Habit Formation in Labor Supply*

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Abstract

Among low income workers, labor supply is often irregular: frequent shocks disrupt work spells, absenteeism is high, and many workers prefer flexible casual work to formal jobs. We examine the possibility that labor supply is habit forming—so that past labor supply levels affect preferences for future supply. We undertake a field experiment with casual urban laborers in Chennai, India. We randomly provide some workers with small financial incentives for attendance over 7 weeks, leading to a 26% increase in labor supply. We test for habit formation by examining subsequent impacts *after* the incentives are removed. First, we see a persistent 18% increase in labor supply over the next 2 months, resulting in a 10% increase in employment. Second, treated workers exhibit a higher willingness to accept work contracts that are of longer duration and less flexible. They also self-report an increase in automaticity—suggesting a change in preferences. Third, shocks that temporarily pull workers out of the labor market lead subsequent treatment effects to collapse to zero; in the absence of these shocks, we cannot reject that there is no decay in effects over time. Fourth, in incentivized measures, employers accurately predict treatment effects, and prefer hiring workers who have been treated with a stronger habit stock in the past. Finally, in supplementary data from other settings, we replicate short-run persistent effects of transitory labor supply shocks—indicating the broader generalizability of state dependence in labor supply. Together, our results suggest that the intermittent nature of employment and frequent shocks experienced in low-income settings may inhibit workers from becoming habituated to regular work—with potential implications for the transition to formal regular work in poor countries.

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1 Introduction

A large body of work in both psychology and economics postulates the importance of habits for determining human behavior (James, 1890; Mazar and Wood, 2018; Waller Jr, 1988; Charness and Gneezy, 2009; Wood and Rünger, 2016).[1] Under this view, actions are self-reinforcing, so that undertaking an activity increases one's desire to undertake it in the future. In this paper, we empirically examine the possibility of habit formation for a core economic behavior: the supply of labor.

This possibility has potentially broad implications for labor market phenomena in both poor and rich countries. For example, it could help explain hysteresis in labor markets—the long-standing observation that transitory shocks to employment lead to persistent changes in employment levels (e.g. Blanchard and Summers, 1986; Kroft et al., 2016). Relatedly, it could exacerbate the extent of duration dependence or the "unemployment scar" (Vendrik, 1993).[2] More generally, a sizable body of work in labor and macroeconomics has examined the potential relevance of state dependence in labor supply for explaining the functioning of the labor market (e.g. Hyslop, 1999; Card et al., 2007).

Habit formation may have particularly important implications in low-income settings, where workers experience frequent shocks—for example, due to unpredictable shift schedules, higher reliance on casual informal work, seasonality in labor demand, and heightened absenteeism due to illness, childcare gaps, and family and social obligations (e.g. Collins et al., 2009; Morduch and Schneider, 2017; Choper et al., 2022). These external constraints may not only make it difficult for low-income workers to regularly supply labor; under habit formation, they may also impact workers' underlying preferences for supplying labor regularly. In testing for habit formation, we examine the potential mediating role of these external shocks.

We conduct our test among casual urban workers in Chennai, India. On days they would like to work, workers show up to labor stands—centralized public spaces where

[1] The psychologist William James is famously quoted as saying "Ninety-nine hundredths or, possibly, nine hundred and ninety-nine thousandths of our activity is purely automatic and habitual, from our rising in the morning to our lying down each night." (James, 1983)

[2] This is the observation that workers who have experienced a spell of unemployment are less likely to be hired for work in the future. Existing work establishes selection reasons for this phenomenon (Kroft et al., 2013). As we discuss using results from surveys with employers below, an erosion of labor supply, for example on the intensive margin of effort provision or attendance at work, could be another and not mutually exclusive contributing factor.

workers gather in the mornings to await potential employers. Such labor stands are ubiquitous in developing countries, providing employment for hundreds of millions of workers. The modal work contract lasts one day, so workers essentially choose their labor supply daily. Consequently, this setting offers two key advantages for studying habit formation: a high degree of discretion in labor supply levels, and a revealed preference measure of labor supply—attendance at the labor stand.

We design a field experiment with 225 workers across 11 labor stands (i.e. local labor markets) in Chennai, India. The average worker in our sample has over 10 years of experience at his given labor stand.[3] Workers' mean attendance at the labor stand is 2.3 days per week, with an employment rate of about 2.9 days per week (across all employment sources).[4] Despite having irregular employment, 60% of workers state they prefer casual work to a stable formal job because they prefer having free time or flexibility during the workweek.

To test for habit formation, we use temporary financial incentives to increase labor supply for some workers, and then examine persistence of impacts *after* the incentives have been removed.[5] We randomize workers at the individual level, treating less than 10% of workers at any given labor stand to avoid general equilibrium effects. We directly measure labor supply by stationing a staff member at each labor stand to visually confirm attendance. We supplement this with regular surveys to measure employment (whether found at the stand or elsewhere), beliefs, etc.

The initial incentive phase ("Phase 1") lasts for 7 weeks. During this phase, treated workers are offered Rs. 50 ($\sim$ 12% of average daily earnings) for each day they arrive at the stand by 8 am.[6] Control group workers receive unconditional payments of the same magnitude: in each week, each control worker is matched with a randomly chosen treatment worker and is paid the incentive earned by that treatment worker. During Phase 1, treated workers increase their overall attendance at the stand by 0.777 days per week (26%) relative to control workers ($p<0.001$). In addition, they arrive before 8

[3]Due to cultural reasons, the vast majority of workers who come to the labor stands where we work are male.

[4]Workers find employment at the stand on 70-80% of the days where they arrive by 8 am. They may also find employment by directly receiving a call from an employer or contractor, but their primary means of actively searching for work is via stand attendance.

[5]This follows the design approach in much of the previous behavioral economics literature on habit formation (e.g. Charness and Gneezy, 2009).

[6]The job finding probability is 77% for workers who arrive before 8 am, and declines substantively with arrival time after 8 am.

am on 1.761 more days per week (126%, $p<0.001$).

In Phase 2, we remove the incentives for labor supply and corresponding unconditional payments to control participants. We continue to track workers daily for 8 weeks. Under standard models of labor supply, when incentives are no longer in place, treated workers should either return to baseline levels of labor supply, or decrease labor supply (under the common presumption of a negative inter-temporal supply elasticity). In contrast, if labor supply is habit forming and strong enough to overcome these negative effects, then we would observe hysteresis: an elevated level of labor supply in Phase 2.

Consistent with this prediction, we see a remarkable degree of persistence over the 8 weeks in Phase 2. Relative to control workers, treated workers come to the stand 0.466 more days per week overall (18%, $p = 0.019$). In addition, they come to the stand by 8 am on more days per week than control workers (37%, $p = 0.009$). Along both these measures, the labor supply of treated workers first order stochastically dominates that of control workers in Phase 2 ($p \leq 0.005$). This heightened labor supply is consequential for employment levels: treated workers hold a job on an additional 0.317 days per week, corresponding to a 10.4% increase in their employment rate ($p = 0.036$).

We continue to track workers less intensively for an additional 2-4 months (i.e. 3-5 months after incentives have ended), which we refer to as "Phase 3". During this period, we visit stands on only 1-2 days per week and track stand attendance only. In this longer follow-up period, we see an estimated 15% treatment effect on labor supply. However, these effects are not significant—due to some combination of decay in effects over time and increased noise from the less frequent observations in Phase 3.

To understand decay in effects over time, we examine the role of shocks. In our setting, workers frequently face disruptions to stand attendance. This includes correlated shocks across workers within a stand—such as festivals, weddings, funerals and other social events which are often held on weekdays. We test whether these shocks erode workers' habit stock. To avoid potential endogeneity problems associated with self-reported shocks, we construct a data-driven measure of shocks using stand attendance. For each participant, we define shocks as occurring in weeks where the attendance of all *other* workers at the labor stand is below the 25th percentile (after residualizing with stand fixed effects and workers' baseline attendance). Because stands are rolled out in a staggered fashion over the course of a year, there is considerable variation across stands in whether there is a shock, and in which week in Phase 2 it occurs. We then use an

event study design to examine what happens to Phase 2 treatment effects in the weeks after such a shock occurs.

We find that shocks play a strong role in mediating the persistence of habit formation effeccts in Phase 2. After the week in which workers at a stand are temporarily pulled out of the labor market, treatment effects immediately collapse to zero: the attendance levels of treatment vs. control workers are indistinguishable in the next week, and all the subsequent weeks. In contrast, we see no evidence for a decay in effects over time in the absence of these shocks. These shocks have little impact on the control group's attendance, which rebounds after the transitory shock is over—indicating that there is nothing persistent about the shocks themselves; they simply erode the habit stock built up by the treatment group.

These patterns provide empirical insight into how to conceptualize the equilibrium under habit formation. Given the instantaneous collapse of treatment effects, our findings do not follow the gradual decline one would expect under traditional "addiction" models (Becker and Murphy, 1988).[7] Rather, they match work from psychology on automaticity and habit discontinuity (e.g. Wood et al., 2005; Wood and Rünger, 2016). This work predicts instantaneous abandonment of new habits in response to an environmental change or temporary removal of cues.

Further consistent with this view, treated workers report increased automaticity around stand attendance in Phase 2. For example, they are more likely to agree with the statement, "Going to the labor stand is something I do without thinking"—suggesting a change in the psychological default associated with attendance. Moreover, in stands where workers were asked this question after the stand had experienced a negative attendance shock, we see no difference in self-reported automaticity between treatment and control workers. While only suggestive, these patterns strengthen our interpretation of the relevance of a discrete change in one's psychological default for sustaining a habit.

In contrast, persistence in labor supply does not seem to be driven by external factors such as fixed cost changes to individuals' morning routines or sleep patterns; using detailed time use data, we find that treated participants undertake similar activities as

[7]Like prior work on habit formation, we find a gradual decay in average effects over time across our sample. However, these aggregate patterns can be consistent with gradual decay across all individuals, or instantaneous full reversion to old behaviors among different individuals occuring at different times.

control participants. In addition, we rule out that our results are driven by learning—which could affect the expected returns of attending the stand—or through employers changing their likelihood of hiring treated workers. For example, we find no change in workers' perceived job finding probability. This is unsurprising, given that average tenure of participants at their labor stand is 10 years, so that workers are quite accurate at baseline in their understanding of how labor stands function. Finally, we use supplementary tests to rule out displacement or general equilibrium effects at stands, or habit formation in consumption rather than labor supply.

To examine whether our impacts reflect a shift in underlying preferences around labor supply, we test for changes in preferences for flexible work arrangements during Phase 2. We find that treated workers are substantially more willing to take on more rigid work contracts. Specifically, they are 17.4 p.p. more likely to choose a six-day contract job at the prevailing wage with wage penalties for missed days, relative to the status quo of casual daily work (p=0.035). In addition, when offered an incentivized choice between a less flexible contract with more restrictive attendance requirements but higher pay, versus a more flexible contract with lower pay, treated participants are 10.6 p.p. (20.4%) more likely to select the less flexible contract (p=0.082). Moreover, as before, we find no difference between treated and control participants if they are offered this choice after their labor stand has experienced a negative attendance shock—tracking the labor supply and automaticity results above. While only suggestive, these findings open the door to the idea that habit formation may have relevance for understanding the lack of labor supply to the formal sector in developing countries.

Finally, to assess the generalizability of habit formation in labor supply, we test for persistence after exogenous labor supply shocks in other datasets. We exploit randomized variation in incentives to increase labor supply among data entry workers in India in Kaur et al. (2015), and among cashew processing factory workers in Cote D'Ivoire in Carranza et al. (2024). In both cases, we document evidence that short-term exogenous transitory increases in labor supply lead to persistently higher labor supply in future periods—with quick decay. This indicates the possibility that the inter-temporal labor supply elasticity could actually be positive (rather than negative) in a variety of settings.

To understand implications for the demand side, we carry out a series of exercises with 167 employers who hire from the labor stands in our setting. We document that

employers have remarkably accurate beliefs about habit formation. Specifically, in incentivized surveys, most employers accurately predict the magnitude of persistence in our treatment effects on labor supply in Phase 2, as well the magnitude of decay. In addition, in a real stakes hiring decision, we elicit employers' willingness to pay to hire a treated vs. control worker from our experiment, where the employer receives a wage subsidy for hiring their chosen worker only if that worker shows up to the labor stand on the workday. We find that 79.7% of surveyed employers are willing to pay 11-22% of the daily wage for a chance at hiring a worker that has been treated with greater habit stock. Moreover, employers state that they would be more likely to offer more longer-term work contracts if they could more readily find workers who were willing to reliably supply labor for such contracts. While speculative, this suggest the potential relevance of habit formation for understanding not only the supply, but also the demand side of the labor market. Building off this idea, we conclude the paper with a more speculative discussion of the potential relevance of habit formation for understanding the way in which the structure of the labor market changes with the process of development.

A long-standing body of work in economics has discussed the relevance of habit formation for economic behavior (Becker and Murphy, 1988; Laibson, 2001; Rabin, 2011). A growing set of studies has empirically tested for habit formation, in contexts such as gym attendance (Charness and Gneezy, 2009; Acland et al., 2015; Royer et al., 2015), hand washing (Hussam et al., 2022; Steiny Wellsjo, 2022), water usage during showers (Byrne et al., 2022), blood donations (Bruhin et al., 2015), and social media usage (Allcott et al., 2020). We build on and complement this prior body of work by documenting the presence of habit formation within the context of a core high-stakes economic behavior: labor supply and full-time earnings. In addition, through detailed auxiliary data collection, we are able to shed light on mechanisms—offering positive evidence for a change in underlying preferences, finding support for psychological underpinnings that help sustain preference change, and ruling out alternative explanations such as learning or fixed cost investments (e.g. in time use). Moreover, we provide direct evidence for the role of shocks in leading to a decay in the persistence of habit stock. This has relevance for both informing the right way to model habit formation, as well as for policy design.

Our study also contributes to the literature on the causes for unemployment and underemployment in developing countries. A growing body of work documents that many workers prefer irregular work over stable factory jobs (Blattman and Dercon, 2018;

Donald and Grosset, 2024), and exhibit large levels of absenteeism in both informal traditional sectors such as farm labor (Cefala et al., 2024) as well as in formal factories (Adhvaryu et al., 2024). We document that habituation to regular labor supply changes workers' preferences for inflexible work. We also find that external constraints and shocks faced by workers may inhibit their ability to build up habit stock for regular work. This view accords with historical theories on the transition of workers to formal factory work during the Industrial Revolution (Pollard, 1963; Thompson, 1967; Clark, 1994). While many factors are likely to play a role in determining whether workers shift to more formal, less flexible work arrangements, our findings suggest that habituation to regular work may be one such potential factor.

Finally, inter-temporal complementarities in labor supply are also relevant for understanding labor market dynamics. Our findings suggest a potential supply-side explanation for unemployment hysteresis (Blanchard and Summers, 1986). This includes the observed correlation between joblessness duration and the transition to non-participation (Krueger et al., 2014), which are particularly important after recessions (Katz, 2010). Given our results that employers believe in the habit-forming effects of prior labor market experience, our study also has relevance for the debate on the reasons for duration dependence: the observation that employers are less likely to hire workers if they are in an unemployment spell (Kroft et al., 2013). Additionally, intertemporal complementarities in labor supply could also explain why many papers measure low Frisch elasticity of labor substitution (Martinez et al., 2021): habit formation in labor supply could reduce workers' sensitivity to temporary changes in wages. These implications are of course speculative. However, the strength of our findings provides impetus for future work examining the links between habit formation in labor supply and these empirical phenomena in labor markets.

The remainder of the paper proceeds as follows. Section 2 describes the empirical context of our study as well as the sampling frame. Section 3 lays out the experimental design and Section 4 outlines the data. Section 5 describes the results and possible underlying mechanisms. Section 6 discusses implications for the labor market and Section 7 concludes.

2 Context

Markets for casual daily labor employ a large fraction of the world's poor and are extremely active in both urban and rural areas of LMICs (ILO, 2018). Our experiment takes place at labor stands — public spaces where casual workers gather to search for employment — across Chennai, India. Employment at labor stands is typically short-term, with the modal contract being one day and very few contracts longer than one week. Workers at the labor stands in our study predominantly engage in construction — a sector that employs 54.3 millions of laborers in India, equivalent to 21% of its non-agricultural labor force (Mehrotra and Parida, 2019).[8] The stands typically operate only in the morning, allowing for a full day of work.

Labor supply among workers at labor stands is often sporadic, consistent with workers' reported preferences for flexible jobs. At baseline, only 33% of workers in our study state that they are likely to accept a long-term, formal job if offered one (Figure 2a), citing a preference for flexibility of daily jobs and an appreciation of free time among main reasons they engage with casual jobs found at labor stands (Figure 2b).

Importantly, labor stands provide an excellent opportunity to measure labor supply cleanly. Workers decide every day whether to attend the labor stand to search for work — effectively revealing that they are willing to supply labor for that day — without any of the typical challenges of separating supply from demand when only work itself is observed. In addition, attendance is directly observable, providing a well-measured outcome that does not rely on self-reports.

3 Experiment: Design and Implementation

Experimental design. To test the hypothesis that workers' preferences over regular labor supply are malleable, we build on the typical design used in the habit formation literature (e.g., Charness and Gneezy, 2009; Hussam et al., 2022): temporarily incentivizing the desired behavior — timely labor supply — and then measuring the persistence *after incentives are removed*.

Treated participants receive a financial incentive for every day they attend the stand

[8] While the stands used in this study cater to construction work, the stands are a broader feature of many labor markets with professions ranging from agriculture to loading and unloading vehicles.

before a pre-specified cut-off time.[9] These incentives are offered for seven weeks, a period referred to as Phase 1. The payment for verified timely arrival at the stand is 50 Indian Rupees ($\sim$ \$0.60) per day, or roughly 12% of average daily earnings for a typical worker.[10]

Control participants receive unconditional payments that do not depend on their attendance in Phase 1. These payments are designed to avoid differential wealth effects by randomly matching a Control participant to a Treated participant each week and paying the Control participant an amount equal to the incentive payment earned by his matched counterpart. To avoid any attempt of collusion, Control participants are told that their weekly compensation is determined by a lottery.

Payments to both groups are disbursed on Saturday evenings to avoid altering incentives to attend the stand.

Experimental timeline. The study was implemented with a rolling enrollment in 11 labor stands from the spring of 2022 to the spring of 2023. We operate in each stand for approximately six to eight months, with each stand cycling through six periods: stand selection, initial screening, baseline, Phase 1 (incentives), Phase 2 (measurement of persistence), and Phase 3 (observation only follow-up).

Stand selection. Stands are first selected for broad suitability for the study. We enroll from stands with a daily population of at least 100 workers, and exclude stands with a high prevalence of non-construction jobs (to ensure an adequate population of interest), a high proportion of migrant laborers (due to language barriers), or a geographic layout that precludes good visual observation of the entire stand.

Initial screening: After a stand is selected, enumerators approach workers to conduct a short survey, allowing us to screen workers for eligibility. Workers are screened out based on the following criteria: workers (i) for whom an increase in work induced by the study could be detrimental, including workers under 18 or above 55 years of age and workers who self-report high alcohol consumption, (ii) who have a high likelihood of attrition, including those without stable housing and those who moved to Chennai

[9]The cut-off time is determined based on when the liklihood of finding a job that day dropped noticeably. This stand-specific time was determined by repeatedly visiting each labor stand and recording the number of workers and employers present every 15 minutes. In most stands, the cut-off time was 8 AM. See Figure 1 for average patterns over arrival times and job-finding probabilities across all stands.

[10]This excludes Sundays and holidays, when we do not staff the stands, as well as rare days with major disruptions such as floods.

less than 2 years ago, (iii) without access to a mobile phone, (iv) who do not speak the local language (Tamil), (v) who do not work primarily in construction. Eligible workers who consent to be a part of the study proceed to the baseline phase.

Baseline: Baseline is used to capture data to improve precision as well as for additional screening. Enumerators record workers' attendance and arrival time at the labor stand daily. Additionally, workers complete a demographic survey and daily surveys regarding labor supply when observed at the stand. If not observed at the stand for three days, the worker is interviewed by telephone. Each of these brief daily surveys was compensated with Rs. 10 ($\sim$ \$0.13) immediately following the survey to build trust.

At the end of baseline, a second round of screening takes place prior to randomization. Workers who attend the stand less than 10% or more than 55% of days are excluded.

Eligible participants are then individually randomized into the Treatment or Control arm, each with 50% probability. The randomization is stratified by stand, baseline stand attendance, and baseline average wage. Following randomization, participants are provided with information about their experimental arms, asked comprehension questions to ensure understanding, and are provided with additional details of the timeline for the study. Comprehension of the study conditions is extremely high.

Phase 1: Phase 1 lasts 7 weeks. Enumerators record study participants' attendance and arrival time at the labor stand daily. Participants are also asked to respond to a brief survey each day they attend the stand. Following baseline protocols, if a participant is absent from the stand for more than three days, study staff conduct the survey by phone. If a participant is absent on days between surveys, they are asked to provide data retrospectively.[11] All participants continue to be paid Rs. 10 per short survey, however, the payments are now lumped together with the weekly payments on Saturdays.

At the end of Phase 1, participants are informed that the next phase of the study would begin and daily survey payments would continue, but there would no longer be weekly attendance (treated) or "lottery" (control) payments. Participants are asked several questions to ensure they understand that payments are ending — overall comprehension is 95.8% on average, with no differential comprehension by treatment status.

[11]The daily recall period is capped at 7 days, at which point the participant is asked to provide summary measures of labor supply (i.e. we last saw you X days ago. Can you tell us how many days did you work since that day?").

Phase 2: Phase 2 lasts 8 weeks. Enumerators continue to record study participants' attendance and arrival time at the stand and conduct brief surveys daily, as in Phase 1. However, participants no longer receive weekly payments for attendance in the treatment group or matching payments in the control group. In addition, on designated days in Phase 2, supplementary surveys and activities aimed at understanding mechanisms behind persistence in labor supply or ruling out confounds are conducted. At the end of Phase 2, participants are informed that their active participation in the study has ended.

Phase 3 Follow-up: Enumerators return to the stand to record study participants' attendance on random days over several months. No participant surveys are conducted during this period and no compensation is offered. The duration of Phase 3 varied from 5 to 12 weeks per stand depending on staffing needs.

Sample size at each stand. Because the intervention could potentially induce general equilibrium effects, in turn depressing the labor supply of Control participants, we limit enrollment at each stand. The cap was set to be no more than 20% of the regular daily attendance at the stand, such that no more than 9.6% of workers at a stand are treated. This cap limits the potential increase in total labor at the stand to 1-2% even during peak attendance in Phase 1.

4 Data

Key outcomes. Our key outcomes of interest – labor supply and work – are measured through a combination of direct observations and self-reported data.

Attendance at stand. Attendance at the labor stand is a directly-observable revealed preference measure for labor supply: by coming to the stand, participants reveal their willingness to supply labor independent of demand. We measure labor supply directly by training enumerators to recognize the workers and to record their attendance as they arrive. Survey enumerators are stationed at the stands between 6 to 10AM from Monday to Saturday from Baseline through Phase 2, with rare exceptions for holidays and unforeseen disruptions (e.g. floods).

We implement several strategies to ensure that attendance is measured accurately. First, we allocate staff such that the ratio of participants to enumerators at each stand is low and all parts of the stand are easily visible. Second, we have a two week baseline

period to provide sufficient time for surveyors to become familiar with all participants at that stand. Third, enumerators take picture quizzes – where they are shown pictures of study participants and asked to name the participant and their study ID number – several times during baseline and early stages of Phase 1 to ensure that they recognize participants correctly. Accuracy on these quizzes was over 90% and well balanced across Treatment and Control.

Further, Treatment and Control participants have an incentive to announce their presence to enumerators: for every daily survey they complete, they receive Rs. 10, or roughly the price of a cup of tea. This amount is small enough such that it would not induce participants to come to the stand specifically to take surveys, but – if they are already at the stand – it provides a motivation to approach an enumerator.

Finally, we also elicit *self-reported* attendance and compare it to our direct measure. Mis-measurement is both low and balanced across arms in Phase 2 by this measure.

In short, the direct observation provides complete and accurate data on attendance, and thereby labor supply, by both Treated and Control participants throughout the study.

Arrival time at stand. Surveyors visually scan the stand at high frequency and record participants' arrival times immediately upon noting their presence. Participants are also regularly reminded to communicate with field staff once they arrive at the stand in order to receive their survey compensation.

Work. All outcomes related to work are measured through the high-frequency surveys described above. Participants are asked to recall day-by-day whether they worked for pay each day since their last survey.[12] Participants are also asked to report wages, the job role, how they found the job, and whether the job was part of a multi-day job. For days when the participant did not work and did not attend the stand, they are asked how they spent their day.

Baseline characteristics. Table 1 presents means and standard deviations for base-

[12]If we have not collected data for more than 7 days, workers do a day-by-day recall for the most recent 7 days as well as a comprehensive recall, where they report a total number of days of work during the period for which we do not have data. In Phase 2, approximately 5% of the work observations come from "comprehensive" recall data, and this is not differential between Treated and Control participants. (p-value 0.32).

line characteristics and tests for balance between Treatment (Column 1) and Control (Column 2) participants. Column (3) reports p-values of a comparison of means between the two arms, obtained from a univariate regression with stand and strata fixed effects. As expected given randomization, Treatment and Control participants are well-balanced on covariates.

The average participant in our study is 43 years old, is married, and has children. 19% of participants have no formal schooling. However, participants have extensive experience: the average worker has worked in construction for more than 15 years and has an average tenure at the stand of 10 years. Participants attend the stand an average of 4.2 out of 11 days, and report working on 4.9 out of 10 days with an average daily wage of Rs. 842.[13]

5 Results

Phase 1. The incentives provided to Treated participants during Phase 1 are effective at increasing labor supply and timeliness of attending the stand. Figure 3a plots the cumulative distribution function of average weekly attendance by treatment status in Phase 1. The attendance distribution for Treatment participants is shifted to the right relative to Control participants (p-value <0.001), with a relatively consistent shift across the distribution suggesting that incentives were effective at increasing labor supply across a wide range of attendance levels. Figure 3b plots the distribution of arrival time at the stand during Phase 1 by treatment status. In addition to an overall shift in the distribution to the right, there is clear bunching before the pre-specified cut-off time (standardized to 8 AM in the figure) among Treated participants.

To provide a quantitative assessment of the results, Table 2 presents the corresponding regression results. Treated participants arrive before the cut-off time 1.761 days per week more often (Column 1, p-val= 0.000), a 126% increase relative to Control participants. Similarly, treated participants attend the stand an average of 0.777 more days per week in Phase 2 (Column 2, p-val = 0.000), a 26% increase relative to Control participants.

Phase 2. While Treated participants respond to direct incentives and attend the stand

[13]Days of work can exceed days at the stand due to multi-day contracts, typically lasting 2-6 days. Nonetheless, stand attendance is crucial to locating work: at baseline, the probability of finding a job at the stand is 38 percentage points higher if a worker attended the stand that day.

more often and earlier when incentivised to do so, models of labor supply without habituation would predict that labor supply would fall back to baseline levels after incentives are removed. Hence, our core test for the hypothesis that labor supply is habit forming is the following: do Treated participants have higher attendance in Phase 2 once incentives are removed?

Figure 4a plots the cumulative distribution function of average weekly attendance by treatment status in Phase 2. We see that the attendance distribution for Treatment participants is shifted to the right relative to Control participants (p-value=0.005), indicating persistence in increased stand attendance for Treatment participants, even after incentives are removed. Figure 4b plots the distribution of arrival time at the stand during Phase 2 by treatment status. We see a significant shift in arrival time, with similar bunching before the pre-specified cut-off time (standardized to 8 AM in the figure) — Treatment participants who attend the stand thus continue to do so significantly earlier.

Regression results in Table 2 demonstrate that this persistence in Phase 2 is sizeable. Treatment participants arrive before the cut-off time on average 0.474 days more often (Column 3, p-val= 0.009), a 37% increase relative to Control participants. Treatment participants attend the stand an average of 0.466 more days per week in Phase 2 (Column 4, p-val = 0.019), a 18% increase relative to Control participants.

Does higher labor supply translate to increased work for Treatment participants? We see that Treatment participants work 0.317 more days per week relative to Control participants (Column 5, p-value 0.036), which indicates a 10% increase in total work relative to Control participants.

Effects over Time. Figure 5 shows the evolution of treatment effects over time on residualized weekly attendance by treatment status over the full time span of the study. The treatment effects in Phase 2 appear to persist in the follow up Phase 3 period. At the same time, there is evidence of a decay in habit, consistent with prior literature (e.g. Charness and Gneezy, 2009). We test for potential decay within Phase 2 more formally in Columns 1 and 2 of Table 3. We interact Treatment with the week number in Phase 2 (Column 1) and with an indicator for the second month of Phase 2 (Column 2). Both interaction terms are negative, which suggests that treatment effects deteriorate over time within Phase 2.

Workers in this empirical setting often face continuous disruptions to labor supply.

These disruptions arise from planned events such as festivals, weddings and personal obligations, as well as unplanned events such as illnesses. We test whether such shocks to labor supply have the ability to erode habit stock. To do so, we first construct a proxy for labor supply disruptions in Phase 2 using residualized attendance of all other workers (leave one out mean). We classify weeks where others' stand attendance falls below the 25th percentile as weeks where a shock occurs. We then examine what happens to treatment effects in the weeks after this shock temporarily pulls workers out of the labor market.

We find that treatment effects quickly dissipate to 0 after a shock pulls workers out of the labor market — the coefficient on Treatment x Post shock is large and negative in Column 3 of Table 3. The coefficient on the interaction term of Treatment and week number in phase 2 is close to zero and insignificant in Column 4, which suggests that there is no discernible decline in treatment effects over time — instead, the decay occurs through the shocks. Finally, the coefficients on the interaction term of Treatment with an indicator for 1 week post shock (Column 5) and Treatment with an indicator for 2+ weeks post shock (Column 6) suggest that treated workers revert back to the old equilibrium immediately in the week following a shock.

Mechanisms. We consider several channels that could possibly drive persistence in labor supply in Phase 2 and the follow up Phase 3 period after incentives are removed.

Changes in Time Use. One possible explanation is that Treatment participants rearrange their schedules and adjust their morning commitments (for example, they shift childcare responsibilities to another member of the household) so that they are able to arrive at the stand before the pre-specified cut-off time in Phase 1. These shifts in schedules and adjustments to morning commitments may persist and remain in place throughout Phase 2. We explore this possibility by asking participants to report their time use in the morning. Figure 6a shows the distribution of participants who typically carry out each of the activities listed in the morning, by treatment status. It does not appear to be the case that Treatment participants have cut back on morning chores in order to go to the stand earlier – both groups report doing morning household duties (fetching water, cooking breakfast, grocery shopping) at similar frequencies. Participants also appear to have similar routines involving prayers and getting ready for the day (eating breakfast, bathing). Another possibility is that Treatment participants

adopt technologies that enable them to be at the stand more regularly and on time during Phase 1 – one such technology is the alarm clock — and they continue to use them throughout Phase 2. Figure 6b shows that Treatment and Control participants report similar rates of usage of an alarm to wake up in the morning. Taken together, we find limited evidence for persistence being driven by permanent changes in morning routines for Treatment participants.

Learning. Another possible explanation is that timely and more frequent attendance at the stand during Phase 1 for Treatment participants could facilitate learning. Participants may learn about the stand and update their beliefs about the relationship between arrival time and job finding probability. Employers may identify "good" workers at the stand, and this in turn would increase returns to stand attendance. If the incentives facilitate learning either among workers or employers, we should expect to see changes in expected job finding probability rates for Treatment participants relative to Control. We fail to detect any significant differences in expected job find probability by treatment status, which suggests that learning is limited. This is unsurprising given that the average worker in our study has been attached to the stand for over 10 years — there might thus be limited scope for learning in the first place.

Shift in Preferences. Next, we present several pieces of evidence that suggest a shift in preferences for more consistent labor supply and a stronger taste for regular work among Treatment participants. To understand whether habituation to more frequent (i.e. more regular) labor supply reduce the desire for flexibility among workers, we conduct several supplementary exercises in Phase 2 and summarize these findings in Table 5.

The first exercise is a hypothetical test, where we ask workers to make a choice between status quo (i.e. searching for a job daily at the labor stand) and a six-day contract job at prevailing wage where work is guaranteed for all six days, but a 25% pay cut is applied for every day worked if the worker were to take any leaves. It is worth highlighting that interest in the 6-day job with penalty is low — only 15% of Control participants choose this over status quo. Treatment participants are more than twice as likely as Control participants to be willing to accept the 6-day job with penalty (+117.4%, Column 1).

The second exercise is a revealed preference test, where we ask workers to make choices in two different scenario pairs. In the first pair, workers choose between a contract

where they come to the stand on any two (flexible) days in following week, or come to the stand on two pre-set (inflexible) days for a Rs.10 premium. In the second pair, workers choose between an amount of money for sure or a contract where they come to stand on two pre-set (inflexible) days for a Rs.20 premium. One of the two pairs is randomly implemented, making these choices incentive-compatible. We find that Treatment participants are 10.6% more likely to choose the pre-set (inflexible) contract (Column 2). We further explore whether the treatment effect on willingness to accept inflexible work contracts varies after a shock temporarily pulls workers out of the labor market. We find that Treatment participants are more likely to choose pre-set (inflexible) contracts only in the absence of shocks — the coefficient on Treatment x Post shock is large and negative in Column 3 of Table 5.

Taken together, these pieces of evidence suggest that persistent increases in labor supply following Phase 1 are driven by *internal* changes, with individuals demonstrating a stronger preference for more consistent labor supply.

6 Implications for the Labor Market

We conduct three exercises with employers at labor stands that are similar to those used in the main study in order to understand broader labor market implications of labor irregularity.

Implications for Firm Behavior. In the first exercise, we survey 167 recruiters who typically frequent labor stands to hire workers. The goal of the survey is to understand whether employers in this setting anticipate labor irregularity, and the implications labor irregularity has, if any, on the functioning of firms.

The modal recruiter in the survey sample attends the stand 5 days per week and hires workers for multiple roles and multiple employers. Contracts are short — the modal contract duration (40% of contracts) is a single day, with another 22% of contracts lasting one week.

A majority of recruiters anticipate labor irregularity and find it to be costly. When asked to predict what fraction of days a worker on a 10-day contract would not attend work, the median response is 20% (Figure 7). This irregularity has a variety of costs, as illustrated in Figure 8: 30 to 40% of recruiters report spending at least 30 to 90 minutes to search for a replacement worker when a worker doesn't show up, and another 30 to

90 minutes to onboard a new worker and help them understand the work that needs to be done. Recruiters also take a number of steps to avoid these time costs – on average, they report hiring 30% more workers than they expect to need for a job. Additionally, they shift to hiring migrant laborers (who typically do not speak the local language), because of their greater regularity. In fact, recruiters cite "working more often" (45%) and "arriving on time" (57%) as at least as common or more common reasons to hire migrants than the 10-15% lower wages (43%) offered to these workers. When asked "if such regular workers existed, would this change anything else about how you offer work?", recruiters state providing less training (30%) and forgoing business opportunities and opportunities for expansion (30%) as common unprompted responses. This data suggests that although the welfare effects of irregularity are ambiguous for workers, they are costly for employers both in the short-term as they search for new workers and make costly adjustments to hiring practices as well as in the long-term as they forgo opportunities and reduce investment.

Beliefs regarding Habit Formation. In the second exercise, we conduct an incentivized survey with 115 employers to shed light on employer beliefs regarding forces driving labor irregularity. In the survey, we explain the design of our randomized experiment and describe the magnitude of treatment effects on labor supply during Phase 1 to employers. We then provide the total number of Control participants (out of 100) attending the stand at two weeks, two months and fourth months after the end of Phase 1, and ask employers to predict the number of Treatment participants attending at those time points. To incentivize truth-telling, we provide a large monetary prize ranging from INR 1000 - 5000 to the top three most accurate employers.

Table 7 summarizes findings from this incentivized survey exercise. Columns 2 and 3 summarize the total number of Control and Treatment participants (out of 100) respectively attending the stand at various time points after the end of Phase 1, as indicated in Column 1. The median employer is able to correctly anticipate treatment effects of the intervention on labor supply following Phase 1, as summarized in Column 4. The median employer is also able to correctly anticipate decay — they report that treated participants are 19.6% more likely to attend at 2 weeks, 11.1% more likely to attend at 2 months, and 50.0% more likely to attend at 4 months (Column 5).[14]

[14]The increased gap in expected attendance at 4 months following the intervention was driven by a large decline in attendance among Control participants, rather than an increase in expected attendance among Treatment participants. Employers report an expected attendance of 50% at 2 months and

Willingness to Pay for Workers with Habit Stock. In the third exercise, we conduct an incentivized survey with 69 employers to estimate willingness to pay to hire Treatment participants who have undergone our intervention. In the survey, we describe the design of our randomized experiment, describing it as a "training" intervention for workers. We then offer employers a chance to enter a lottery for a voucher to help with hiring a worker via a wage subsidy if the worker shows up at the stand by 8:30. Employers are told that they have a 1 in 10 chance of winning the lottery. Before the lottery is drawn, we vary the size of the subsidy we offer depending on whether a worker is trained or untrained, and elicit employers' preferences. For example, we start with a baseline offer of INR 300 subsidy for either a trained or untrained worker, and ask employers to state who they prefer. Then, we reduce the subsidy for the trained worker by INR 25-50 (holding fixed the subsidy for the untrained worker at INR 300), and ask employers to state who they prefer.

Table 8 summarizes findings from this incentivized elicitation exercise. We find a high willingness to pay for a trained worker. 79.7% of employers are willing to pay 11-22% of the daily wage bill for a chance at hiring a trained worker with habit stock.

7 Conclusion

In this paper, we provide experimental evidence that time-limited financial incentives to supply labor generate persistent increases in labor supply *even after incentives are removed* for a sample of casual workers in urban India. We find that a 26% increase in labor supply during the first two months of the study (Phase 1) leads to a persistent 18% increase in labor supply in the two months after incentives are removed. It also generates an increase in 0.317 days of employment per week, equivalent to a 10% increase in the employment rate. In addition, we find that shocks that temporarily pull workers out of the labor market erode their capital stock, leading treatment effects to collapse to zero. In the absence of such shocks, we find suggestive evidence that the effects can persist—at least for some workers—for up to 5 months.

We present evidence which suggests that the persistence we find is driven by *internal* changes in workers' preference for consistent labor supply and attitudes towards longer-term work, rather than *external* changes in routines or use of time. We rule out several

45% at 4 months among Treatment participants.

important confounds, namely that persistence is due to learning about the stand or learning about worker type, and that incentives induce a change in expected returns from searching for work at labor stands.

Labor irregularity has important equilibrium consequences for the labor market. We document that labor irregularity is costly for employers both in the short-term as they make costly adjustments to hiring practices to cope with sporadic labor supply, as well as in the long-term as they forgo opportunities and reduce investment. We also find that employers have accurate beliefs about the role of habit formation in labor supply, and are willing to pay for workers that have accumulated this habit stock.

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Figures

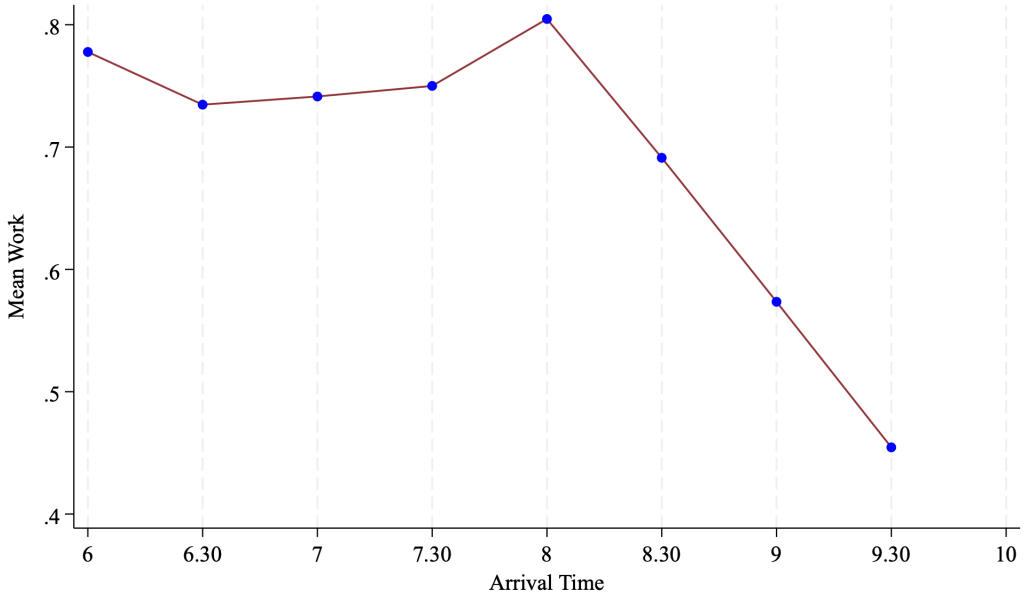
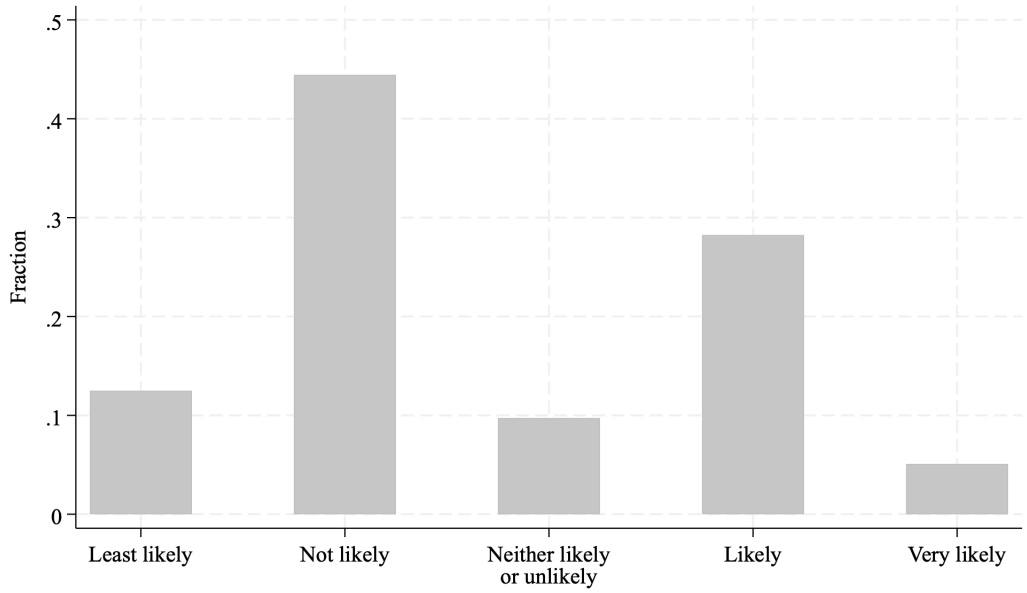
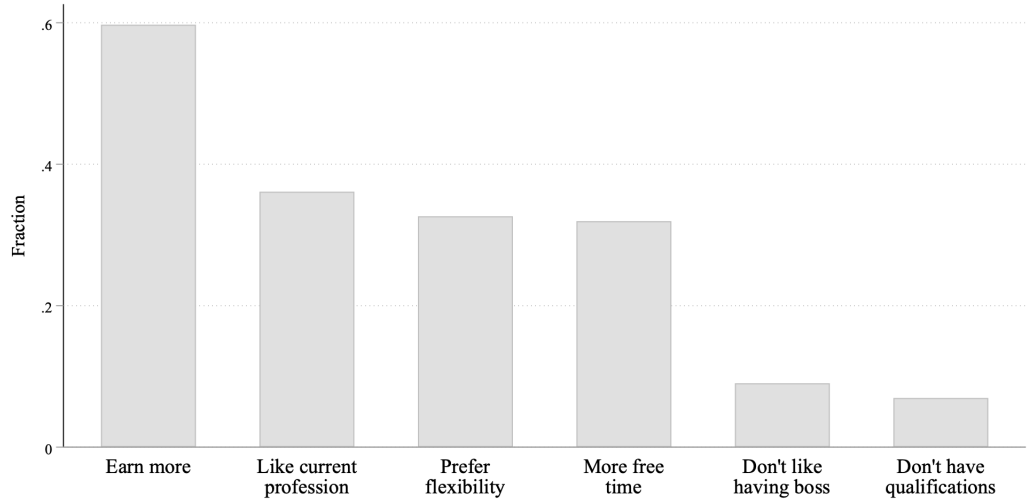


Figure 1: Probability of finding a job by arrival time

Notes: Figure plots the relationship between arrival time and work using baseline data.

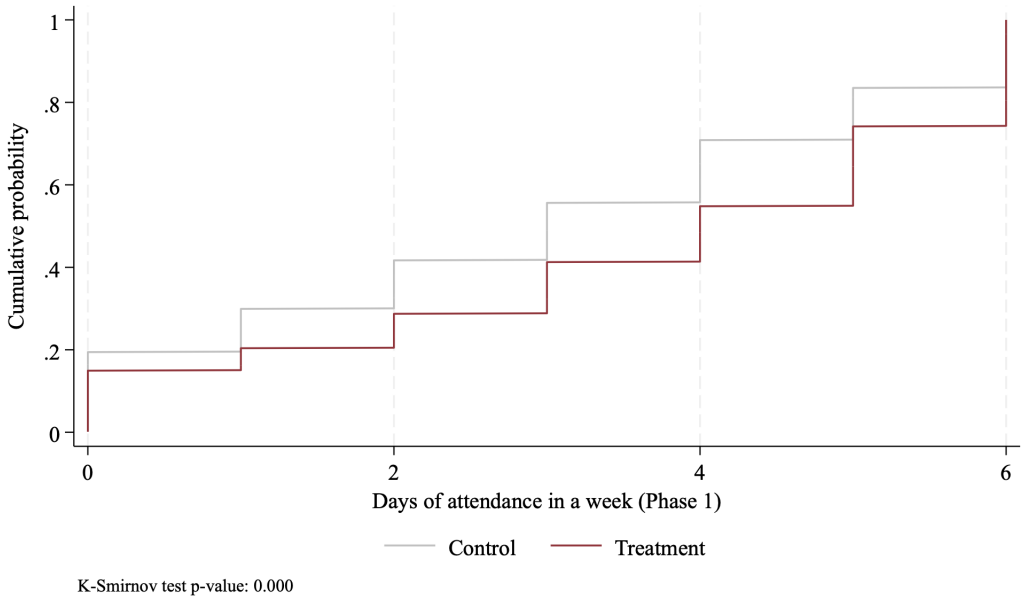


(a) Likelihood of accepting a long-term, formal job if offered one

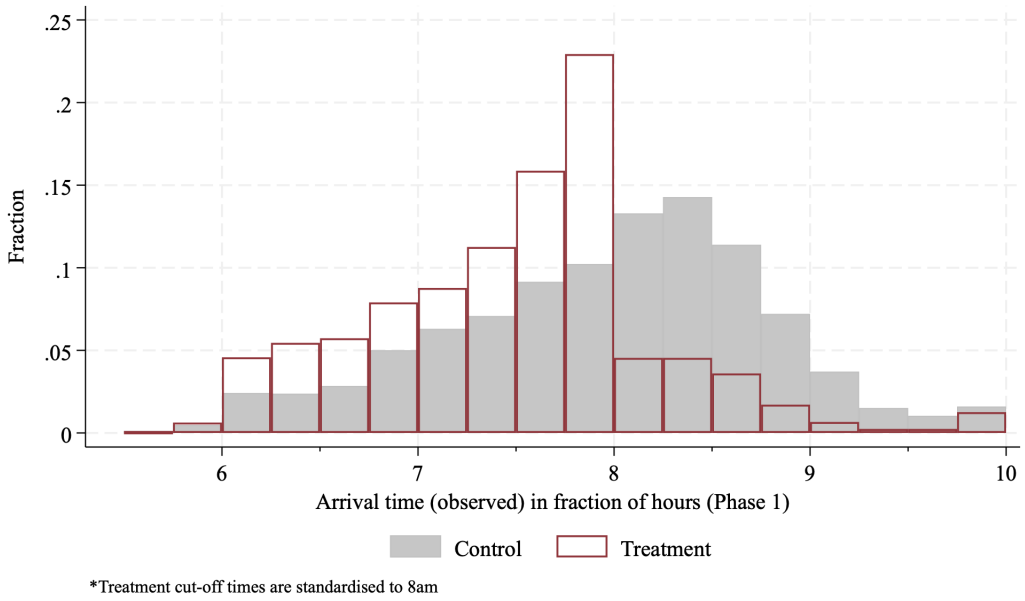


(b) Characteristics of casual jobs found at the stands most appreciated by participants

Figure 2: Job preferences at baseline



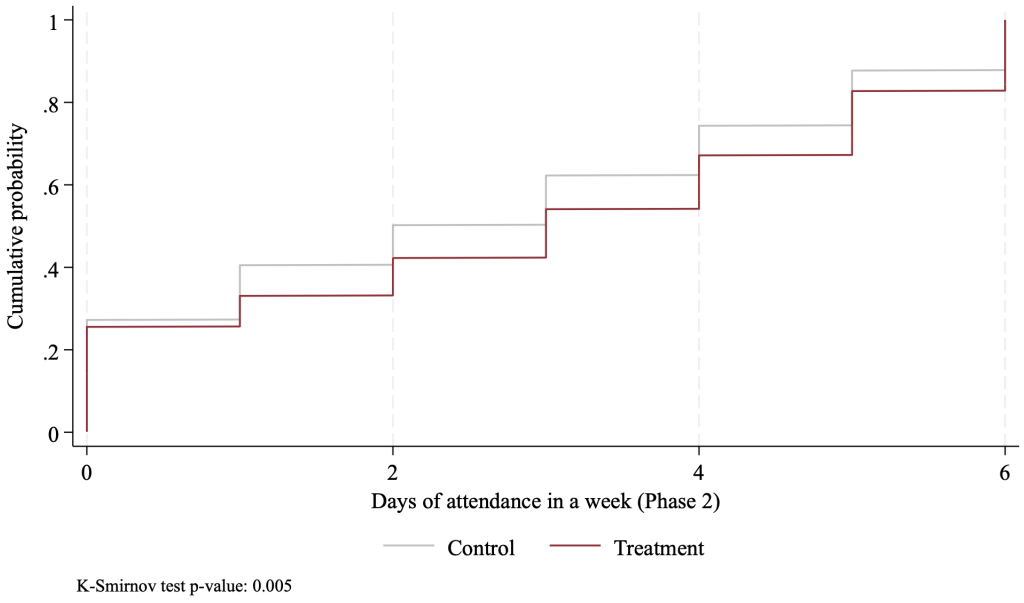
(a) Weekly attendance



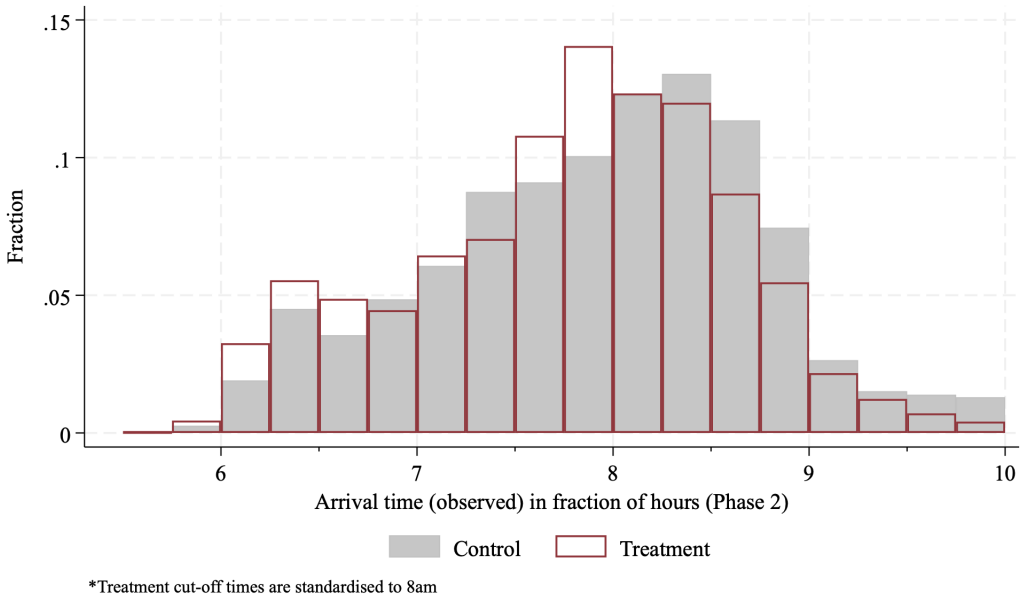
(b) Arrival time at labor stand

Figure 3: Treatment effect on attendance and arrival time in Phase 1

Notes: Panel 3a plots the cumulative distribution of weekly attendance at the stand and Panel 3b plots the distribution of arrival time at the stand during Phase 1 (when participants receive incentives).



(a) Weekly attendance



(b) Arrival time at labor stand

Figure 4: Treatment effect on attendance and arrival time in Phase 2

Notes: Panel 4a plots the cumulative distribution of weekly attendance at the stand and Panel 4b plots the distribution of arrival time at the stand during Phase 2 (after incentives are removed).

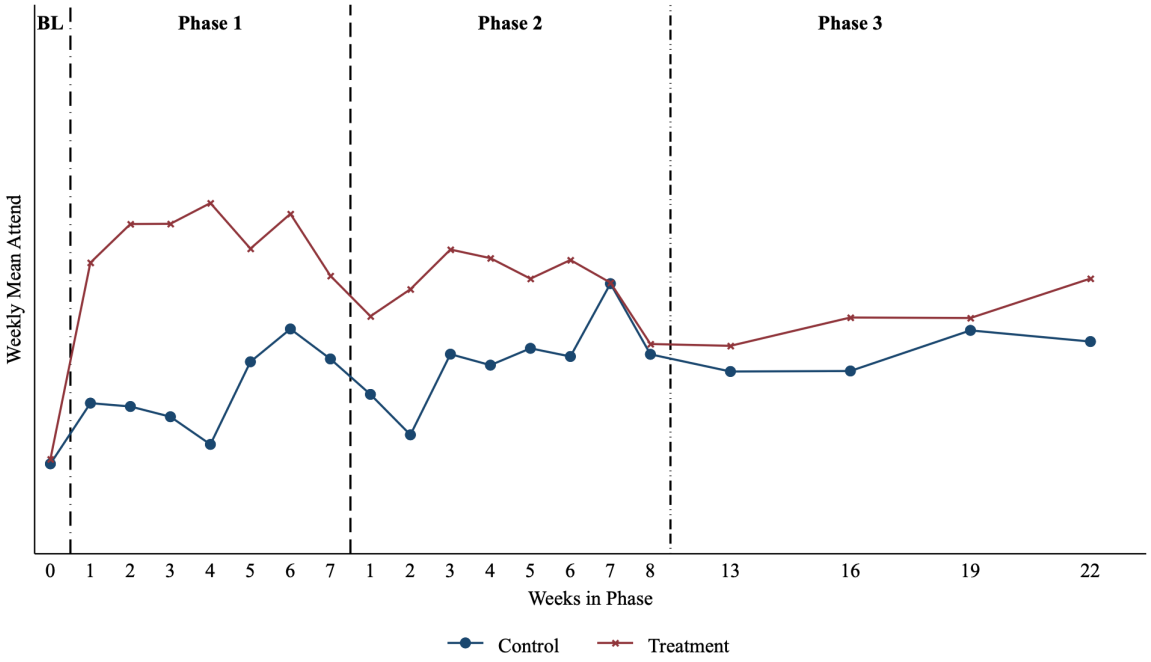
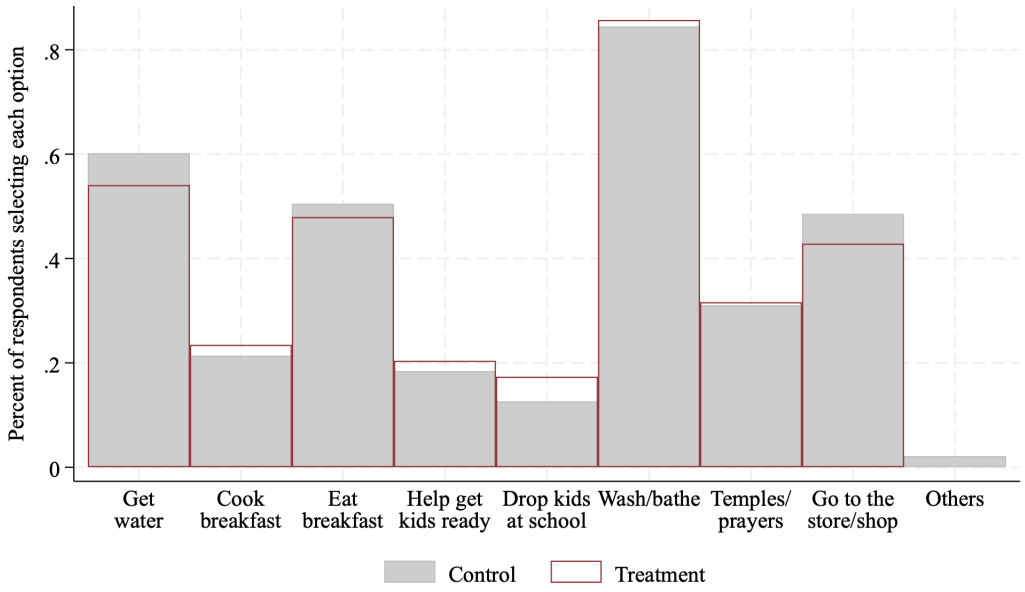
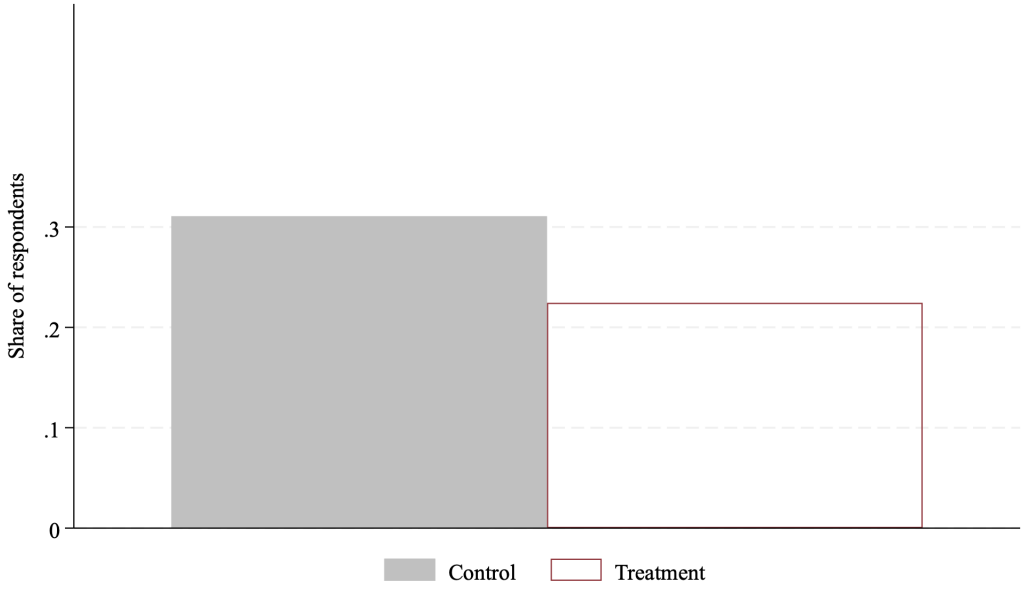


Figure 5: Attendance

Notes: Figure plots residualized weekly attendance (controlling for stand and calendar week fixed effects) for each phase of the study, by treatment status.



(a) Activities done in the morning between 5.30am to 9am



(b) Usage of an alarm to wake up in the morning

Figure 6: Treatment effect on morning routines during Phase 2

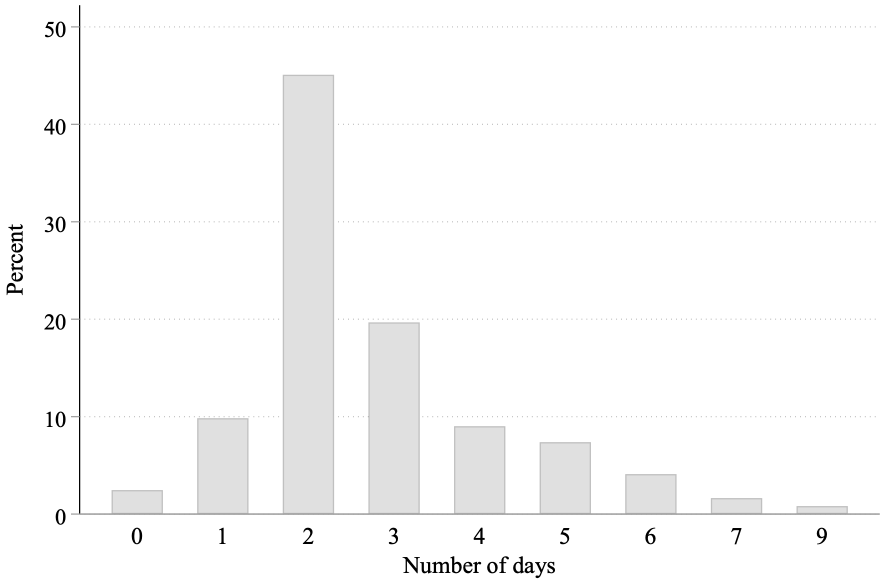
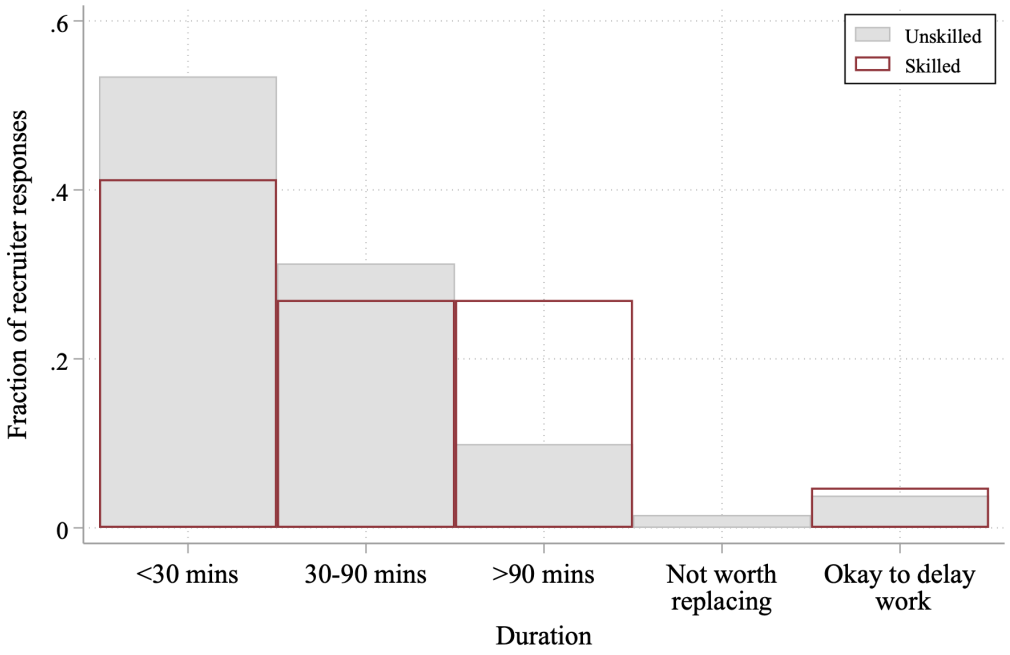
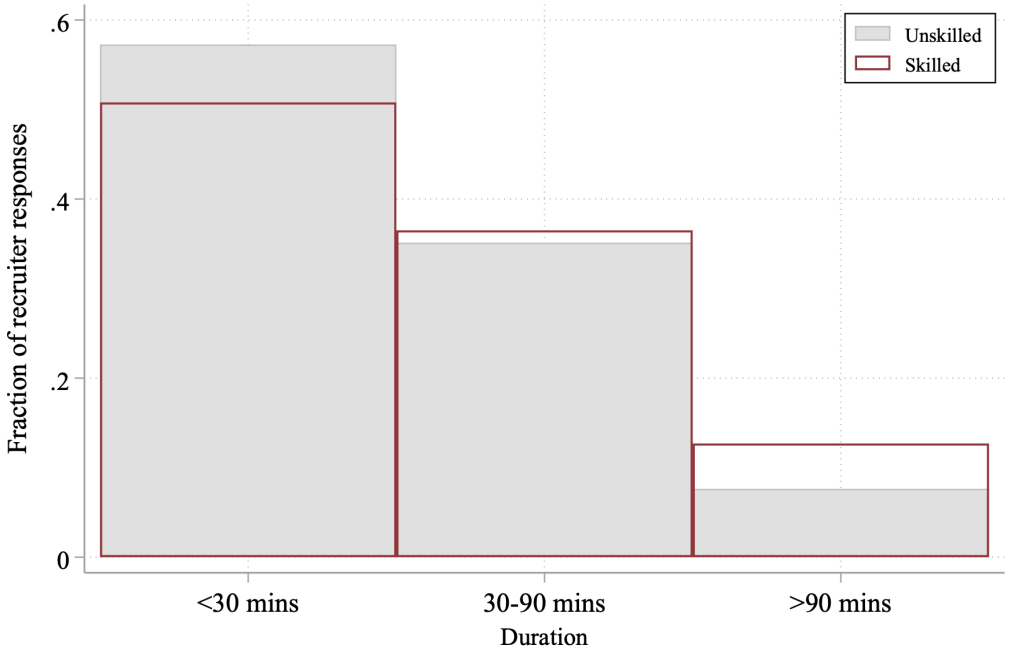


Figure 7: Predicted Worker Absenteeism

Notes: Figure summarize survey responses to the following question: "Suppose you hired one worker for a 10 day contract (so that the worker said he would come on all days when he took the job). Out of these 10 workdays, on how many days do you think the worker would be absent from work?"



(a) Time taken to find a replacement worker



(b) Time taken to onboard a new worker

Figure 8: Costs incurred by employers

Notes: Figure summarizes response to the following survey questions: "How much time does it typically take to find a worker to replace someone who was supposed to come to work but didn't?" (panel (a)) and "How much time do you typically spend helping a new worker understand what needs to be done and how to do it?" (panel (b))

Tables

Table 1: Baseline Characteristics

	(1) Control Mean	(2) Treatment Mean	(3) Regression P-value
Age	42.4	42.6	0.597
	(9.5)	(9.3)	
No schooling	0.2	0.2	0.950
	(0.4)	(0.4)	
Has spouse/children	0.9	0.9	0.751
	(0.4)	(0.3)	
Years at stand	10.2	10.4	0.732
	(8.2)	(8.0)	
Years in current profession	16.3	15.9	0.765
	(10.5)	(9.6)	
Days attended stand	4.2	4.2	0.929
	(1.6)	(1.6)	
Days worked	4.9	4.9	0.965
	(2.2)	(2.4)	
Average daily wage (in rupees)	843.8	840.4	0.389
	(122.3)	(133.7)	
Total earnings (in rupees)	4162.1	4157.1	0.791
	(1986.4)	(2159.6)	
N: workers	112	113	

Notes: This table presents baseline characteristics for study participants. Columns (1) and (2) present baseline means and standard deviations of characteristics for participants in the control and treatment group respectively. Column (3) reports p-values of a comparison of means across treatment and control participants, obtained from regressing the covariate in each row on a dummy for treatment with stand and strata fixed effects and robust standard errors.

Table 2: Labor Supply Effects

	Phase 1		Phase 2		
	Attend by 8am	Attend	Attend by 8am	Attend	Worked (Total)
	(1)	(2)	(3)	(4)	(5)
Treatment	1.761	0.777	0.474	0.466	0.317
	(0.186)	(0.179)	(0.180)	(0.196)	(0.150)
	[0.000]	[0.000]	[0.009]	[0.019]	[0.036]
Control mean	1.395	2.990	1.265	2.577	3.041
N: worker-weeks	1572	1572	1800	1800	1357

Notes: This table reports estimates of the treatment effect on attendance and employment in Phase 1 and Phase 2. Observations are at the worker-week level. Dependent variable: Number of days attended or worked per week. Controls include strata, stand, week-in-phase, and calendar week fixed effects, as well as number of days where attendance is recorded (weekly), baseline attendance rate at worker level, and baseline mean earnings at worker level. Standard errors are clustered at the worker level.

Table 3: Shocks Erode Habit Stock

	Attendance					
	(1)	(2)	(3)	(4)	(5)	(6)
Treat	0.467	0.603	0.791	0.760	0.792	0.766
	(0.196)	(0.220)	(0.243)	(0.257)	(0.243)	(0.259)
	[0.018]	[0.007]	[0.031]	[0.025]	[0.030]	[0.023]
Treat × Post shock			-0.791	-0.714		
			(0.348)	(0.420)		
			[0.038]	[0.072]		
Treat × 1 week post shock					-0.641	-0.605
					(0.357)	(0.389)
					[0.115]	[0.136]
Treat × 2+ weeks post shock					-0.838	-0.763
					(0.378)	(0.472)
					[0.024]	[0.077]
Treat × Second month of phase 2		-0.272				
		(0.162)				
		[0.095]				
Treat × Week in phase 2	-0.0712			-0.0339		-0.0289
	(0.040)			(0.056)		(0.059)
	[0.080]			[0.617]		[0.689]
Week in phase FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Week FE	Yes	Yes	Yes	Yes	Yes	Yes
N: worker-weeks	1800	1800	1612	1612	1612	1612

Notes: In this table, columns (1)-(2) test for potential decay in treatment effect within Phase 2, and columns (3)-(6) estimate the effect of shocks on treatment effect within Phase 2. Observations are at the worker-week level. Dependent variable: Number of days attended per week. Controls include strata, stand, week-in-phase, and calendar week fixed effects, as well as number of days where attendance is recorded (weekly), baseline attendance rate at worker level, and baseline mean earnings at worker level. Columns (2)-(6) treat the first week after the worker's labor stand received a shock (as proxied by low attendance among other workers) as baseline. Standard errors are clustered at individual level in parentheses. Columns (1)-(2) repsort p-values in brackets. Columns (3)-(6) report bootstrapped p-values.

Table 4: Automaticity: Change in Psychological Default

	(1)	(2)
Treatment	0.308	0.467
	(0.151)	(0.181)
	[0.043]	[0.011]
Treatment x Post shock		-0.655
		(0.355)
		[0.067]
Control mean	3.865	3.865
N: worker	175	175

Notes: The table reports the estimated effect of treatment and shocks on automaticity, measured by the extent to which workers agree with the following statement: "Going to the stand is something I do without thinking." The variable is on a 5-point scale, ranging from strongly disagree (1) to strongly agree (5). Both columns control for strata and stand fixed effects. Column 2 additionally controls for post-shock, the indicator for being surveyed in the first week post-shock, and the interaction between the indicator and treatment. Standard errors are clustered at the worker level.

Table 5: Preference for Flexibility

	Contract job	Fixed choice	Fixed choice
	(1)	(2)	(3)
Treatment	0.174	0.119	0.152
	(0.081)	(0.068)	(0.081)
	[0.035]	[0.082]	[0.063]
Treatment x Post shock			-0.123
			(0.158)
			[0.439]
Control mean	0.148	0.523	0.523
N	109	278	278

Notes: Observations are at the worker level in Column 1 and at the worker-question level in Columns 2 and 3. Regressions include stand and strata fixed effects. Standard errors are clustered at individual level in parentheses, and p-values are in brackets.

Table 6: Evidence for Persistence in Other Datasets

	Kaur et al. 2015		Carranza et al. 2022	
	(1) Output	(2) Output	(3) Ln(Output)	(4) Ln(Output)
Lagged highest target imposed	706	484		
	(229)	(185)		
	[0.003]	[0.010]		
Lagged log piece rate			1.056	1.139
			(0.532)	(0.527)
			[0.048]	[0.031]
Day FE	No	Yes	No	Yes
N: worker-days	2964	2964	1296	1296
N: workers	96	96	276	276
Dep. variable mean	5278	5278		

Notes: Columns (1)-(2) are from Kaur et al. (2015) on full-time data entry workers over 1 year, where randomized incentives to work hard have persistent effects *the next day*. Columns (3)-(4) are from Carranza et al. (2024) on daily piece rate randomization among full-time factory workers in a multinational firm, showing that a 1% piece rate increase causes 1% effort (output) increase *the next day*. The table supports the possibility that inter-temporal labor supply elasticity can be positive.

Table 7: Employer beliefs

Time Point	Control	Treatment	Treatment (median)	% Change
(1)	(2)	(3)	(4)	(5)
2 Weeks	46	55	55	19.6%
2 Months	45	50	50	11.1%
4 Months	30	33	45	50.0%

Notes: This table presents results from an incentivized survey with employers where we elicit beliefs regarding the impact of our intervention. Column 1 indicates several time points after the end of Phase 1. Columns 2 and 3 indicate counts of control and treatment participants (out of 100) respectively attending the stand at the different time points, based on the experimental data. Column 4 summarizes employers' median responses to the number of treated workers attending the stand each day at the time points indicated in Column 1, while Column 5 presents the corresponding percentage change in expected attendance between treatment and control participants.

Table 8: Employer Willingness to Pay for Workers with Habit Stock

Subsidy: untrained (INR)	300	300	300	300	300
Subsidy: trained (INR)	300	250-275	200-250	150-225	100-200
% choose trained	97.1	85.5	82.6	82.6	79.7

Notes: This table presents results from an incentivized survey with employers where we elicit willingness to pay for "trained" workers with habit stock.