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# BRINGING WORK HOME: FLEXIBLE ARRANGEMENTS AS GATEWAY JOBS FOR WOMEN IN WEST BENGAL

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# Bringing Work Home: Flexible Arrangements as Gateway Jobs for Women in West Bengal \*

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(Job Market Paper)

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## Abstract

Several hundred million women want a job but are out of the labor force, often because available opportunities are incompatible with traditional norms about their household roles. In a field experiment with 1,670 households in West Bengal, we offer flexible, short-term data entry jobs which meet households where they are in terms of expectations on women's domestic responsibilities. We find three sets of results. First, 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 and at the hours they choose. Second, working from home reduces worker efficiency. Third, flexible jobs act as a gateway to outside-the-home jobs for women initially out of the labor force: experience with flexible jobs makes women more likely to accept inflexible work several months after the initial intervention. This gateway effect may be explained by changes in attitudes about appropriate behavior for men and women: flexibility makes a larger difference to the labor supply of women who hold more traditional pre-intervention attitudes, and work experience in turn shifts the gender attitudes of these women and their children to become less traditional. Thus, flexible work arrangements can both attract women to the labor force and provide a gateway to less flexible jobs.

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# 1 Introduction

Several hundred million women across the world want paid work but are out of the labor force.<sup>1</sup> This has implications for both equity and efficiency: independent income increases the agency and wellbeing of women and girls, and while the nearly universal gender gap in labor force participation could in theory be driven entirely by comparative advantage, recent studies find that men’s and women’s labor are misallocated (Hsieh et al., 2019; Ashraf et al., 2023). This misallocation, and consequently the potential gains, are particularly pronounced in South Asia, the Middle East, and North Africa. Despite advancements in women’s education, these regions exhibit low female employment rates compared to other areas with similar levels of economic development.

In countries with lagging female labor force participation rates, social norms are thought to be a key barrier to women’s paid work (Jayachandran, 2021). The mismatch between available jobs and jobs women could do without violating norms suggests two potential strategies: (1) change *norms* so that existing jobs incur a smaller cost, or (2) change *jobs* to be more compatible with existing norms. While past attempts to directly influence attitudes toward women and work have met with limited success (e.g. Dean and Jayachandran, 2019), we know little about how the second approach — modifying jobs to cater to current norms — would affect women’s employment and other outcomes related to empowerment. Beyond immediate employment consequences, aligning jobs to existing norms could have important effects on attitudes about gender roles, as well as whether women are willing and allowed to do outside-the-home work in the future.

Using a field experiment in West Bengal, we test the effects of offering jobs that are more compatible with existing norms of women’s behavior. Given the widespread expectation that women take sole responsibility for household chores, the experiment includes flexible jobs that allow women to work from home, multitask work with childcare, and/or choose their work hours. These jobs are increasingly realistic: paid work fitting this description has become more common, accelerated by the covid-19 pandemic. Online gig work opportunities are increasing in developing countries (Datta et al., 2023), and a silver lining of the pandemic is that many salaried jobs also now grant greater flexibility (Goldin, 2021). Early 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. Our study participants are offered an example of this emerging type of work: piece-rate data entry jobs on an online platform.

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<sup>1</sup>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.

Motivated by this growth in flexible work arrangements — combined with their potential to bring previously excluded women into the labor force — we design our experiment 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, to assess the viability of firms introducing flexible work arrangements, what are the effects of flexibility on job performance and the composition of workers who are drawn into the firm? Third, given that many jobs cannot easily be made remote, and that many (more secure) salaried jobs require in-person attendance, can flexible jobs act as a gateway to less flexible jobs for women in more traditional households who are initially only allowed to work from home?

We randomly assign women from 1,670 lower-middle-income households in a mix of urban, peri-urban, and rural areas to either receive a job offer or to a control condition with 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 working 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 [Project Karya](#) at Microsoft Research, which distributes smartphone-based data tasks to gig workers in India. The job involves tasks that contribute to Bangla or Hindi datasets that can train language models, and most are copies of tasks that previous clients requested and paid for. In order 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 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 women’s gender attitudes, as well as spillovers to their children. Two to three months later, we measure subsequent take-up of different work opportunities (“jobs round 2”), including some outside-the-home jobs.

To be eligible for our study, women could not be in the labor force or enrolled in skills training. This requirement allows us to study the extensive margin of labor force participation. 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. In general, participants have little previous work experience: 69% never worked for pay before 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. On average, participants are thirty years old, and thus are nearly all married (93%) and finished with their education. 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. Additionally, 48% have childcare responsibilities for a young child (under eight), and a quarter have a child aged eight to eighteen.

We find three main sets of results. First, flexible work arrangements greatly increase 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 — more than triples job take up from 15% to 48% ( $p < 0.001$ ). To contextualize this 33 pp effect size, even highly effective previous interventions designed to increase women’s job take up find smaller effects. For example, a promotional video shown to women’s family members in rural Uttar Pradesh increased job take up 78% (7 pp) (McKelway, 2023), and correcting Saudi men’s second-order beliefs about women working outside the home increased job sign ups by 36% (9 pp) (Bursztyn et al., 2020).

By randomly varying job characteristics, we estimate the contribution of each of the three flexibility dimensions examined in this study — a choice of work hours, the ability to combine work with childcare, and the freedom to work from home — to the overall effect of flexibility on job take up. Work from home, even without multitasking work with childcare, doubles job take up from 14% to 28% ( $p = 0.004$ ). The ability to multitask work with childcare is also important, increasing job take up by 59% ( $p < 0.001$ ). The ability to choose work hours does not make a significant difference to job take up. Therefore, decomposing the 33 pp effect of flexibility, approximately one half (16 pp) comes from the ability to work from home, and the other half (17 pp) stems from the ability to multitask work with childcare.

We see that flexible work arrangements seem to work well for women. Our second set of results asks whether the flexible work arrangements also work well for firms. Beyond fixed costs and feasibility concerns, employers considering the adoption of flexible work arrangements would likely want to understand how these arrangements would affect the selection of workers drawn into the firm as well as the job performance of their existing employees. 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 job acceptance and onboarding, (ii) quality-adjusted output—the total amount the worker produces during the job, adjusted by Karya’s accuracy and volume metrics, and (iii) efficiency—the speed with which workers complete their assigned tasks.

Overall, we find that flexible work arrangements turn out well for employers that pay piece rate wages, with more ambiguous implications for salaried work. Flexibility — particularly the ability to work from home — improves reliability. The ability to work from home decreases the fraction of workers who accept the job but never start work by 25 pp ( $p = 0.004$ ). In addition, flexibility does not affect the average quality-adjusted output produced by each worker. Since pay is based on quality-adjusted output, the firm gets the same amount of output per worker at the same wage cost under any work arrangement. In fact, including all workers who accepted the job, working

from home actually increases quality-adjusted output per worker by 23% ( $p = 0.038$ ). However, one drawback of work-from-home is decreased efficiency: home-based workers spend more time completing the same amount of work. In this context of piece rate wages, the efficiency cost is borne by the worker, but could pose concerns for a firm that pays according to days worked.

Flexible work arrangements include similar advantages and disadvantages for firms in terms of the effects on worker composition. Women who accept work only when it is flexible are equally reliable and produce the same amount of quality-adjusted output, but they work 7.7% more slowly ( $p = 0.092$ ). Again, this is neutral for an employer that pays piece-rate wages, but may be negative for an employer offering salaried pay. Examining work patterns to understand the mechanisms underlying these differences in efficiency, we find that women who can only accept at-home jobs have shorter periods of uninterrupted work. These fragmented work patterns result in lower efficiency because of “flow effects”: we find that workers speed up and complete tasks more accurately when they work for a longer stretch of time without pauses. This suggests that women whose labor supply is marginal to flexibility (i.e. will only accept paid work when it is flexible) may not have lower “ability,” but rather are more likely to be on call at home to switch between home production tasks and paid work more frequently. Flow effects can also explain the negative effect of working from home on efficiency, as working from home causes women to work in shorter spurts, suggesting that women’s attention is called upon for other activities more often when they are at home.

Third, given that the transition from unpaid home production as a full-time homemaker to working outside the home is a large leap — both for a woman herself and for her family members — could flexible work arrangements act as a stepping stone to outside-the-home jobs? These more flexible work arrangements come closer to meeting households where they are in terms of their expectations of women’s household roles, raising the possibility of creating a stairway of jobs that allows women to take multiple smaller steps into the broader labor market. To test whether flexible work can act as a “gateway job,” we offer all study participants another job two to three months after the endline survey. These jobs vary in their flexibility, as well as in the type of work (the same online gig work, or non-digital work that involves sewing masks and making jewellery). Consistent with flexible work acting as a gateway job, women who are assigned to a flexible job in the initial intervention are 6 pp more likely to take up a less flexible job assignment during the second round of work than women who did not get a similar gateway experience ( $p = 0.031$ ). The efficacy of flexible work arrangements as an intermediate step to outside-the-home work is important for several reasons. First, although flexible jobs are effective at bringing women into the labor force without tradeoffs for (at least some) firms, remote work will not be feasible for all jobs. Second, a greater presence of women in outside-the-home jobs is likely to be important for achieving an equal role for women in the public sphere.

One potential mechanism consistent with this gateway effect is a change in attitudes about ap-

appropriate behavior for women. Flexibility makes the biggest difference to job take up for women with more traditional gender attitudes, and in turn, the flexible work arrangements shift the views of these more traditional women to become less traditional. 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 ( $p = 0.038$ ), as measured 1-2 weeks after the end of the intervention. This effect is large considering the short-term nature of the jobs, low take up of treatment, and the lack of direct focus on gender attitudes. The effects on gender attitudes are concentrated on participants who had held more traditional pre-intervention attitudes. Women more traditional than the median become less traditional by 0.11 SDs on average ( $p = 0.001$ ), while there is no significant impact on attitudes for participants who were already less traditional than the median.<sup>2</sup>

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.<sup>3</sup> These changes could help to explain why women who first have some experience with flexible jobs are more willing to take up inflexible jobs than those without the “gateway” treatment. When the intervention ends, our survey team asks children about their attitudes and their family members’ behaviors during the last month. Children with mothers in the treatment group are 16 pp more likely to say that their mother had a job ( $p < 0.001$ ) and no more or less likely to say that their father had a job. Treatment group children also report that other household members 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 ( $p = 0.045$ ), as compared to the 42% of children in the control group who say their fathers *never* help with these home production activities. The gender attitudes of older children also shift to become 0.1 SDs less traditional ( $p = 0.034$ ).

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. 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. A natural question

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<sup>2</sup>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.14).

<sup>3</sup>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%).



then is to what extent women's preferences for flexibility explain low female labor force participation in these countries. In this study, we focus on women who are *not* labor market participants and show that women's greater preference for flexible work arrangements is not important just for explaining gender wage gaps, but also for explaining low female labor force participation. In addition, although women's greater preference for flexible work arrangements is well-documented in the literature, these preferences are taken as exogenous. Our experiment shows that these preferences are malleable and endogenous to women's own labor supply: work experience and a shift in gender attitudes can lessen the magnitude of these preferences for flexibility.

Second, 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). However, few experimental interventions have effectively 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. Our results are promising in that working appears to kickstart a virtuous cycle for female employment, in which women working and less traditional gender roles mutually reinforce each other.

Third, we contribute to a growing literature about the effects of flexible work arrangements on job performance (Bloom et al., 2015; Choudhury et al., 2021; Bloom et al., 2022; Choudhury et al., 2022; Atkin et al., 2023; Aksoy et al., 2023; Emanuel and Harrington, 2023). Many of these papers examine trends in and impacts of post-pandemic increases in remote and hybrid work. Our project differs from previous work in our focus on the extensive margin of labor force participation, as well as our focus on developing countries. While other studies estimate which incumbent workers choose flexible work arrangements, we aim to shed light on how flexible job arrangements would change the gender composition of the labor force in a developing country context.

Lastly, we contribute to an active literature on strategies to increase female labor force participation in developing countries (Bernhardt et al., 2018; Khanna and Pandey, 2021; Field et al., 2021; McKelway, 2021; Lowe and McKelway, 2021; Agte and Bernhardt, 2023). 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.



The rest of the paper proceeds as follows: Section 2 describes the study population and experimental design. Section 3 presents results on the effects of flexible work arrangements on take up of work. Section 4 presents the impacts of flexible work arrangements on job performance and worker composition. Section 5 presents the results on effects of work experience on take up of future jobs, and Section 6 shows how increased willingness to do less flexible jobs could be explained by changes in household attitudes and division of home production. Section 7 concludes.

## 2 Experimental Design

We designed our experiment with three broad goals in mind. First, we want to estimate the importance of flexible work arrangements to female labor force participation, and to isolate the relative importance of different dimensions of flexibility. Second, we aim to understand what types of women are marginal to job flexibility, and to understand what the effects of flexibility would be on the job performance of inframarginal women. Finally, we aim to measure the effects of paid work experience on women and their children, including whether paid work makes women any more likely to participate in market labor in the future.

Households are recruited for the study from eight areas in and near Kolkata, West Bengal with the support of our partner NGO, Calcutta Foundation.<sup>4</sup> 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 our study if there is a woman who consents to participate who 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.<sup>5</sup>

### 2.1 Description of the Intervention Jobs

We partner with Project Karya at Microsoft Research (later Karya Inc) to implement the smartphone-based gig jobs. The jobs involve piece-rate paid microtasks to contribute to Hindi or Bangla speech datasets, which require participants to speak into their phones and record their voices speaking in Hindi or Bangla. Karya builds these and similar speech databases to better train speech recognition algorithms. One advantage of this task is that we can verify from the voice if a woman seems to be completing the tasks, even if the participant works remotely.

**Tasks.** 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

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<sup>4</sup>Calcutta Foundation was founded in 1994 and has worked on projects related to education, disaster relief, health, and more recently gender.

<sup>5</sup>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 housewives in the absence of the experiment.

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 desire.

There are four types of paid microtasks, which participants can choose to do in either Hindi or Bengali. The simplest tasks involve reading aloud a sentence which appears on the screen (“simple”). These sentences are selected from a database of common phone or computer commands (e.g. “set an alarm for 7am tomorrow morning”). The second type of task involves reading a sentence backwards, which we introduce to require more concentration (“backwards”). 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 (“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 (“open-ended”). Each week, the 1000 tasks are ordered in the following way: 200 simple, 100 paragraph, 150 simple, 100 backwards, 10 open-ended, 150 simple, 40 paragraph, 50 backwards, and 200 simple.

**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 score each task on a scale of 0-2 in terms of fluency, accuracy, and volume. This means that participants could earn up to Rs 4000 (approximately \$50 USD) over the course of the intervention jobs, which is equivalent to 36% of the average household monthly income in the study.

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

<b>Work Arrangement</b>	<b>Time Flexibility</b>	<b>Multitasking with Childcare</b>	<b>Work From Home</b>
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. Working for three hours each day would be more than enough to complete the weekly assigned tasks.

*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.

*Working From the Office.* In the location-flexible jobs, participants work from home (or another location of their choice), 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, and we do not allow participants to bring their children.<sup>6</sup> 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 Saturday-Thursday in the Muslim-majority area).

## 2.2 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 takes 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.

The baseline survey also includes an incentivized job preferences elicitation. Surveyors explain that an employer in the area is looking for female workers for a month-long, part-time job, and that the jobs involve reading Hindi or Bangla sentences to help engineers make computers understand these languages. Participants are told (truthfully) that there are already many recordings of men’s voices, but not enough female voices, which is why the employer is looking for female workers.

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<sup>6</sup>In Jalota and Ho (2023), 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.

Surveyors then ask participants whether or not they would accept each of the five different work arrangements for the job. Participants are told to choose carefully because their decision will have an effect on whether they will be offered a job. The work arrangements are presented one at a time, and the order of the jobs is randomly varied between participants to avoid order effects. Surveyors explain that accepting or turning down one work arrangement will not affect their chance of getting assigned to a different work arrangement, and that job assignment is decided by lottery. To check comprehension, participants are asked three (of seven possible) questions to ensure they understand what the jobs involve, the different requirements of each work arrangement, and that their choice for one work arrangement would not affect their probability of receiving a different work arrangement.

**Jobs Round 1: Two-Stage Randomization (Initial Job Offers and Surprise Upgrades).** After the baseline survey, we randomly assign households to the control group or one of the five job groups for their initial job offers. Randomization is stratified by three characteristics: area, smartphone ownership, and whether the participant has a child under age eight.<sup>7</sup> If participants said during the job preferences elicitation that they would accept the job they were assigned to if offered it, then we offer them the job. This follows the “strategy method”; in effect, we randomly select one of the questions from the job preferences elicitation and implement the participant’s choice. Participants can accept or reject the job offer, or else ask us to call back in a day or two if they need more time.

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 (e.g. quality-adjusted output) 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 job that participants are doing, while comparing participants who are “flexibility compliers” (i.e. women who will only work when the job is flexible) to those who are not. 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 the type of worker – an always-taker on this dimension of flexibility – and assesses

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<sup>7</sup>One region (New Alipore) was not stratified further for randomization, because of the small number of participating households (66).

how their job performance changes if given greater work flexibility. After the upgrades, the jobs are implemented for one month as described in Section 2.1.

**Endline Surveys and Jobs Round 2.** After the randomized jobs intervention, participants complete an endline survey (within two weeks of job completion for treated participants). The endline survey takes approximately one hour to complete and covers gender attitudes, agency, and psychological wellbeing. We also survey children when possible, covering their aspirations, participation in household activities, and gender attitudes. All women completing the endline survey also take part in an incentivized job preferences elicitation. This job preferences elicitation again uses the “strategy method” to make participants’ choices have real stakes that will be implemented as Jobs Round 2. The job choices are identical to those in the initial jobs intervention (Jobs Round 1), except for two major differences:

1. *Non-Digital Jobs.* We introduce two non-digital jobs, bringing the total number of jobs that we ask each participant to make a choice about to seven. One of the non-digital jobs could be done from home, while multitasking with childcare, and at any hours the participant wishes. The other non-digital job required participants to work from an office. The purpose of these non-digital jobs is to assess whether any treatment effects on interest in work are restricted to digital work, or whether the effects hold more broadly. The non-digital jobs involve constructing newspaper bags, making earrings, and sewing masks. As in the digital jobs, payment is piece rate and dependent on quality.
2. *Costly Choices.* At endline, participants do not just state whether they would accept or reject each job; instead, they choose between each job and a gift (a pressure cooker), which introduces a cost of saying yes to the job. A pressure cooker (worth approximately Rs 750, or \$9) was selected as the gift because they are very common, useful household items that participants would already have at home. An additional pressure cooker is unlikely to change how a household operates, and so it would act more as a small transfer, perhaps postponing when the participating household next buys a pressure cooker.

We randomly select one of the seven job versus gift questions for each participant and implement their choice on that question. This means each study participant receives either a job offer or a pressure cooker after the endline survey. We measure the take-up rates in Jobs Round 2 to shed light on treatment effects of work experience on future interest in work.

### **2.3 Sample Description and Intervention Fidelity**

This subsection will summarise participant characteristics, balance, and attrition across the experimental groups. Table A.8 presents the mean and standard deviations of important characteristics in

the control and treatment groups after randomization, and Table 1 presents the same for participants who completed the endline survey.

**Summary of Participant Characteristics.** The average participant is 30 years old and married. Half have completed 10th standard, and 13% have at least started an undergraduate degree. Three quarters of the participants are Hindu, with the remaining coming from Muslim households. 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. Average household income is Rs. 11,791 per month (approximately \$142), meaning that these women mostly belong to lower-middle-income households in which women do not need to work for subsistence. The majority (69%) of participants have never worked for pay. If participants were to get a job offer, 36% report that they would have the final say in whether or not to take the job.

**Balance and Endline Completion.** As seen in Table A.8, randomization produced a control and treatment group which were balanced across most major characteristics of interest, although the treatment group is more likely to be Hindu (significant at the 10% level).

After the job treatments, we completed our endline survey with 1525 households, a completion rate of 91%. As seen in Table 1, attrition is balanced between the treatment and control groups, and the groups are mostly balanced on important covariates. The control group is less likely to be Hindu ( $p = 0.064$ ) and more likely to have parents-in-law in the household ( $p = 0.098$ ). To control for any imbalances, we include covariates selected by double post LASSO in regressions estimating the effects of treatment (Belloni et al., 2014).

### 3 Effects of Flexibility on Job Take Up

#### 3.1 Measuring Job Take Up

We measure job take-up in three ways: (i) from our baseline survey, (ii) from the job offer stage, and (iii) whether participants actually start work. Each measure of job take up illuminates different parts of the decision making process about labor supply.

First, participants' baseline survey responses act as well-powered indicator of job interest. On the baseline survey, each participant is asked – for each of the five work arrangements – whether or not she would take the job if offered it. Participants respond to these questions on the spot. Because participants are unable to discuss the decision with family members or to spend more time reflecting on the benefits and costs of the job, we interpret this baseline survey measure as an

indicator of tentative interest in the job. One benefit of asking every participant about every job is that the baseline survey measure is better powered to detect differences in take up between work arrangements than at later stages when each participant is offered exactly one job (or not offered a job at all).

Second, a participant’s response when the intervention team calls her with a job offer is the measure that most closely simulates her job acceptance behavior if a firm tries to hire her. After randomization, the jobs team calls the participant with a job offer that she can respond to immediately or take a couple of days to decide on, allowing her to discuss the decision with family members if she wishes. If she decides to accept the job, she is expected to begin the job as soon as possible and no later than one week from the time of her job offer. In order to make a participant’s baseline survey responses meaningful, she is only contacted at the job offer stage if she indicated on the baseline survey that she would take up that job if offered it.

Third, the job take-up measure of starting work is the most real-stakes measure of labor supply: we say that a participant started work if she submitted job tasks to the employer for review. This is the measure we use when estimating the effects of employment, as well as the measure we use when presenting results from just one measure of job take up.

### 3.2 Estimation

Because the job offers are randomly assigned, estimating the effects of different work arrangements on job take up is straightforward. We estimate an equation of the following form using ordinary least squares (OLS):

$$\begin{aligned} \text{Takeup}_{ij} = & \alpha_0 + \alpha_1 \text{TimeInflexible}_j + \alpha_2 \text{ChildInflexible}_j \\ & + \alpha_3 \text{TimeChildInflexible}_j + \alpha_4 \text{Office}_{ij} + \varepsilon_{ij} \end{aligned} \quad (1)$$

where  $i$  represents a participant and  $j$  represents a job.  $\text{Takeup}_{ij}$  represents take up of job  $j$  for participant  $i$ , and is measured at three stages as described in Section 3.1. The regressors are dummy variables for each job type  $j$ , with the most flexible job excluded. When using the baseline survey measure for Takeup, we include household fixed effects and cluster standard errors at the household level because each participant is asked about each of the five jobs.

### 3.3 Results: Effects of Flexible Arrangements on Job Take Up

Flexibility dramatically increases women’s labor supply, from 15% for an office-based job to 48% for the maximally flexible, work-from-home job. Despite the relatively comfortable conditions of the study’s office-based job, only 14.6% of participants who were offered the office-based job showed up to work. The study’s office-based job offered relatively high pay, flexible hours, low



commuting costs, work that was not physically tasking or dangerous, and less exposure to non-family males compared to other jobs in the area. The piece-rate wage of Rs 1 per task meant that participants could earn up to Rs 4000 throughout the month, which was equivalent to 36% of average household income, while working only around 12 hours per week. The offices were open from 10am-6pm on Mondays through Saturdays, which gave participants the ability to come into work during the middle of the day, which we learned from our pilot study was the most convenient time to come to the office, as that was when their husbands were most likely to be at work.<sup>8</sup> We set up 1-2 offices in each area, so in urban areas participants could walk to the office within 15 minutes, and in peri-urban and rural areas participants could take short rides on an auto-rickshaw which the employer would reimburse. In addition, because the work did not involve strenuous manual labor or tasks associated with a particular religious or caste group, the job would not result in physical injuries or be seen as belonging to an identity group incongruent with the participant's family (Oh, 2023). Lastly, the participant's coworkers and managers would be entirely female, shutting down concerns about harassment or other potential negative consequences of a workplace with men.

The maximally flexible, work-from-home job increased take up to 47.9%, a 33 pp (or 228%) increase from the take up rate for the office-based job. This job ("Flex") allows participants to work from home, multitask work with childcare, and choose their work hours flexibly. This is a very large effect compared to other interventions that attempt to increase women's labor supply. In comparison, even very effective previous interventions, such as correcting Saudi men's perceptions of other men's beliefs about the appropriateness of women being employed outside the home, increased sign up for a job matching service by 36% (9 pp) (Bursztyn et al., 2020), and a promotional informational video shown to women's family members in rural Uttar Pradesh increased take up of an employment program by 78% (7 pp) (McKelway, 2023).

To understand why the most flexible job increased take up by such a large amount, we separately estimate the effects of time-flexibility, childcare-flexibility, and work-from-home on job take up by comparing take up rates between the jobs which we offer in the study. Starting with the most flexible job, each job we offer toggles off one job attribute at a time, holding fixed all other characteristics.

Figure 2 and Table 2 present the take up rates for each of the five jobs, with the following main takeaways about the effects of flexible work arrangements on job take up:

- *Time flexibility.* The ability to choose one's hours flexibly does not make a significant difference to women's take up of the jobs. At the baseline survey stage, the ability to choose one's hours flexibly increases take up by a small amount (4-5 pp,  $p < 0.001$ ). At the job offer and

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<sup>8</sup>In Tiljala, the office was open on Saturdays and closed on Fridays, as this was more convenient for the study participants in the area, 94% of whom were Muslim.

starting work stages, the effect of time flexibility is even smaller and no longer significant.

- *Childcare flexibility.* The ability to multitask work with childcare increases job take up substantially. The fraction of women who start work increases by 59% (17 pp,  $p < 0.001$ ) when women can multitask work with childcare. The effect of childcare flexibility on job take up is also large and significant at the baseline survey (42 pp,  $p < 0.001$ ) and job offer (21 pp,  $p < 0.001$ ) stages.
- *Work-from-home.* The ability to work from home – even at fixed hours and without multitasking with childcare – also has a substantial effect on job take up, increasing the fraction of women who start work by 100% (14pp,  $p = 0.004$ ). This difference is also large and significant at the baseline survey (10 pp,  $p < 0.001$ ) and job offer (10 pp,  $p = 0.020$ ) stages.

In sum, decomposing the large difference in fraction of women who start work between the office-based job and most flexible job, 6% can be attributed to time flexibility, 52% to childcare flexibility, and 42% to the ability to work from home (independent of childcare concerns).

### 3.4 Whose labor supply is most likely to be marginal to flexibility?

Participant characteristics mediate both the level of job take up as well as the importance of flexibility for job take up. In this subsection we explore how participant characteristics predict (i) the take up of inflexible jobs and (ii) the effect of flexibility on job take up. We start by discussing the differential effects of flexibility on job take up, and find that flexibility makes a bigger difference to the job take up of women with more traditional attitudes about household gender roles.

#### 3.4.1 Differential importance of flexibility: empirical strategy and results

To estimate the heterogeneous effects of flexibility on job take up, we use the following regression, interacting job flexibility with participant characteristics:

$$\text{Takeup}_{ij} = \beta_0 + \beta_1 \text{Flexible}_j + \beta_2 \text{Characteristic}_i + \beta_3 \text{Flexible}_j \times \text{Characteristic}_i + \varepsilon_{ij} \quad (2)$$

where  $\text{Flexible}_j$  is a dummy variable equal to one if  $j$  is the flexible or time-inflexible job.<sup>9</sup> The omitted group consists of participants who do not satisfy  $\text{Characteristic}_i$  and who are not assigned to a flexible job offer. The coefficient of interest in  $\beta_3$ , which represents the differential importance of job take up if a participant satisfies the given characteristic.

Figure 3 plots the  $\beta_3$  coefficients for six binary characteristics hypothesized to be important to women’s labor supply: (i) having gender-related attitudes that are more traditional than the median,

<sup>9</sup>We pool the flexible and time-inflexible jobs because they have indistinguishable take-up rates. For a replication of this analysis where  $\text{Flexible}$  stands for the most flexible job only, see Figure A.12. The results are very similar.

(ii) not having the final say in one's own labor supply, (iii) having a young child, (iv) completing education to at least 10th standard, (v) having higher than median income, and (vi) cohabitating with at least one parent-in-law. The characteristics are defined so that each one is associated with a larger importance of flexibility for job take-up.

The characteristic with the largest  $\beta_3$  point estimate is more traditional gender attitudes ( $\beta_3 = 0.17, p = 0.001$ ), and this is the only characteristic which has a  $\beta_3$  coefficient that is significant at the 10% confidence level. Below, we discuss in more detail the heterogeneous importance of flexibility for each of these characteristics.

### 3.4.2 Baseline gender attitudes.

*Overview.* Table 3 and Figure 4 compare job take up rates between more traditional and less traditional participants across work arrangements grouped by their level of flexibility. Participants are classified by whether their gender attitudes index score is more traditional or less traditional than the median participant. Jobs with similar take up rates are pooled, creating three levels of flexibility: (i) Flex and Time, (ii) Child and Timechild, and (iii) Office.

*Measuring gender attitudes.* Gender-related attitudes are measured on the baseline survey in a module with sixteen questions. Two of the questions ask participants to make hypothetical choices between two possible daughters-in-law (one who works or one who doesn't work) and between two possible sons-in-law (one who allows his wife to work and one who does not). Two questions directly ask about the participant's beliefs about whether or not women should have jobs (one about jobs that they would do from home, and one about jobs that they would do outside the home). The remaining questions ask participants to respond on a likert scale expressing their agreement or disagreement with statements in three domains: (1) appropriate household roles for men and women, (2) women's participation in the labor market, and (3) gender and technology use.

*Comparing take up rates by jobs and attitudes.* Women with less traditional baseline gender attitudes have higher job take up in every work arrangement, and especially in the less flexible arrangements. As shown in Figure 4, the difference in job take up by baseline gender attitudes is smallest (5.5 pp,  $p = 0.109$ ) for the most flexible jobs, Flex and Time. The difference is much larger for the childcare-inflexible jobs (27.8 pp,  $p < 0.001$ ) and office job (18 pp,  $p < 0.001$ ). The less traditional participants were 360% more likely to start the office job than their more traditional counterparts, only 5% of whom began the office job.

As can be seen most clearly in Table 3, flexibility still makes a large (+34 pp,  $p < 0.001$ ) difference to job take up for less traditional participants, but flexibility makes an even larger difference for more traditional participants (+51 pp). The results of the regressions presented in this table control for age, marital status, work experience, cohabitation with parents-in-law, religion, number

of household members, whether the participant has a child under age eight, smartphone ownership, and area.

*Heterogeneity by attitudes with and without having a young child.* To investigate whether the correlations between gender attitudes and job take up are due to a difference in likelihood of having young children, Table 3 shows the results of the same regressions interacting flexibility and attitudes for two separate subsamples: participants with and without a child under age eight. Even within both of these subsamples, participants with less traditional gender attitudes have higher job take-up. Being less traditional is associated with +19 pp (84%) higher take up of inflexible jobs for participants without young children ( $p < 0.001$ ), and associated with +26 pp (520%) higher take up for participants with young children ( $p < 0.001$ ). In addition, less traditional participants without children under age eight were 24 pp (67%) more likely to start the more flexible jobs than their counterparts who were more traditional ( $p = 0.002$ ).

### **3.4.3 Final Say in Labor Supply.**

Women who have the final say in their own labor supply are more likely to start both the flexible and inflexible jobs, and flexibility is less likely to determine their interest in working (see Table A.10). During the baseline survey, a minority (36%) of women report that they would have the final say in whether or not to take a job. When offered a flexible job, women who have the final say are 7 pp (16%) more likely to start work ( $p = 0.051$ ). When offered an inflexible job, women who have the final say are 15 pp (94%) more likely to start work ( $p = 0.001$ ). These results are consistent with other work which finds women are more supportive of themselves working than their family members are (Bernhardt et al., 2018; Field et al., 2021; Bursztyn et al., 2023). In addition, our findings suggest that some of the importance of flexibility to female labor supply is due to other family members' opinions. The gap in take up between those with and without a final say in their own labor supply is smaller for the more flexible jobs, although this differential importance of flexibility is only significant ( $p < 0.001$ ) at the baseline survey stage.

### **3.4.4 Young Children.**

Participants with a young child (under age eight) are less likely to start the inflexible jobs, but are equally likely to start the flexible jobs (see Table A.11). Women with young children are 9 pp (35%) less likely to start inflexible jobs ( $p = 0.021$ ), which do not allow them to multitask work with childcare, than their counterparts without young children. Women with young children are no less likely (1 pp,  $p = 0.770$ ) to start the flexible jobs, however. This differential importance of flexibility is positive at all stages of take up, but only significant ( $p < 0.001$ ) at the baseline stage.

### **3.4.5 Education**

More educated participants – those who have completed at least 10th standard – are more likely to start both the inflexible and flexible jobs than their less educated counterparts, and flexibility is not differentially important by education (see Table A.12). More educated women are 12 pp ( $p = 0.004$ ) more likely to begin inflexible jobs and 19 pp ( $p < 0.001$ ) more likely to begin flexible jobs.

### **3.4.6 Household Income**

Participants in higher income households are a bit more likely to start flexible and inflexible jobs than their lower income counterparts, although the difference is only significant for the flexible jobs (see Table A.13). Women in higher income households are 7 pp more likely to start the flexible jobs ( $p = 0.067$ ) and 3 pp more likely to start the inflexible jobs (not significant). The difference in take up rates between the flexible and inflexible jobs, for women who are higher versus lower income, is small (4 pp) and not significant.

### **3.4.7 Cohabitation with In-Laws**

Participants who live with their in-laws are no more or less likely to start work, whether the job is flexible or inflexible (see Table A.14).

## **4 Effects of Flexibility on Worker Composition and Performance**

In addition to the effect of flexible work arrangements on women starting work, we may want to understand the effects on whether women actually persist through the work, and how well they do the job. This is important because if women who start work due to flexible work arrangements quickly drop out, then there are muted benefits from introducing flexible work arrangements. In addition, a firm that is considering introducing flexible work arrangements will want to understand the costs and benefits associated with transitioning to remote work, including whether they can expect the same levels of job performance from their workers. In our experiment, we assess the effects of flexible work arrangements on worker composition and job performance. Using the randomly assigned surprise upgrade to the most flexible job, we estimate what types of women will only accept jobs when they are flexible (“flexibility compliers”), as well as the effects of flexibility on job performance for inframarginal workers.

### **4.1 Measuring job performance**

We use three main measures of job performance: (i) reliability, (ii) output, and (iii) efficiency.

Worker **reliability** is measured by whether or not the worker shows up to the job after accepting it. In most specifications, we measure reliability as a dummy variable equal to 1 if the participant accepted the job and later started work (i.e. submitted tasks for the employer to review) and equal to 0 if the participant accepted the job but never started work. We call these participants who accept the job but never start work “no shows.” No shows could be costly to the employer, because recruitment and onboarding are costly in terms of time and wages paid to the firm’s hiring officers. If the participant turned down the job, she does not enter into this measure of job performance.

Our main measure of output is **quality-adjusted output**, defined by the number of tasks completed by the worker, adjusted for quality metrics defined by our partners at Karya. Because workers are paid according to quality-adjusted output, this measure is also equal to earnings. If a worker submitted a task to Karya that achieved a full score in both accuracy and volume, she was paid 1 rupee for the task. If she did not complete the task satisfactorily, then pay was lower according to the following formula: if the task submission was not accurate, then she received 0.5 rupees, and if the task was accurate but the volume of her voice recording was not sufficiently loud, then she received 0.75 rupees. The accuracy and volume scores were determined by a separate team of validators who listened to each task. We calculate the effects of flexibility on quality-adjusted output per worker in two ways (i) only including participants who started work, and (ii) including all workers who accepted the job, whether or not they were reliable (i.e. started work).

**Efficiency** is measured as quality-adjusted output produced by a worker per minute. We measure minutes worked using the timestamps provided to us by Karya. During any given minute in the intervention month, we counted that minute towards time worked if the participant took any action during that minute (e.g. starting a task, listening to one of their tasks to check it for accuracy, or submitting a task). For this measure of job performance, we only include participants who started work.

## 4.2 Selection into flexible work arrangements

*Estimation.* 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 a less flexible 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:

$$y_i = \beta_0 + \beta_1 \mathbb{1}\{\text{InitialOffer}_i = \text{LessFlexible}_j\} + \varepsilon_i \quad (3)$$

where  $y_i$  is a measure of job performance, and the comparison group is participants who are initially offered and accept the most flexible job. The coefficient of interest,  $\beta_1$ , is the difference in job performance between participants who were randomly selected for an upgrade to the most flexible job, with those who were initially randomly assigned to the most flexible job. Comparing these two groups allows us to assess whether participants who are willing to work in a less flexible job perform systematically differently from flexibility compliers.

*No selection in who starts work.* Flexibility compliers are no less likely to start work after accepting the job (Figure A.15). 67% of participants who accept the most flexible job begin work, which is similar and statistically indistinguishable from the rates of starting work for participants who accept a less flexible job and then are upgraded (initially accepted time-inflexible: 66%; child-inflexible: 70%; time- & child-inflexible: 66%; office: 70%). These similar rates of starting work conditional on accepting the job across different treatment groups means that we can interpret any differences between these groups in terms of their task completion as being driven by worker composition rather than the probability of starting work.

*Quality-adjusted output and hours worked.* There is suggestive evidence that flexibility compliers are less efficient (in terms of quality adjusted output per minute) than workers who would accept less flexible work arrangements. Workers who initially accepted the most flexible job produce 0.17 Rs less output per minute than workers who were upgraded from a less flexible job ( $p = 0.106$ ).

### 4.3 Effects of flexible work arrangements

*Estimation.* To estimate the effects of work arrangements, we compare the job performance between two groups who were initially assigned to a less flexible job offer: (1) those who accept 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 use the following regression to estimate the effects of the upgrade to the most flexible job:

$$y_{is} = \beta_s + \beta_1 \mathbb{1}\{\text{Upgrade}_i = 1\} + \theta_{is} + \varepsilon_{is} \quad (4)$$

where  $y_i$  is a measure of job performance, and the comparison group is participants who initially accept a less flexible job and are not randomly selected for an upgrade. We estimate this regression separately for each of the less flexible jobs: Time-Inflexible, Child-Inflexible, Time-&-Child-Inflexible, and Location-Inflexible. The coefficient of interest,  $\beta_1$ , is the difference in performance between participants who were upgraded and participants who were not — in other words,  $\beta_1$  is the impact on job performance of relaxing the relevant dimension of flexibility.



*Starting work.* Working from home greatly increases the fraction of workers who start the job after accepting it, while time flexibility and the ability to multitask work with childcare do not make a significant difference to starting work (see Figure A.15). Workers who are upgraded from the office to work-from-home are 25 pp more likely to start work ( $p = 0.035$ ). On the other hand, workers who are upgraded from time-inflexible to most flexible are no more likely to start work. Participants who are upgraded from child-inflexibility and time- and child-inflexibility appear to start work more often overall (by 6 pp and 11 pp, respectively), but neither of these effects are significant, even when the child- and timechild-inflexibility are pooled together. Taken together, this means that the upgrades provide us with a clean measure of the impact of time flexibility on job performance, but that we may need to compute bounds in order to separate worker selection from the impacts of flexibility for groups involving childcare inflexibility and office-based work. In these bounding exercises, we will assume the difference in starting work is driven by the worst performing workers (following Lee, 2009).

*Quality-adjusted output and hours worked.* Working from home decreases worker speed, but does not affect total quality-adjusted output. Working from home decreases the average quality-adjusted output produced per minute by 19% ( $p = 0.028$ ) while keeping total quality adjusted output produced over the month-long job the same. However, as discussed above, given the different rates of starting work for workers who were upgraded from the office to work from home, this difference in hours worked could be driven by either worker composition or treatment effects of work. In order to isolate treatment effects alone, we compute Lee bounds and find that, even assuming that the would-be no-shows in the upgraded group are the least productive workers, working from home results in the same quality adjusted output per minute. This makes it likely that work-from-home has a negative treatment effect on efficiency.

Flexibility in time and childcare, on the other hand, do not have a significant effect on total output or productivity: there is no effect of time flexibility and childcare flexibility on quality adjusted output or time spent on work.

#### **4.4 Mechanisms: Interruptions and Flow Effects**

The results on job performance show that working from home increases the time required to do the same quantity of work, and suggests that flexibility compliers may be less productive than their counterparts who are willing to accept less flexible jobs. What about participants' work patterns explains these differences in job performance?

One of the major differences between working at home and working from an office is the possibility for interruptions. 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 participant’s tasks into worksessions in which consecutive tasks are completed at most ten minutes apart (following [Adams-Prassl and Berg, 2017](#)). Consistent with this hypothesis, working from the office causes a large increase in the concentration of work, in the sense that worksessions are longer and more tasks are completed in each worksession. As reported in Figure 7 (Panel B), participants who work from the office complete 68% more tasks per worksession on average ( $p < 0.001$ ).

If there are no task-switching costs, then the length of worksession should not matter for job performance. However, based on the amount of time that tasks require at the beginning of a worksession versus later on, as well as on the accuracy scores for the first few tasks in each worksession, it appears that there are performance costs to switching back and forth between job tasks and other activities (Figure 7, 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, the tasks also take longer for the worker to complete.

These results suggest that costly task switching may at least partially explain the negative impact of work-from-home on worker efficiency. Although only suggestive, the ability to work uninterrupted may also explain why flexibility compliers have lower productivity than women who are willing to accept inflexible jobs. Flexibility compliers have shorter worksessions than participants who say that they are willing to work from an office, which may reflect that participants who are not willing to work from the office know that their household responsibilities will not allow them to work in a focused way for a sustained interval of time.

In this piece-rate context, the lower efficiency of home-based workers is unimportant to an employer. However, 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, then this may prove to be more of a barrier for jobs that require working in teams or quick turnarounds.

## **5 Gateway jobs**

The effect of flexible work arrangements on take up of work in Round 1, combined with the shift in attitudes, suggests a large potential for work experience in Round 1 to affect Round 2 labor supply decisions. Women and other members of their households may learn that they like it more (or less) than they expected when women in the household work for pay, and the treatment effect on gender attitudes could change the set of jobs that women can or would want to take up. This raises the possibility that short-term, part-time flexible jobs could act as “gateway jobs” insofar that shifting people into flexible work may change beliefs or preferences, and that may actually increase their interest in and ability to do future work that is less flexible.

We now test this hypothesis of gateway jobs by testing whether people who are shifted into paid work by being offered a flexible job are more likely to take a less flexible job afterwards than people who have not been shifted into any job yet. We test this by comparing round 2 job take up between (i) households that are “dropped into the deep end,” as in offered an inflexible job without first having prior work experience with a more flexible job, and (ii) households that were offered an inflexible job only after first experiencing working in a more flexible job.

We operationalize this test by regressing round 2 job take up on whether the round 1 job offer was more flexible than the round 2 job offer. For example, if a household’s round 2 job offer is the office job, then we say that the household was offered a “gateway job” if in round 1 they were offered the most flexible job, the time-inflexible job, the child-inflexible job, or the time- & child-inflexible job, and we say that the household was “dropped in the deep end” if they were offered the office job or no job during round 1. Similarly, if a household is offered the child-inflexible job during round 2, then we say that the household was offered a “gateway job” if in round 1 the household was offered the most flexible job or the time-inflexible job, and we say that the household was “dropped in the deep end” if in round 1 they were offered the child-inflexible job, the time- & child-inflexible job, the office job, or no job. These variables are defined similarly for the rest of the round 2 jobs. The regression specification to test this “gateway jobs” hypothesis is defined as follows, with  $\beta_1$  as the coefficient of interest:

$$R2\_Job\_TakeUp_{ij} = \beta_0 + \beta_1 R1\_MoreFlex\_R2_{ij} + \beta_2 R2=Flex_j + \beta_3 R2=TimeInflex_j + \beta_4 R2=ChildInflex_j + \beta_5 R2=TimechildInflex_j + \beta_6 R2=Office_j + X_i + \varepsilon_i \quad (5)$$

$\beta_1 > 0$  means that women are more likely to start a less flexible job if they were first offered a more flexible job as a stepping stone to that job. Table 7 presents results of this regression (columns 1 and 2), showing that  $\hat{\beta}_1 = 0.06$  ( $p = 0.032$ ). Columns 3 and 4 present the results of a similar specification that interacts  $R1\_MoreFlex\_R2_{ij}$  with each of the round 2 work arrangements excluding the most flexible job. The coefficients on these interactions are also all positive but more noisily estimated due to smaller sample sizes, but the coefficient on the interaction with a round 2 office-based job is still positive and significant ( $p = 0.044$ ).

The results suggest that a one-time experience with flexible jobs did not reduce future willingness to take up less flexible work, and in fact increased willingness to take up less flexible jobs. This means that potential negative effects of getting accustomed to flexible work or the negative income effects on labor supply are, if present, overwhelmed by a positive effect that may be driven by learning or changes in attitudes. Survey evidence suggests that households where women worked during round 1 had, overall, a positive experience with the jobs. 97% of women who worked said that the pay for the job was fair or even generous, and 77% said that the work was less difficult

than the cooking, cleaning, and childcare that they are responsible for. In a sense, flexible work arrangements may subsidize households' trial of women working, and so flexible work arrangements could increase future interest in women's work if paid jobs are an experience good.

## 6 Effects of jobs intervention

The gateway job effect may act through effects of the jobs intervention on women's and their families' attitudes about appropriate behavior for each gender. In this section, we explore the effects of receiving a job offer on gender attitudes of women and their children. We also examine effects on a few other pre-specified outcomes, including agency and psychological wellbeing.

### 6.1 Estimation

We estimate the effects of the job treatments using variations on the following participant-level intent-to-treat (ITT) regression:

$$y_{is} = \gamma_s + \gamma_{ITT} \text{JobOffer}_{is} + \gamma_3 \theta_{is} + \varepsilon_{is} \quad (6)$$

where  $y_{is}$  is the relevant outcome variable (e.g. gender-related attitudes, agency, or future labor supply);  $\gamma_s$  is strata ( $s$ ) fixed effects;  $\text{JobOffer}_{is}$  is the randomization into receiving a job offer; and  $\theta_{is}$  is a vector of control variables selected using a double LASSO (least absolute shrinkage and selection operator) (Belloni et al., 2014). In some specifications, we pool together all job offers and compare participants who received any job offer to the control group. In other specifications, we compare the effects of being offered a flexible job with the effects of being offered an inflexible job. As before, "flexible" will encompass the most flexible job and the time-inflexible job, because they have indistinguishable take up rates, while "inflexible" will encompass all other jobs. The coefficient of interest is  $\gamma_{ITT}$ , which represents the effect of receiving a job offer.

We also report two-stage least squares (2SLS) estimates in some cases, for which we instrument job take up with being assigned to a treatment group. The first stage equation is

$$\text{JobStart}_{is} = \pi_s + \pi_{FS} \text{JobOffer}_{is} + \pi_3 \theta_{is} + u_{is} \quad (7)$$

and the second stage equation is

$$y_{is} = \beta_s + \beta_{2SLS} \widehat{\text{JobStart}}_{is} + \beta_3 \theta_{is} + e_{is} \quad (8)$$

where the coefficient of interest  $\beta_{2SLS}$  is the effect of work experience on the outcome of interest. For the exclusion restriction to hold, the job offer needs to affect the outcome only through work

experience.

## 6.2 Effects of work on gender-related attitudes

*Measuring endline gender attitudes.* The endline survey gender-related attitudes index is composed of 15 questions, which were similar to those on the baseline survey. Respondents were asked to rate whether they strongly disagreed, disagreed, agreed, or strongly agreed with each of 15 statements across four domains (household roles, employment, technology use, and ability). In the main specifications, the gender attitudes index is constructed following the strategy outlined in [Anderson \(2008\)](#). The effects of work on attitudes are similar when we use other methods of aggregating the gender attitudes questions, such as taking a simple average (see [Table A.16](#)).

*Overall treatment effects.* As reported in [Figure 9](#) and [Table 4](#), the jobs intervention shifts women's gender attitudes to become less traditional. Assignment to a job offer group causes women's gender attitudes to become 0.05 SDs less traditional on average ( $p = 0.038$ ). Assuming the job offers affect gender attitudes only through work experience, having a job shifts gender attitudes to be less traditional by 0.12 SDs (2SLS estimate).

*Heterogeneity by baseline gender attitudes.* The treatment effect on gender attitudes is concentrated nearly entirely on participants whose views were more traditional at baseline (results reported in [Table A.19](#) and [Figure 9](#)). Job offers shifted gender attitudes of more traditional women by 0.11 SDs on average ( $p = 0.001$ ), translating to a 2SLS job experience effect of 0.31 SDs. In contrast, the estimated treatment effect for women who were already less traditional at baseline is small (-0.02 SDs) and insignificant ( $p = 0.557$ ). [Figure A.14](#) 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 concentration of effects on more traditional women is not driven by ceiling effects.

*Heterogeneity by work arrangement.* The effect on gender-related attitudes is driven largely by the more flexible jobs (Fully-Flexible and Time-Inflexible), as seen in [Table 4](#). Being offered a flexible job shifts women's gender attitudes to become 0.07 SDs less traditional on average ( $p = 0.007$ ), which translates to a 0.13 SDs effect of work experience according to the 2SLS estimate. The treatment effects on attitudes of being assigned to any other less flexible job arrangement is not significant, which could be due to lower job take up in the less flexible groups. We can reject that the being offered a flexible job has the same effect as being offered a childcare-inflexible job ( $p = 0.048$ ), although we cannot reject that being offered a flexible job has the same effect as being offered an office job ( $p = 0.231$ ).

### 6.3 Spillovers to children

*Measuring effects on children.* At both the baseline and endline survey, we attempted to speak to one child aged eight to eighteen in each household. Children were asked about their parents' labor supply, their gender attitudes, how often they and their father helped with household chores, and their educational aspirations. The gender attitudes module was combined into an index, following the same procedure as for adult women. Questions about attitudes were similar to those asked to mothers, with 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.").

*Noticing that mother has started a paid job.* As shown in Table 6 (column 1), 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 ( $p < 0.001$ ). As a placebo check, we also ask children if their fathers had a job, and treatment has no effect on this outcome (column 2). Age and gender do not predict whether children take note that their mother has started a paid job.

*Effects on children's attitudes.* As reported in Table 6 (columns 3-5), 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 (age  $> 12$ ), who become less traditional by 0.11 SDs ( $p = 0.034$ ). On the contrary, younger children's attitudes remain unchanged.

Examining differences by gender, boy children have attitudes that are 0.13 SDs less gender-equal than girl children on average ( $p = 0.062$ ), showing that differences in gender-based preferences and beliefs start at a young age. However, the treatment does not affect the attitudes of boy children differentially from girl children, and so the gap in average gender attitudes between boys and girls is the same in the treatment and control group at endline ( $p = 0.935$ ).

To put this effect in context, this is a very large effect on attitudes given the short-term nature of the intervention and the lack of direct focus on attitudes in the intervention. Finding an effect on adolescents but not younger children is consistent with the results found by [Dhar et al. \(2022\)](#), who find that an intensive two-year classroom intervention in Haryana about gender equity shift the attitudes of older children's by 0.18 SDs on average. 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 gender role in society, while still being young enough that their views are relatively malleable ([Kohlberg, 1976](#); [Markus and Nurius, 1986](#)). One caveat, however, is that the effects were measured approximately one week after the end of

the intervention, and it is possible that the effects would fade in the medium- to long-run without reinforcement.

*Effects on contribution to home production.* Older children assigned to treatment report helping more with cleaning. As reported in Table A.25, while control group children report on average helping with cleaning less half the time, the average treatment group child reports helping with cleaning closer to half the time ( $p = 0.064$ ). Younger children do not report helping more with any chores when their mothers are assigned to the treatment group. Examining heterogeneity by child gender, girl children are more likely than boy children to report helping with cooking ( $p = 0.016$ ) and cleaning ( $p = 0.013$ ). Boys on average say that they help with cooking and cleaning between never and less than half the time, while girls report helping with both somewhere between less than half and half the time. However, there is no differential effect on boy versus girl children, and so this gap remains in the treatment group.

In addition to asking children how often they help, we also ask children to what degree their fathers help with cooking, cleaning, and childcare. As reported in column 4 of Table A.25, treatment group children are 9 pp more likely to report that their fathers helped at least occasionally (more than never) during the treatment period ( $p = 0.045$ ). This effect is particularly concentrated on older children. In the control group, half of older children report that their fathers never help with cooking, cleaning, or childcare. Treatment increases the fraction of older children saying their fathers help by 17 pp ( $p = 0.005$ ). On the contrary, there is no treatment effect on the fathers of younger children, but younger children's fathers are 19 pp more likely to help with home production even in the absence of treatment ( $p = 0.016$ ).

*No effects on children's aspirations.* Treatment had no significant impact on children's aspirations. As reported in Table A.26, treatment had no effect on whether children report wanting to get an undergraduate degree or a master's degree. There are no heterogeneous effects by child age or gender.

*Discussion.* When their mothers begin working, children notice. Older children are more affected by their mothers receiving a job offer: they are more likely (i) to report helping with cleaning, (ii) to say that their fathers are helping with cooking, cleaning, and childcare, and (iii) to hold less traditional gender attitudes.

The effects on children could be driven either by the job offer or by their mothers doing 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 intel-



ligent or able than they previously believed. This could affect their willingness to help with home production, as well as their attitudes about the relative abilities of men and women. Similarly, women’s husbands might learn from the job offer that their wives have better outside options than they previously believed, which could increase women’s bargaining power in the household and cause their husbands to do more housework.

#### **6.4 Effects on Other Outcomes**

*Psychological wellbeing.* Treatment had no effects on psychological wellbeing (see Table A.21). This was true for the overall index as well as the four index components individually: (i) sleeping peacefully, (ii) feeling overwhelmed, (iii) feeling happy, and (iv) feeling worried. In this module, participants were asked to rate whether over the last month (i.e. during the treatment period), they felt this way never, a few days, around half the days, more than half the days, or nearly every day. At baseline, the average participant has moderately high levels of psychological wellbeing. The average participant sleeps peacefully most days, feels overwhelmed occasionally to half of the time, feels happy around half to most days, and feels anxious occasionally to half the time. Examining each work arrangement separately, we do not see important heterogeneity by the work arrangement (Panel B). There are also no heterogeneous treatment effects by participants’ baseline wellbeing (Panel C).

*Self Perceptions.* Treatment participants are more likely to believe that their potential and talents are put to good use. In the control group, more than one quarter of participants say that their potential and talents are put to good use “not at all.” In comparison, the treatment group is more likely to believe that their potential and talents are put to good use “a little bit,” “somewhat,” or “very much” (see Figure 10). Even in the treatment group, however, fewer than 15% of participants say that their potential and talents are put to good use “very much.” Coding “not at all” as 0, “a little bit” as 1, “somewhat” as 2, and “very much” as 3, treatment increased participants’ self-reported ability/talent use score by 9.5% ( $p = 0.021$ ). This corresponds to an 8 pp (32%) decrease in participants who said that their talents/abilities were put to use “not at all” ( $p = 0.001$ )

*Agency.* As reported by Table A.20, treatment had no significant effect on the women’s agency index. The agency index was composed of 6 questions: (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. In Panels B and C, we test for heterogeneous treatment effects on agency by work arrangement or by baseline agency and do not find any effects. Because the job

was short-term, it may be less likely to affect women's bargaining power in the household.

## 7 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 accommodate 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, the difference in take up rates between jobs shows that flexible work arrangements are effective at increasing labor supply for women, particularly those from traditional households. Varying different dimensions of job flexibility, we document that the ability to multitask (combining work with childcare) and to work from home are the deciding factor in whether or not to work for many women. Second, flexible work arrangements work well for employers that pay piece rate: flexibility increases worker reliability while maintaining the same amount of quality-adjusted output. However, working from home causes women to work more slowly, and the women drawn into the firm by flexible work arrangements also work more slowly. Third, flexible work arrangements act as a gateway to less flexible jobs, including outside-the-home work. Flexibility makes the biggest difference to labor supply for women from more traditional households, and experience with flexible jobs in turn shifts the gender attitudes of women and children to become less traditional. 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 work: 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?

In our study, all of the job opportunities are part time. Another avenue for future research is to test whether part-time, at-home work could act as a stepping stone to *full-time* work. If it is most effective to offer jobs which are incrementally more demanding on women’s time use and attention, then there is likely too large a leap between the jobs involved in our study and the jobs currently available to women outside the study, as all of the jobs in our study involved part-time work. Would intermediate steps in terms of how time consuming a job is be an effective stepping stone between part-time and full-time work, or does a different strategy need to be taken for overcoming this large step?

One policy implication of these findings is that offering flexible work arrangements could 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, and workers who can work from home are more likely to complete their work assignments than workers who are required to come to the office. If workers who have some work-from-home experience with a firm are more likely to later accept office-based jobs at the firm, employers might even be able to offer home-based training which would lead to office-based work. This could allow employers to tap a larger potential workforce while the training period could allow them to screen for the most competent workers. Firms might not be aware of these potential benefits of offering flexible work arrangements, and so temporary subsidies and assistance for firms transitioning jobs to become remote could have long-term positive effects on women’s employment.

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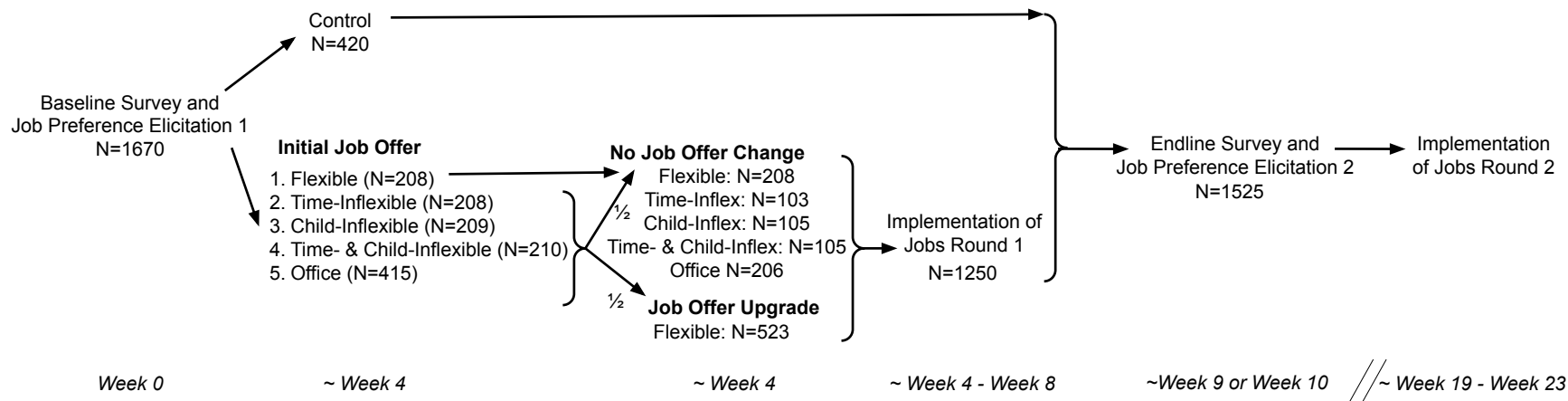
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## Figures

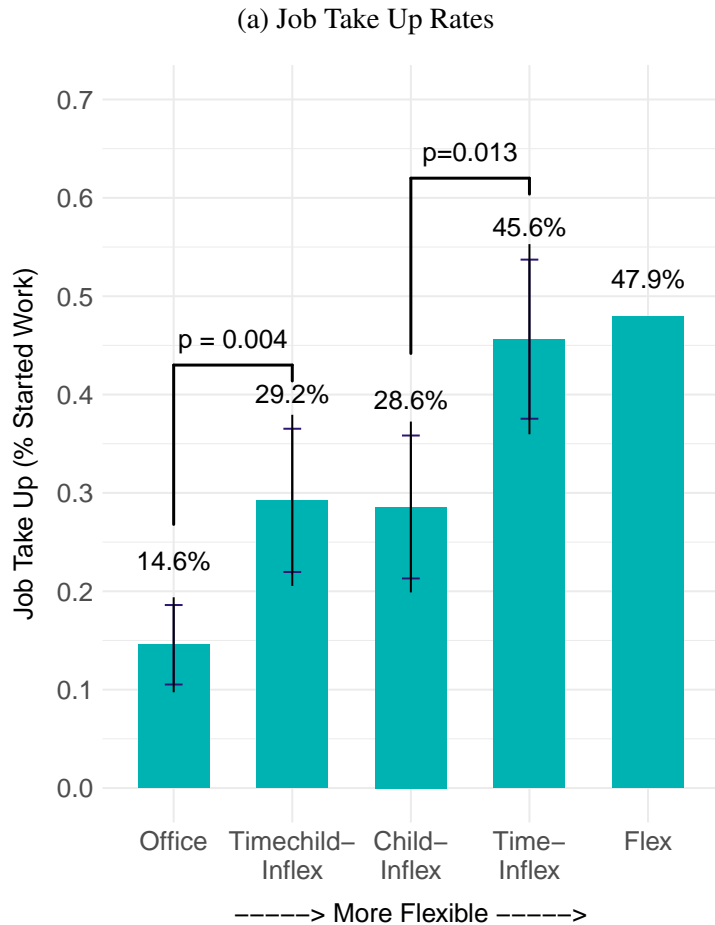
Figure 1: Experimental Design



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 the 5 jobs involved in the treatment. The baseline survey for children includes modules about (i) aspirations, (ii) gender attitudes, and (iii) help with 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 work arrangements and the treatment effects of those work arrangements, mirroring the design in [Karlan and Zinman \(2009\)](#). After this final job offer, participants start their part-time, month-long job.
- 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. One of their choices is randomly selected 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). 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



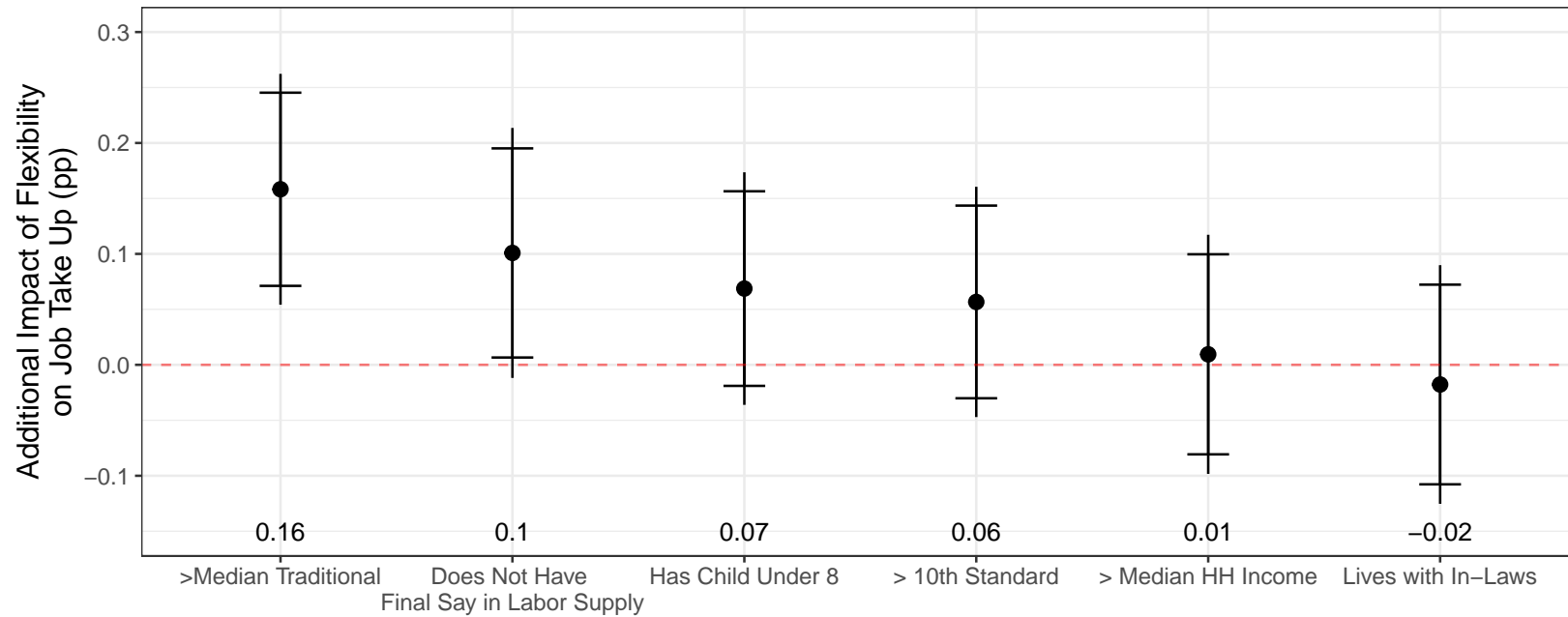
(b) Job Descriptions

Work Arrangement (1)	Choose Hours (2)	Multitasking (Childcare) (3)	Work from Home (4)
Flex	Yes	Yes	Yes
Time-Inflexible	No	Yes	Yes
Child-Inflexible	Yes	No	Yes
Timechild-Inflexible	No	No	Yes
Office	(Yes)	No	No

Notes: This figure plots the take up rate for each of the five jobs during intervention.

- 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.” 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 2.
- 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: Heterogeneous Importance of Flexibility for Take Up of Work



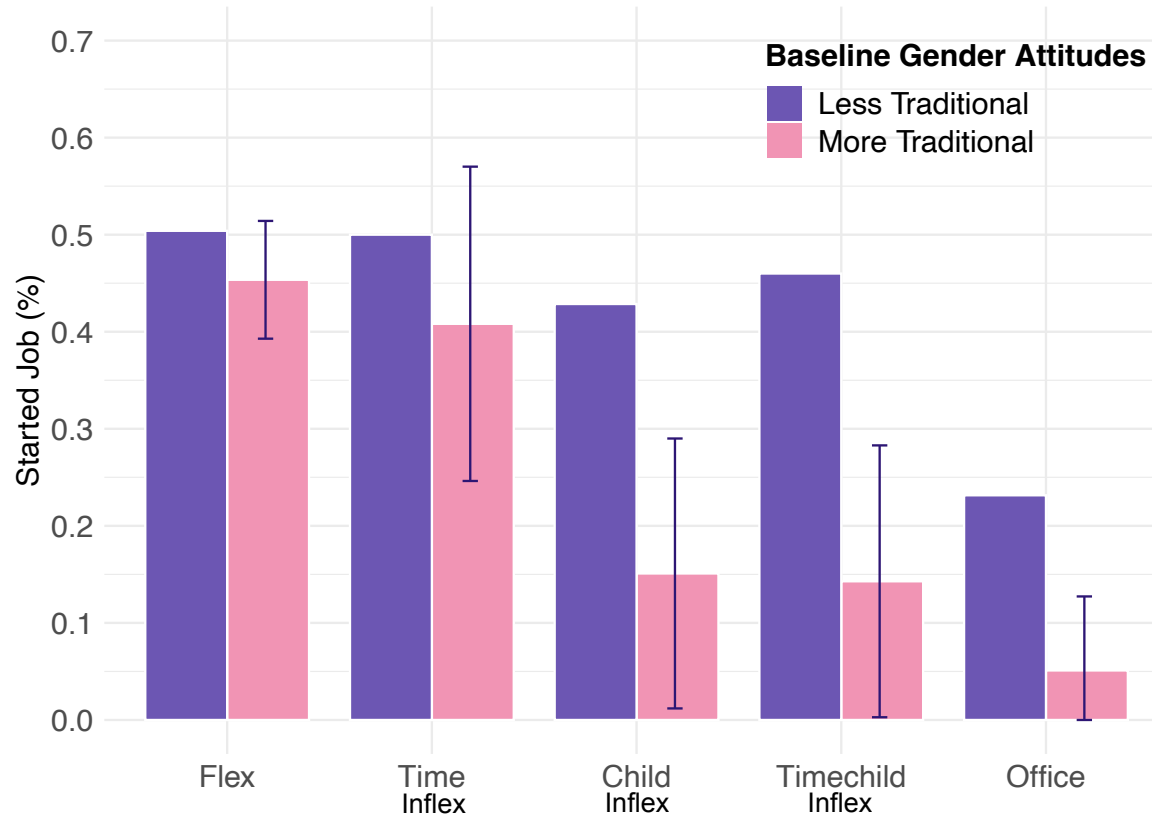
38

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

- The plotted coefficients are the  $\beta_3$  estimates from regressions that take the following form:  

$$y_{ij} = \beta_0 + \beta_1 \text{flexible}_j + \beta_2 \text{characteristic}_i + \beta_3 \text{flexible}_j \times \text{characteristic}_i + \varepsilon_{ij}$$
 where  $y_{ij}$  is a binary variable equal to one if participant  $i$  starts job  $j$  after being offered it.  $\text{flexible}_j$  is a dummy variable equal to one for if job  $j$  is one of the jobs with the the highest take up (i.e. the most flexible job) and equal to zero for all other jobs.
- The meaning of the  $\beta_3$  coefficient is the additional importance of flexibility to job take up for women satisfying a particular characteristic (from left to right: having gender attitudes more traditional than the median participant; not having the final say in one's own labor supply; having a child younger than eight; being educated to at least 10th standard; having household income greater than the median; living with at least one parent-in-law). Gender attitudes are computed as a weighted average of 16 questions on the baseline survey, in which the weights take into account the covariance structure of the components (Anderson, 2008). All characteristics are binary.
- Standard errors are heteroskedasticity-robust. Confidence intervals at the 90% and 95% levels are reported.

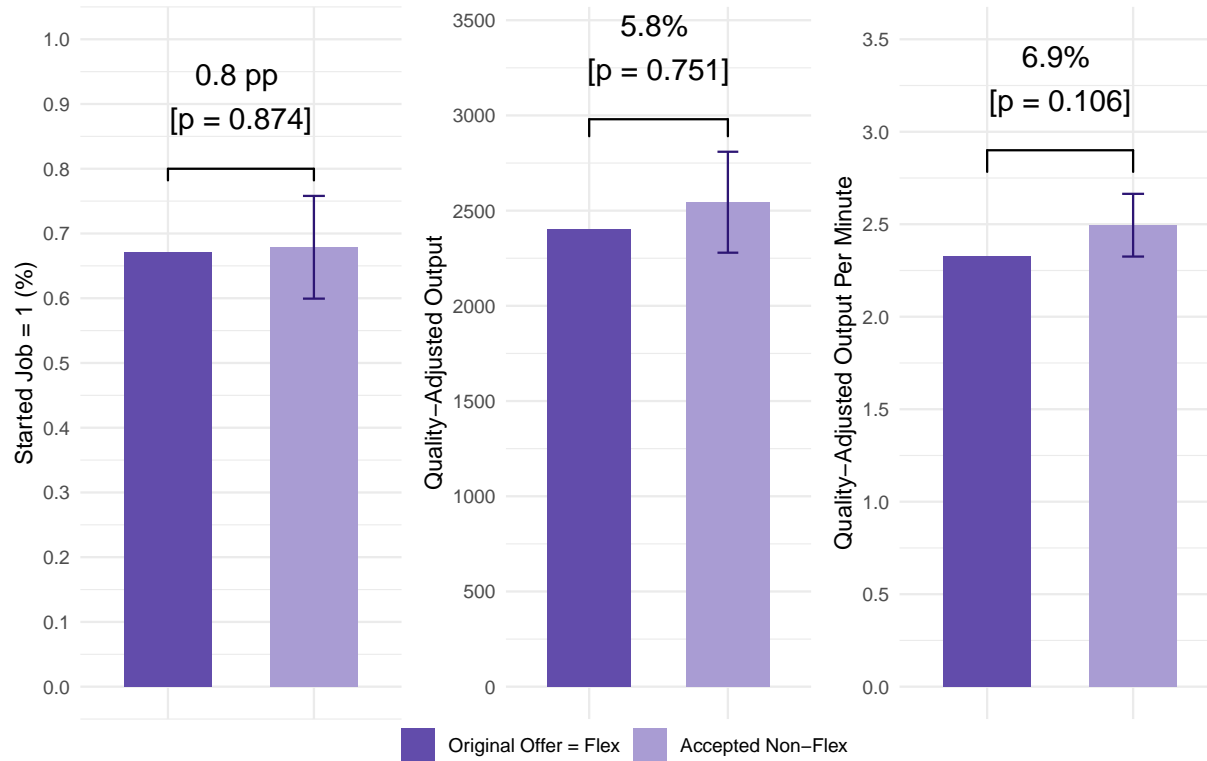
Figure 4: Heterogeneity in Job Take Up by Baseline Gender Attitudes



Notes: This figure shows how job take up (starting work) varies 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).
- Confidence intervals at the 90% level are shown.

Figure 5: Effects of Flexibility on Worker Composition



Notes: This figure shows how flexible work arrangements affect worker composition in terms of job performance.

- Three job performance measures are included: on the left, reliability (i.e. the likelihood that the worker starts the job after accepting it); in the middle, quality-adjusted output (i.e. the total output produced by the worker during the month-long job, adjusted by Karya’s quality metrics); and on the right, efficiency (i.e. quality-adjusted output divided by minutes spent actively working). Minutes spent working are measured according to number of unique minutes during which the worker took an action on the job app.
- All workers in these figures worked in the most flexible arrangement. The performance measures are plotted separately for workers initially offered the most flexible job (Flex), and the workers who were initially offered and accepted a less flexible job (Time for time-inflexible, Child for child-inflexible, Timechild for time- and child-inflexible, and Office or office-based) and then were randomly selected for an upgrade to the most flexible job. All workers who were initially offered and accepted a less flexible job before their upgrade are pooled together. See Appendix Figure A.27 for a version where these different dimensions of flexibility are separated out.
- The brackets report the magnitude and  $p$ -value for the difference between performance of the Flex group and performance of all the less flexible groups pooled together. Whiskers indicate 90% confidence intervals.

Figure 6: Effects of Flexibility on Job Performance



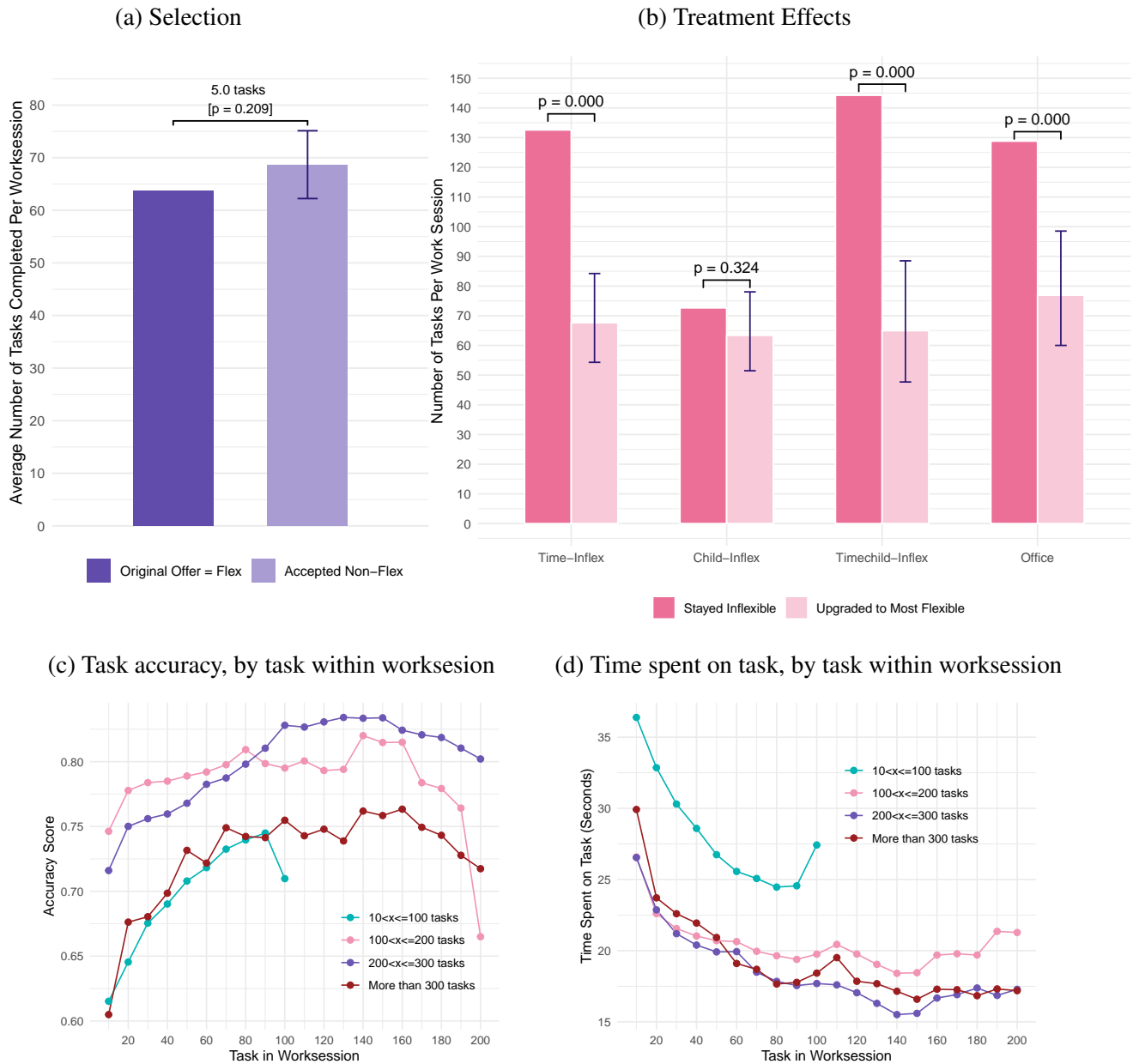
Notes: This figure shows the impact of flexible work arrangements on job performance of inframarginal workers (i.e. those who would accept the job with or without flexibility).

- Three job performance measures are included: In panel A, reliability (i.e. the likelihood that the worker starts the job after accepting it); in Panel B, quality-adjusted output (i.e. the total output produced by the worker during the month-long job, adjusted for Karya’s quality metrics that determine pay); and in Panel C, efficiency (i.e. a measure of work efficiency that is quality-adjusted output divided by minutes spent actively working). Minutes spent working are measured according to number of unique minutes during which the worker took an action on the job app. To see the effects of flexibility on output, including the “no-show” workers (those who accepted the job but never started it), see Appendix Figure A.28.
- Each subfigure shows the impact of four different types of flexibility, with a pair of dark and light bars for each dimension (from left to right: flexibility to choose work hours, flexibility to multitask work with childcare, the combination of flexibility to choose work hours and multitask work with childcare, and the ability to work from home). The darker bars plot job performance for workers who accepted a less flexible job and did not have their job offer changed, while the lighter bars plot job performance for workers who accepted a less flexible job and were randomly selected for a surprise upgrade to the most flexible job.
- The brackets report the magnitude and *p*-value for the difference in job performance between workers who continued in the less flexible jobs, and those who were surprised with an upgrade to the most flexible job. Whiskers indicate 90% confidence intervals.





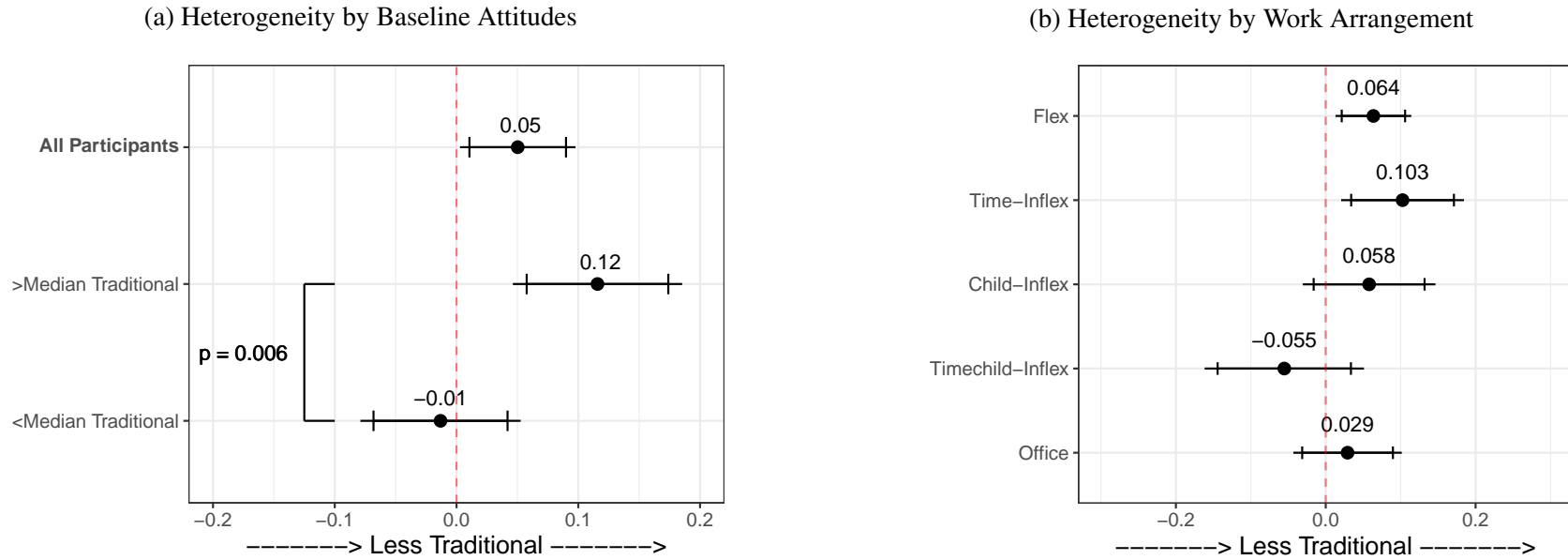
Figure 7: Flow Effects



Notes: This figure offers descriptive evidence for mechanisms driving the differences in job performance.

- Panels A and B report number of tasks completed per worksession between different treatment groups. 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 a less flexible job and are upgraded to the most flexible job (lighter bar) with workers who were initially assigned to the most flexible job (darker bar). Panel B compares workers who accept a less flexible job and are upgraded to the most flexible job (lighter bars) with workers who accept a less flexible job and are not randomly selected for an upgrade (darker bars). The brackets report the  $p$ -value for the difference in job performance between workers who continued in the less flexible jobs, and those who were surprised with an upgrade to the most flexible job. Whiskers indicate 90% confidence intervals.
- 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.

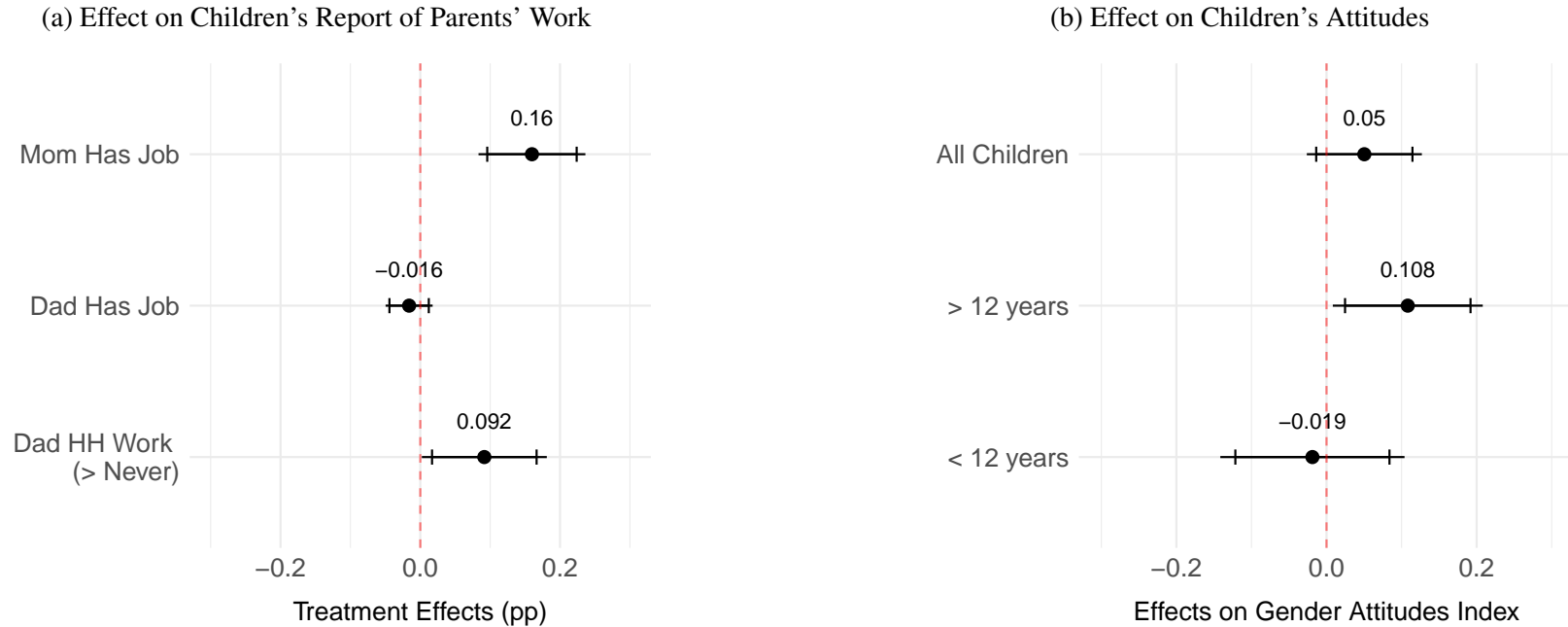
Figure 8: Treatment Effect of Jobs Intervention on Women's Own Gender Attitudes



Notes: This figure reports intent-to-treat effects on gender attitudes, with heterogeneity by baseline attitudes (Panel A) and randomly assigned work arrangement (Panel B).

- In both Panels A and B, participants' scores on the endline gender attitudes index are regressed on treatment assignment. In Panel A, the regression is first on all participants (top), and then separately on participants with pre-intervention attitudes more traditional than the median (middle) and less traditional than the median (bottom). The  $p$ -values correspond to a test of whether the treatment effect on gender attitudes is equal in the two subsamples. In Panel B, the effect on each participants randomly assigned to each work arrangement is estimated separately by work arrangement. All specifications include lasso-selected controls and strata fixed effects.
- 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](#)).
- In Panel A, participants are categorized according to whether their baseline gender attitudes are more (or less) traditional than the median study participant.
- In Panel B, participants are categorized according to the work arrangement they were randomly assigned to.
- Standard errors are heteroskedasticity robust. Estimates are plotted along with corresponding 90% and 95% confidence intervals.

Figure 9: Treatment Effect of Jobs Intervention on Children

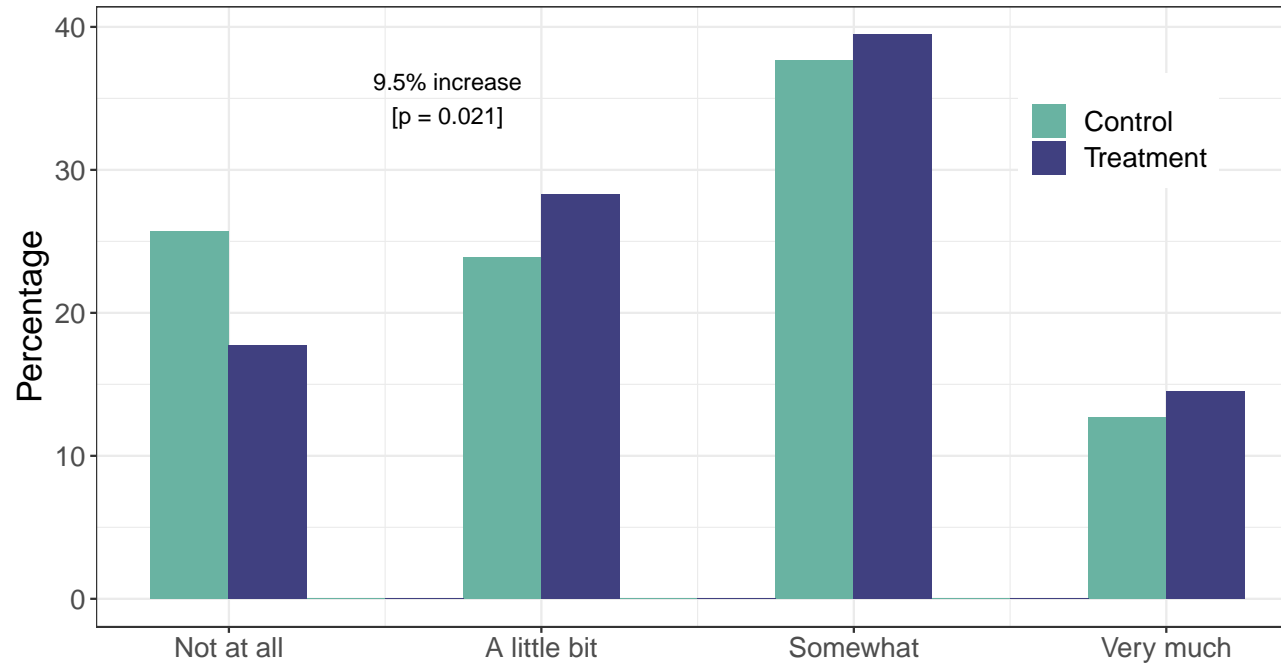


45

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 10: Treatment Effect on Perception of Talent and Ability 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 effect 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.

## Tables

Table 1: 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
<i>Number of participants</i>	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 2: Effect of Flexible Job Attributes on Take Up of Work

	<b>Job Take Up</b>		
	Baseline (1)	Job Offer (2)	Start Work (3)
Time-inflexible	-0.04*** (0.01) [0.00]	-0.04 (0.04) [0.38]	-0.02 (0.05) [0.66]
Child-inflexible	-0.27*** (0.01) [0.00]	-0.17*** (0.05) [0.00]	-0.19*** (0.05) [0.00]
Time- & Child-inflexible	-0.32*** (0.01) [0.00]	-0.15*** (0.05) [0.00]	-0.19*** (0.05) [0.00]
Office	-0.42*** (0.01) [0.00]	-0.25*** (0.04) [0.00]	-0.33*** (0.03) [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 participant had been randomized to one work arrangement. The number of observations in Column (1) is 8290 = 1658 × 5 because there are 12 respondents who did not respond to the location-inflexible question.
- Standard errors in parentheses (·) are robust to heteroscedasticity and clustered at the participant level for column (1). Brackets [·] report unadjusted p-values, as do stars next to coefficients (\* significant at 10%; \*\* at 5%; \*\*\* at 1%).

Table 3: Differential Importance of Flexibility by Gender Attitudes and Young Children

	Baseline (1)	Job Offer (2)	Start Work (3)
<i>Panel A: All Participants</i>			
Flexible Job	0.34*** (0.01)	0.19*** (0.04)	0.34*** (0.03)
>Median Traditional Gender Atts	-0.06*** (0.02)	-0.12*** (0.03)	-0.23*** (0.04)
Flexible Job × >Median Traditional Gender Atts	0.04** (0.02)	0.02 (0.06)	0.17*** (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
<i>Panel B: Participants without children under 8</i>			
Flexible Job	0.25*** (0.01)	0.17*** (0.06)	0.24*** (0.05)
>Median Traditional Gender Atts	-0.07*** (0.02)	-0.10** (0.05)	-0.19*** (0.06)
Flexible Job × >Median Traditional Gender Atts	0.05*** (0.02)	0.03 (0.08)	0.04 (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
<i>Panel C: Participants with children under 8</i>			
Flexible Job	0.42*** (0.02)	0.21*** (0.06)	0.44*** (0.04)
>Median Traditional Gender Atts	-0.02 (0.03)	-0.13*** (0.05)	-0.26*** (0.05)
Flexible Job × >Median Traditional Gender Atts	0.00 (0.03)	0.02 (0.08)	0.30*** (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 4: Treatment Effect of Jobs Intervention on Gender Attitudes - Heterogeneity by Work Arrangement

	Gender Attitudes Index					
	ITT Estimates			2SLS Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A: All Treatments Pooled</i>						
Treatment	0.06**	0.06**	0.05**			
	(0.03)	(0.03)	(0.02)			
Started Work				0.15**	0.15**	0.12**
				(0.06)	(0.06)	(0.06)
<i>B: Heterogeneity by Work Arrangement</i>						
Arrangement = Flex or Time-Inflex	0.08***	0.08***	0.07***			
	(0.03)	(0.03)	(0.03)			
Arrangement = Child- or Child- & Time-Inflex	0.00	0.00	0.00			
	(0.04)	(0.04)	(0.04)			
Arrangement = Office	0.05	0.05	0.03			
	(0.04)	(0.04)	(0.04)			
Started Flex or Time-Inflex				0.16***	0.16***	0.13***
				(0.05)	(0.05)	(0.05)
Started Child- or Child- & Time-Inflex				0.00	-0.01	-0.01
				(0.13)	(0.13)	(0.12)
Started Office				0.34	0.30	0.18
				(0.25)	(0.25)	(0.23)
Strata Fixed Effects		Yes	Yes		Yes	Yes
Lasso Selected Controls			Yes			Yes
Observations	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

Notes: This table presents intent-to-treat (ITT) and two-stage-least-squares (2SLS) results for the effect of the intervention on gender attitudes. Panel A presents results for the effects when pooling all the jobs groups together, while Panel B examines heterogeneity by work arrangement.

- The gender attitudes index is computed as a standardized, weighted average of questions from the endline survey, in which the weights take into account the covariance structure of the components (Anderson, 2008).
- The dependent variable is gender attitudes measured on the endline survey, which took place within two weeks of the end of the job treatments.
- Heteroskedasticity-robust standard errors are reported in parentheses. Stars next to coefficients represent unadjusted p-values (\* significant at 10%; \*\* at 5%; \*\*\* at 1%).

Table 5: Treatment Effect of Jobs on Gender Attitudes - Heterogeneity by Baseline Attitudes

	Gender Attitudes Index					
	ITT Estimates			2SLS Estimates		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.10*** (0.04) [0.008]	0.10*** (0.04) [0.006]	0.11*** (0.04) [0.001]			
<Traditional at Baseline	0.31*** (0.04) [0.000]	0.30*** (0.04) [0.000]	0.06 (0.05) [0.215]	0.31*** (0.04) [0.000]	0.30*** (0.04) [0.000]	0.07 (0.05) [0.153]
Treatment × <Traditional at Baseline	-0.10** (0.05) [0.041]	-0.11** (0.05) [0.025]	-0.13*** (0.05) [0.006]			
Started Work				0.26*** (0.10) [0.008]	0.27*** (0.10) [0.006]	0.31*** (0.10) [0.001]
Started Work × <Traditional at Baseline				-0.27** (0.12) [0.025]	-0.29** (0.12) [0.016]	-0.34*** (0.12) [0.004]
Strata Fixed Effects		Yes	Yes		Yes	Yes
Lasso-Selected Controls			Yes			Yes
Observations	1,525	1,525	1,525	1,525	1,525	1,525
Control Mean (Gender Attitudes, Endline)	0.01	0.01	0.01	0.01	0.01	0.01
P-val: equality of coefficients						
Treatment Effect (< Traditional) == 0	0.880	0.767	0.557	0.880	0.784	0.566

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.
- The dependent variable is gender attitudes measured on the endline survey, which took place within two weeks of the end of the job treatments. The gender attitudes index is 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).
- Participants are categorized as “less traditional” if their baseline gender attitudes index value is lower than the median participant.
- Standard errors in parentheses (·) are robust to heteroskedasticity. Brackets [·] report unadjusted p-values, as do stars next to coefficients (\* significant at 10%; \*\* at 5%; \*\*\* at 1%).



Table 6: Treatment Effects on Children's Gender Attitudes

	Mom has job	Dad has job	Children's Gender Attitudes		
	(1)	(2)	(3)	(4)	(5)
<i>Panel A: All Children</i>					
Treatment	0.16*** (0.04) [0.000]	-0.02 (0.02) [0.352]	0.06 (0.04) [0.160]	0.07* (0.04) [0.080]	0.04 (0.04) [0.198]
Control Mean	0.19	0.97	0.00	0.00	0.00
<i>Panel B: Heterogeneity by Age</i>					
Treatment	0.13** (0.05) [0.011]	-0.02 (0.03) [0.368]	0.10* (0.06) [0.076]	0.12** (0.05) [0.029]	0.11** (0.05) [0.034]
Treatment × Age < 12	0.03 (0.08) [0.707]	0.01 (0.03) [0.821]	-0.08 (0.09) [0.346]	-0.10 (0.09) [0.243]	-0.13 (0.08) [0.120]
Age < 12	-0.03 (0.06) [0.667]	0.03 (0.03) [0.231]	-0.01 (0.08) [0.856]	0.02 (0.07) [0.795]	0.07 (0.07) [0.307]
Control Mean, Age > 12	0.20	0.96	0.01	0.01	0.01
<i>Panel C: Heterogeneity by Gender</i>					
Treatment	0.19*** (0.05) [0.000]	0.00 (0.03) [0.895]	0.06 (0.07) [0.342]	0.06 (0.06) [0.340]	0.05 (0.06) [0.046]
Treatment × Male	-0.09 (0.07) [0.244]	-0.05 (0.03) [0.187]	-0.02 (0.09) [0.803]	0.00 (0.08) [0.973]	-0.01 (0.08) [0.935]
Male	0.16*** (0.06) [0.008]	0.02 (0.03) [0.505]	-0.14* (0.08) [0.070]	-0.17** (0.07) [0.025]	-0.13* (0.07) [0.062]
Control Mean, Female	0.13	0.96	0.08	0.08	0.08
Observations	601	597	606	606	606
Strata Fixed Effects	Yes	Yes		Yes	Yes
Children's Baseline Attitudes					Yes
Mom Baseline Job Status	Yes				
Dad Baseline Job Status		Yes			

*Notes:* This table presents results on whether children notice the intervention and subsequent effects on children's gender attitudes.

- In addition to the main effect (Panel A), we present heterogeneous treatment effects by children's age compared to the median age of 12 (Panel B) and child gender (Panel C). In this analysis, all treatment groups (work arrangements) are pooled together.
- Columns (1) and (2) test whether children notice that their mothers are working. In column (1), the outcome variable is a dummy equal to one if children say that their mother had a job in the last month. Column (2) asks the same question, but about their father, as a placebo check. Children who say that they do not know whether or not their mother (or father) is working are dropped. Both of these regressions control for the children's baseline report of their parents' labor supply.
- In columns (3)-(5), the outcome variable is children's gender attitudes. Gender attitudes are computed as a standardized, weighted average of questions from the children's surveys, in which the weights take into account the covariance structure of the components (as in [Anderson, 2008](#)).
- Standard errors in parentheses (·) are robust to heteroskedasticity. Brackets [·] report unadjusted  $p$ -values, as do stars next to coefficients (\* significant at 10%; \*\* at 5%; \*\*\* at 1%).

Table 7: “Gateway jobs”: Effect of experience with more flexible job on less flexible job take up

	Started Work in Round 2			
	(1)	(2)	(3)	(4)
R1 More Flexible Than R2	0.06** (0.03)	0.06** (0.03)		
R2: Time-Inflexible × R1 More Flexible Than R2			0.03 (0.07)	0.02 (0.06)
R2: Child-Inflexible × R1 More Flexible Than R2			0.11 (0.07)	0.10 (0.06)
R2: Time- & Child-Inflexible × R1 More Flexible Than R2			0.02 (0.07)	0.01 (0.06)
R2: Office × R1 More Flexible Than R2			0.08** (0.04)	0.08** (0.04)
R2: Time-Inflexible	-0.06 (0.04)	-0.06 (0.04)	-0.04 (0.05)	-0.04 (0.05)
R2: Child-Inflexible	-0.10** (0.04)	-0.10** (0.04)	-0.12** (0.05)	-0.12** (0.05)
R2: Time- & Child-Inflexible	-0.19*** (0.04)	-0.18*** (0.04)	-0.16*** (0.05)	-0.16*** (0.05)
R2: Office	-0.38*** (0.04)	-0.38*** (0.03)	-0.39*** (0.04)	-0.39*** (0.04)
Observations	1,524	1,524	1,524	1,524
Strata fixed effects	✓	✓	✓	✓
Lasso selected controls		✓		✓

Notes: This table presents results on effects of the initial intervention on take up in the second round of jobs.

- Round 2 job take up is measured as whether or not the participant started work.
- Columns (1) and (2) test the gateway jobs hypothesis: whether women are more likely to take up a job in round 2 if they initially had experience with a more flexible job in round 1. This test is operationalized using the empirical specification:  $R2\_Job\_TakeUp_{ij} = \beta_0 + \beta_1 R1\_MoreFlex\_R2_{ij} + \beta_2 R2\_Arrangement_j + X_i + \epsilon_{ij}$ . The coefficient of interest is  $\beta_1$ , the estimate of which is reported in the top row of the table. This coefficient is the additional probability of taking up the randomly assigned job in round 2 — conditional on the round 2 work arrangement — if randomly assigned to a more flexible job in the initial intervention.
- Columns (3) and (4) report the gateway jobs test separately for each work arrangement in round 2, as in the following specification:  $R2\_Job\_TakeUp_{ij} = \beta_0 + \beta_1 R1\_MoreFlex\_R2_{ij} + \beta_2 R2\_Arrangement_j + \beta_3 R1\_MoreFlex\_R2_{ij} \times R2\_Arrangement_j + X_i + \epsilon_{ij}$ . The  $\beta_3$  coefficients are reported in rows 2-5, and represent the additional probability that a participant takes up a specific job in round 2 if she was assigned to a more flexible job in the initial intervention. For example, the coefficient in row 5 is the additional probability of taking up the office-based job in the second round of work if initially assigned to a more flexible job during the initial intervention.
- 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%).

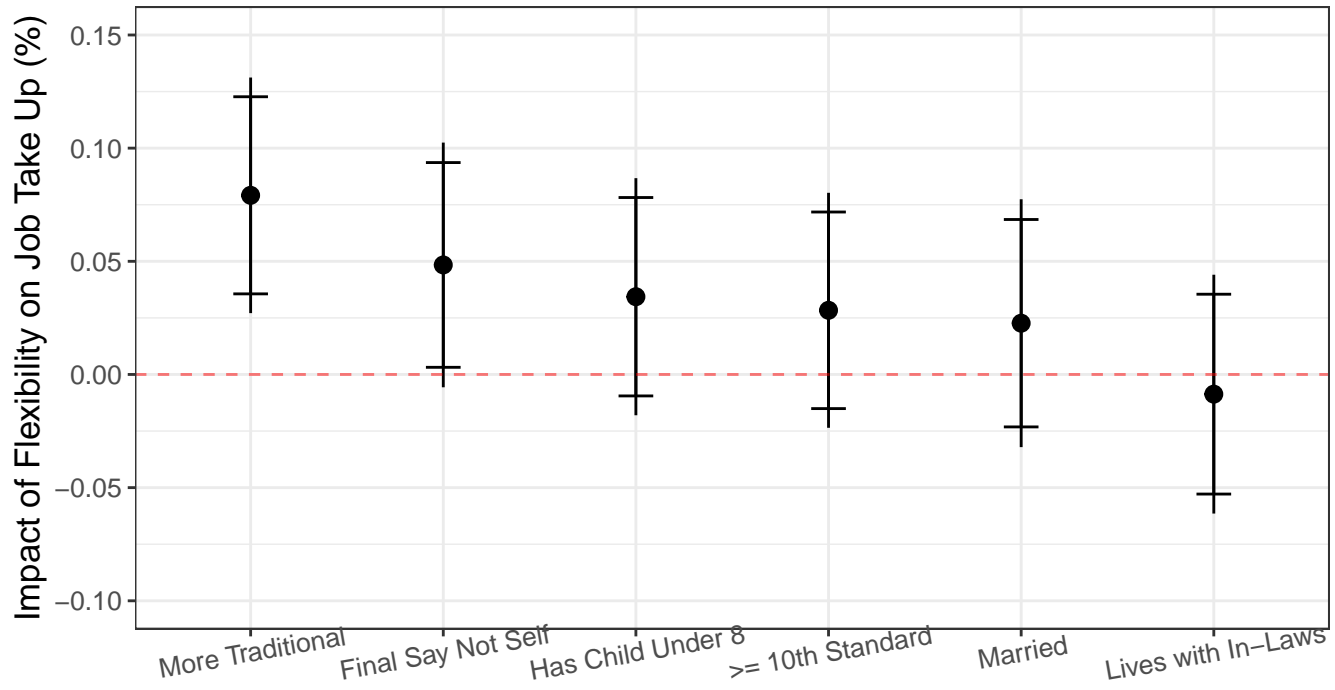
# A Appendix

## Appendix Figures

Figure A.11: Take Up of Jobs: All Work Arrangements, All Take-Up Measures



Figure A.12: Heterogeneous Importance of Flexibility for Take Up of Work



Notes: Robustness check where “Flexibility” defined as the most flexible job only.

Figure A.13: Gender Attitudes at Baseline and Endline (Histogram)

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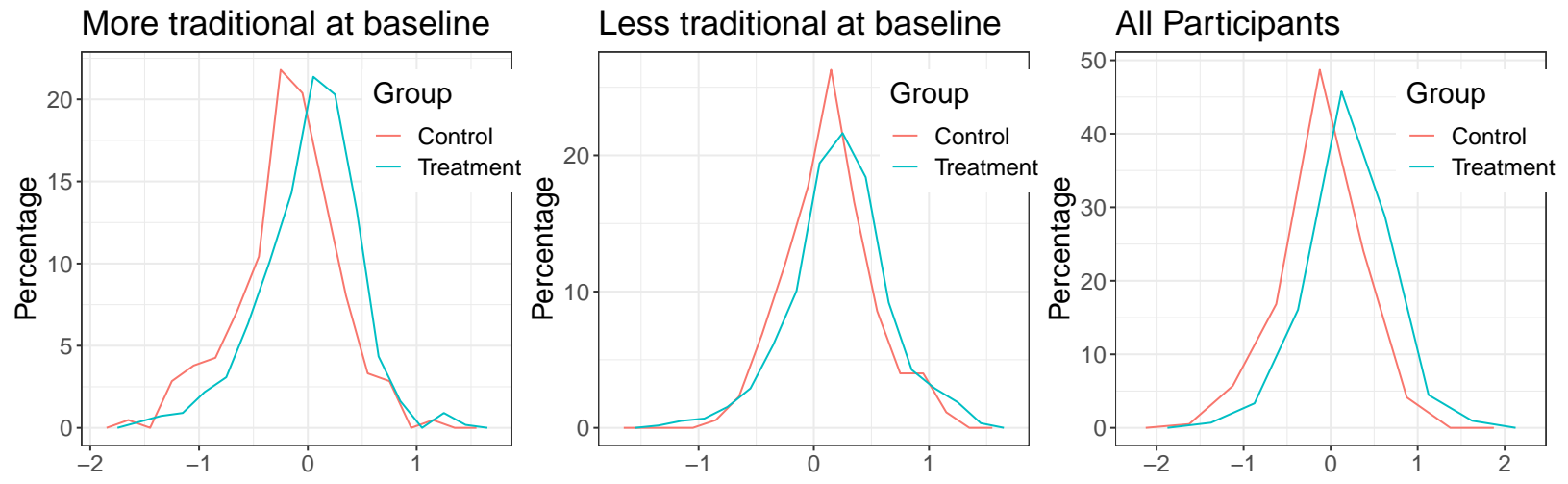
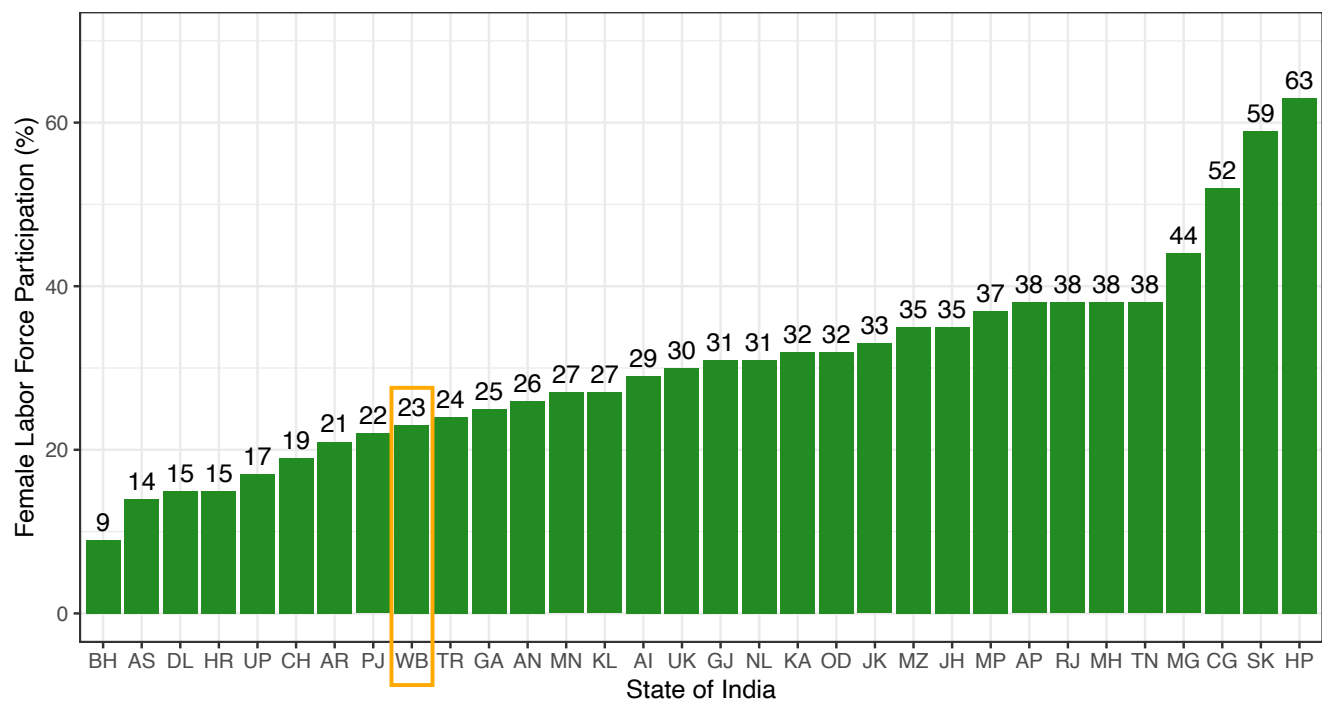


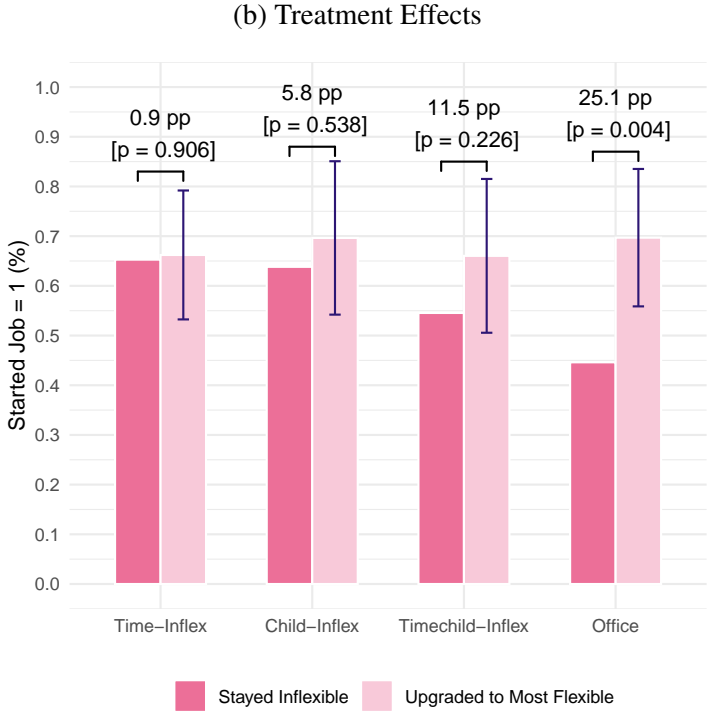
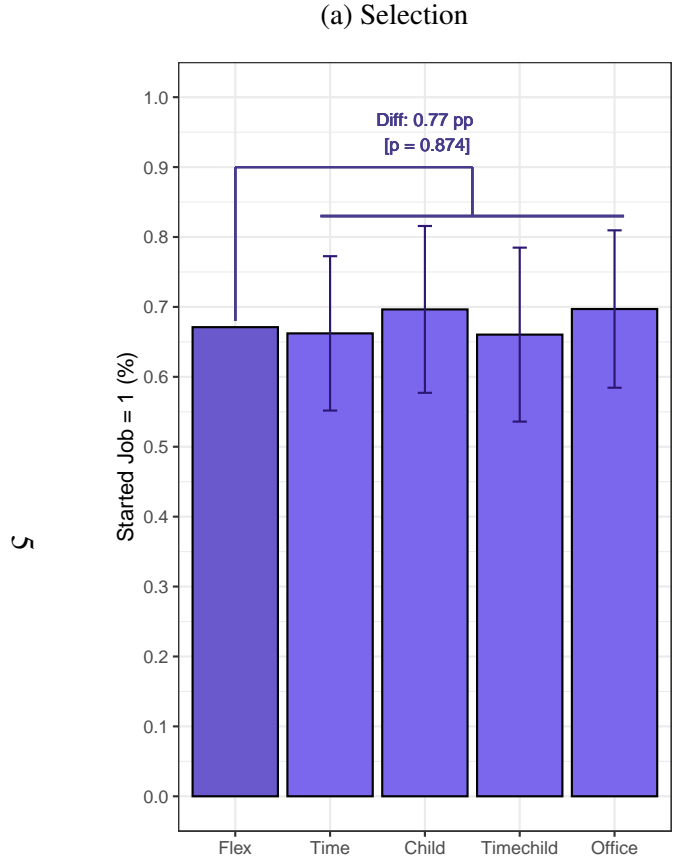


Figure A.14: Female Labor Force Participation Rates (States of India)



Notes: Data from the Periodic Labor Force Survey 2019-2020. People aged 15 and over included.

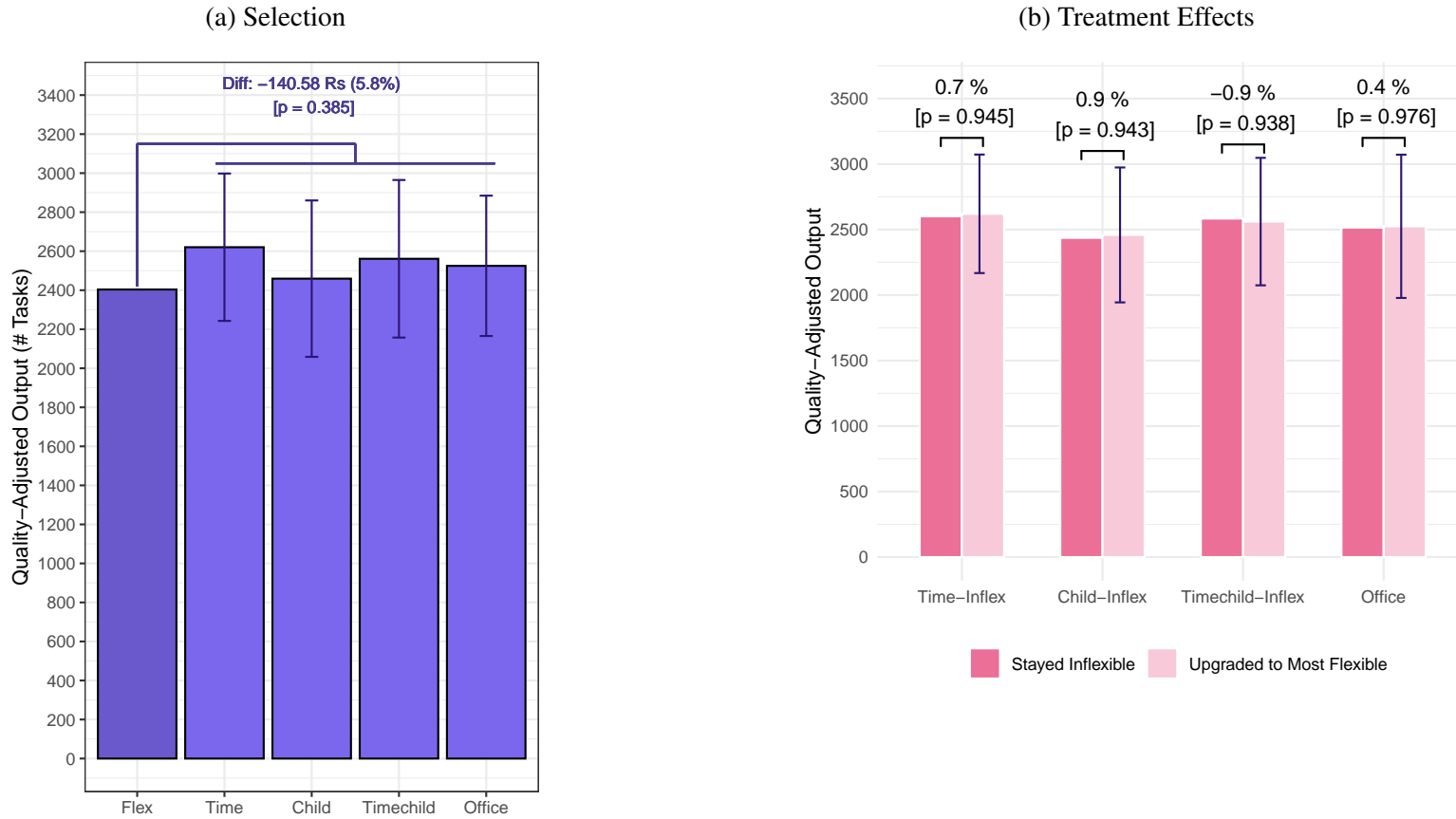
Figure A.15: Started Job



Notes:

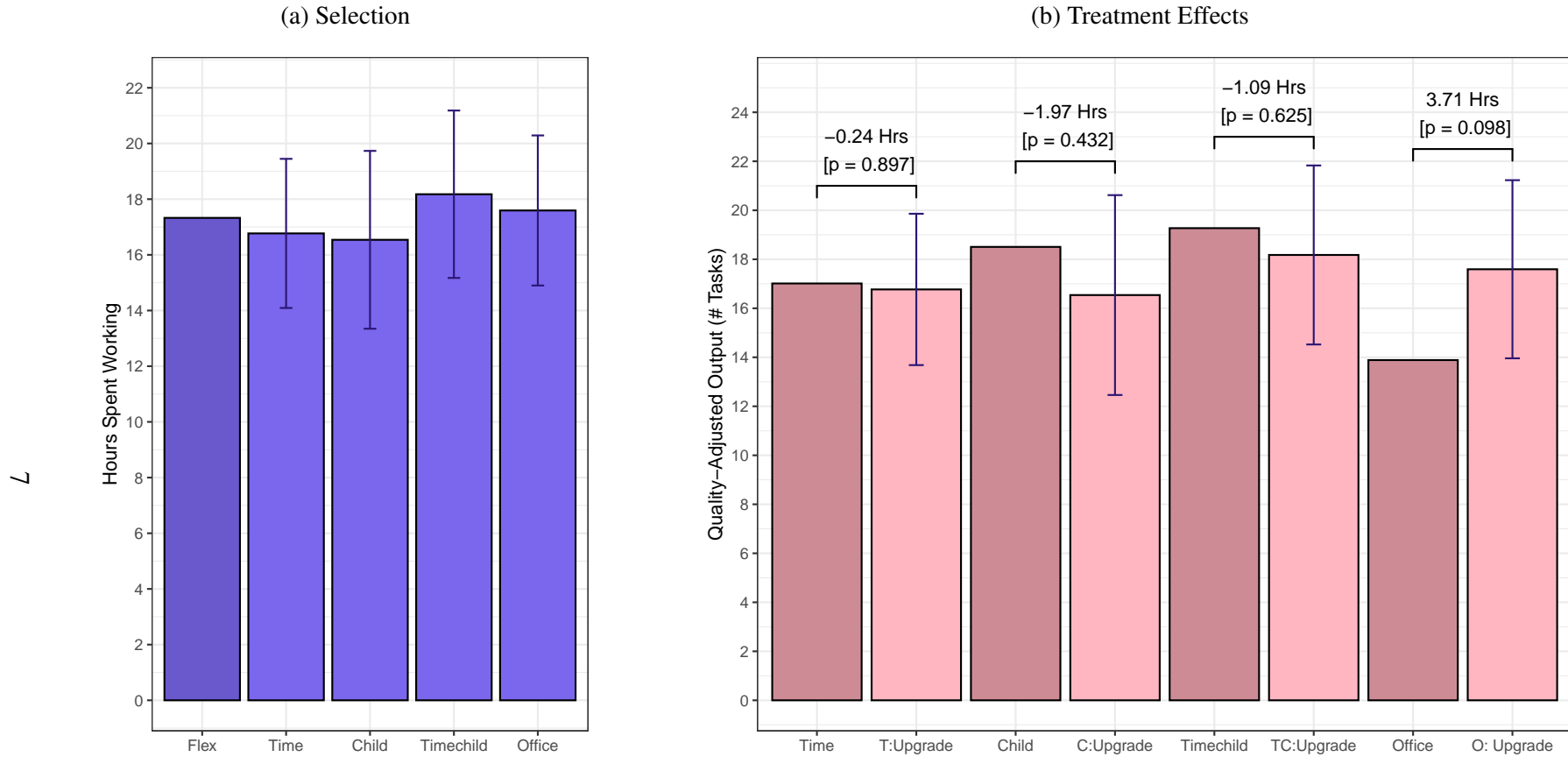
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Figure A.16: Quality-Adjusted Output (Conditional on Starting Work)



Notes: This figure reports the effects of assignment to flexible and inflexible jobs on gender attitudes (Panel A) and subsequent take up of inflexible jobs during the second round (Panel B). The effects are reported side-by-side to easily allow for comparing effects on attitudes versus job take up.

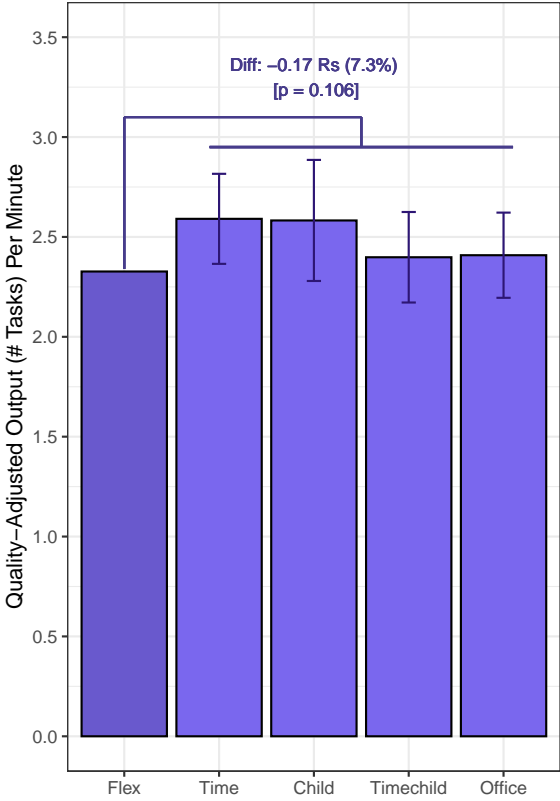
Figure A.17: Hours Spent (Conditional on Starting Work)



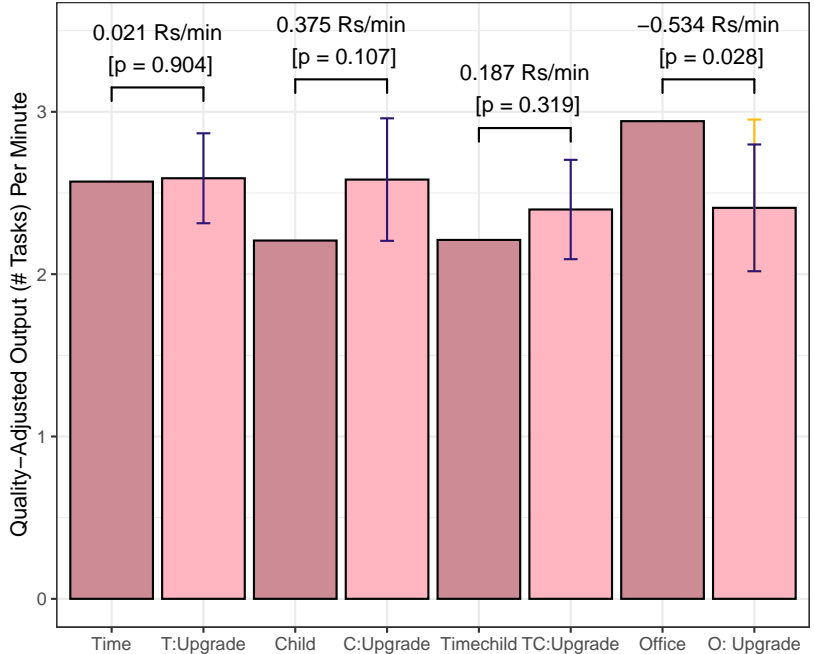
Notes: This figure reports the effects of assignment to flexible and inflexible jobs on gender attitudes (Panel A) and subsequent take up of inflexible jobs during the second round (Panel B). The effects are reported side-by-side to easily allow for comparing effects on attitudes versus job take up.

Figure A.18: Quality Adjusted Output (Earnings) Per Minute

(a) Selection



(b) Treatment Effects

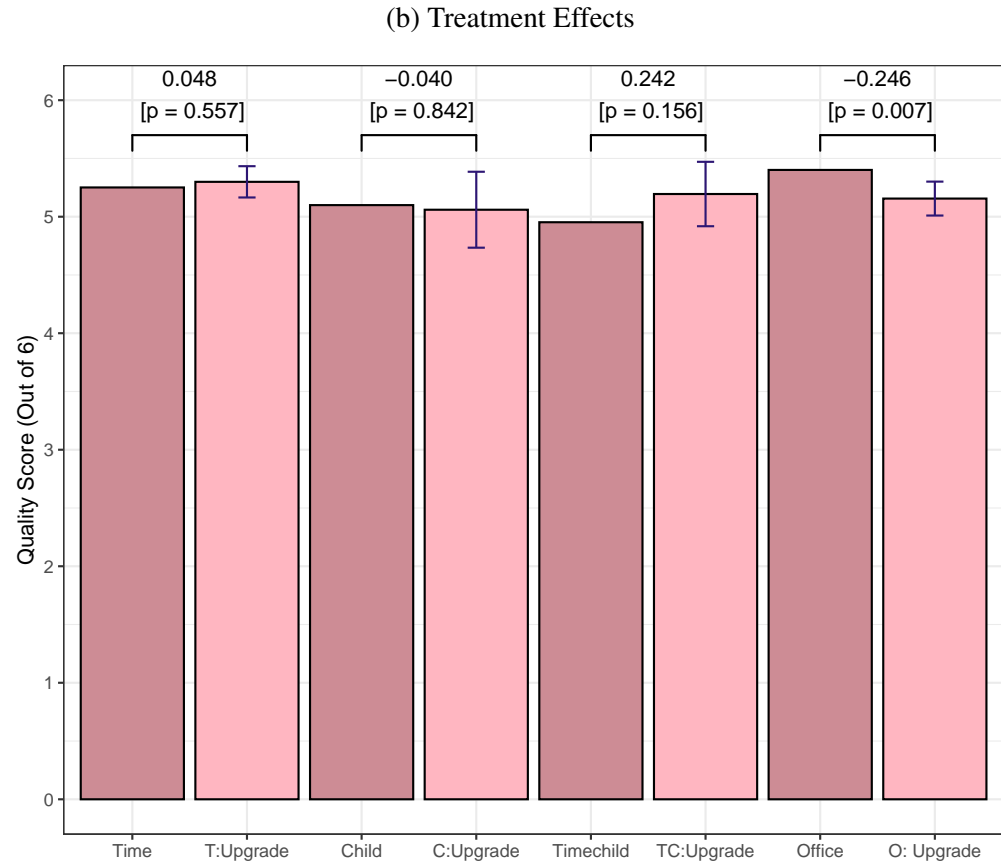
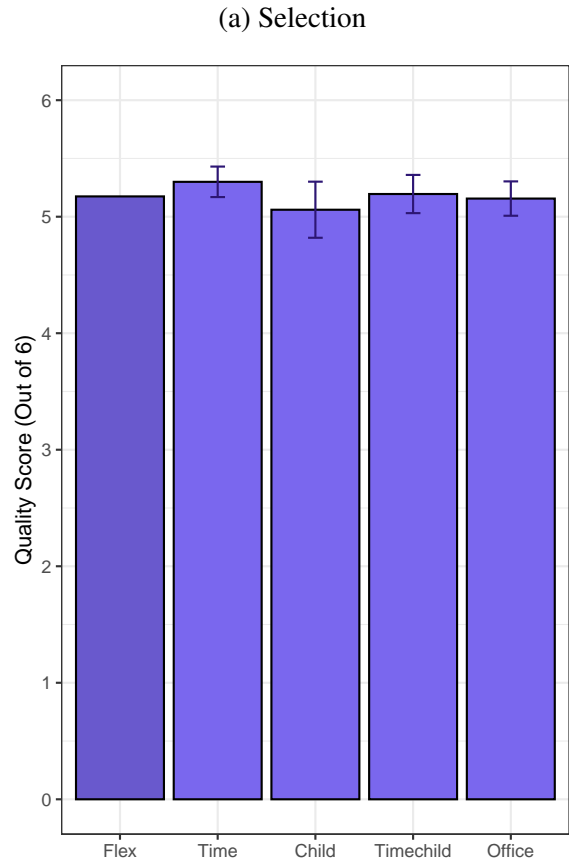


8

Notes:

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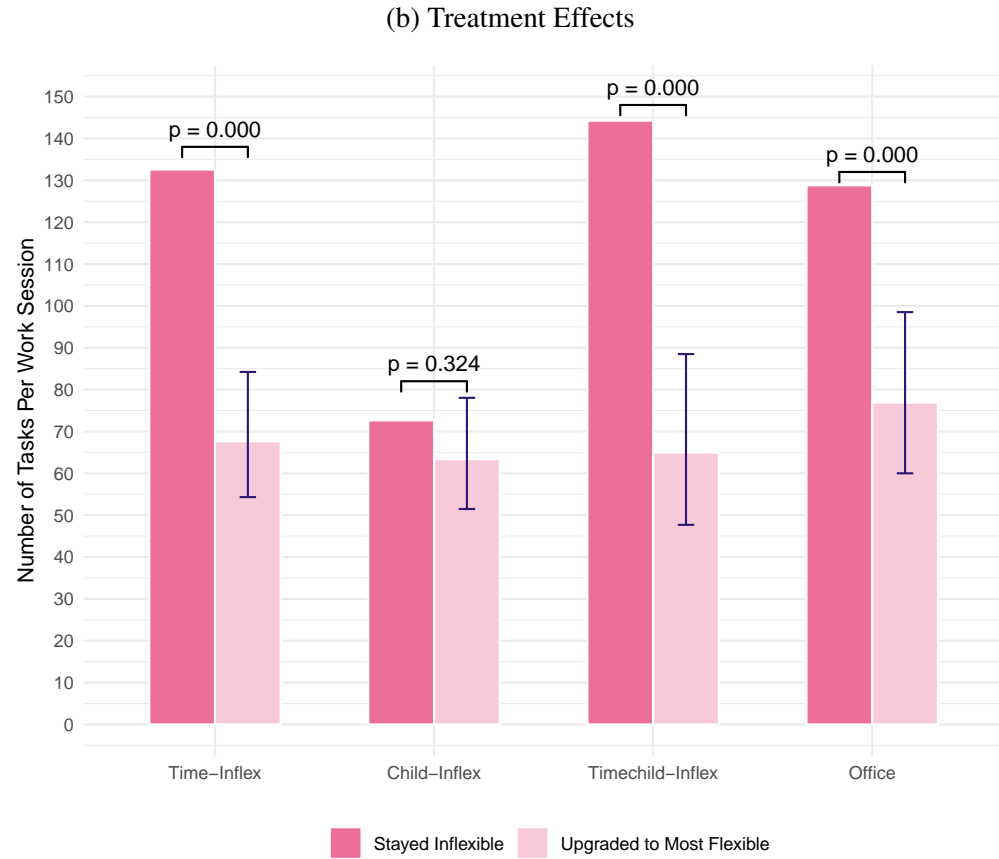
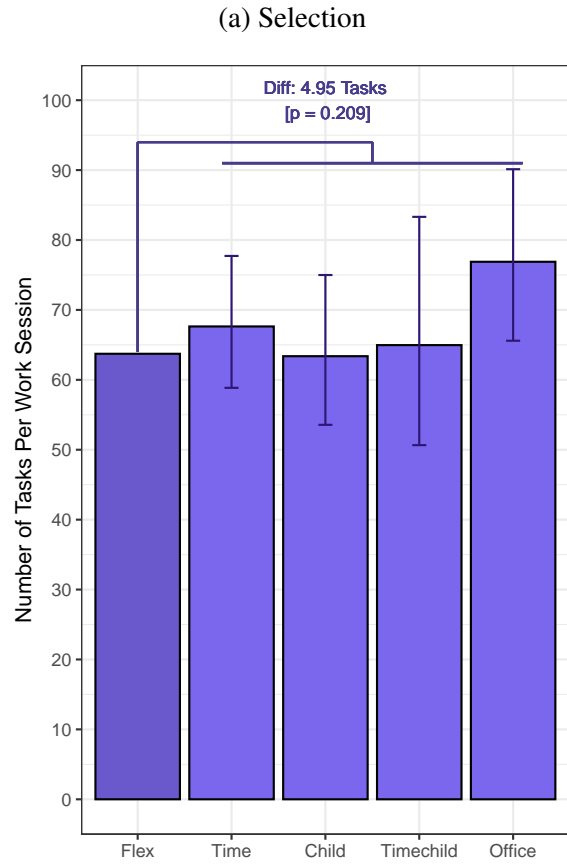
Figure A.19: Average Task Quality



Notes:

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Figure A.20: Tasks Per Worksession



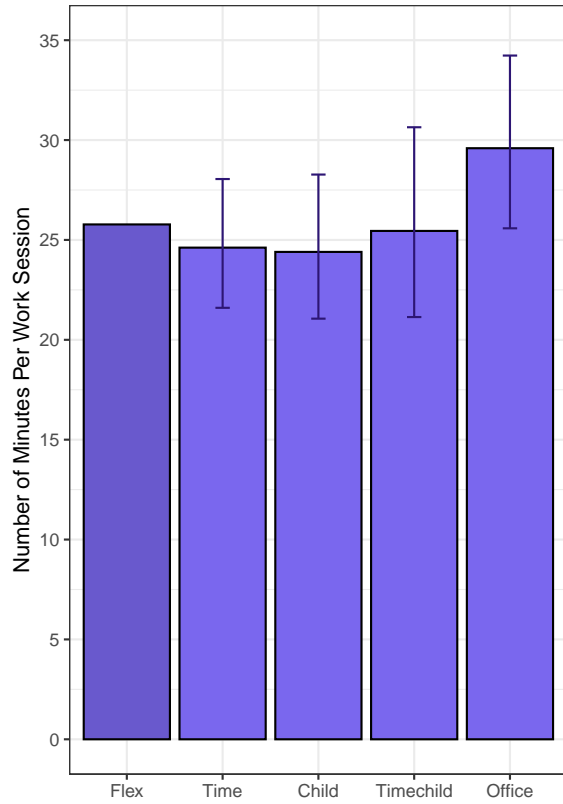
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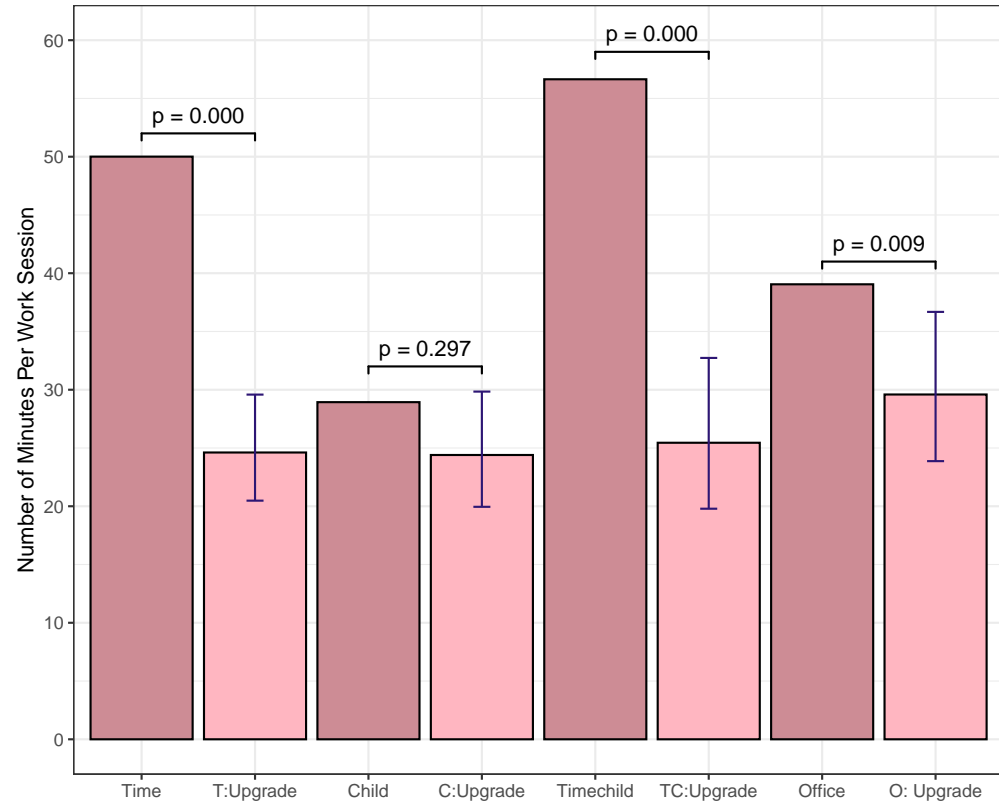
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Figure A.21: Minutes Per Worksession

(a) Selection



(b) Treatment Effects



11

Notes:

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Figure A.22

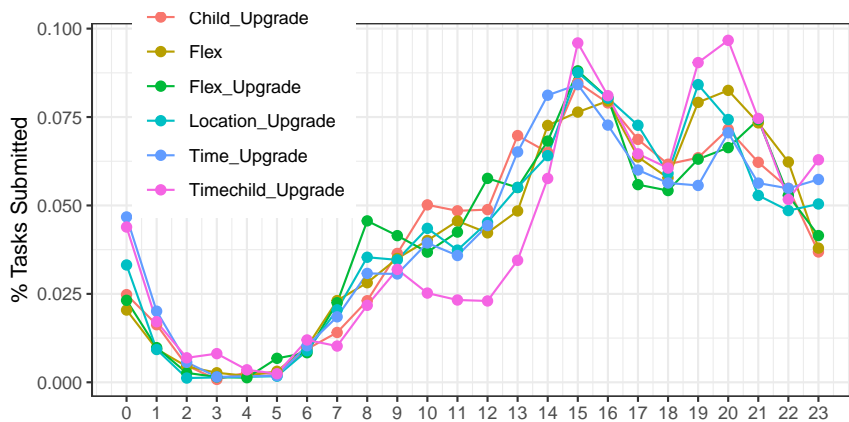
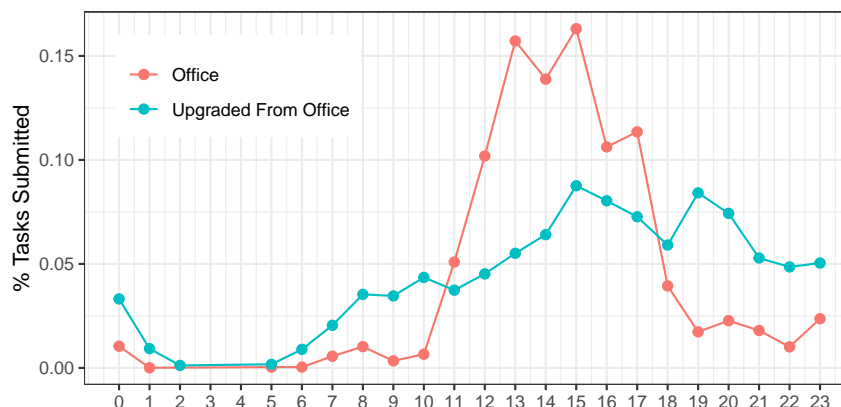
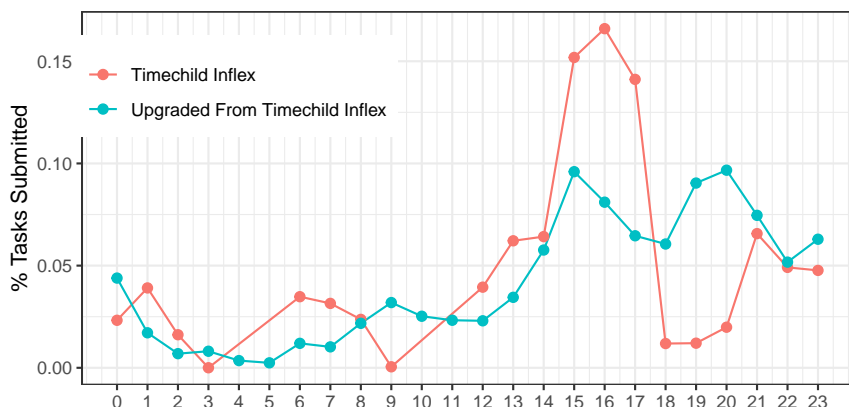
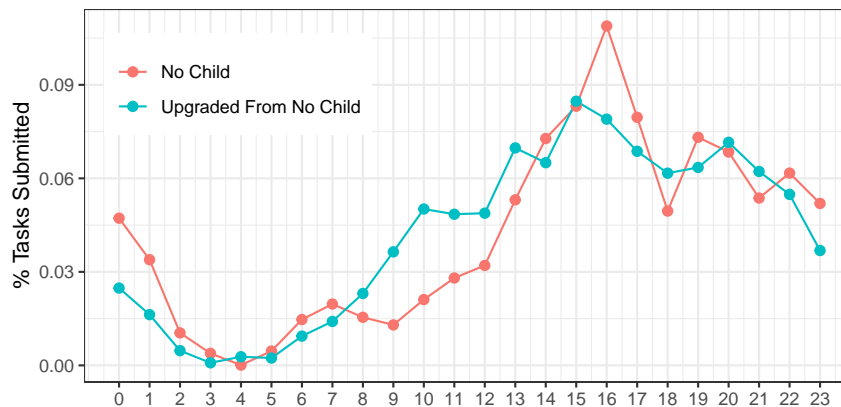
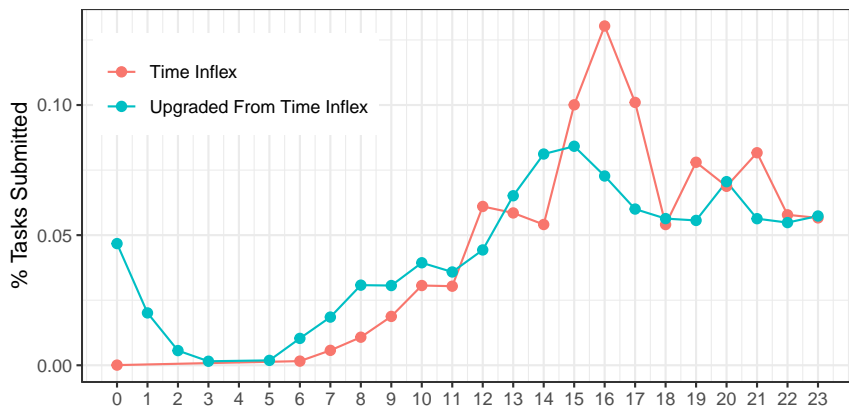


Figure A.23

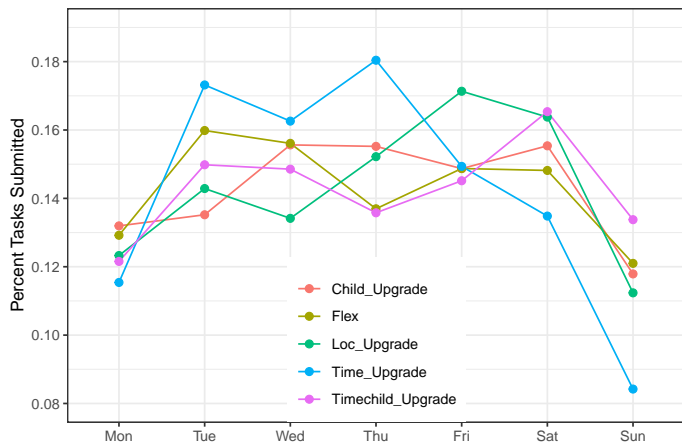
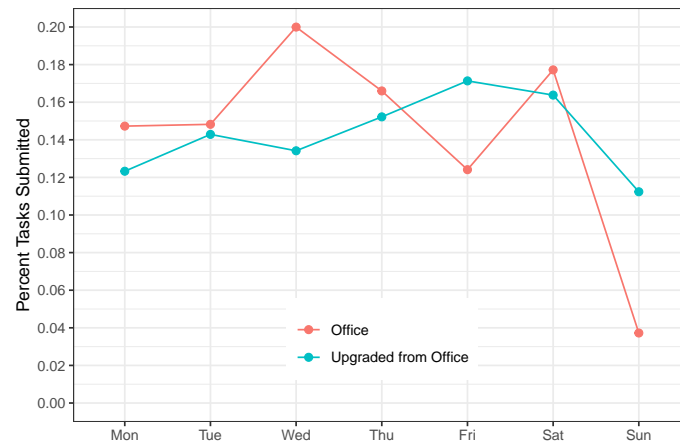
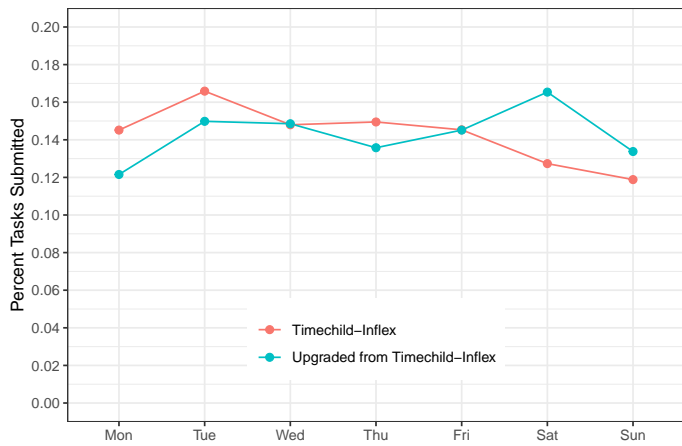
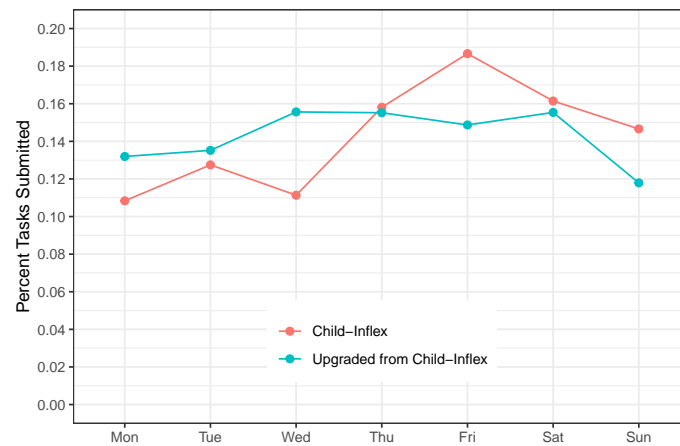
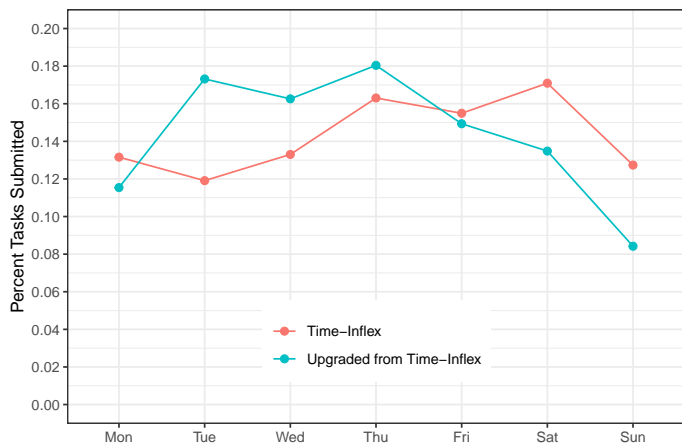


Figure A.24

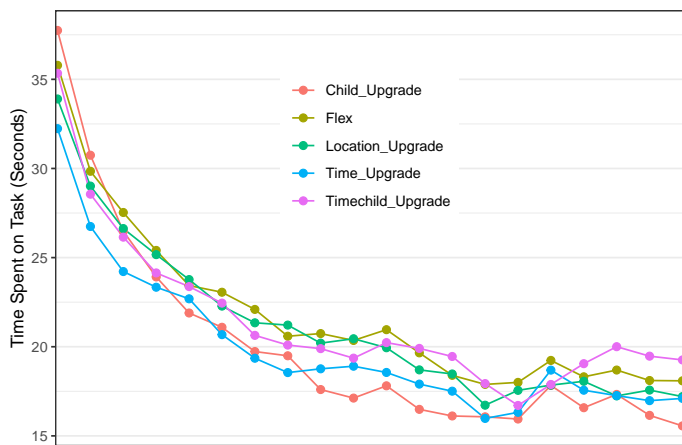
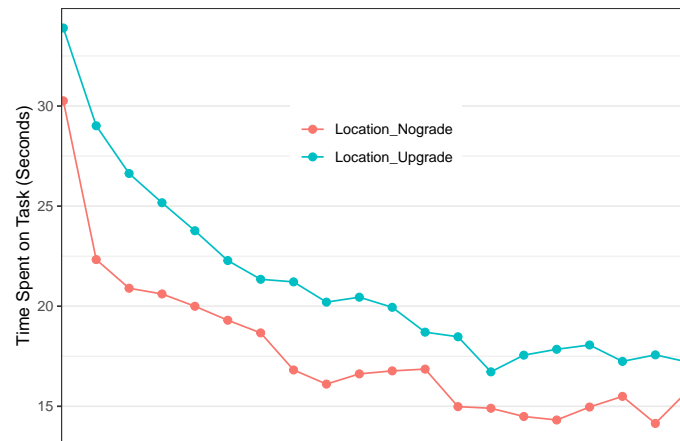
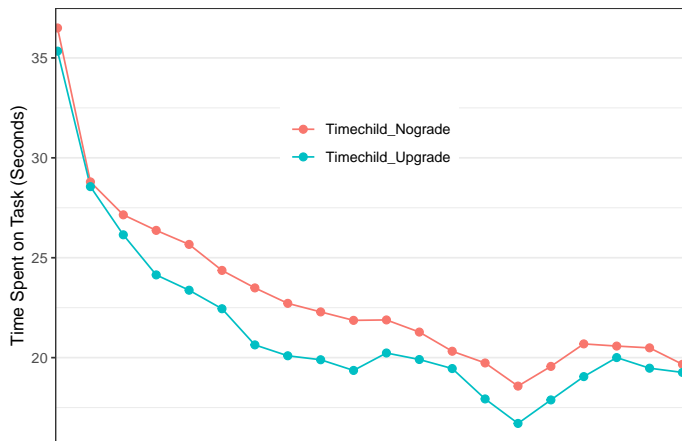
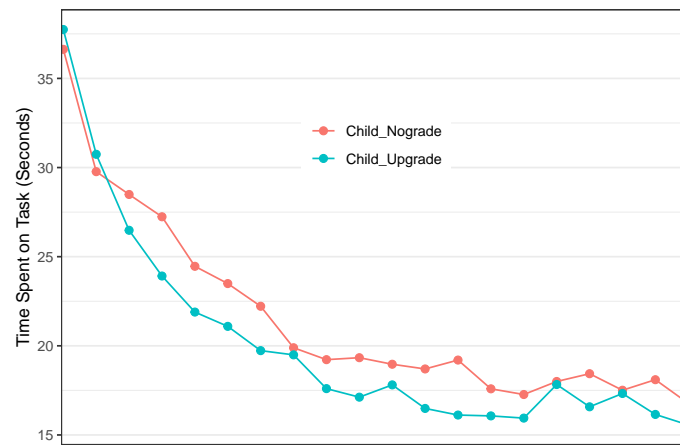
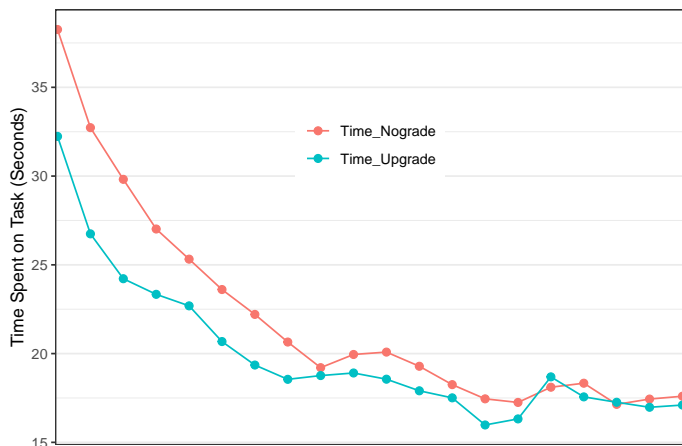


Figure A.25: “Flow” effects over the course of the worksession

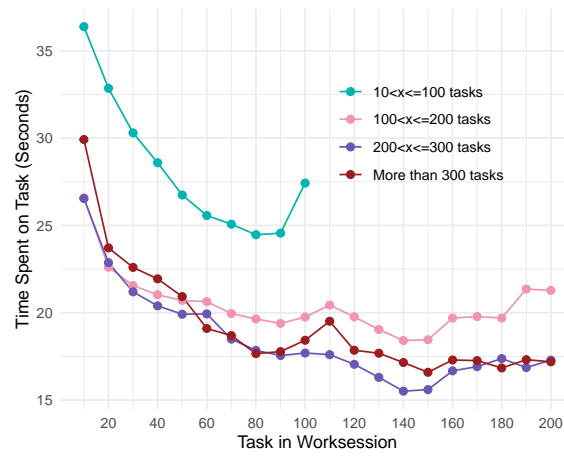
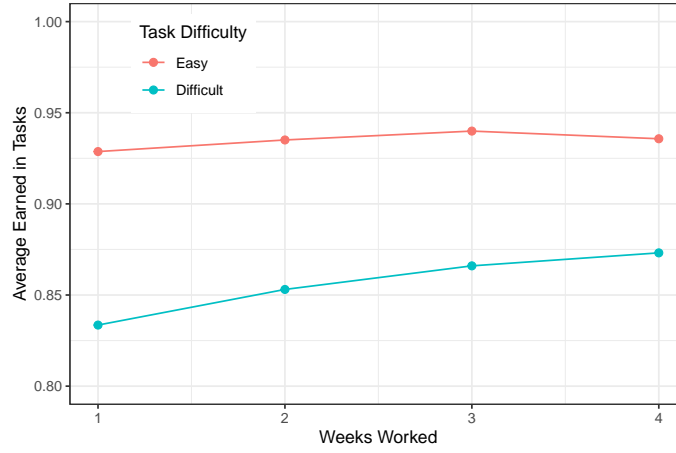
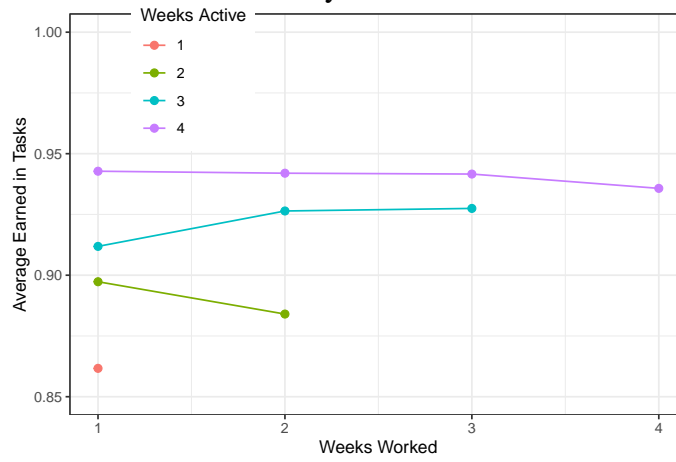


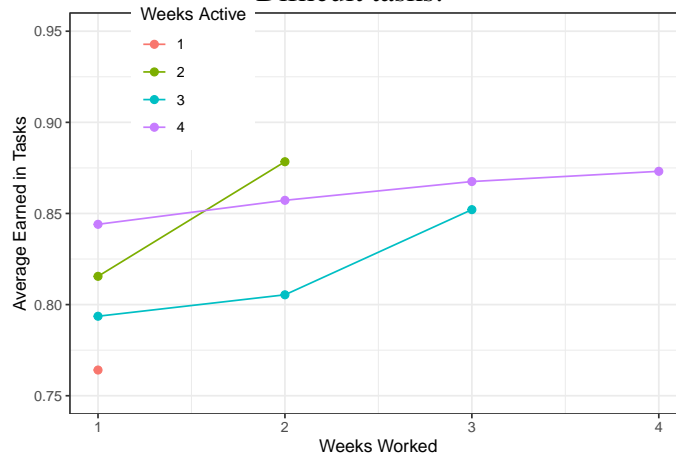
Figure A.26: Learning over weeks



Easy tasks:



Difficult tasks:



## Appendix Tables

Table A.8: 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
<i>Number of participants</i>	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.9: Effect of job attributes on take up (with controls)

Dependent Variables: Model:	Baseline (1)	Job Offer (2)	Start Work (3)
<i>Variables</i>			
Time inflexible	-0.04*** (0.01)	-0.04 (0.04)	-0.01 (0.05)
Child inflexible	-0.27*** (0.01)	-0.17*** (0.04)	-0.20*** (0.05)
Time & child inflexible	-0.32*** (0.01)	-0.13*** (0.04)	-0.17*** (0.05)
Location inflexible	-0.42*** (0.01)	-0.25*** (0.04)	-0.34*** (0.03)
<i>Fixed-effects</i>			
strata_control	Yes	Yes	Yes
<i>Fit statistics</i>			
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 take up. 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: 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*** (0.01)	0.18*** (0.04)	0.29*** (0.03)
Final Say in Job Decision = Self	0.14*** (0.02)	0.09** (0.04)	0.15*** (0.04)
Flexible Job $\times$ Final Say in Job Decision = Self	-0.12*** (0.02)	0.01 (0.06)	-0.08 (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 take-up. 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.11: Heterogeneous Impact of Flexibility on Take-Up by Having Young Child

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.22*** (0.01)	0.16*** (0.04)	0.22*** (0.04)
Has Child Under 8	-0.20*** (0.02)	-0.06* (0.03)	-0.09** (0.04)
Flexible Job $\times$ Has Child Under 8	0.19*** (0.02)	0.05 (0.06)	0.08 (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.12: Heterogeneous Impact of Flexibility on Take-Up by Educational Attainment

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.33*** (0.01)	0.19*** (0.04)	0.22*** (0.03)
>10th Standard	0.03* (0.02)	0.11*** (0.03)	0.12*** (0.04)
Flexible Job $\times$ >10th Standard	-0.03* (0.02)	-0.02 (0.06)	0.07 (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 take up. 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.13: Heterogeneous Impact of Flexibility on Take-Up by Household Income

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.33*** (0.01)	0.20*** (0.04)	0.24*** (0.04)
High Income	0.05*** (0.02)	0.05 (0.04)	0.03 (0.04)
Flexible Job $\times$ High Income	-0.03 (0.02)	-0.02 (0.06)	0.04 (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.14: 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*** (0.01)	0.22*** (0.03)	0.25*** (0.03)
Lives with In-Laws	-0.03 (0.02)	0.02 (0.04)	-0.03 (0.04)
Flexible Job $\times$ Lives with In-Laws	0.02 (0.02)	-0.11* (0.06)	0.02 (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.15: Heterogeneous Impact of Flexibility on Take-Up by Baseline Gender Attitudes (Continuous)

	Baseline (1)	Job Offer (2)	Start Work (3)
Flexible Job	0.32*** (0.01)	0.18*** (0.03)	0.25*** (0.03)
Baseline Gender Attitudes (Continuous)	0.11*** (0.02)	0.19*** (0.04)	0.22*** (0.05)
Flexible Job $\times$ Baseline Gender Attitudes (Continuous)	-0.08*** (0.02)	-0.04 (0.07)	-0.11* (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 take-up. 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.16: Effect of Job Treatments (Pooled) on Gender-Related Attitudes

Dependent Variables: Model:	All Gender Attitudes (1)	HH Roles (2)	Women & Work (3)	Technology (4)
<i>Variables</i>				
Treatment	0.10* (0.05)	0.05 (0.05)	0.08 (0.05)	0.09* (0.06)
Baseline Gender Attitudes	0.91*** (0.06)	0.79*** (0.05)	0.48*** (0.06)	0.66*** (0.06)
<i>Fixed-effects</i>				
Strata FE	Yes	Yes	Yes	Yes
<i>Fit statistics</i>				
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.17: 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*** (0.03)	0.08*** (0.03)	0.06** (0.03)	0.06** (0.03)				
arrangement = Time-Inflex	0.11** (0.05)	0.11** (0.05)	0.10** (0.04)	0.10** (0.04)				
arrangement = Child-Inflex	0.08 (0.05)	0.08* (0.05)	0.06 (0.05)	0.06 (0.05)				
arrangement = Time- & Child-Inflex	-0.07 (0.06)	-0.08 (0.06)	-0.06 (0.05)	-0.06 (0.05)				
arrangement = Office	0.05 (0.04)	0.05 (0.04)	0.03 (0.04)	0.03 (0.04)				
Started Flex					0.15*** (0.05)	0.15*** (0.05)	0.12** (0.05)	0.12** (0.05)
Started Time					0.23** (0.10)	0.23** (0.10)	0.22*** (0.08)	0.21** (0.09)
Started Child					0.26* (0.15)	0.26* (0.15)	0.19 (0.14)	0.18 (0.14)
Started TimeChild					-0.23 (0.19)	-0.25 (0.19)	-0.15 (0.17)	-0.18 (0.17)
Started Office					0.34 (0.25)	0.30 (0.25)	0.21 (0.23)	0.18 (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.18: Effect of Job Treatments (Pooled) on Gender Attitudes: Heterogeneity by Baseline Gender Attitudes

Dependent Variables: Model:	All Gender Attitudes Index (1)	HH Roles (2)	Women & Work (3)	Tech (4)	Ability (5)
Treatment	0.70* (0.37)	0.19 (0.16)	0.27* (0.14)	0.18 (0.16)	0.06 (0.09)
Less Traditional $\times$ Treatment	-0.02 (0.43)	0.03 (0.19)	-0.13 (0.15)	0.07 (0.18)	0.02 (0.10)
Baseline Gender Attitudes	0.44*** (0.04)	0.18*** (0.01)	0.09*** (0.01)	0.11*** (0.02)	0.05*** (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.19: 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** (0.02)	0.05** (0.02)	0.05** (0.02)	0.05** (0.02)				
Treatment × Baseline Gender Attitudes	-0.08 (0.07)	-0.09 (0.07)	-0.10 (0.06)	-0.12* (0.07)				
Started Work					0.14** (0.06)	0.14** (0.06)	0.15** (0.06)	0.15** (0.06)
Started Work × Baseline Gender Attitudes					-0.25 (0.18)	-0.28 (0.18)	-0.33* (0.18)	-0.36** (0.18)
Baseline Gender Attitudes	0.45*** (0.06)	0.43*** (0.06)	0.26*** (0.06)	0.26*** (0.06)	0.45*** (0.06)	0.43*** (0.06)	0.26*** (0.06)	0.26*** (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.20: Effect of Job Treatments on Women’s Agency

	Agency Index		
	(1)	(2)	(3)
<i>Panel A: Treatments Pooled</i>			
Treatment	0.04 (0.04)	0.04 (0.04)	0.02 (0.03)
Control Mean	-0.01	-0.01	-0.01
<i>Panel B: Heterogeneity by Work Arrangement</i>			
Arrangement = Flex or Time-Inflex	0.05 (0.04)	0.05 (0.04)	0.03 (0.04)
Arrangement = Child- or Child- & Time-Inflex	0.03 (0.06)	0.03 (0.06)	0.01 (0.05)
Arrangement = Office	0.00 (0.06)	0.00 (0.06)	-0.03 (0.05)
Control Mean	-0.01	-0.01	-0.01
<i>Panel C: Heterogeneity by Baseline Agency</i>			
Treatment	0.03 (0.05)	0.03 (0.05)	0.03 (0.04)
Treatment × Baseline Agency	-0.01 (0.07)	-0.01 (0.07)	-0.02 (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.21: Effect of Job Treatments on Women's Psychological Well-Being

	Psych Index (1)	Sleep Peacefully (2)	(Not) Overwhelmed (3)	Happy (4)	(Not) Worried (5)
<i>Panel A: Treatments Pooled</i>					
Treatment	0.02 (0.04)	-0.02 (0.06)	0.07 (0.06)	-0.02 (0.06)	-0.01 (0.06)
Control Mean	0.00	0.00	0.00	0.00	0.00
<i>Panel B: Heterogeneity by Work Arrangement</i>					
Arrangement = Flex or Time	0.03 (0.04)	0.03 (0.06)	0.10 (0.06)	-0.02 (0.06)	-0.01 (0.06)
Arrangement = Child or Timechild	-0.01 (0.05)	-0.12 (0.09)	0.01 (0.09)	0.04 (0.08)	-0.07 (0.09)
Arrangement = Office	-0.02 (0.06)	-0.13 (0.09)	0.02 (0.09)	-0.04 (0.09)	0.03 (0.09)
<i>Panel C: Heterogeneity by Baseline Wellness</i>					
Treatment	0.02 (0.05)	-0.06 (0.07)	0.09 (0.07)	0.03 (0.07)	-0.02 (0.08)
Treatment $\times$ >Well at Baseline	0.00 (0.06)	0.08 (0.09)	-0.04 (0.08)	-0.08 (0.08)	0.02 (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.22: Treatment Effect on Take Up of Digital vs Non-Digital Jobs

	<b>Started Work in Round 2</b>					
	R2: Flexible		R2: Inflexible		Interaction	
	Digital (1)	Non-Digital (2)	Digital (3)	Non-Digital (4)	Both (5)	Both (6)
Treatment	-0.09 (0.08)	-0.09 (0.09)	0.01 (0.06)	0.06 (0.08)	-0.09 (0.08)	0.05 (0.07)
R2 = Digital					0.02 (0.10)	-0.11 (0.08)
Treatment × R2 = Digital					0.03 (0.11)	-0.05 (0.09)
Observations	218	218	217	218	436	435
Strata Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Lasso Selected Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes:

Table A.23: Spillovers to Control Group Job Take Up

	<b>Round 2: Started Work</b>					
	R2: Any Job			R2: Flexible Jobs		
	(1)	(2)	(3)	(4)	(5)	(6)
Num Friends (Flexible Jobs)	0.05 (0.05) [0.333]			0.16** (0.07) [0.018]		
>= 1 Friend (Flexible Jobs)		0.04 (0.08) [0.598]			0.15 (0.15) [0.305]	
Fraction Friends (Flexible Jobs)			0.03 (0.08) [0.693]			0.10 (0.13) [0.439]
Num Friends (Any Group)	-0.01 (0.04) [0.879]	0.01 (0.04) [0.703]	0.02 (0.03) [0.608]	-0.07 (0.05) [0.167]	-0.01 (0.05) [0.893]	0.01 (0.04) [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 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 (·) are robust to heteroskedasticity. Brackets [·] report unadjusted *p*-values, as do stars next to coefficients (\* significant at 10%; \*\* at 5%; \*\*\* at 1%).

Table A.24: 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 (0.06)			0.20*** (0.07)		
Number Friends (Inflexible Jobs)	0.07* (0.04)			0.07 (0.04)		
>=1 Friend (Flexible Jobs)		0.04 (0.09)			0.15 (0.15)	
>=1 Friend (Inflexible Jobs)		0.00 (0.09)			0.01 (0.14)	
Fraction Friends (Flexible Jobs)			0.03 (0.08)			0.10 (0.13)
Fraction Friends (Inflexible Jobs)			0.02 (0.06)			0.01 (0.08)
Number Friends (Any Group)	-0.05 (0.04)	0.01 (0.04)	0.02 (0.04)	-0.11** (0.05)	-0.01 (0.06)	0.01 (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.25: 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 (0.11) [0.924]	0.10 (0.11) [0.355]	0.03 (0.15) [0.853]	0.09** (0.05) [0.045]
Control Mean (Endline)	1.13	1.09	2.47	0.58
<i>Heterogeneity by Child Age</i>				
Treatment	0.15 (0.14) [0.284]	0.26* (0.14) [0.064]	-0.13 (0.19) [0.494]	0.17*** (0.06) [0.005]
Treatment × < 12	-0.29 (0.23) [0.201]	-0.31 (0.23) [0.168]	0.36 (0.30) [0.237]	-0.19** (0.09) [0.037]
< 12	-0.04 (0.20) [0.826]	-0.17 (0.20) [0.388]	-0.57** (0.25) [0.026]	0.19** (0.08) [0.016]
Control Mean, > 12	1.39	1.38	2.51	0.51
<i>Heterogeneity by Child Gender</i>				
Treatment	-0.22 (0.17) [0.211]	-0.02 (0.17) [0.925]	-0.01 (0.19) [0.958]	0.15** (0.07) [0.029]
Treatment × Male	0.40* (0.22) [0.076]	0.17 (0.22) [0.439]	0.06 (0.28) [0.820]	-0.10 (0.20) [0.274]
Male	-0.48** (0.20) [0.016]	-0.49** (0.20) [0.013]	-0.15 (0.24) [0.536]	0.13 (0.08) [0.104]
Control Mean, Female	1.39	1.38	2.51	0.51
Observations	606	606	315	603
Strata Fixed Effects	×	×	×	×
Baseline Behavior	×	×	×	×

Table A.26: 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
<i>Heterogeneity by Child Age</i>		
Treatment	-0.04 (0.04)	0.01 (0.06)
Treatment $\times$ Age < 12	0.09 (0.06)	-0.08 (0.09)
Age < 12	-0.06 (0.05)	0.04 (0.08)
Control Mean, Age > 12	0.91	0.33
<i>Heterogeneity by Child Gender</i>		
Treatment	0.00 (0.04)	0.01 (0.07)
Treatment $\times$ Male	-0.02 (0.06)	-0.07 (0.09)
Male	-0.01 (0.05)	-0.02 (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 (2) is a dummy variable equal to 1 if the child says they would like to finish at least a master's degree.

Table A.27: Quality of Tasks (Selection)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
Accept Time-Inflex, Then Upgrade	0.03 (0.12)	0.00 (0.04)	0.02 (0.04)	0.02 (0.05)	0.01 (0.01)
Accept Child-Inflex, Then Upgrade	-0.10 (0.13)	-0.04 (0.05)	-0.01 (0.04)	-0.05 (0.05)	-0.01 (0.01)
Accept Timechild-Inflex, Then Upgrade	0.00 (0.09)	-0.02 (0.03)	0.02 (0.03)	0.00 (0.04)	0.00 (0.01)
Accept Office, Then Upgrade	0.02 (0.08)	0.00 (0.03)	0.03 (0.03)	-0.01 (0.03)	0.00 (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.28: Quality of Tasks (Impact)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
<i>Time-Inflexible</i>					
Upgraded to Flex	-0.05 (0.13)	-0.02 (0.05)	-0.02 (0.04)	-0.01 (0.05)	0.00 (0.01)
<i>Child-Inflexible</i>					
Upgraded to Flex	-0.03 (0.19)	0.24*** (0.09)	-0.12*** (0.05)	-0.15* (0.08)	0.05** (0.02)
<i>Timechild-Inflexible</i>					
Upgraded to Flex	0.20 (0.16)	0.39*** (0.09)	-0.12*** (0.02)	-0.07 (0.08)	0.08*** (0.02)
<i>Office</i>					
Upgraded to Flex	-0.20*** (0.08)	-0.08*** (0.02)	-0.05** (0.02)	-0.08* (0.04)	-0.03*** (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.29: 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 (0.12)	0.00 (0.04)	0.02 (0.04)	0.02 (0.05)	0.01 (0.01)
Accept Child-Inflex, Then Upgrade	-0.06 (0.12)	-0.02 (0.04)	0.00 (0.04)	-0.04 (0.05)	0.00 (0.01)
Accept Timechild-Inflex, Then Upgrade	0.04 (0.08)	0.00 (0.03)	0.03 (0.03)	0.01 (0.04)	0.00 (0.01)
Accept Office, Then Upgrade	0.02 (0.07)	0.01 (0.02)	0.03 (0.03)	-0.01 (0.03)	0.01 (0.01)
Difficult Task	-0.52*** (0.07)	-0.26*** (0.04)	-0.11*** (0.02)	-0.15*** (0.02)	-0.07*** (0.01)
Difficult Task × Accept Time-Inflex, Then Upgrade	-0.03 (0.13)	-0.01 (0.06)	0.00 (0.03)	-0.02 (0.03)	0.00 (0.02)
Difficult Task × Accept Child-Inflex, Then Upgrade	-0.13 (0.15)	-0.07 (0.07)	-0.04 (0.04)	-0.03 (0.04)	-0.02 (0.02)
Difficult Task × Accept Timechild-Inflex, Then Upgrade	-0.12 (0.12)	-0.06 (0.06)	-0.03 (0.03)	-0.03 (0.03)	-0.02 (0.02)
Difficult Task × Accept Office, Then Upgrade	0.01 (0.10)	-0.01 (0.05)	0.01 (0.03)	0.01 (0.03)	0.00 (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.30: Quality of Tasks By Task Difficulty (Impact)

	Quality Index (1)	Accuracy (2)	Volume (3)	Fluency (4)	Earned in Task (5)
<i>Time-Inflexible to Flex</i>					
arrangement = Time	0.02 (0.12)	0.01 (0.04)	0.01 (0.04)	0.00 (0.05)	0.00 (0.01)
Difficult Task	-0.55*** (0.10)	-0.27*** (0.05)	-0.11*** (0.03)	-0.17*** (0.03)	-0.07*** (0.01)
Difficult Task × arrangement = Time	0.10 (0.15)	0.04 (0.08)	0.01 (0.04)	0.05 (0.04)	0.01 (0.02)
<i>Child-Inflexible to Flex</i>					
arrangement = Child	0.04 (0.17)	-0.22*** (0.08)	0.09** (0.04)	0.17** (0.08)	-0.04* (0.02)
Difficult Task	-0.65*** (0.13)	-0.32*** (0.06)	-0.15*** (0.03)	-0.18*** (0.04)	-0.08*** (0.02)
Difficult Task × arrangement = Child	0.00 (0.17)	-0.07 (0.09)	0.11*** (0.04)	-0.04 (0.06)	-0.01 (0.02)
<i>Timechild-Inflexible to Flex</i>					
arrangement = Timechild	-0.19 (0.15)	-0.36*** (0.09)	0.09*** (0.02)	0.09 (0.07)	-0.08*** (0.02)
Difficult Task	-0.64*** (0.10)	-0.32*** (0.05)	-0.14*** (0.03)	-0.18*** (0.03)	-0.08*** (0.01)
Difficult Task × arrangement = Timechild	-0.04 (0.13)	-0.10 (0.09)	0.12*** (0.03)	-0.06 (0.05)	-0.02 (0.02)
<i>Office to Flex</i>					
arrangement = Loc	0.10 (0.08)	0.03 (0.02)	0.02 (0.02)	0.06 (0.04)	0.01 (0.01)
Difficult Task	-0.51*** (0.07)	-0.27*** (0.04)	-0.10*** (0.02)	-0.14*** (0.02)	-0.07*** (0.01)
Difficult Task × arrangement = Loc	0.33*** (0.10)	0.19*** (0.05)	0.08*** (0.02)	0.06* (0.03)	0.05*** (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.24.

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.

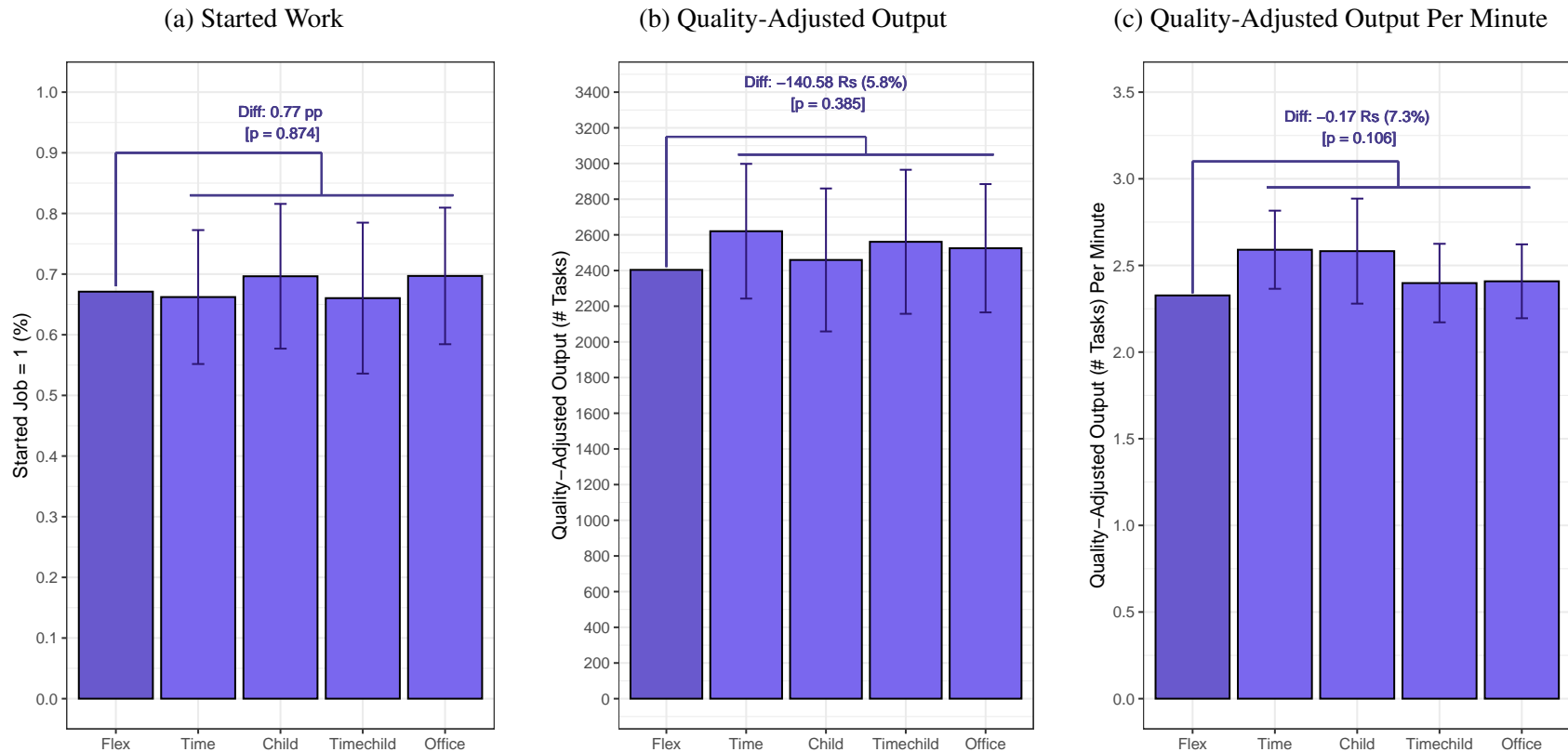
*Estimation.* We test for the presence of spillovers from the treatment group to the control group using the following regression:

$$\text{TakeUp}_{is} = \gamma_s + \gamma_1 \text{JobExposure}_{is} + \gamma_2 \text{NumFriends}_{is} + \gamma_3 \theta_{is} + \varepsilon_{is} \quad (\text{A.9})$$

where  $\text{TakeUp}_{is}$  is a dummy variable equal to one if participant  $i$  started her job in Round 2;  $\text{NumFriends}_i$  is the total number of friends that  $i$  has in the study;  $\text{JobExposure}_i$  is one of the three measures of exposure to the treatment group described above; and  $\theta_{is}$  is a vector of control variables selected using a double LASSO. The coefficient of interest is  $\gamma_1$ , 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.23 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.24). There are no effects of having friend exposure to the inflexible jobs on take up of flexible jobs in Round 2.

Figure A.27: Effects of Flexibility on Worker Composition



Notes: This figure shows how flexible work arrangements affect worker composition in terms of job performance.

- Three job performance measures are included: In panel A, the likelihood that the worker starts the job after accepting it; in Panel B, the total quality-adjusted output produced by the worker during the month-long job; and in Panel C, a measure of work efficiency that is quality-adjusted output divided by minutes spent actively working. Minutes spent working are measured according to number of unique minutes during which the worker took an action on the job app.
- All workers in these figures worked in the most flexible arrangement. The performance measures are plotted separately for workers initially offered the most flexible job (Flex), and the workers who were initially offered and accepted a less flexible job (Time for time-inflexible, Child for child-inflexible, Timechild for time- and child-inflexible, and Office for office-based) and then were randomly selected for an upgrade to the most flexible job.
- The brackets report the magnitude and  $p$ -value for the difference between performance of the Flex group and performance of all the less flexible groups pooled together. Whiskers indicate 90% confidence intervals.



Figure A.28: Effect of flexible work arrangements, including “no shows”

