender attitudes are included in this analysis as this continuous index variable with mean zero and standard deviation one. The dependent variable is gender attitudes measured on the endline survey, which took place 1-2 weeks after the end of the job treatments. Columns (1)-(4) present intent-to-treat (ITT) estimates, while columns (5)-(8) present two-stage least squares (2SLS) estimates. Heteroskedasticity- robust standard errors are reported in parentheses. Stars next to coefficients represent unadjusted p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A.21: Effect of Job Treatments on Women's Agency

	Agency Index		
	(1)	(2)	(3)
Panel A: Treatments Pooled			
Treatment	0.04	0.04	0.02
	(0.04)	(0.04)	(0.03)
Control Mean	-0.01	-0.01	-0.01
Panel B: Heterogeneity by Work Arrangement			
Arrangement = Flex or Time-Inflex	0.05	0.05	0.03
	(0.04)	(0.04)	(0.04)
Arrangement = Child- or Child- & Time-Inflex	0.03	0.03	0.01
	(0.06)	(0.06)	(0.05)
Arrangement = Office	0.00	0.00	-0.03
	(0.06)	(0.06)	(0.05)
Control Mean	-0.01	-0.01	-0.01
Panel C: Heterogeneity by Baseline Agency			
Treatment	0.03	0.03	0.03
	(0.05)	(0.05)	(0.04)
Treatment × Baseline Agency	-0.01	-0.01	-0.02
	(0.07)	(0.07)	(0.05)
Control Mean, Low Baseline Agency	-0.18	-0.18	-0.18
Observations	1,525	1,525	1,525
Strata FE fixed effects		✓	✓
Lasso Selected Controls			✓

Notes: This table presents results about the treatment effect of the jobs intervention on participants' agency. In this analysis, all treatment groups are pooled and compared to the control group. The dependent variable in column (1) is a weighted average of seven questions on the endline survey, while the dependent variables in columns (2)-(5) are subsets of the variables in the column (1) index. The weights on the different index components is informed by their covariance, as in Anderson (2008). Column (2) is a binary variable equal to one if the participant names herself as the person who would have the final say in her own labor supply. Column (3) is a physical mobility index composed of three questions (how often the participant leave homes alone, the participant's ability to leave home without asking permission, and the participant's ability to meet friends without permission). Column (4) is an individual purchases index composed of two questions (to what extent the participant can purchase clothes independently, and to what extend she can buy things from the market without asking). Column (5) is the standardized variable for how much of a say the participant has in significant household purchases. The sample size for the sub-indices varies depending on whether participants indicated that the question was relevant to them (e.g. for column (4) whether or not they had made any purchases in the last month, and for column (5) whether their household had made any significant purchases in the last month). All regressions include lasso-selected controls and strata fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. Stars next to coefficients represent unadjusted p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A.22: Effect of Job Treatments on Women's Psychological Well-Being

	Psych Index	Sleep Peacefully	(Not) Overwhelmed	Happy	(Not) Worried
	(1)	(2)	(3)	(4)	(5)
Panel A: Treatments Pooled					
Treatment	0.02	-0.02	0.07	-0.02	-0.01
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Control Mean	0.00	0.00	0.00	0.00	0.00
Panel B: Heterogeneity by Work Arrangement					
Arrangement = Flex or Time	0.03	0.03	0.10	-0.02	-0.01
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)
Arrangement = Child or Timechild	-0.01	-0.12	0.01	0.04	-0.07
	(0.05)	(0.09)	(0.09)	(0.08)	(0.09)
Arrangement = Office	-0.02	-0.13	0.02	-0.04	0.03
	(0.06)	(0.09)	(0.09)	(0.09)	(0.09)
Panel C: Heterogeneity by Baseline Wellness					
Treatment	0.02	-0.06	0.09	0.03	-0.02
	(0.05)	(0.07)	(0.07)	(0.07)	(0.08)
Treatment $\times >$Well at Baseline	0.00	0.08	-0.04	-0.08	0.02
	(0.06)	(0.09)	(0.08)	(0.08)	(0.09)
Control Mean, $<$ Well at Baseline	-0.16	-0.15	-0.10	-0.21	-0.21
Observations	1,524	1,524	1,524	1,524	1,524
Strata FE	✓	✓	✓	✓	✓
Lasso Selected Controls	✓	✓	✓	✓	✓

Notes: This table presents results about the treatment effect of the jobs intervention on participants' psychological wellbeing. In this analysis, all treatment groups are pooled and compared to the control group. The dependent variable in column (1) is a weighted average of four questions on the endline survey, while the dependent variables in columns (2)-(5) are the components making up the column (1) index. The weights on the different index components in column (1) are informed by their covariance, as in Anderson (2008). For the questions corresponding to columns (2)-(5), participants were asked about how often they felt a certain way in the last month (i.e. during the treatment). Column (2) is how often they slept peacefully, column (3) is how often they were generally feeling happy, column (4) is how often they were feeling anxious, and column (5) is how often they felt overwhelmed. The outcomes in columns (4) and (5) are negated so that positive values always correspond to better psychological wellbeing. 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. The answers are all standardized to have mean zero and standard deviation equal to one. All regressions include lasso-selected controls and strata fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. Stars next to coefficients represent unadjusted p-values (* significant at 10%; ** at 5%; *** at 1%).

Table A.23: Treatment Effect on Take Up of Digital vs Non-Digital Jobs

	Started Work in Round 2	
	(1)	(2)
Round 2 Job = Digital	0.02	0.05
	(0.03)	(0.06)
Treatment		0.03
		(0.06)
Round 2 Job = Digital × Treatment		-0.03
		(0.07)
Double Post Lasso Controls + Strata FE	✓	✓
Observations	1,524	1,524

Notes: This table reports the results of regressions comparing job take up between the digital and non-digital jobs during Jobs Round 2. The outcome variable is a dummy equal to one if the participant started the job she was randomly assigned to during round 2, and otherwise equal to zero. In column (1), the only regressor is a boolean variable (whether or not the round 2 job is a digital job or non-digital job). In column (2), this variable is interacted with treatment assignment (whether the participant was assigned to the control group or one of the job groups during Round 1).

Table A.24: Gateway jobs: Heterogeneity by work arrangement, first-time workers

	Started Work in Round 2	
R1 More Flexible Than R2	0.07**	
	(0.03)	
R2: Time-Inflexible × R1 More Flexible Than R2		**0.02**
		(0.06)
R2: Child-Inflexible × R1 More Flexible Than R2		**0.10**
		(0.06)
R2: Time- & Child-Inflexible × R1 More Flexible Than R2		**0.01**
		(0.06)
R2: Office × R1 More Flexible Than R2		**0.08****
		(0.04)
R2 Most Flexible Job Take Up Rate	0.59	0.59
Observations	1,524	1,524

Notes: Coefficients shown for first-time workers only.

Table A.25: Spillovers to Control Group Job Take Up

	Round 2: Started Work					
	R2: Any Job			R2: Flexible Jobs		
	(1)	(2)	(3)	(4)	(5)	(6)
Num Friends (Flexible Jobs)	0.05			0.16**		
	(0.05)			(0.07)		
	[0.333]			[0.018]		
>= 1 Friend (Flexible Jobs)		0.04			0.15	
		(0.08)			(0.15)	
		[0.598]			[0.305]	
Fraction Friends (Flexible Jobs)			0.03			0.10
			(0.08)			(0.13)
			[0.693]			[0.439]
Num Friends (Any Group)	-0.01	0.01	0.02	-0.07	-0.01	0.01
	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)
	[0.879]	[0.703]	[0.608]	[0.167]	[0.893]	[0.841]
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Lasso Selected Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	367	367	367	149	149	149
Control, No Friends Take Up	0.43	0.43	0.43	0.64	0.64	0.64

Notes: This table presents results on spillovers to the control group in terms of starting work during the second round of jobs.

- These regressions only include participants who were in the control group.
- The dependent variable is a binary variable equal to one if the participant started the job that she was assigned to during Jobs Round 2. This outcome is regressed on the participant's total number of friends in the study, along with a measure of these friends' exposure to the flexible jobs (most flexible and time-inflexible, the arrangements with the highest job take up).
- Exposure is measured in three ways: (1) number of friends assigned to flexible jobs, (2) a binary variable equal to one if at least one friend was was assigned to a flexible job, and (3) the fraction of a participant's friends who were assigned to a flexible job.
- Friend lists are collected at baseline as well as during a follow-up survey 4-6 months after the endline survey. In this table, we include any friend who was listed on the baseline survey, as well as friends listed on the follow-up survey who (i) has been friends with the participant for more than one year, and (ii) is a relative or neighbor of the participant, to alleviate concerns that they met these friends through the study.
- Columns (1)-(3) consider the take up of any job arrangements in Round 2, while columns (4)-(6) consider the take up of flexible jobs in Round 2.
- Standard errors in parentheses $(\cdot)$ are robust to heteroskedasticity. Brackets $[\cdot]$ report unadjusted p-values, as do stars next to coefficients (* significant at 10%; ** at 5%; *** at 1%).

Table A.26: Spillover Effects (Flexible & Inflexible Friends) on Round 2 Job Take Up

	Started Work in Round 2					
	All Arrangements			Flex or Time		
	(1)	(2)	(3)	(4)	(5)	(6)
Number Friends (Flexible Jobs)	0.09			0.20^{***}		
	(0.06)			(0.07)		
Number Friends (Inflexible Jobs)	0.07^{*}			0.07		
	(0.04)			(0.04)		
>=1 Friend (Flexible Jobs)		0.04			0.15	
		(0.09)			(0.15)	
>=1 Friend (Inflexible Jobs)		0.00			0.01	
		(0.09)			(0.14)	
Fraction Friends (Flexible Jobs)			0.03			0.10
			(0.08)			(0.13)
Fraction Friends (Inflexible Jobs)			0.02			0.01
			(0.06)			(0.08)
Number Friends (Any Group)	-0.05	0.01	0.02	-0.11^{**}	-0.01	0.01
	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.05)
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Lasso Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	367	367	367	149	149	149
Control, No Friends	0.43	0.43	0.43	0.39	0.39	0.39

Notes:

Table A.28: Effect of Job Treatments (Pooled) on Children's Household Help

	Help Cook	Help Clean	Help Childcare	Dad Helps (=1) (More Than Never)
	(1)	(2)	(3)	(4)
Treatment	0.01	0.10	0.03	0.09**
	(0.11)	(0.11)	(0.15)	(0.05)
	[0.924]	[0.355]	[0.853]	[0.045]
Control Mean (Endline)	1.13	1.09	2.47	0.58
Heterogeneity by Child Age				
Treatment	0.15	0.26*	-0.13	0.17***
	(0.14)	(0.14)	(0.19)	(0.06)
	[0.284]	[0.064]	[0.494]	[0.005]
Treatment $\times < 12$	-0.29	-0.31	0.36	-0.19**
	(0.23)	(0.23)	(0.30)	(0.09)
	[0.201]	[0.168]	[0.237]	[0.037]
< 12	-0.04	-0.17	-0.57**	0.19**
	(0.20)	(0.20)	(0.25)	(0.08)
	[0.826]	[0.388]	[0.026]	[0.016]
Control Mean, > 12	1.39	1.38	2.51	0.51
Heterogeneity by Child Gender				
Treatment	-0.22	-0.02	-0.01	0.15**
	(0.17)	(0.17)	(0.19)	(0.07)
	[0.211]	[0.925]	[0.958]	[0.029]
Treatment $\times$ Male	0.40*	0.17	0.06	-0.10
	(0.22)	(0.22)	(0.28)	(0.20)
	[0.076]	[0.439]	[0.820]	[0.274]
Male	-0.48**	-0.49**	-0.15	0.13
	(0.20)	(0.20)	(0.24)	(0.08)
	[0.016]	[0.013]	[0.536]	[0.104]
Control Mean, Female	1.39	1.38	2.51	0.51
Observations	606	606	315	603
Strata Fixed Effects	$\times$	$\times$	$\times$	$\times$
Baseline Behavior	$\times$	$\times$	$\times$	$\times$

Note: Each question is scored from 0-4, where 0 is "never," 1 is "less than half the days," 2 is "around half the days," 3 is "more than half the days," and 4 is "every day."

Table A.29: Effect of Job Treatments (Pooled) on Children's Aspirations

	Aspire - UGrad (1)	Aspire - Masters (2)
Treatment	0.00	-0.03
	(0.03)	(0.04)
Control Mean (Endline)	0.88	0.34
Heterogeneity by Child Age		
Treatment	-0.04	0.01
	(0.04)	(0.06)
Treatment $\times$ Age $<$ 12	0.09	-0.08
	(0.06)	(0.09)
Age $<$ 12	-0.06	0.04
	(0.05)	(0.08)
Control Mean, Age $>$ 12	0.91	0.33
Heterogeneity by Child Gender		
Treatment	0.00	0.01
	(0.04)	(0.07)
Treatment $\times$ Male	-0.02	-0.07
	(0.06)	(0.09)
Male	-0.01	-0.02
	(0.05)	(0.08)
Control Mean, Female	0.89	0.36
Observations	602	602

Note: The outcome variable in column (1) is a dummy variable equal to 1 if the child says they would like to finish at least an undergraduate degree. The outcome variable in column (1) is a dummy variable equal to 1 if the child says they would like to finish at least a master's degree.

Table A.30: Quality of Tasks (Selection)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
Accept Time-Inflex, Then Upgrade	0.03	0.00	0.02	0.02	0.01
	(0.12)	(0.04)	(0.04)	(0.05)	(0.01)
Accept Child-Inflex, Then Upgrade	-0.10	-0.04	-0.01	-0.05	-0.01
	(0.13)	(0.05)	(0.04)	(0.05)	(0.01)
Accept Timechild-Inflex, Then Upgrade	0.00	-0.02	0.02	0.00	0.00
	(0.09)	(0.03)	(0.03)	(0.04)	(0.01)
Accept Office, Then Upgrade	0.02	0.00	0.03	-0.01	0.00
	(0.08)	(0.03)	(0.03)	(0.03)	(0.01)
Observations	949,543	949,543	949,543	949,543	949,543
Flexible Mean	1.79	1.83	1.55	5.17	0.93
Task type fixed effects	✓	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓	✓

Notes:

Table A.31: Quality of Tasks (Impact)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
Time-Inflexible					
Upgraded to Flex	-0.05	-0.02	-0.02	-0.01	0.00
	(0.13)	(0.05)	(0.04)	(0.05)	(0.01)
Child-Inflexible					
Upgraded to Flex	-0.03	0.24***	-0.12***	-0.15*	0.05**
	(0.19)	(0.09)	(0.05)	(0.08)	(0.02)
Timechild-Inflexible					
Upgraded to Flex	0.20	0.39***	-0.12***	-0.07	0.08***
	(0.16)	(0.09)	(0.02)	(0.08)	(0.02)
Office					
Upgraded to Flex	-0.20***	-0.08***	-0.05**	-0.08*	-0.03***
	(0.08)	(0.02)	(0.02)	(0.04)	(0.01)
Observations	273,942	273,942	273,942	273,942	273,942
Flexible Mean	1.79	1.83	1.55	5.17	0.93
Task Type fixed effects	✓	✓	✓	✓	✓
Week fixed effects	✓	✓	✓	✓	✓

Notes:

Table A.32: Quality of Tasks By Task Difficulty (Selection)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
Accept Time-Inflex, Then Upgrade	0.04	0.00	0.02	0.02	0.01
	(0.12)	(0.04)	(0.04)	(0.05)	(0.01)
Accept Child-Inflex, Then Upgrade	-0.06	-0.02	0.00	-0.04	0.00
	(0.12)	(0.04)	(0.04)	(0.05)	(0.01)
Accept Timechild-Inflex, Then Upgrade	0.04	0.00	0.03	0.01	0.00
	(0.08)	(0.03)	(0.03)	(0.04)	(0.01)
Accept Office, Then Upgrade	0.02	0.01	0.03	-0.01	0.01
	(0.07)	(0.02)	(0.03)	(0.03)	(0.01)
Difficult Task	-0.52***	-0.26***	-0.11***	-0.15***	-0.07***
	(0.07)	(0.04)	(0.02)	(0.02)	(0.01)
Difficult Task × Accept Time-Inflex, Then Upgrade	-0.03	-0.01	0.00	-0.02	0.00
	(0.13)	(0.06)	(0.03)	(0.03)	(0.02)
Difficult Task × Accept Child-Inflex, Then Upgrade	-0.13	-0.07	-0.04	-0.03	-0.02
	(0.15)	(0.07)	(0.04)	(0.04)	(0.02)
Difficult Task × Accept Timechild-Inflex, Then Upgrade	-0.12	-0.06	-0.03	-0.03	-0.02
	(0.12)	(0.06)	(0.03)	(0.03)	(0.02)
Difficult Task × Accept Office, Then Upgrade	0.01	-0.01	0.01	0.01	0.00
	(0.10)	(0.05)	(0.03)	(0.03)	(0.01)
Observations	949,543	949,543	949,543	949,543	949,543
Flexible Mean	5.33	1.87	1.86	1.60	0.95
Week fixed effects	✓	✓	✓	✓	✓

Notes:

Table A.33: Quality of Tasks By Task Difficulty (Impact)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
Time-Inflexible to Flex					
arrangement = Time	0.02	0.01	0.01	0.00	0.00
	(0.12)	(0.04)	(0.04)	(0.05)	(0.01)
Difficult Task	-0.55***	-0.27***	-0.11***	-0.17***	-0.07***
	(0.10)	(0.05)	(0.03)	(0.03)	(0.01)
Difficult Task × arrangement = Time	0.10	0.04	0.01	0.05	0.01
	(0.15)	(0.08)	(0.04)	(0.04)	(0.02)
Child-Inflexible to Flex					
arrangement = Child	0.04	-0.22***	0.09**	0.17**	-0.04*
	(0.17)	(0.08)	(0.04)	(0.08)	(0.02)
Difficult Task	-0.65***	-0.32***	-0.15***	-0.18***	-0.08***
	(0.13)	(0.06)	(0.03)	(0.04)	(0.02)
Difficult Task × arrangement = Child	0.00	-0.07	0.11***	-0.04	-0.01
	(0.17)	(0.09)	(0.04)	(0.06)	(0.02)
Timechild-Inflexible to Flex					
arrangement = Timechild	-0.19	-0.36***	0.09***	0.09	-0.08***
	(0.15)	(0.09)	(0.02)	(0.07)	(0.02)
Difficult Task	-0.64***	-0.32***	-0.14***	-0.18***	-0.08***
	(0.10)	(0.05)	(0.03)	(0.03)	(0.01)
Difficult Task × arrangement = Timechild	-0.04	-0.10	0.12***	-0.06	-0.02
	(0.13)	(0.09)	(0.03)	(0.05)	(0.02)
Office to Flex					
arrangement = Loc	0.10	0.03	0.02	0.06	0.01
	(0.08)	(0.02)	(0.02)	(0.04)	(0.01)
Difficult Task	-0.51***	-0.27***	-0.10***	-0.14***	-0.07***
	(0.07)	(0.04)	(0.02)	(0.02)	(0.01)
Difficult Task × arrangement = Loc	0.33***	0.19***	0.08***	0.06*	0.05***
	(0.10)	(0.05)	(0.02)	(0.03)	(0.01)
Week fixed effects	✓	✓	✓	✓	✓

Notes:

A.1 Spillovers to the control group

There is suggestive evidence that control group participants who had more friends who worked during Round 1 were more likely to start work in Round 2.

Measuring spillovers. To measure exposure to treatment, we estimate the impact of having friends in the treatment group, conditional on total number of friends in the study. We use three methods to measure having friends in the treatment group: (i) number of friends, (ii) having at least one friend, and (iii) fraction of friends assigned to treatment. Because of low take up of work in inflexible jobs, to measure exposure to friends working rather than just receiving a job offer, in our main specification we include only friends assigned to the flexible jobs (Flex or Time), but we also show results of specifications including friends assigned to inflexible jobs in Table A.26.

To collect information on who the participant knew in the study, we ask at both baseline and endline for participants to list anyone else they knew who took part in our surveys. We then ask for their relationship to this person, how long they have known this person for, and how often they speak with each other. Because recruitment and baseline were done on a rolling basis, the initial lists collected at baseline are very incomplete, and we use the endline lists in our main specifications, eliminating anyone who the participant said they met within the last year (i.e. after the start of the study). In order to mitigate concerns that the endline lists could still include people who the participant met as a result of the study, we use only people who are either relatives or neighbors of the participants, with the idea being that it is very unlikely they met a relative or neighbor as a result of the study.

Estimation. We test for the presence of spillovers from the treatment group to the control group using the following regression:

$$\text{TakeUp}_{is} = \gamma_s + \gamma_1 \text{JobExposure}_{is} + \gamma_2 \text{NumFriends}_{is} + \gamma_3 \theta_{is} + \varepsilon_{is} \tag{A.4}$$

where TakeUp_{is} is a dummy variable equal to one if participant i started her job in Round 2; NumFriends_i is the total number of friends that i has in the study; JobExposure_i is one of the three measures of exposure to the treatment group described above; and θ_{is} is a vector of control variables selected using a double LASSO. The coefficient of interest is γ_1, which isolates the causal effect of control participants' exposure to the treatment group on their subsequent job take up. Holding fixed i's total number of friends in the study, i's exposure to the treatment group (e.g. number of friends randomly assigned to the treatment group) is as good as randomly assigned. *Spillover effects.* Overall, the estimates from these regressions suggest that participants with more friends assigned to the flexible jobs in Round 1 are more likely to take up flexible jobs in Round 2 than participants with less friend exposure. Table A.25 presents results of the spillover regressions, looking at take up of any Round 2 job (columns 1-3) and take up of flexible jobs (columns 4-6). Participants in the control group are 16 pp ($p = 0.018$) more likely to start work in the flexible jobs for each of their friends who was assigned to the flexible jobs, conditional on the number of friends they had in the study. The point estimates are also positive, but smaller and insignificant, for take up of any round 2 job. For the other measures of treatment exposure, the point estimates are similar but less precisely estimated. Results from comparing the effects of friend exposure to the flexible jobs versus inflexible jobs are consistent with the idea that control group participants are more likely to take up jobs if they heard about those jobs from their friends (Table A.26). There are no effects of having friend exposure to the inflexible jobs on take up of flexible jobs in Round 2.

Figure A.13: Effects on Work on attitudes, heterogeneity by Work Arrangement

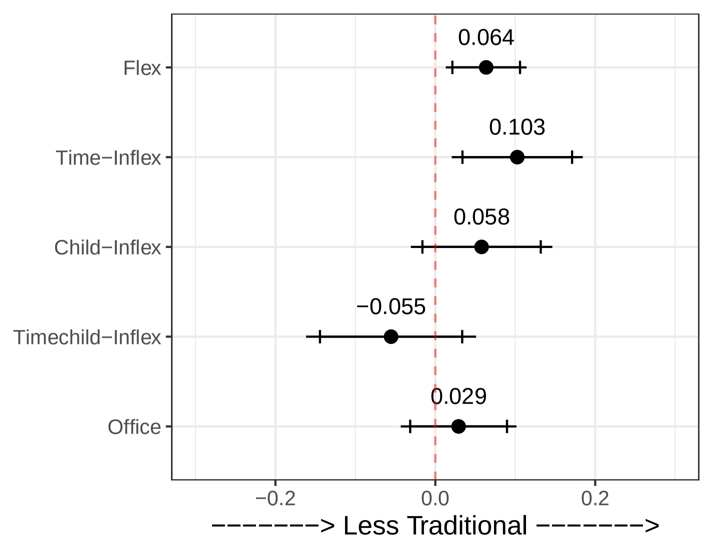


Table A.34: Assumptions for back-of-the-envelope firm tradeoffs

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

Figure A.14: Home-Office Tradeoff (Firm’s Perspective)