Habit Formation in Labor Supply*

Luisa Cefala† Supreet Kaur Heather Schofield Yogita Shamdasani

July 22, 2026

Most recent version **here**.

Abstract

We examine the possibility of habit formation in labor supply. Using a field experiment with casual urban laborers in India, we randomly provide treated workers with small financial incentives for attendance at labor stands for 7 weeks, leading to a 26% increase in labor supply. We then test for the persistence of impacts *after* the incentives are removed. First, we see a persistent 18% increase in labor supply over the following 2 months, resulting in a 10% increase in overall employment days. Second, treated workers exhibit a higher willingness to accept work contracts that are of longer duration and less flexible. Third, labor market disruptions deplete habit stock: shocks that temporarily pull workers out of the labor market instantly eliminate treatment effects on labor supply and work contract choice; in the absence of these shocks, we cannot reject that there is no decay in persistence over time. Fourth, we see no "fixed cost" changes in household time use, or learning among workers or employers—consistent with "true" state-dependence in labor supply. Rather, workers self-report an increase in automaticity—suggesting a change in their psychological default. Fifth, employers accurately predict treatment effects, and are willing to pay to hire workers who have been treated with our habit stock intervention. Our results offer evidence for habit formation as a micro-foundation for state-dependence in labor supply and labor market hysteresis. They also suggest that in low income settings, intermittent employment and frequent shocks may inhibit workers from becoming habituated to regular work—with potential implications for absenteeism and barriers to structural transformation in developing countries.

*IRB of record: UC Berkeley Protocol # 2019-06-12300, AEA Trial Registration # AEARCTR-0009402.

†Cefala: Cornell (lc2295@cornell.edu); Kaur: UC Berkeley (supreet@berkeley.edu); Schofield: Cornell (hws44@cornell.edu); Shamdasani: NUS (yogita@nus.edu.sg).

1 Introduction

"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."

— William James (James, 1983)

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). Under this view, actions are self-reinforcing, so that undertaking an activity increases one's propensity 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. A sizable body of work in labor and macroeconomics has discussed the potential relevance of state dependence in labor supply (e.g. Hyslop, 1999; Card et al., 2007). For example, economists have long observed that labor markets tend to exhibit hysteresis: employment rates fall during recessions, and remain sluggishly low after the recession ends (e.g. Bowen and Finegan, 1969; Yagan, 2019). Habit formation may offer one (not mutually exclusive) explanation for why a laid off worker may choose to remain unemployed even when labor demand recovers (e.g. Kroft et al., 2016).

In addition, habit formation may have particularly important implications for low-income workers. In both poor and rich countries, such workers often experience irregular employment patterns—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; Ganong et al., 2025). These external constraints may make it difficult to build up a habit of regular labor supply—with potential implications for understanding high absenteeism levels and low take-up of formal jobs in poor countries (Breza and Kaur, 2025). In testing for habit formation, we explicitly examine the mediating role of shocks and employment disruptions.

We conduct our test among casual urban workers at labor stands in Chennai, India. Such labor stands are ubiquitous in developing countries, providing the primary means of employment for hundreds of millions of workers. Labor stands are also found in high income settings, including many large cities in the U.S., for example, in the parking lots of Home Depot (Valenzuela, 2014). 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 directly observable 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. The typical worker at these stands has over 10 years of experience at his given labor stand. The median worker attends the stand 3.8 days per week and, as is typical for casual workers, has an employment rate of about 2.7 days per week (across all employment sources).[1] Despite having irregular employment, at baseline 60% of workers state they would not take up a stable formal job if offered, because they value flexibility and free time during the workweek.

To test for habit formation, we use temporary financial incentives to increase labor supply for treated workers, and then examine persistence of impacts *after* the incentives have been removed.[2] We randomize workers at the individual level, treating between 2-7% 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 seven 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 a.m.[3] To address potential income effects, 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 a.m. on 1.761 more days per week (126%, $p<0.001$).

[1] Workers find employment on 77% of the days they arrive at the stand by 8 a.m. Our experiment restricts the sample to workers who attend fewer than 5 days per week during the baseline period.

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

[3] The job finding probability declines substantively with arrival time after 8 a.m., with most stands "closing" by 10 or 10:30 a.m.

In Phase 2, we remove the incentives for stand attendance among treated workers and corresponding unconditional payments to control participants. We continue to track workers daily for eight 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 intertemporal 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 among treated workers in Phase 2.

Consistent with this prediction, we see substantial 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 a.m. on 0.474 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 (p =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 ($p = 0.036$). However, we also see some evidence of decay in treatment effects over time during Phase 2.

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 to track attendance. In this longer follow-up period, we estimate a 15% treatment effect on labor supply. However, these effects are no longer significant (p=0.264) — due to some combination of decay in effects over time and reduced power from the less frequent observations in Phase 3.

To understand why the persistence in treatment effects begins to erode over time, we examine the role of shocks and other labor market disruptions — forces that create irregularity in employment in low-income settings. These may include major holidays (e.g. Diwali), festivals and weddings (which are often held on weekdays), weather shocks, illness, or trips back to one's village to take care of family obligations. To avoid potential endogeneity in self-reported shocks, we construct a data-driven measure of disruptions at the labor stand level: weeks where attendance among *other* workers at the stand is unusually low (i.e. below the 25th percentile). Because stands are rolled out in a staggered fashion over the course of a year, and workers are enrolled over time within a stand, there is considerable variation across workers and stands in whether there is a disruption in Phase 2, and in which week the disruption occurs. We then use an event study design to examine what happens to Phase 2 treatment effects in the weeks after such a disruption occurs.

We find that disruptions play a strong role in mediating persistence in Phases 2. After 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 week after the disruption, and in 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 future attendance, which rebounds after the transitory shock is over—indicating that there is nothing persistent about the disruptions themselves; they simply knock the treated participants back to their original labor supply behavior (i.e., before our experiment began). These estimates are robust to a wide range of shock cut-offs and empirical specifications.

These patterns provide empirical insight into how to conceptualize habit formation. Our findings do not follow the gradual individual decline one would expect under traditional "addiction" models in economics, which model habits via complementarities in the utility function (Becker and Murphy, 1988). Rather, they match work from psychology on automaticity and habit discontinuity (e.g., Wood et al., 2005; Wood and Rünger, 2016; Webb et al., 2024). This work predicts instantaneous abandonment of new habits in response to removal of cues, environmental changes, or "shocks" which cause the individual to reevaluate automatic behaviors and actively choose a new behavior.

Also consistent with this view, treated workers report increased automaticity around stand attendance in Phase 2. For example, they are more significantly 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 disruption, we see no difference in self-reported automaticity between treatment and control workers—mirroring the effects on labor supply. While only suggestive, these patterns strengthen our interpretation 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 "fixed cost" changes, such as adjustments to individuals' morning routines (e.g. who drops off the kids at school), commuting patterns, or technology (alarm clocks) adoption. Using detailed time use data, we find that treated participants undertake similar morning activities as control participants. And the vast majority of participants live quite close to the stand with over two-thirds of participants commuting on foot or by bike or motorbike, such that they are unlikely to change commuting patterns. Finally, the use of alarm clocks is similar, but slightly lower, among Treated workers. Of course, we cannot measure every type of fixed cost change; moreover, making changes to routine could be construed as a part of what enables a habit—so this is not a confound per se. However, the evidence we have suggests that changes in household duties or morning routine are not driving our effects.

In addition, we rule out that our results stem from worker or employer learning — either of which could affect the expected returns to attending the stand. At baseline, worker beliefs about job finding probabilities are quite accurate. This fact is unsurprising: the average participant's tenure at their stand is 10 years. These beliefs are maintained over time: in Phase 2 we find no difference in workers' perceived job finding probability between treated and control workers. Such confounds also cannot explain the immediate dissipation of effects after a short disruption. Finally, we do not find evidence that effects are driven by habit formation in consumption: by exploiting random variation in Phase 1 payments within the control group, we find no evidence that control workers who had more disposable income in Phase 1 supply more labor in Phase 2.

Habituation to higher labor supply also changes the kinds of job contracts workers are willing to take up. The employers who hire at the stands often have projects which span a week or longer and require a consistent labor supply. Nonetheless, the modal contact is only one day — in large part due to an expectation of high but uncertain absenteeism. We construct two tests on contract choice. First, in Phase 2, among the control group, only 14.8% of workers say they would accept a 6-day job contract with a wage penalty for absences. This number increases to 32.2% among treated workers (118% effect, p=0.035). Second, we offer workers an incentivized choice between a higher-paying but less flexible contract that requires attendance on two specific fixed days, versus a lower-paying contract with more flexibility in attendance. Treated participants are 10.6 p.p. (20.4%) more likely to select the higher paying but less flexible contract (p=0.082). Conditional on selecting the inflexible contract, treated workers are also 41% more likely to complete it successfully than control workers. Moreover, as before, if participants are offered the incentivized contract choice *after* their labor stand has experienced a temporary disruption, there is no longer a treatment effect on contract choice—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 impediments to labor supply to the formal sector in developing countries.

To assess generalizability, we complement our analysis by testing for habit formation effects in other other contexts: data entry workers in India from Kaur et al. (2015) and cashew processing factory workers in Côte D'Ivoire from Carranza et al. (2024). In each of these datasets, we exploit randomized variation in daily incentives that exogenously increase labor supply on a given day. In both contexts, we find that these transitory increases in labor supply lead to persistently higher labor supply in future days — but with quick decay — a result that is consistent with earlier work on habit in water usage (Byrne et al., 2022). This indicates the possibility that the inter-temporal labor supply elasticity could actually be positive (rather than negative) in a variety of settings.

In the final part of the paper, we engage 300 employers who hire from similar labor stands to undertake one of three additional exercises. First, we document that employers have remarkably accurate beliefs about habit formation. Specifically, in incentivized surveys, employers accurately predict both persistence in labor supply and decay in the magnitude of the effects over time. Second, in an incentive-compatible hiring decision, we elicit employers' willingness to pay to hire a treated vs. control worker from our experiment without providing information regarding treatment effects in Phase 2. 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.[4] Finally, in response to open-ended survey questions, employers indicate that irregular attendance causes them to offer daily spot contracts with few amenities. Specifically, they state that if attendance were more reliable, they would prefer to offer longer-term work contracts, offer more training to workers and amenities such as loans, and also expand the nature of their business by taking on additional types of work. Although speculative, these results suggest that labor supply behavior may endogenously shape the structure and organization of labor market arrangements.

A long-standing body of work in psychology and economics has discussed the relevance of habit formation for human behavior (e.g., Wood and Rünger, 2016; Webb et al., 2025; Rabin, 2011). Economists have traditionally modeled habit formation as intertemporal

[4]The willingness to pay exercise was implemented as a subsidy payment, which would only be paid out if the treated or control worker came to the labor stand on the agreed upon day of work. Thus, employers' willingness to pay reflects their beliefs in the Phase 2 attendance treatment effects.

complementarities in the utility function (Becker and Murphy, 1988), although a small number of theoretical papers has pursued more psychologically-grounded approaches based on cues or memory (Laibson, 2001; Bordalo et al., 2025). A growing literature has empirically tested for habit formation in contexts such as gym attendance (Charness and Gneezy, 2009; Acland and Levy, 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), social media usage (Allcott et al., 2020), and expenditures (Lyu, 2025).

We build on and complement this prior body of work in three ways. First, we document the presence of habit formation within the context of a core high-stakes economic behavior: labor supply and full-time earnings. This is an important domain in its own right, given the potential implications of habit formation for understanding labor market phenomena, as discussed above. Second, through detailed auxiliary data collection and tests, our experiment design enables us to rule out a variety of potential channels for persistence — including learning about the external environment and many types of fixed cost changes in routine — suggesting that our findings likely reflect true state dependence. Third, we provide evidence on the underpinnings of persistence. We find that temporary shocks can cause a discrete jump back to one's old equilibrium, rather than gradual decay. This rapid and complete dissipation of habit is inconsistent with standard economic models of habit formation via intertemporal complementarities in utility and instead supports a psychological account in which habits are sustained by automaticity.

Our study also has relevance for the literature on labor markets 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 high 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; Goraya et al., 2025). We find that external constraints and shocks faced by workers may inhibit their ability to build up habit stock for regular work. We also document that habituation to regular labor supply changes workers' willingness to take up longer or more inflexible work contracts. 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) well as work arguing that institutions such as schooling served to cultivate the discipline and habits of punctuality required by formalized labor markets (**?**). 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.

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 7 discusses implications for the labor market and Section 8 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).[5]

The stands typically operate only in the morning, typically from 6 a.m. to 10 a.m., allowing for a full day of work. Roughly 75-80% of those who attend early in the morning secure work. However, the likelihood of finding work declines steeply after 8 a.m. (Figure 1).

Workers at the stands we operate in have a stated preference to attend the stand 5.4 days per week on average.[6] However, consistent with both the low overall level of labor supply typically found among causal laborers in many low-income countries as well as high absenteeism rates, labor supply among workers at labor stands is often sporadic with workers attending roughly 4 days per week on average (Breza and Kaur, 2025; Cefala et al., 2024; Adhvaryu et al., 2024; Goraya et al., 2025). Further, at baseline 60% of workers state they would not take up a stable formal job if offered, citing a preference for the flexibility of daily jobs and an appreciation of free time.

[5]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.

[6]The standard workweek in India remains six days for many professions.

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' may habituate to different (higher) levels of labor supply, 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 before a pre-specified cut-off time.[7] 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.[8]

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.[9]

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

Experimental timeline. The study was implemented in 11 labor stands. We operate

[7] 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 a.m. See Figure 1 for average patterns over arrival times and job-finding probabilities across all stands.

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

[9] Note that as the match was randomized, this statement is not deceptive.

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). Enrollments were rolling both between and within stands such that the study began in the spring of 2022 and concluded the spring of 2023.

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 less than two 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 over two weeks. Additionally, workers complete a one-time demographic survey as well as brief daily surveys regarding labor supply when observed at the stand. Each of these brief daily surveys was compensated with Rs. 10 ($\sim$ \$0.13) immediately following the survey to build trust.[10]

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 to avoid enrolling individuals who are likely to provide incomplete data or those for whom it is difficult to increase labor supply during Phase 1. This screening results in a sample that attends 3.4 days per week on average during the baseline period.

[10]For participants not observed at the stand for three consecutive days, we also attempted to collect work data via phone survey. We ultimately do not use this data, however, as participants had difficulty accurately recalling which specific days they had worked without a calendar in front of them or the benefit of the more careful, structured recall prompts we are able to use in person.

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.[11] 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 with an average score of 2.92 out of 3 and no difference across treatment (p-val 0.51). The randomization was well balanced on observables (Appendix Table A.1).

Phase 1: Phase 1 lasts 7 weeks. The duration of this period was based on the existing habit formation literature (Buyalskaya et al., 2023). During this phase, treated participants receive the incentive payments and control participants the matching payments that were detailed above. Enumerators record study participants' attendance and arrival time at the labor stand Monday through Saturday, the days in which the stands are active. Participants are also asked to respond to a brief survey each day they attend the stand. If a participant is absent on days between surveys, they are asked to provide data retrospectively.[12] All participants continue to be paid Rs. 10 per short survey, however, the payments are now lumped together with the incentive/matching payments on Saturday evenings.[13]

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. Payments allocated to treatment and control participants are well balanced (Appendix Table A.3).

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

[11]Because randomization was done on the Sunday between baseline and Phase 1 for each cohort, it was not possible to fully clean the data prior to the randomization. This resulted in a small number of participants having randomization strata that are inconsistent with their baseline attendance measures.

[12]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?").

[13]This payment timing simplifies the logistics of matching payments.

1. However, participants no longer receive weekly payments for attendance in the treatment group or matching payments in the control group. Daily survey compensation is still provided once per week on Saturday evenings. 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. Due to the rolling enrollment, not all participants complete these additional activities, resulting in variable sample sizes. At the end of Phase 2, participants are informed that their active participation in the study has ended and are thanked for participating.

Phase 3 Follow-up: Enumerators return to the stand to record study participants' attendance on random days over several months. These visits are not announced. 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. Between 2 and 7% of each stand was treated, limiting the potential increase in total labor at the stand to less than 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 the end of 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. Importantly, 98.7% of workers frequent only one labor stand, such that it is highly unlikely that non-attendance at a worker's assigned stand reflects labor supply at a different stand rather than a true absence from the labor market.

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. Mismeasurement is both low and balanced across arms in Phase 2 by this measure.

In short, the direct observation provides objective, complete, and accurate data on attendance, and thereby labor supply, by both Treated and Control participants throughout the study without the need to rely on self-reports. Because data on attendance is complete, we do not examine attrition on this measure. However, we do examine formal (explicitly wanting to discontinue) and informal (simply not appearing at the stand for an extended duration) "dropout" and find both to be well-balanced across arms (Table A.2).

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.[14] Participants are also asked to report wages, the

[14]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 = 0.32). In Phase 2, we capture data on work outcomes on approximately 70% of the days. This

job role, how they found the job, and whether the job was part of a multi-day job.

Baseline characteristics. Table A.1 presents means and standard deviations for baseline 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.

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. Screened participants attend the stand an average of 4.2 out of 11 days, and report working on 4.9 with an average daily wage of Rs. 842 (roughly $11.50).[15] As expected given randomization, Treatment and Control participants are well-balanced on covariates.

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 2a plots the cumulative distribution function of average weekly attendance by treatment status in Phase 1. The attendance distribution for Treated participants is shifted to the right relative to Control participants (p-value = 0.000), 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 2b 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 a.m. in the figure) among Treated participants.

To provide a quantitative assessment of the results, Table 1 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

is well-balanced across experimental arms (p = 0.393)

[15]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.

days per week in Phase 1 (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 more often and earlier when incentivised to do so, models of labor supply without state-dependence would predict that labor supply would fall back to baseline levels after incentives are removed. Hence, our core test for the hypothesis is whether labor supply is persistently higher among treated participants in the absence of incentives during Phase 2.

Figure 3a plots the cumulative distribution function of average weekly attendance by treatment status in Phase 2. The attendance distribution for Treated 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. These effects are not only visually apparent, but also economically meaningful and statistically significant. Treated participants attend the stand an average of 0.466 more days per week in Phase 2 (Table 1, Column 4, p-val = 0.019), a 18% increase relative to Control participants.

Effects on arrival time remain large and significant in Phase 2 as well. Figure 3 plots the distribution of arrival time at the stand during Phase 2 by treatment status. The plot shows a continued bunching before the pre-specified cut-off time (standardized to 8 a.m. in the figure), despite the removal of incentives to arrive promptly. Treated 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. Standard models of labor supply typically assume a negative intertemporal elasticity, under which a temporary positive shock to labor supply should be followed by compensating reductions once the shock ends. Our finding of sustained, rather than reduced, attendance in Phase 2 suggests the opposite: a positive intertemporal elasticity of labor supply.

These effects on labor supply translate to consequential increases in employment among Treated workers, who work 0.317 more days per week — a 10% increase in total work — relative to Control participants (Column 5, p-value 0.036). Wages in this environment are quite sticky and remain similar across Treated and Control workers (p = 0.950).

Effects over time. Figure 4 shows the evolution of treatment effects over time on residualized weekly attendance by treatment status over the full time span of the study.[16] Visually, treatment effects in Phase 2 appear to show some persistence in Phase 3. None the less, there is evidence of a decay in habit, consistent with prior literature (e.g. Charness and Gneezy, 2009), and the reduced effect sizes combined with less frequent observations result in a treatment effects that is not robustly statistically significant. We test for potential decay within Phase 2 more formally in Columns 1 and 2 of Table 2 by interacting 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 and marginally significant ($p = 0.080$ and $p = 0.095$, respectively), which suggests that treatment effects deteriorate over time within Phase 2.

Disruptions to labor supply. This deterioration could be the result of a gradual decline in habit within workers over time — consistent with traditional economics models of preference based habit (e.g. Becker and Murphy (1988)). Alternatively, this decline could be the result of individual workers discontinuously jumping to a lower level of labor supply at different points in Phase 2, resulting in gradual aggregate decline — consistent with a traditional psychological view of habit based on automaticity (Webb et al., 2024). To shed light on these competing views of habit and explore how persistence may change over time in low-income settings, we examine worker's reactions to disruptions in labor supply.

Such disruptions are common in this setting. These disruptions arise from planned events such as festivals, weddings and personal obligations (for which it is common to return to one's village), as well as unplanned events such as illnesses. We test whether such shocks to labor supply have the ability to erode habit stock. Because reporting on disruptions is likely to be endogenous to attendance, we rely on a data-driven proxy for disruptions: a leave-one-out mean of residualized attendance at the stand-week level.[17] We classify weeks where *others'* stand attendance falls below the 25th percentile as a week when a shock occurs for a given individual. We then examine what happens to treatment effects in the weeks after this shock temporarily pulls workers out of the labor market.

Importantly, these shocks are well-spaced across Phase 2 due to the rolling enrollment

[16] Attendance is residualized against stand and calendar week fixed effects as well as baseline controls.

[17] The baseline specification uses baseline and Phase 1 data and residualizes against treatment assignment, stand fixed effects, phase fixed effects, stand × phase, treatment × stand, and workers' baseline controls.

both between and within stands A.1. The data-driven shocks also align well with observable disruptions: 78% of the shocks experienced by participants under the data-driven definition correspond to the hottest weeks of the year or major holidays.

We find that treatment effects dissipate, returning the worker to their previous equilibrium, after a shock pulls workers out of the labor market — the coefficient on Treatment x Post shock is similar in magnitude to the treatment effect but negative in Column 3 of Table 2.[18] The coefficient on the interaction term of Treatment and week in Phase 2 is close to zero and insignificant ($p = 0.617$) in Column 4, suggesting that there is no discernible decline in treatment effects over time outside of the decay which occurs via the shocks. We also explore the decline's temporal pattern in Columns 5 and 6 by separating estimate differential treatment effects for the week following the shock and all future weeks in Phase 2. The coefficients on the interaction term of Treatment with an indicator for 1 week post shock and Treatment with an indicator for 2+ weeks post shock are similar in magnitude, suggesting that treated workers revert back to the old equilibrium immediately in the week following a shock and then maintain the previous level of labor supply over the remainder of the period.

As shown in Appendix Tables A.8 and A.9, these effects are robust to a wide range of specifications. For reference, we replicate the primary specification in Column 1. Column 2 predicts attendance using post-double Lasso selected controls. Columns 3 and 4 use alternative thresholds to proxy disruptions. Column 5 restricts draws on only baseline data to predict labor supply in Phase 2. Columns 6 and 7 use rolling averages of 4 and 7 days rather than a calendar week as the period of evaluation for a disruption. In all cases, the coefficient are quite stable: the coefficient on Treatment ranges from 0.697 to 0.833 (main specification: 0.791) and the coefficient on the interaction between Treatment and the disruption remains between -0.661 and -0.882 (main specification: -0.791). We also conduct a leave-one-out robustness check, re-estimating our main specification while excluding each labor stand in turn. Results are consistent across all permutations, indicating that no single stand is driving the findings (Appendix Figure A.2).

Automaticity. The immediate and sharp drop in labor supply following a disruption is inconsistent with traditional models of intertemporal complementarities in prefer-

[18]Interestingly, the negative impact of a disruption is much smaller in magnitude and non-significant if the disruption occurs during Phase 1 when habit is being reinforced via incentives (Appendix Table A.7).

ences which predict a more gradual evolution of labor supply. One common alternative approach to conceptualizing habit is through the lens of automaticity or a psychological default. Psychologists have traditionally viewed habit as a learned association between an action and a reward that may persist even after the reward is removed, typically via automaticity or system 2 thinking. While automaticity is difficult to measure by its nature, the typical approach to capturing automaticity is fairly straightforward: simply asking the individual about how automatic a behavior is.

To provide some positive evidence on this potential channel, we follow this approach and ask workers to respond to the statement "going to the stand is something I do without thinking" on a scale of strongly disagree (1) to strongly agree (5) during Phase 2. The timing of the vignette varied by worker within Phase 2 depending on staffing constraints across stands and the participant's enrollment timing.

We find that treated workers are significantly more likely to agree with this statement (Table 3, column 1), with a roughly 10% increase in their agreement score ($p = 0.043$). However, consistent with the evidence on disruptions, we find that this effect dissipates entirely if the treated worker is asked the question after having experienced a disruption (column 2). While only suggestive, these results are consistent with and provide additional evidence in support of the automaticity view of habit formation.

Alternative drivers of state-dependence. In the previous section, we documented persistence in labor supply among treated workers even after incentives for attendance were removed. However, this state-dependence could be driven by a number of factors beyond habit formation including learning, fixed costs, or habit formation in consumption. Below, we provide evidence against these channels below as well as documenting a lack of general equilibrium effects which could confound the estimates.

Learning. Timely and more frequent attendance at the stand during Phase 1 for Treatment participants could facilitate learning on either side of the market. Participants may learn about the stand and update their beliefs about the relationship between arrival time and job finding probability. Alternatively, employers may identify "good" workers at the stand, increasing the return to attendance for treated workers. But, for this learning on either side of the market to influence the workers decisions about attendance, they must notice a difference in the job finding probability that would motivate a change in behavior.

We test for changes in beliefs about job finding probabilities by asking participants from both arms about the likelihood of finding work by asking "how many days of work would you find if you came to the stand everyday at 8 a.m.?"[19] Consistent with their many years of experience, at baseline workers are very well calibrated on job finding probabilities. For example, workers believe the probability of finding a job if one arrives by 8 a.m. is 79.6% while the true value is 77.2%. Similarly at 9 a.m. workers report a job finding probability of 54.8% while the true probability is 54.2%. Second, using the same elicitation method, we fail to detect any significant differences in expected job find probability by treatment status in Phases 2 and 3: the coefficient on Treated is small but negative and statistically insignificant (Table 4). Additionally, learning could not explain why treatment effects would disappear following a brief disruption to attendance.

In short, these results suggest that little, if any, learning occurs that would impact attendance. This is perhaps unsurprising given that the average worker in our study has been attached to the stand for over a decade and that learning from the employer side is challenging: stands typically have hundreds of workers who attend irregularly and employers frequent multiple stands, making it difficult to notice an increase of roughly one day every other week in attendance for any given worker.

Fixed costs. Another potential driver of state-dependence beyond habit formation is fixed costs. Specifically, treated participants may re-arrange their schedules and adjust their morning commitments (e.g., 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 adjustments to morning commitments could persist and remain in place throughout Phase 2. We explore this possibility by asking participants to report their time use in the morning. Figure 5a shows the distribution of participants who typically carry out each of the activities listed in the morning, by treatment status. There is no clear pattern of Treated participants altering or cutting 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 potential fixed cost is the method by which one commutes. However, 99%

[19] This question was originally asked out of 7 days. But, because it is extremely rare to work on Sunday, we later shifted to a 6 day time frame. We calculate the expected rate for all individuals by dividing by the appropriate number of days.

frequent only one stand and roughly 70% of participants commute by foot, bike, or motorbike with an average commute time of 10-15 minutes. The very local and simple nature of the commute makes changes in commuting unlikely.

Finally, another possibility is that Treated participants pay a fixed cost to adopt a technology that enables them to be at the stand more regularly and on time during Phase 1 – e.g., learning to use an alarm clock — and they continue to use them throughout Phase 2. Figure 5b shows that both groups report similar rates of usage of an alarm to wake up in the morning, with a slightly lower point estimate for usage among the Treated workers.

Of course, altering one's routine could itself be part of what enables a habit to form, rather than a confound to it. None the less, we find no evidence for persistence being driven by paying a fixed cost to alter one's morning routines among Treated participants among a variety of possible changes.

Consumption habit. An additional possibility is that the persistence does reflect habit, but that the habit is not in labor supply, but in consumption. Treated participants higher labor supply in Phase 1 could lead them to increase their consumption correspondingly, forming a habit of high consumption over time. Then, after the incentives are removed, treated participants may continue to work more to finance this higher desired level of consumption.

To examine this possibility, recall that Control participants were randomly matched with Treated participants and were provided matching payments *unconditionally* during Phase 1. We leverage this variation in income provided to the Control group to test whether Control workers who received higher Phase 1 payments exhibit higher labor supply in Phase 2, as a consumption habit would predict. Importantly this variation is substantial: the standard deviation of total Phase 1 consumption is 54.7% of the mean. As shown in Table A.4, Control workers receiving higher incentives during Phase 1 do not attend more during Phase 2 (point estimate = -0.07, p-value = 0.732) suggesting consumption habit is not a primary mechanism driving persistence.

Potential confound: General equilibrium effects? The increase in labor supply induced among the treated participants opens the door to potential concerns around general equilibrium effects which could confound the estimates. We work to avoid this concern first through the design of the study, with only 2-7% of individuals treated at each stand (Table A.5). We also test directly for such effects, but find no significant differences across stands with higher or lower percentages of treated individuals (Table A.6). Finally, general equilibrium effects are inconsistent with the analysis of disruptions and their effects on persistence.

Taken together, these pieces of evidence suggest that persistent increases in labor supply in Phase 2 are driven by *internal* changes, consistent with a habit driven by automatic behaviors.

6 Evidence from Other Datasets

Following roughly two months of habituation, we find persistence in labor supply that lasts for a comparable duration. Other papers have found evidence of state-dependence in domains outside of labor over shorter horizons (Byrne et al., 2022). To explore whether habit formation in labor supply is a more general phenomena and could also occur over shorter time horizons, we conduct two additional analyses which examine the intertemporal elasticity of labor supply over a very short horizon of two days. Specifically, we leverage random variation in the incentive to apply effort at a job generated by two RCTs. In the first study, Kaur et al. (2015) vary the incentives to provide additional labor on a given day among full-time Indian data-entry workers over the course of a year. In the second study, Carranza et al. (2024) have a daily piece rate randomization among full-time factory workers processing cashews in Côte D'Ivoire.

In both studies, we examine whether incentives to work hard **yesterday** influence output **today**, and if so, whether those effects are positive or negative. We find that the lagged incentives have a positive effect on output today (Table 5). The effects are significant and economically meaningful. For example, in Carranza et al. (2024) a 1% increase in the piece rate yesterday results in a 1.1% increase in output today (p = 0.031). Similarly, in Kaur et al. (2015), having a high output target imposed yesterday increases output today by 0.9% (p = 0.010). These results provide additional evidence that the intertemporal elasticity of labor supply can be positive over both short and longer durations and that the effects may be applicable to a range of settings beyond those directly studied in the experiment.

7 Implications for the Labor Market

These results raise a natural question: if habit formation in labor supply is real but easily dissipated, what are its implications for low-income labor markets, where shocks and disruptions are common? In the remainder of the paper, we present suggestive evidence on this question, examining how habituation affects workers' willingness to take up less flexible job contracts, and how employers perceive and respond to habituated labor supply as well as how they adjust business operations in response to irregular labor supply.

Desire for flexibility. Beyond the level of labor provided, a key feature of more regular work is that a worker must typically be willing to work on the schedule preferred by the employer in order to facilitate more complex production. Yet, many workers maintain a strong desire to control their schedules, resulting in high absenteeism and turnover (Adhvaryu et al., 2024; Blattman and Dercon, 2018). Similarly, at baseline, workers in this context also express a reluctance to take up more regular work — only 33% of participants say they are "likely" or "very likely" to take up a long term job if it were offered to them, typically cite a desire for flexibility as the reason for this choice (Figure 6a).

This desire for flexibility is well-known to employers. To document this, we survey 167 recruiters who typically frequent labor stands to hire workers. The modal recruiter in the survey sample attends the stand 5 days per week and hires workers for multiple roles and multiple employers. When asked to predict what fraction of days a worker on a 10-day contract would not attend work, the median response is 20% and the mean response is 27.5% (Figure 7). This irregularity has a variety of costs. Recruiters adjust by hiring 30% more workers than they expect to need for a job. Additionally, many 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 (Figure 8). Finally, "contracts" are purposefully short: the modal contract duration (40% of contracts) is a single day and only 22% of employers offer contracts lasting one week.

Habituation and willingness to forgo flexibility. Habituation to more frequent (i.e. more regular) labor supply has the potential to alter not only the amount of labor provided, but also a worker's willingness to supply labor on the employer's schedule rather than their own. To examine whether habituation changes the kinds of jobs workers are willing to take we conduct several supplementary exercises in Phase 2 (after incentives are removed) and summarize these findings in Table 6.

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 takes any leaves. It is worth highlighting that interest in the 6-day job with penalty is low — consistent with the earlier preference for flexibility, only 15% of Control participants choose this over status quo. However, treated 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 an incentive compatible test in which we ask workers to make choices between two options in each of two different scenarios. In the first scenario, 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 of our choosing for a Rs. 10 premium. We choose 2 days to ensure the days of attendance are inframarginal for the strong majority of workers such that overall attendance is unlikely to confound the results. In the second scenario, workers choose between a fixed amount of money for sure or a contract where they earn a Rs. 20 premium if they attend the stand on two pre-set (inflexible) days. One of the two scenarios is randomly implemented, making these choices incentive-compatible. We find that Treated participants are 10.6% ($p = 0.082$) more likely to choose the pre-set (inflexible) contract (Column 2). Although endogenous, treated workers are also 41% more likely to achieve the higher paying outcome conditional on selecting the inflexible contract.

To provide additional evidence these effects are associated with the treatment and the additional regularity in labor supply, we examine whether take-up of the inflexible work contracts falls after a shock temporarily pulls workers out of the labor market. Consistent with the disruption effects in the level of labor supply, the coefficient on Treatment x Post shock is large and negative in Column 3 of Table 6, shifting Treated workers back to their previous level of take-up after a disruption to labor supply. Taken together, these results indicate that habituation alters not only the level of labor supply, but also workers' willingness to commit to inflexible, employer-set schedules — and that this shift is also contingent on sustained regularity without disruptions.

Beliefs regarding habit formation. Employer beliefs about state dependence in labor supply are also central to the operation of the labor market, with potential implications for contract duration and duration dependence in unemployment. To shed light on employer beliefs regarding forces driving labor irregularity, we conduct an incentivized survey with 115 employers. 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 as a benchmark for seasonal patterns, and ask employers to predict the number of Treated participants attending at those time points. To incentivize thoughtful replies, we provide a large monetary prize ranging from Rs. 1000 - 5000 (wages for roughly 1.2 to 6 days of work) to the top three most accurate employers.

Table 7 summarizes findings from this incentivized survey. Columns 2 and 3 summarize the total number of Control and Treated participants (out of 100) respectively attending the stand at specific time points after the end of Phase 1, as indicated in Column 1. As summarized in Column 4, the median employer is able to correctly anticipate treatment effects of the intervention on labor supply following Phase 1. Notably, employers not only anticipate the state-dependence, but also decay in these effects — they report that treated participants are 19.6% more likely to attend at 2 weeks, 11.1% more likely to attend at 2 months, closely mirroring the actual treatment effects with slight overoptimism about durability over longer horizons. This pattern of employer beliefs is notable in light of the broader debate on duration dependence in unemployment: while adverse selection is one commonly cited explanation for why longer unemployment spells are associated with lower re-employment prospects (e.g., Mueller and Spinnewijn (2025)), our results suggest an additional channel: employers may also perceive a decay in the "habit stock" or regularity of labor supply itself.

Willingness to pay for workers with habit stock. The combination of employer anticipation of habit and the costs associated with irregularity suggest that employers should be willing to pay for habituated workers. To test this hypothesis, we conduct an incentivized survey with 69 employers to estimate their willingness to pay to hire Treated participants, describing our experiment to employers as a worker “training” intervention. Employers are entered into a lottery for a wage-subsidy voucher, usable if the subsidized worker arrives at the stand by 8:30,[20] and we vary the relative subsidy offered for a trained versus untrained worker before the lottery is drawn to elicit employ-

[20]Employers are told they have a 1 in 10 chance of winning the lottery.

ers' willingness to pay. Starting from a baseline subsidy of INR 300 for either worker, we ask employers to state their preferred worker, then reduce the trained worker's subsidy in increments of INR 25-50 (holding the untrained worker's subsidy fixed at INR 300) and again elicit employers' choice.

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.[21]

Other changes to employer behavior. While the willingness-to-pay exercise captures employers' valuation of regularity in monetary terms, we also elicit employers' own unprompted views on how worker regularity might change their hiring and business practices more broadly. When asked "if such regular workers existed, would this change anything else about how you offer work?", recruiters state they would be more likely to provide training (30%) and more able and willing to take up new 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.

Discussion. Although not definitive, these results suggest that the consequences of state-dependence may be broad, especially for low-income labor markets in which it is difficult for workers to maintain habit-stock. Although it is difficult to provide a concrete estimate of the welfare benefits (or costs) of changes in habit to workers themselves, it is clear that employers find irregular labor supply costly in a number of ways. These costs are both immediate and direct (e.g. having to travel to the labor stand, hire a new worker, and orient them to the worksite if a "contracted" worker doesn't arrive as planned) and potentially long-term (e.g. lack of investment in training, inability to take on new types of work). In addition, they suggest that the occurrence of spot labor markets rather than longer-term contracting may be an endogenous consequence of workers' lack of habit, which is in turn potentially the result of facing a highly disrupted environment, making structural shifts more challenging.

[21]While there is no reason to expect the "training" to affect on-the-job productivity, we cannot rule out that employers' willingness to pay also reflects perceived productivity benefits, in addition to beliefs about a worker's likelihood of attending.

8 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 casual workers in urban India. We find that a 26% increase in labor supply during the first two months of the study (Phase 1, with incentives) 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. While there are a number of potential drivers of this state-dependence, we do not find evidence for traditional explanations beyond habit such as learning on either side of the market or fixed costs.

In addition, we find that disruptions 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 disruptions, we find suggestive evidence that the effects can persist—at least for some workers—for up to 5 months. This finding is inconsistent with what one would expect under traditional intertermporal preference complementarities (e.g. Becker and Murphy (1988)). Rather, it more closely matches what one might expect under conceptualizations of habit in the psychology literature — consistent with our suggestive survey evidence for increased automaticity.

Habit formation has important potential equilibrium consequences for the labor market. The irregular patterns of work experienced by workers in low-income countries can hinder the accumulation and maintenance of a labor supply "habit stock". This has implications beyond just the quantity of labor supplied: we find that workers' willingness to take on longer and less flexible employment contracts, which place a premium on regular attendance changes alongside the level of labor supply. Further, 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. This willingness to pay reflects the reported costs associated with irregularity, including both short term costs related to adjustments to hiring practices to cope with sporadic labor supply and long-term costs as they forgo opportunities and reduce investment.

Our findings also speak to policy design around unemployment and job training. If habit stock erodes during unemployment, workers may need more than income replacement (e.g., unemployment insurance) to maintain labor force attachment, policies may also need to safeguard the habit of regular work itself. Relatedly, our results speak to the design of vocational and job training programs. A growing body of work has emphasized that schooling may confer benefits beyond skill acquisition by instilling habits of regularity and punctuality (Bowles and Gintis, 1976). This suggests that training programs may benefit not only from imparting skills, but also from explicitly cultivating and rewarding regular attendance. More speculatively, the growing prevalence of gig economy jobs — characterized by flexible, on-demand work rather than regular schedules — could have long-run consequences for workers' transitions into stable formal employment, to the extent that such work fails to build the habit stock that regular attendance appears to require. We view this as a conjecture rather than a direct implication of our results, worth exploring in future work.

These findings also connect to historical accounts of the industrial revolution, where habituation to regular attendance through practices such as formal schooling and factory lock-outs was viewed as critical for the transition from informal to factory work (Pollard, 1963; Thompson, 1967; Clark, 1994). More broadly, our results raise the possibility that the spot-market structure characterizing many low-income labor markets today may itself be an endogenous consequence of irregular labor supply, which is in turn shaped by the frequent shocks associated with poverty. Our findings open the door to the possibility that this mechanism — the shocks associated with poverty hampering labor supply, and irregular labor supply in turn sustaining spot-market structures — may remain relevant for understanding the barriers to structural transformation in contemporary developing economies.

References

Acland, Dan and Matthew R Levy, "Naivete, Projection Bias, and Habit Formation in Gym Attendance," *Management Science*, 2015, (October 2018).

Adhvaryu, Achyuta, Jean francois Gauthier, Anant Nyshadham, and Jorge Tamayo, "Absenteeism, Productivity, and Relational Contracts Inside the Firm," *Journal of the European Economic Association*, 2024, (1), 128–163.

Allcott, Hunt, Luca Braghieri, Sarah Eichmeyer, and Matthew Gentzkow, "The welfare effects of social media," *American economic review*, 2020, *110* (3), 629–676.

Becker, Gary S and Kevin M Murphy, "A Theory of Rational Addiction," *Journal of Potical Economy*, 1988, *96* (4), 675–700.

Blattman, Christopher and Stefan Dercon, "The impacts of industrial and entrepreneurial work on income and health: Experimental evidence from Ethiopia," *American Economic Journal: Applied Economics*, 2018, *10* (3), 1–38.

Bordalo, Pedro, Giovanni Burro, Nicola Gennaioli, Gad Nacamulli, and Andrei Shleifer, "Ads as Cues," 2025.

Bowen, William G and T Aldrich Finegan, *The economics of labor force participation*, Princeton University Press, 1969.

Bowles, Samuel and Herbert Gintis, "Schooling in capitalist America," 1976.

Breza, Emily and Supreet Kaur, "Labor Markets in Developing Countries," *Annual Review of Economics*, 2025, *17.*

Bruhin, Adrian, Lorenz Goette, Adrian Roethlisberger, Alexander Markovic, Regula Buchli, and Beat M Frey, "Call of duty: the effects of phone calls on blood donor motivation," *Transfusion*, 2015, *55* (11), 2645–2652.

Buyalskaya, Anastasia, Hung Ho, Katherine L Milkman, Xiaomin Li, Angela L Duckworth, and Colin Camerer, "What can machine learning teach us about habit formation? Evidence from exercise and hygiene," *Proceedings of the National Academy of Sciences*, 2023, *120* (17), e2216115120.

Byrne, David P, Lorenz Goette, Leslie A Martin, Lucy Delahey, Alana Jones, Amy Miles, Samuel Schob, Thorsten Staake, Verena Tiefenbeck et al., "The habit forming effects of feedback: Evidence from a large-scale field experiment," *Available at SSRN 3974371*, 2022, *4* (9).

Card, David, Raj Chetty, and Andrea Weber, "The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?," *American Economic Review*, 2007, *97* (2), 113–118.

Carranza, Eliana, Aletheia Donald, Florian Grosset, and Supreet Kaur, "The social tax: Redistributive pressure and labor supply," Technical Report, National Bureau of Economic Research 2024.

Cefala, Luisa, Pedro Naso, Michel Ndayikeza, and Nicholas Swanson, "Under-training by Employers in Informal Labor Markets: Evidence from Burundi," 2024.

Charness, Gary and Uri Gneezy, "Incentives to Exercise," *Econometrica*, 2009, *77* (3), 909–931.

Choper, Joshua, Daniel Schneider, and Kristen Harknett, "Uncertain time: Precarious schedules and job turnover in the US service sector," *ILR Review*, 2022, *75* (5), 1099–1132.

Clark, Gregory, "Factory Discipline," *Journal of Economic History*, 1994, *54* (1), 128–163.

Collins, Daryl, Jonathan Morduch, Stuart Rutherford, and Orlanda Ruthven, *Portfolios of the Poor: How the World's Poor Live on $2 a Day*, Princeton: Princeton University Press, 2009.

Donald, Aletheia and Florian Grosset, "Complementarities in Labor Supply," 2024.

Ganong, Peter, Pascal Noel, Christina Patterson, Joe Vavra, and Alex Weinberg, "Earning Instability," Technical Report 2025. Working Paper.

Goraya, Sampreet Singh, Nandita Krishnaswamy, Suanna Oh, and Yogita Shamdasani, "Worker Absenteeism and Firm Outcomes: Evidence from the Indian Manufacturing Sector," 2025.

Hussam, Reshmaan, Atonu Rabbani, Giovanni Reggiani, and Natalia Rigol, "Rational Habit Formation: Experimental Evidence from Handwashing in India," *American Economic Journal: Applied Economics*, January 2022, *14* (1), 1–41.

Hyslop, Dean R, "State dependence, serial correlation and heterogeneity in intertemporal labor force participation of married women," *Econometrica*, 1999, *67* (6), 1255–1294.

ILO, *Women and men in the informal economy: a statistical picture*, third ed., Vol. 28 2018.

James, William, "The principles of psychology New York," *Holt and company*, 1890.

_ , *Talks to Teachers on Psychology and to Students on Some of Life's Ideals*, Vol. 12, Harvard University Press, 1983.

Jr, William T Waller, "The concept of habit in economic analysis," *Journal of economic issues*, 1988, *22* (1), 113–126.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan, "Self-Control at Work," *Journal of Political Economy*, 2015, *123* (6), 1227–1277.

Kroft, Kory, Fabian Lange, Matthew J Notowidigdo, and Lawrence F Katz, "Long-term unemployment and the Great Recession: the role of composition, duration dependence, and nonparticipation," *Journal of Labor Economics*, 2016, *34* (S1), S7–S54.

Laibson, David, "A cue-theory of consumption," *The Quarterly Journal of Economics*, 2001, *116* (1), 81–119.

Lyu, Junru, "Deferred Burden: Habit Formation and the Hidden Costs of Temporary Debt Relief," 2025.

Mazar, Asaf and Wendy Wood, "Defining habit in psychology," *The psychology of habit: Theory, mechanisms, change, and contexts*, 2018, pp. 13–29.

Mehrotra, Santosh and Jajati K Parida, "India's employment crisis: Rising education levels and falling non-agricultural job growth," 2019.

Morduch, Jonathan and Rachel Schneider, *The Financial Diaries: How American Families Cope in a World of Uncertainty*, Princeton: Princeton University Press, 2017.

Mueller, Andreas I and Johannes Spinnewijn, "The Nature of Long-Term Unemployment: Predictability, Heterogeneity, and Selection," *Journal of Political Economy*, 2025, *133* (12), 3846–3902.

Pollard, Sidney, "Factory Discipine in the Industrial Revolution," *Economic History Review*, 1963, *16* (2), 254–271.

Rabin, Matthew, "Healthy Habits: Some Thoughts on The Role Of Public Policy in Healthful Eating and Exercise Under Limited Rationality," 2011.

Royer, Heather, Mark Stehr, and Justin Sydnor, "Incentives, commitments, and habit formation in exercise: Evidence from a field experiment with workers at a Fortune-500 company," *American Economic Journal: Applied Economics*, 2015, *7* (3), 51–84.

Thompson, E. P., "Time, Work-Discipline, and Industrial Capitalism," *Past and Present*, 1967, *38*, 56–97.

Valenzuela, Abel, "Regulating day labor: Worker centers and organizing in the informal economy," *The informal city: Settings, strategies, responses*, 2014, pp. 261–276.

Webb, Ryan, Jessica Fong, Asaf Mazar, Julia Levine, Alexandra Steiny Wellsjo, Olivia R Natan, Clarice Zhao, Phillippa Lally, Sanne de Wit, John Odoherty et al., "Integrating Neuro-Psychological Habit Research into Consumer Choice Models," 2024.

_ , _ , Peter Landry, Julia Levine, Alex Steiny Wellsjo, Olivia Natan, Asaf Mazar, Clarice Zhao, Phillippa Lally, Sanne de Wit et al., "Integrating neuro-psychological habit research into consumer choice models," *International Journal of Research in Marketing*, 2025.

Wellsjo, Alex Steiny, "Simple Actions, Complex Habits: Lessons from Hospital Hand Hygiene," 2022.

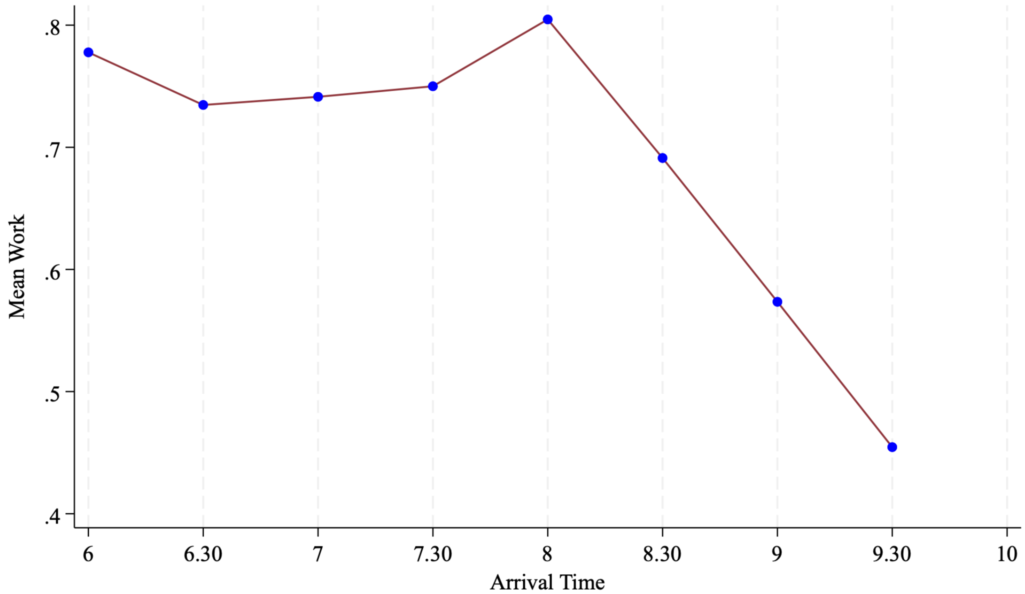
Wood, Wendy and Dennis Rünger, "Psychology of habit," *Annual review of psychology*, 2016, *67*, 289–314.

_ , Leona Tam, and Melissa Guerrero Witt, "Changing circumstances, disrupting habits.," *Journal of personality and social psychology*, 2005, *88* (6), 918.

Yagan, Danny, "Employment Hysteresis from the Great Recession," *Journal of Political Economy*, 2019, *127* (5), 2505–2558.

Figures

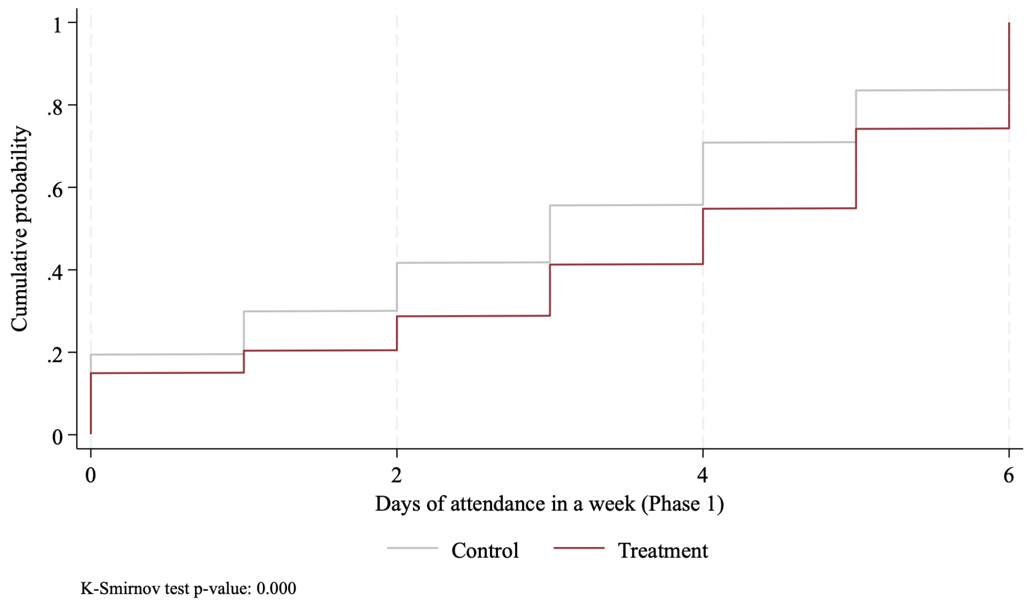
Figure 1: Probability of Finding a Job by Arrival Time



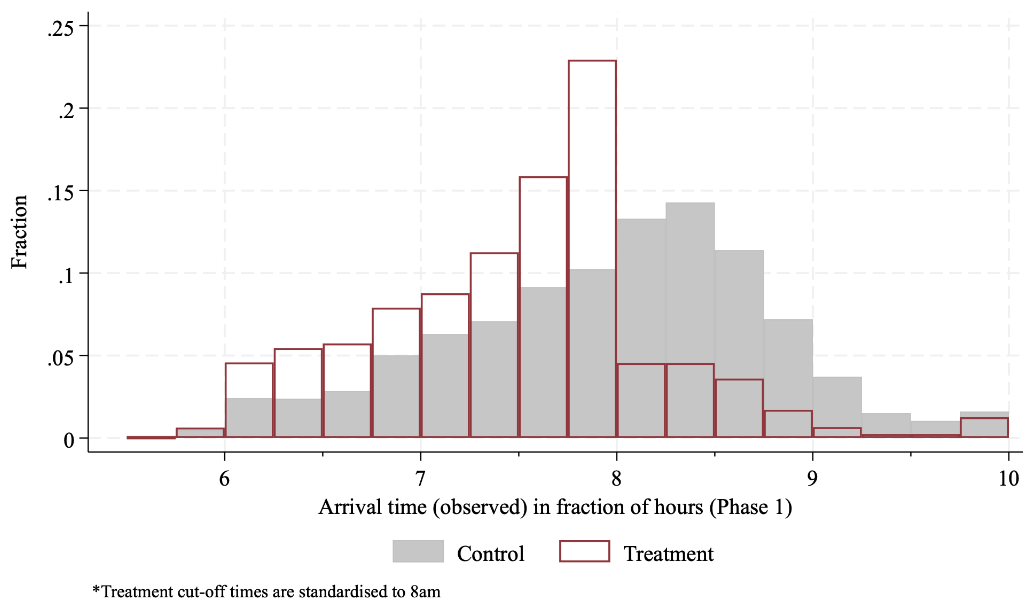
Notes: This figure plots the relationship between a worker's time of arrival at the stand and the probability of securing work using baseline data. Arrival time is captured by surveyors positioned at each stand starting at 6 a.m. Employment status for the day is self-reported by the worker via a survey. To allow for a full day of work, stands typically operate from 6 a.m. to 10 or 10:30 a.m., though few workers arrive after 9:30 or 10 a.m.

Figure 2: Treatment Effect on Attendance and Arrival Time in **Phase 1**

(a) Weekly Attendance



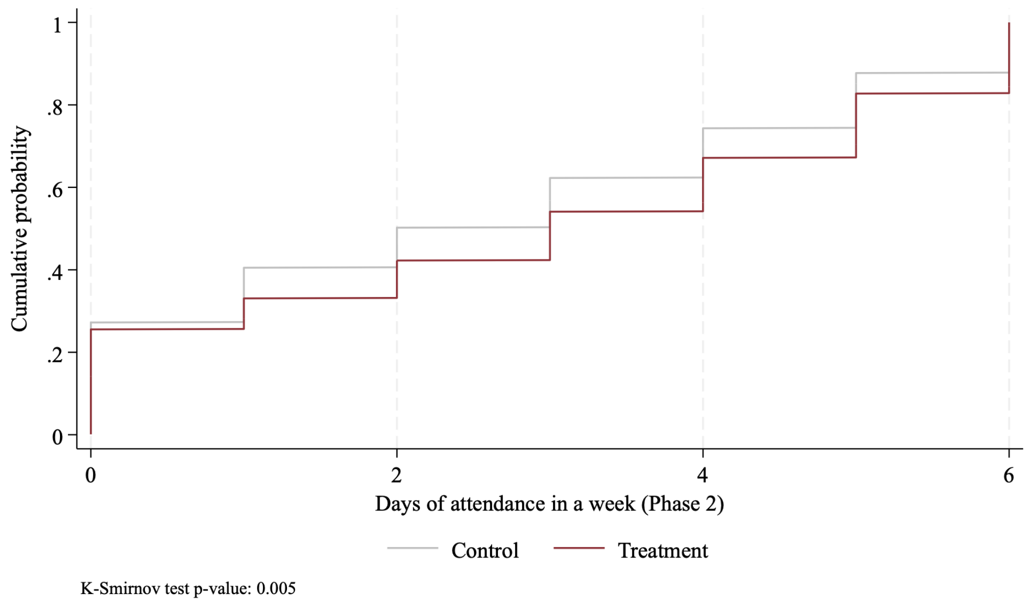
(b) Arrival Time



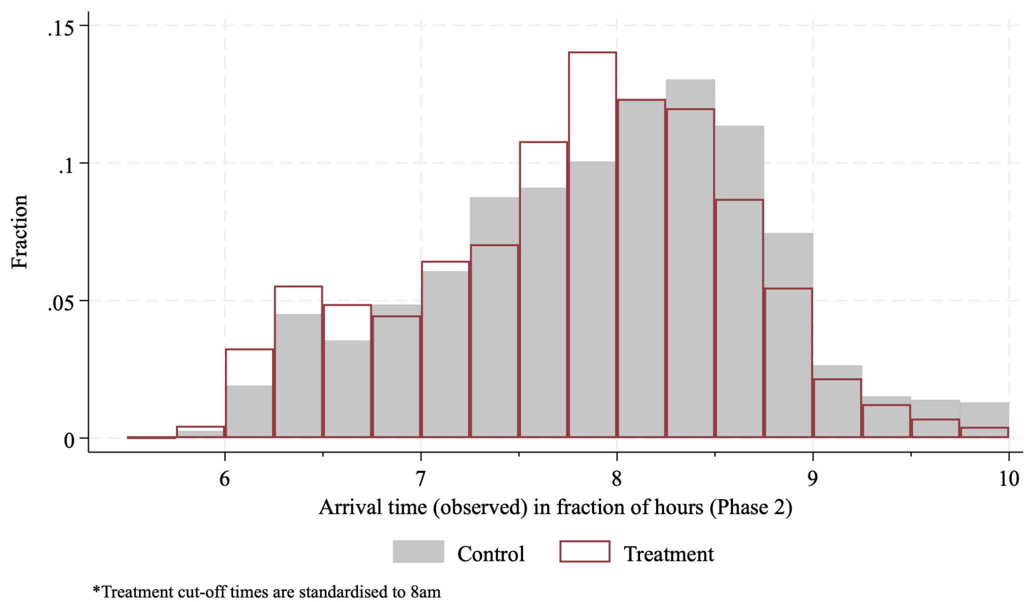
Notes: This figure presents data from Phase 1, during which Treated participants received incentives for timely arrival and Control participants received matching unconditional payments. Panel A plots the cumulative distribution of weekly stand attendance by experimental arm. Panel B plots the distribution of arrival times by arm; because a few stands had peak labor demand periods slightly earlier or later than 8 a.m., the incentivized arrival time was adjusted accordingly for those stands. To facilitate comparison across stands, Panel B normalizes each stand's incentivized arrival time to 8 a.m.

Figure 3: Treatment Effect on Attendance and Arrival Time in **Phase 2**

(a) Weekly Attendance

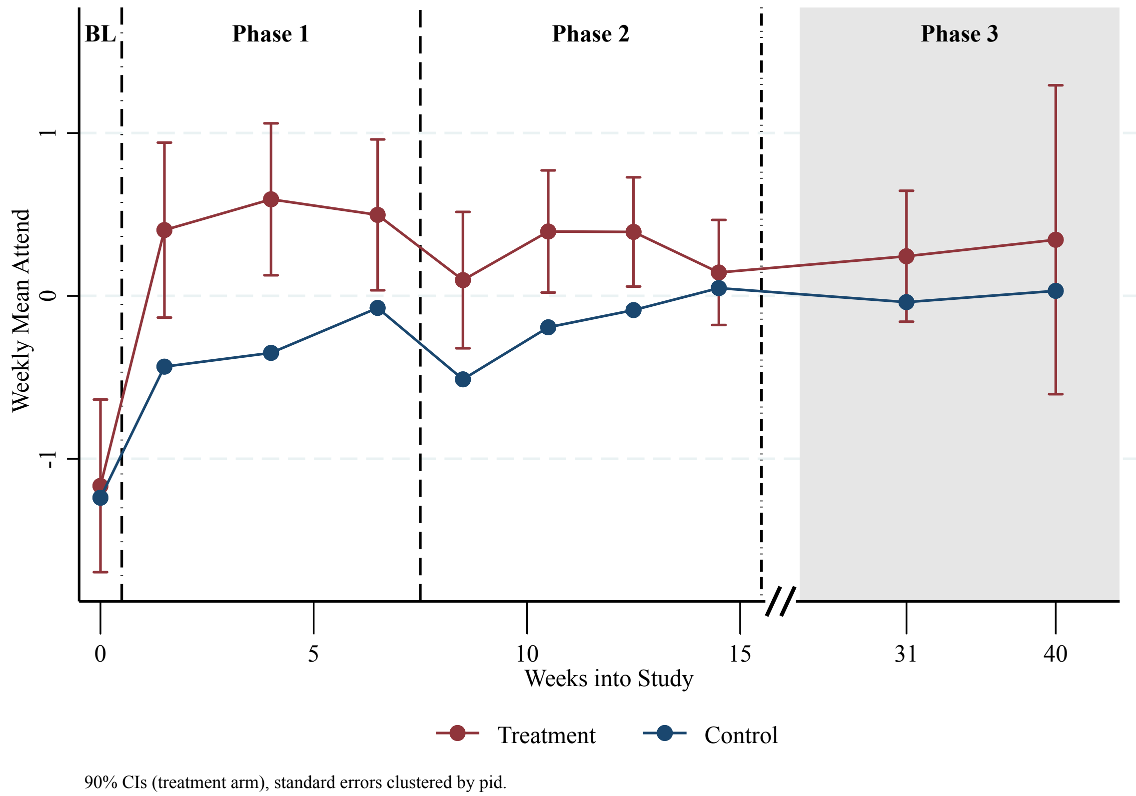


(b) Arrival Time



Notes: This figure presents data from Phase 2, after incentive and matching payments are removed. Panel A plots the cumulative distribution of weekly stand attendance by experimental arm. Panel B plots the distribution of arrival times by arm; because a few stands had peak labor demand periods slightly earlier or later than 8 a.m., the incentivized arrival time was adjusted accordingly for those stands. To facilitate comparison across stands, Panel B normalizes each stand's incentivized arrival time to 8 a.m.

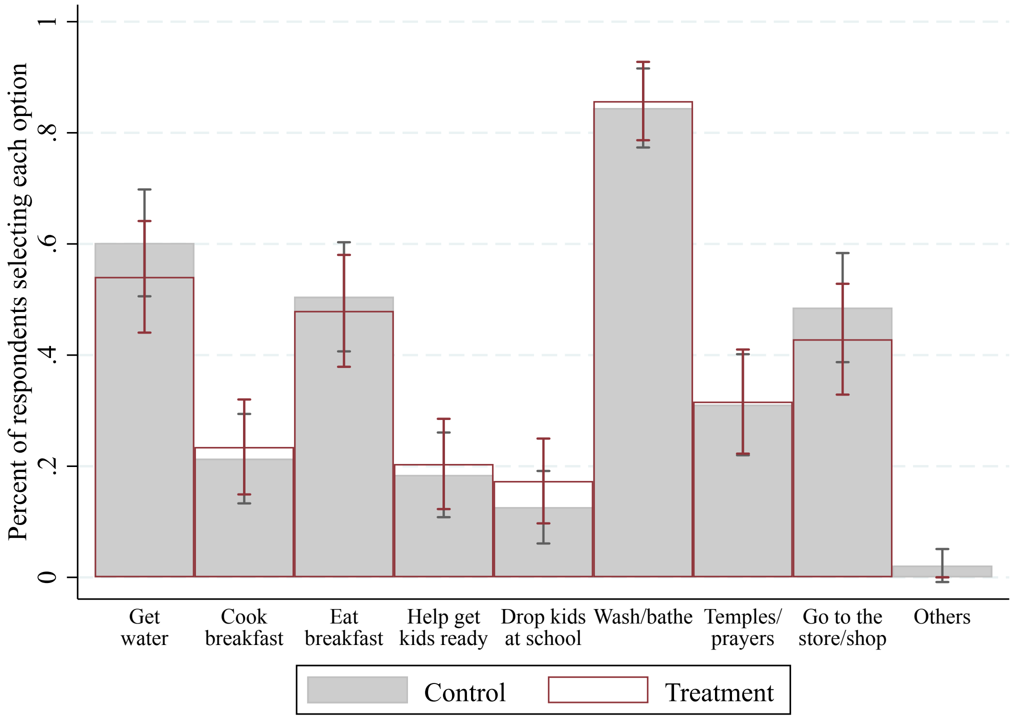
Figure 4: Attendance Over Time



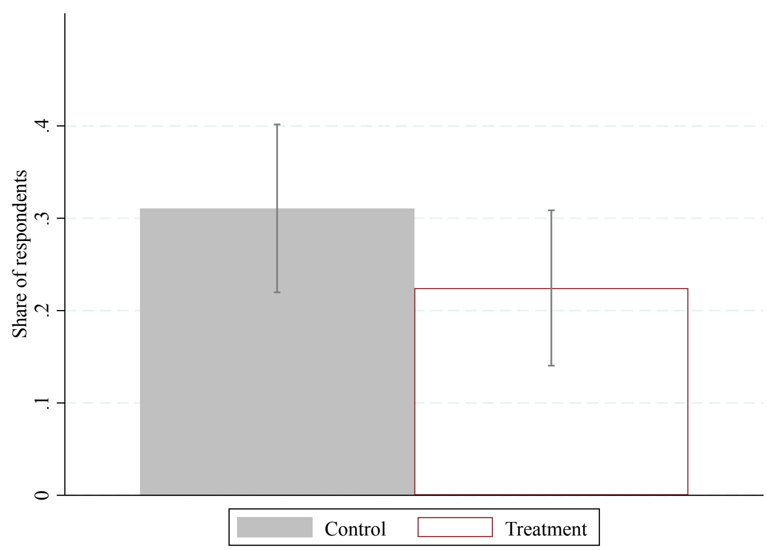
Notes: Figure plots residualized weekly attendance (controlling for baseline controls and stand, strata, and calendar week fixed effects) for each phase of the study, by treatment status. Error bars are 90% confidence intervals with worker level clustering. Phase 3, shaded in gray, uses a condensed time scale as indicated on the x-axis. Given the reduced frequency of data collection in Phase 3, we use only two bins in that phase, evenly dividing the first and second half of the phase.

Figure 5: Treatment Effect on Morning Routines During Phase 2

(a) Activities Done Between 5.30 a.m. and 9 a.m.



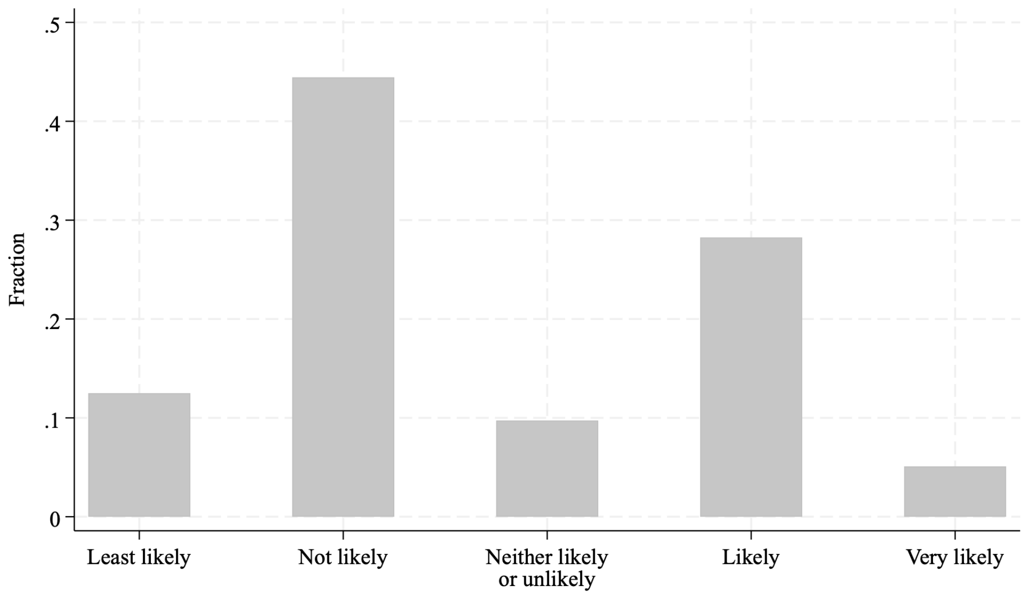
(b) Alarm Clock Usage



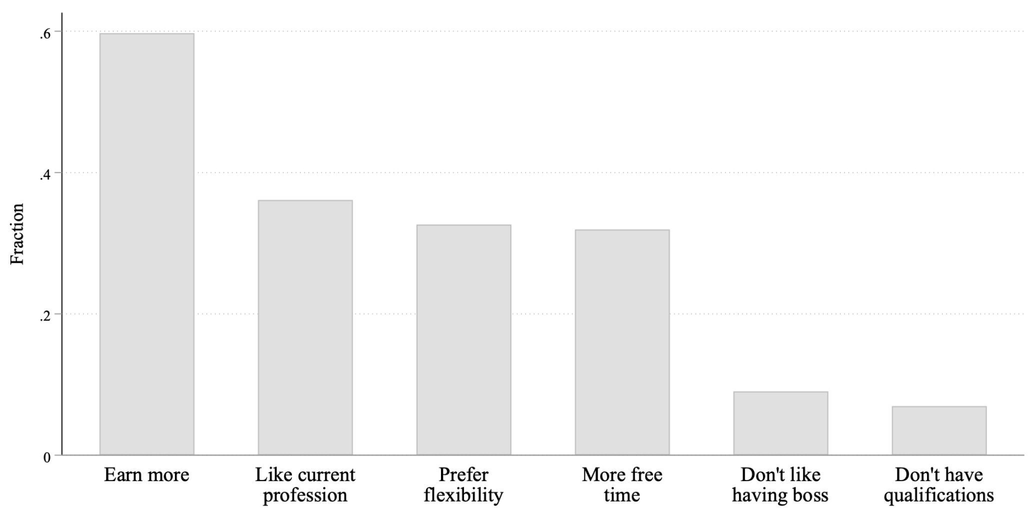
Notes: This figure presents data on morning routines in Phase 2 by treatment status to explore the role of fixed costs in generating the treatment effect. Both panels include 90% confidence intervals and use gray shading for the Control group and a red outline for the Treatment group. Panel A plots the fraction of workers who undertake a given action in the morning. Panel B reports the fraction of workers who use an alarm clock.

Figure 6: Job Preferences at Baseline

(a) Likelihood of Accepting a Long-term, Formal Job if Offered

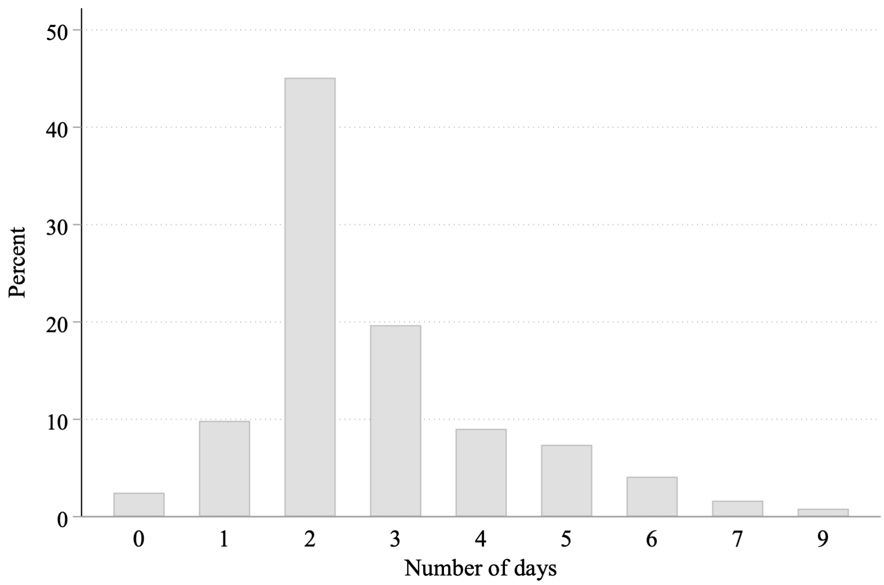


(b) Reasons Worker Prefer Casual Work to Stable Jobs



Notes: Panel A notes participant's responses to the question "If you were offered a long term job today, how likely would you be to take it?" at baseline. Conditional on answering "neither likely nor unlikely," "not likely," or "very unlikely", the participant was then asked why they would not want the stable job. Panel B reports these responses. Participants we allowed to select all answers that applied, such that responses sum to greater to 100%.

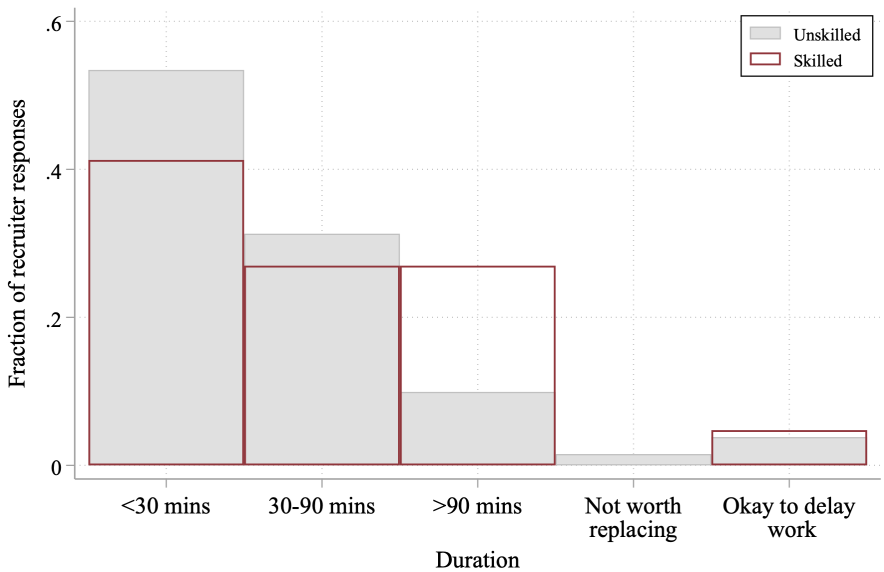
Figure 7: Predicted Worker Absenteeism



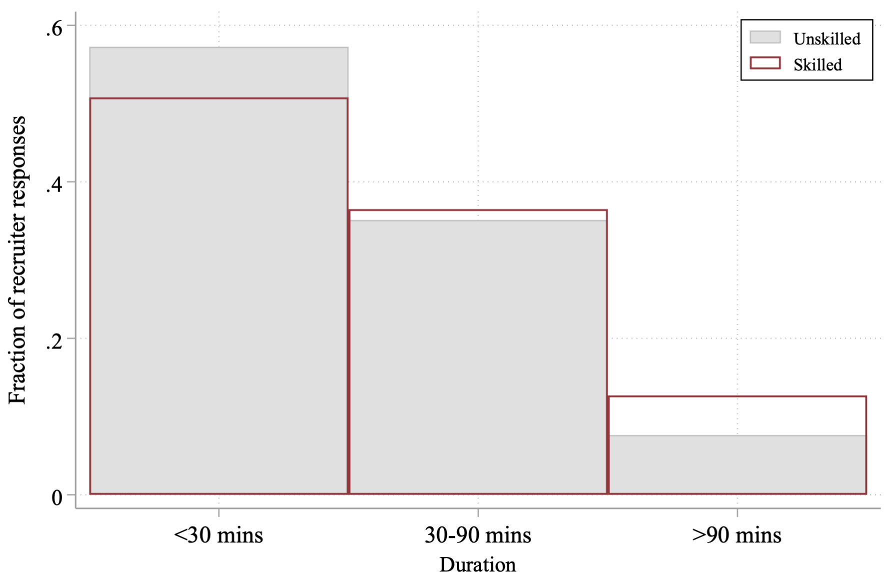
Notes: Figure summarize survey responses from 167 employers to the following question: “Suppose you hired one worker for a 10 day contract (so that the worker said he would come on all days when he took the job). Out of these 10 workdays, on how many days do you think the worker would be absent from work?"

Figure 8: Costs Incurred by Employers

(a) Time Taken to Find a Replacement Worker

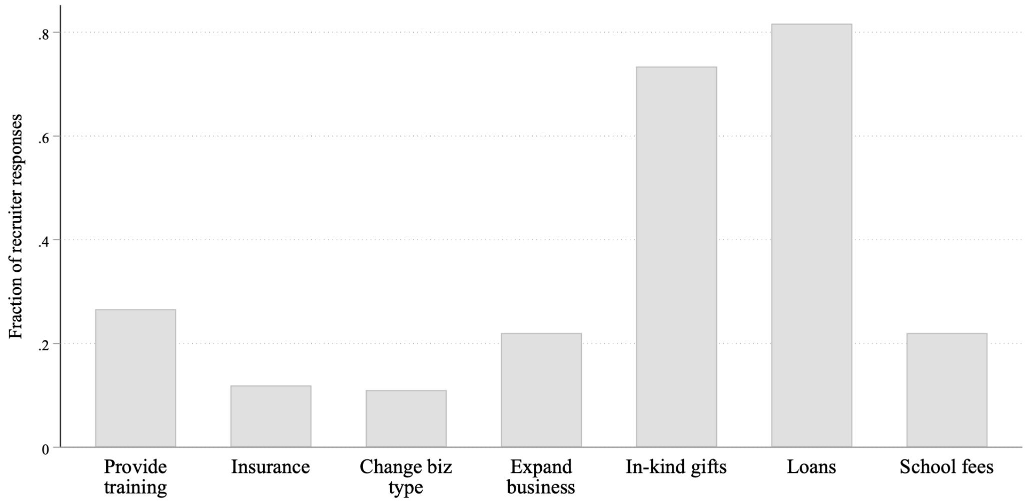


(b) Time Taken to Onboard a New Worker



Notes: Panel A summarizes responses from 167 employers to the question: "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 B displays the fraction of employers indicating a given amount of time for the question: "How much time do you typically spend helping a new worker understand what needs to be done and how to do it?" In both panels, responses are provided for unskilled workers in gray and skilled workers in red.

Figure 9: Potential Implications for Labor Market Structure



Notes: Figure summarizes responses by 167 employers to the following survey question: "If such regular workers existed, would this change anything else about how you offer work?"

Tables

Table 1: Labor Supply Effects

	Phase 1		Phase 2		
	(1) Attend by 8am	(2) Attend	(3) Attend by 8am	(4) Attend	(5) Worked (Total)
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	1409

Notes: This table reports treatment effects on attendance and employment in Phase 1 and Phase 2, at the worker-week level. In Columns 1 and 3, the dependent variable is the fraction of days a participant arrived before 8 a.m. (the incentive cutoff for Treated participants) as observed by surveyors at the stand, with non-attendance days coded as zero. In Columns 2 and 4, the dependent variable is days attended per week, also observed by the surveyor. In Column 5, the dependent variable is days worked per week, as self-reported by participants. 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 and baseline mean earnings at the worker level. Standard errors are clustered at the worker level.

Table 2: 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	-0.071			-0.034		-0.029
	(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: Columns 1-2 test for potential decay in treatment effect within Phase 2 and columns 3-6 estimate the effect of disruptions to attendance on treatment effects within Phase 2. Observations are at the worker-week level. The dependent variable is days attended per week, as observed by the surveyor. The week of the disruption is omitted from the regression at the worker-week level. Controls include strata, stand, week-in-phase, and calendar week fixed effects, as well as number of days where attendance is recorded (weekly), and baseline attendance rate baseline mean earnings at worker level. Standard errors are clustered at individual level in parentheses. p-values are reported in brackets, with clustering at the stand level in Columns 3-6.

Table 3: 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 labor disruptions 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, with higher values indicating greater agreement. Both columns control for strata and stand fixed effects. Column 2 additionally includes controls for the shock-week itself, a dummy for all weeks post-shock, and the interactions between these indicators and the treatment. Standard errors are clustered at the worker level.

Table 4: Perceived Job Finding Probability

	(1)	(2)
	Fraction of days	Fraction of days
Treatment	-0.00826	-0.0107
	(0.039)	(0.036)
	[0.833]	[0.766]
Survey date FE	No	Yes
Control mean	0.744	0.744
N: worker-survey	115	115

Notes: The table reports the estimated effect of treatment on participants' perceived job finding probability in Phase 2 and 3. For ease of interpretation, participants answer the question "How many days of work would you find, if you come to the stand everyday at 8 a.m.?". We then convert their response to percentage terms. A small number of participants responded to this survey twice due to an administrative error. We retain all data, apply analytical weights, and cluster standard errors by individual.

Table 5: 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: The table explores the short run inter-temporal elasticity of labor supply, using random variation yesterday to predict today's output. Columns 1 and 2 are from Kaur et al. (2015) on full-time data entry workers, where workers are randomly assigned high productivity targets, incentivizing hard work that day. The dependent variable in these columns is daily output. Columns 3-4 are from Carranza et al. (2024) where workers face a daily randomization of the piece rate offered for factory work. The dependent variable in these columns in log daily output.

Table 6: Willingness to Forgo Flexibility

	Contract job (1)	Fixed choice (2)	Fixed choice (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. The survey of Column 1 was administered separately from the survey for Columns 2 and 3, resulting in different sample sizes. Regressions include stand and strata fixed effects. Standard errors are clustered at individual level in parentheses, and p-values are in brackets.

Table 7: Employer Beliefs

Time Point	Control (Actual)	Treatment (Actual)	Employer Prediction (median)	Employer Prediction (% effect)
(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 eliciting beliefs regarding the impact of our intervention. Column 1 indicates 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 provides 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 increase 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 69 employers where we elicit willingness to pay for "trained" workers with habit stock. We describe the intervention and offer a wage subsidy voucher, varying the extent of the subsidy for the Treated ("trained") worker, relative to a constant Rs. 300 voucher for a control worker. The final row in the table indicates the percent of employers choosing the trained worker at that subsidy level.

Appendix Tables

Table A.1: Baseline Characteristics

	(1) Control Mean	(2) Treatment Mean	(3) Regression P-value
Age	42.45	42.63	0.60
	(9.52)	(9.30)	
No schooling	0.20	0.19	0.95
	(0.40)	(0.39)	
Has spouse/children	0.85	0.87	0.75
	(0.36)	(0.33)	
Years at stand	10.23	10.45	0.73
	(8.18)	(7.96)	
Years in current profession	16.35	15.91	0.76
	(10.48)	(9.56)	
Days attended stand	4.23	4.24	0.93
	(1.63)	(1.61)	
Days worked	4.91	4.92	0.96
	(2.17)	(2.43)	
Average daily wage (in rupees)	843.76	840.36	0.39
	(122.26)	(133.66)	
Total earnings (in rupees)	4162.05	4157.08	0.79
	(1986.35)	(2159.64)	
Observations	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 with robust standard errors.

Table A.2: Balance in Attrition

	(1) Formal Dropouts	(2) Informal Dropout
Treatment	0.0183	0.0164
	(0.022)	(0.036)
Control mean	0.0179	0.0804
N	225	225

Notes: This table presents a test of equality in dropout rates by experimental arm. Column 1 examines "formal dropouts," defined as telling the study staff they no longer wished to participate. Column 2 examines "informal dropouts," defined as not attending the stand for at least three consecutive weeks. Regressions include stand and strata fixed effects and robust standard errors. p-values are included in brackets.

Table A.3: Weekly Incentive Amount in Phase 1 (Nominal Rs.)

Variable	(1) Control Mean/(SE)	(2) Treatment Mean/(SE)	(1)-(2) Pairwise t-test P-value
Payment allocated	163.967	159.671	.460
	(4.136)	(4.087)	
N	784	791	1575

Notes: This table provides summary statistics on study payments during Phase 1. Both columns present baseline means and standard deviations of the payments accrued to participants by experimental arm, demonstrating similar levels and variation across the groups.

Table A.4: Habit Formation in Consumption?

	Attend	By 8
Incentive Paid	-0.0754	0.0603
	(0.220)	(0.190)
	[0.732]	[0.751]
Control Mean	2.576	1.250
N: worker-weeks	896	896

Notes: This table explores whether habit formation in consumption could be driving the observed results by leveraging random variation in payment to Control participants in Phase 1. We test whether Control participants receiving higher payments in Phase 1 have higher labor supply in Phase 2. To accomplish this, we regress attendance (column 1) and attendance before 8 a.m. (column 2) in Phase 2 on payments in Phase 1, instrumenting actual payments with earned payments to avoid endogeneity in payments received.

Table A.5: Stand Size and Treatment Intensity

Stand ID	Workers Approached	Finalized Stand Size	Assigned Treatment	Treatment Intensity
1	215	215	16	.0744
2	130	350	11	.0315
3	156	231	7	.0303
4	150	197	9	.0456
5	146	173	7	.0405
6	156	376	8	.0213
7	120	230	6	.0261
8	177	280	17	.0607
9	266	309	19	.0615
10	122	352	9	.0256
11	112	154	4	.0259

Notes: Stand size was estimated by combining time-series observations of the number of people at each stand (recorded every 15–30 minutes between 6:00 AM and 10:00 AM) with survival functions derived from baseline survey data. For each stand, individuals were divided into "early" (arrived by 8:00 AM) and "late" (after 8:00 AM) groups, each with distinct job-finding rates and exponential survival functions $S(t) = e^{-\lambda t}$ estimated from the reported time spent at the stand before finding or leaving without a job. The number of new arrivals in each time interval was inferred from observed counts and these survival functions, and total stand size was the sum of new arrivals adjusted by a correction factor accounting for non-daily attendance. Treatment intensity was calculated as the ratio of treated individuals at the stand to the estimated stand size obtained from the exponential survival specification.

Table A.6: General Equilibrium Effects

	Phase 1	Phase 2	
	(1)	(2)	(3)
	Attend	Attend	Work
Panel A — Median and Below Intensity			
Treatment	1.333	0.436	0.483
	(0.291)	(0.292)	(0.233)
	[0.000]	[0.140]	[0.041]
Control mean	1.461	1.314	3.033
Treatment Effect (%)	91.3	33.1	15.9
N: worker-weeks	586	672	508
Panel B — Above Median Intensity			
	(1)	(2)	(3)
Treatment	1.970	0.504	0.220
	(0.235)	(0.235)	(0.192)
	[0.000]	[0.034]	[0.255]
Control mean	1.360	1.238	3.046
Treatment Effect (%)	144.8	40.7	7.2
N: worker-weeks	986	1128	849
Panel C — Difference in TE in Panel A and B			
	(1)	(2)	(3)
P-value	0.296	0.434	0.365

Table A.7: Disruptions During Phase 1

	(1)	(2)	(3)	(4)
Treat	0.778	0.873	0.942	0.919
	(0.178)	(0.181)	(0.237)	(0.245)
	[0.000]	[0.000]	[0.026]	[0.078]
Treat × Post shock			-0.383	-0.319
			(0.339)	(0.360)
			[0.416]	[0.614]
Treat × Second month of phase 1		-0.224		
		(0.173)		
		[0.196]		
Treat × Week in phase 1	-0.0667			-0.0444
	(0.053)			(0.061)
	[0.213]			[0.585]
Week in phase FE	Yes	Yes	Yes	Yes
Calendar Week FE	Yes	Yes	Yes	Yes
N: worker-weeks	1572	1572	1420	1420

Table A.8: Shock Analysis—Robustness

	(1) Main Spec	(2) Lasso Residual	(3) 20^{th} Percentile Threshold	(4) 30^{th} Percentile Threshold	(5) Residual Baseline Only	(6) 4-Day Rolling Average	(7) 7-Day Rolling Average
Treat	0.791	0.786	0.731	0.811	0.803	0.833	0.773
	(0.243)	(0.241)	(0.222)	(0.253)	(0.246)	(0.232)	(0.234)
	[0.031]	[0.102]	[0.077]	[0.072]	[0.075]	[0.043]	[0.076]
Treat × Post shock	-0.791	-0.716	-0.760	-0.756	-0.785	-0.882	-0.745
	(0.348)	(0.336)	(0.372)	(0.339)	(0.346)	(0.341)	(0.377)
	[0.038]	[0.070]	[0.050]	[0.056]	[0.056]	[0.048]	[0.073]
Week in phase FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N: worker-weeks	1612	1618	1637	1605	1612	1618	1636

Notes: This table reports robustness analysis results testing different specifications for column 3 of Table 2. Column 1 reports the results using the main specification, as in Table 2. Column 2 reports the results from a specification that uses Post-Double Selection Lasso to select additional covariates from demographic variables when residualizing attendance. Column 3 defines a shock as the weekly mean attendance rate of the stand falling below the 20th percentile of all stand-week attendance rates, rather than the 25th percentile used in the main specification. Column 4 uses a 30% threshold instead of the 25% threshold used in the main specification. Column 5 trains the linear regression model that residualizes attendance on Baseline attendance data only, rather than on Baseline and Phase 1 attendance data as in the main specification. Columns 6 and 7 smooth residualized attendance using 4-day and 7-day rolling averages, respectively, prior to computing the 25th-percentile shock.

Table A.9: Shock Analysis—Robustness

	(1) Main Spec	(2) Lasso Residual	(3) 20^{th} Percentile Threshold	(4) 30^{th} Percentile Threshold	(5) Residual Baseline Only	(6) 4-Day Rolling Average	(7) 7-Day Rolling Average
Treat	0.760	0.741	0.697	0.779	0.775	0.819	0.754
	(0.257)	(0.254)	(0.228)	(0.271)	(0.260)	(0.243)	(0.239)
	[0.025]	[0.131]	[0.088]	[0.102]	[0.099]	[0.039]	[0.083]
Treat × Post shock	-0.714	-0.608	-0.661	-0.681	-0.717	-0.848	-0.692
	(0.420)	(0.407)	(0.422)	(0.419)	(0.417)	(0.411)	(0.416)
	[0.072]	[0.155]	[0.108]	[0.122]	[0.094]	[0.064]	[0.115]
Treat × Week in phase 2	-0.0339	-0.0486	-0.0500	-0.0318	-0.0303	-0.0148	-0.0298
	(0.056)	(0.057)	(0.052)	(0.058)	(0.056)	(0.056)	(0.050)
	[0.617]	[0.460]	[0.394]	[0.681]	[0.650]	[0.823]	[0.641]
Week in phase FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Calendar Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N: worker-weeks	1612	1618	1637	1605	1612	1618	1636

Notes: This table reports robustness analysis results testing different specifications for column 4 of Table 2.

A Appendix Figures

Figure A.1: Timing First Disruption in Phase 2 by Week

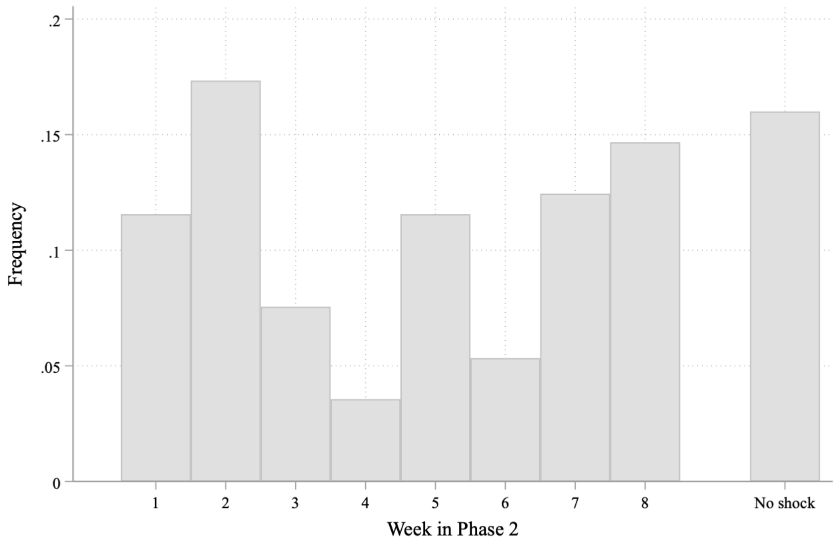


Figure A.2: Robustness: Leave-one-stand-out Effect of Disruption