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ABSTRACT

Workers who are worried about their personal finances may find it hard to focus at work. If so, reducing financial concerns could by itself increase productivity. We test this hypothesis in a sample of low-income Indian piece-rate manufacturing workers. We stagger when wages are paid out: some workers are paid earlier and receive a cash infusion while others remain liquidity constrained. The cash infusion leads workers to reduce their financial concerns by immediately paying off debts and buying household essentials. Subsequently, they become more productive at work: their output increases by 7% (0.11 SD), and they make fewer costly, unintentional mistakes. Workers with more cash-on-hand thus not only work faster but also more attentively, suggesting improved cognition. These effects are concentrated among more financially constrained workers. We argue that mechanisms such as gift exchange or nutrition cannot account for our results. Instead, our findings suggest that financial strain, at least partly through psychological channels, has the potential to reduce earnings exactly when money is most needed.

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1 Introduction

Low-income individuals frequently report feeling stress, worry, or anxiety about their finances. When money is tight, people cannot stop thinking about how they will afford groceries, avoid eviction, care for a sick child, or repay a moneylender (e.g. Collins et al., 2009; Morduch and Schneider, 2017). These worries—because of their poignancy—can intrude into everyday functions of life. For example, the average American reports spending 6.4 working hours each week distracted by thoughts of finances (Sergeyev, Lian and Gorodnichenko, 2023). Recent work in economics and psychology has attempted to understand such worries, arguing that financial concerns can adversely impact how people think, reason, and choose (Mullainathan and Shafir, 2013; Haushofer and Fehr, 2014).

We study the economic consequences of these psychological concerns. In our study sample of rural Indian workers, 70% state that they are "very worried" about their finances. Importantly, they carry these psychological burdens to work: they report being distracted at work by these financial worries on 50% of days. In this paper, we test the obvious hypothesis implied by this data: financial concerns may sufficiently distract workers so as to meaningfully reduce their productivity; and, as a consequence, reduce earnings exactly when money is most needed.

We run a field experiment with 408 low-income male workers in rural Odisha, India to measure the effects of lowering financial constraints on worker productivity. The experiment takes place during the lean season, when there is little agricultural work and people instead work as casual laborers in other sectors. Such jobs are intermittent and typically of short duration, ranging from one day to a couple of weeks. We partner with local contractors to employ workers in such a contract job, making disposable plates for restaurants, for two weeks during the lean season. Workers are paid piece rates, so that their productivity directly impacts their earnings. These earnings are workers' primary source of income during the experiment and—given the intermittent nature of lean season employment—comprise the bulk of their income for the month. Consequently, workers are highly motivated to be productive.

As the experiment takes place during the lean season, workers enter the experiment with high levels of financial strain. At baseline, 86% report being worried or very worried about their finances. The two most commonly reported sources of worries are daily expenses and loans, with 71% of workers carrying outstanding debt. In addition, workers indicate low levels of liquidity, with 66% saying they would have difficulty coming up with Rs. 1,000 (4 days of wages) in an emergency. As noted above, our survey data show that workers bring these financial concerns with them to work: on a given day, about half of workers report worrying about their finances while engaged in making plates at work.

Our financial strain manipulation is motivated by evidence that receiving money reduces

financial strain, even when the payment is fully anticipated (Mani et al., 2013; Pew Charitable Trusts, 2016; Ellwood-Lowe et al., 2022). For liquidity-constrained workers, the anticipation of income may not be enough to alleviate financial strain. In qualitative interviews, workers in our sample indicate that they feel sadness or guilt after saying no when their children ask for a perceived essential. They may feel harassed by a moneylender or embarrassed around relatives until they can repay them. They also indicate feeling vulnerable and anxious about the prospect of shocks like illness, which require cash-on-hand to address. Knowing that cash is coming does not fully eliminate these concerns—the money lender does not relent until the loan is actually paid off. As a result, the actual arrival of resources can reduce financial strain beyond the effects of anticipating them.

We leverage this idea to construct our empirical test. Using a modest and naturalistic manipulation, we experimentally vary the timing of when workers receive their (expected) wage payments. Specifically, control workers receive their earnings at the end of the two-week contract period. In contrast, treatment workers receive their earnings in two installments: an interim payment of earnings-to-date four days before the end of the contract period, and the remainder on the final day. Consequently, for a four-day window, treatment workers have received a large cash infusion while control workers have not. This design eases financial strain while holding constant both the incentive to work (i.e. the piece rate) and wealth—providing a test of whether financial strain in and of itself affects productivity.

We first gauge whether the cash infusion meaningfully affects financial strain. First, we examine workers' expenditure patterns. After receiving their interim cash payment, treatment workers immediately pay off loans and increase household expenditures—the two most common sources of lean season financial stress cited by workers in our sample. In the three days after interim cash receipt, treated workers are 40 percentage points (222%) more likely to repay any loan ($p < 0.001$), with a 287% increase in loan payment amounts ($p < 0.001$). The majority of these payments occur on the very same day as the cash disbursement. In addition, on the day they receive their interim payment, treated workers increase spending by 70% on household items, such as food, clothing, soap, and fuel ($p < 0.001$). Second, we report suggestive evidence from worker self-reports. After the cash infusion, treatment workers report being more focused on their work task and being less likely to have thought of financial worries while at work. Together, these patterns suggest that the early payment treatment generated a meaningful reduction in financial strain.

The reduction in financial strain is accompanied by a sharp increase in workers' actual productivity. The day after receiving a cash infusion, treated workers increase output by 0.109 standard deviations (SD), or 6.9%, relative to the control group ($p = 0.020$). These gains persist throughout the remaining days of the contract period. They are also concentrated among workers who are poorer at baseline, measured both by having fewer assets and less

liquidity. Early payment increases productivity for these poorer workers by 0.204 SD ($p = 0.003$).[1] Because work hours are fixed and attendance is high (98.3%), these output increases reflect improvements in productivity: how quickly workers produce plates in each hour.

The cash infusion not only increases total plates produced, it also changes *how* workers produce those plates; they appear to plan and focus better. We measure attentiveness at work by examining the physical plates themselves. Producing a leaf plate requires assembling irregularly sized leaves into a clean circle. Doing so efficiently requires planning (how will the leaves fit together) and focus (making sure each stitch is in line with that plan). Failing to do so creates extra work: stitches must be removed or additional leaves added, slowing the worker and reducing overall production and earnings. As a result, finished leaf plates contain traces of how attentive a worker was in making them—the number of leaves or stitches used, and pairs of holes that indicate where mistaken stitches were removed—which we measure, unbeknownst to workers. After treatment workers receive their interim payment, such "attentional lapses" decline by 0.08 SD ($p = 0.092$). As with the productivity results, these effects persist until the end of the contract period and they are concentrated among the poorer workers, whose attentional lapses fall by 0.13 SD ($p = 0.037$). The reduction in attentional lapses is particularly striking given that workers are working faster: more cash-on-hand increases pace while simultaneously reducing the rate of mistakes.

Are workers more attentive because they are less weighed down by financial concerns or because they are simply more motivated? Could any increase in worker motivation or effort mechanically increase attentiveness? To test this, we experimentally vary the piece rate, adjusting the base wage to hold overall earnings constant. Each one-rupee increase in the piece rate raises output by 0.020 SD. However, this is not accompanied by any discernible change in attentional lapses: the estimated effect on the attentiveness measures is essentially zero and significantly different from the effect on output ($p = 0.001$). These findings suggest that attentiveness and effort can operate independently.

Could this pattern of results be explained by mechanisms other than the psychological benefits of relieving financial strain? Our simple framework in Section 3 shows how these results are inconsistent with a neoclassical model, even accounting for liquidity constraints and an effort margin. In addition, in Section 7 we argue that they cannot be explained by other factors, including fairness, trust, nutrition, and sleep. For example, in contrast with predictions of fairness or gift exchange models, we find no evidence of treatment effects of announcing pay schedules; effects only arise once treatment workers actually receive the interim payment. Moreover, exploiting random variation in which day treated workers receive their interim payment within each round, we find no evidence that control workers decrease

[1] All workers in our sample are fairly poor and report feeling financial strain. This heterogeneity may reflect larger strain among poorer workers, or more likely, simply reflect the fact that the magnitude of the interim payment is relatively more meaningful for poorer workers.

effort upon seeing others paid before them. Similarly, we argue our findings cannot be explained by nutritional changes, which biologically cannot generate increased productivity overnight. In addition, since all food at the worksites is provided by us, we argue short-run blood sugar spikes from differential food consumption cannot explain our findings—evidenced, for example, by the stability of treatment effects over the course of the workday, as well as direct data on breakfast consumption patterns.

Note that we do not take a stance on the specific psychological mechanism—such as worry, anxiety, or affect—that gives rise to productivity effects we observe. Our experiment is designed to test whether workers are less attentive at work, but not to tease apart the exact psychological reasons for that reduced focus—primarily because many of the economic implications are the same irrespective of the exact psychology. Rather, our goal is to provide a clean proof of concept for whether productivity effects can occur in a high-stakes setting where workers' behavior determines their income. We find that a relatively modest manipulation of financial strain produces meaningful effects on productivity. The magnitude of our findings suggests that examining the productivity implications of broader interventions—for example, different pay structures or consumption smoothing technologies—presents interesting directions for further research.

This paper contributes to the growing literature on the psychological impacts of economic conditions (Haushofer and Fehr, 2014; Schilbach, Schofield and Mullainathan, 2016). One set of studies has focused on effects on well-being or mental health (Haushofer and Shapiro, 2016, 2018; Ridley et al., 2020). A second set of studies, more directly related to our work, examines cognitive effects—measured primarily through psychometric tests (such as Raven's Matrices or executive control games) or through laboratory measures of preferences and decision-making (Mani et al. 2013; Shah, Shafir and Mullainathan 2015; Carvalho, Meier and Wang 2016; Ong et al. 2019; Lichand and Mani 2020; Bartoš et al. 2021; Fehr, Fink and Jack 2022).[2] Building on these studies, recent work examines whether inducing financial thoughts during an academic test can affect test performance, or alter the demand for an educational intervention (Duquennois, 2022; Lichand et al., 2022).

Our work provides direct evidence for the impacts of financial strain in a high-stakes field context on an outcome of central interest to economics: worker productivity (and earnings). A priori, it is unclear whether financial strain will lower individuals' earnings capacity when their need for money is high. In such periods, workers will also be most motivated, so strain may reduce the capacity to focus while also increasing the desire to focus—making the net

[2]Carvalho, Meier and Wang (2016) find no differences in cognition and decision-making among low-income individuals in the US when comparing them up to seven days before vs. after their regular bi-weekly paydays. The differences in results across studies may be driven by differences in the relative reduction in financial strain, by differences in the absolute level of poverty and other characteristics across study populations, or by differences in the outcome measures.

effect on productivity unclear. This motivational channel is absent when looking at outcomes like paper-and-pencil cognitive tests: unlike in the work domain, performance on these tests is not consequential for addressing the worker's financial constraints. In contrast, worker earnings in our experiment constitute a large fraction of the household's overall income for that month. Seeing effects on productivity in this context indicates financial concerns can have material consequences when the stakes are high. Because we find that such impacts can occur even when the receipt of cash is expected, this suggests that even the predictable cycles of transient liquidity crunches that are often experienced by the poor can have meaningful consequences beyond consumption.

These findings also complement research on asset transfers to the poor (Banerjee et al., 2020).[3] We find productivity effects while holding constant labor supply and investment channels, both of which could be affected by asset transfers. The potential presence of these additional channels suggests a broader set of potential pathways through which alleviating financial constraints could increase earnings.

2 Context: Financial Concerns

We undertake our study with low-income workers engaged in small-scale manufacturing in Odisha, India. In this area, laborers work in agriculture during peak planting and harvesting periods, which comprise about 4 to 6 months of the year. In the remaining lean agricultural months, they typically seek short-term contract employment in non-agricultural jobs, such as manufacturing and construction. These jobs are of short duration—with the modal job lasting one day, and lengths typically ranging from one day to a couple of weeks (Breza, Kaur and Shamdasani, 2021). Contract jobs may pay wages daily, at interim intervals, or as a lump sum at the end of the contract period. During lean months, jobs are not easy to find and employment rates are low, with workers finding wage employment only 1.9 days per week on average (Table I Panel A, Col. 1). Combined with intermittent employment, this leads to low and variable income in lean months, the time of our experiment. Consequently, workers report high levels of financial constraints, especially those who are dependent on wage labor for their primary earnings (i.e., who own little or no farmland).

In our sample, 68% of control group workers report outstanding loans at baseline (Table I Panel B, Col. 1). Nearly 54% have outstanding credits with local shops for basic household consumption, consistent with difficulties in meeting basic daily expenditures. In addition, 64% of workers say they would have difficulty coming up with Rs. 1,000 (i.e., 4 days of

[3]Banerjee et al. (2020) find that individuals who receive a large livestock asset transfer—shifting them from being wage laborers to farmers—are more willing to engage in and more productive in a piece-rate bag-sewing task. Related work also documents increased employment and earnings caused by transfer programs, albeit in the presence of increased productive assets that may be complementary with labor (Banerjee et al., 2015; Balboni et al., 2022).

wage labor income) in case of an emergency—indicating a low level of cash-on-hand. These patterns, while stark, are not unique to our setting. The poor report low levels of liquidity and difficulty in financially coping with shocks in a range of contexts, including in the U.S. and in developing countries (Collins, Morduch, Rutherford and Ruthven 2009; Lusardi, Schneider and Tufano 2011; Morduch and Schneider 2017).

These financial burdens are reflected in high levels of worries. Figure I depicts workers' self-reports of how thoughts about finances interact with their daily lives.[4] When asked how concerned they are about their (future) finances, 70% of workers say they are "very worried." This number rises to 86% when also including those who say they are "quite worried" (Panel A). Worries arise top of mind often: more than half (52%) report they worry about finances at least once per day, and almost all report worrying at least a few times per week (Panel B). When finances do rise top of mind, workers say they ruminate anywhere from a few minutes (29%) to a few hours (43%) to a whole day (10%) (Panel C). In Panel D, we depict workers' responses to an open-ended qualitative question asking them "What makes you worry about money issues?" Surveyors entered workers' responses in short phrases or sentences. We visualize their raw text responses with no processing, except removing stop words and typos, using a word cloud (e.g., Fellows, 2012). Larger text denotes phrases that appear more frequently. The results indicate that the struggle to meet daily expenses and pay off loans is especially prominent for workers.

As an additional window into workers' mindsets, we conduct a small exploratory exercise building on Shah et al. (2018). We show workers two pictures, whose facial expressions have similar affect; one of them, though, is visibly low-income while the other is more affluently dressed (see Appendix Figure A.1 for the photos). After seeing a photo, workers are asked what they think the person is feeling, and why they think the person might be feeling this way. Coding these responses reveals very different attributions: 98% of workers say the poor individual looks sad, worried, or anxious, and 92% guess financial concerns as a reason. For the more affluent person, only 20% report any negative affect or emotions, with the overwhelming majority (77%) instead stating that he looks happy, and 92% of workers attribute the feeling to having enough money or having a good job. These results illustrate what workers view as most important: in interpreting the emotional states of others, they reflexively turn to financial concerns first.

Perhaps most relevant to our hypothesis, workers bring worries with them to work. At the end of one workday, we ask workers an open-ended question about what they were thinking about that day while working—with no prompts related to finances, so workers could talk about anything, such as their weekend plans. On a given day, one out of two workers reports

[4] As our goal is not to distinguish between particular psychological mechanisms, we use the words "worry," "anxiety," and "rumination" in their lay sense. Psychologists have more precise definitions and measurement constructs for each these (e.g., Zebb and Beck, 1998; Fresco et al., 2002).

ruminating about financial concerns while at work. After this unprompted question, we then ask workers specifically whether they thought about their finances while working, and 83% of workers report doing so.

These patterns are consistent with qualitative interviews with workers. For example, workers state that when they arrive home, their children may beg them to purchase something in the market or their spouse may point out the need for a household essential like fuel; having to turn down such requests leads to feelings of sadness, guilt, or inadequacy that can linger. When workers have outstanding overdue loans, harassment from the moneylender in the village or interacting with a relative who lent them money can generate stress or humiliation. In addition, shocks like illness occur frequently, generating immediate cash emergencies, such as needing to pay a deposit before a loved one can be admitted to a hospital or clinic. Consequently, not having immediate access to cash can create a feeling of vulnerability or anxiety about the prospect of not being able to handle a potential emergency. Receiving income, and therefore being able to spend funds or have cash-on-hand, has the potential to reduce mental burdens, even when the receipt of those funds is expected. This matches empirical patterns documented elsewhere in the literature (Mani et al., 2013; Pew Charitable Trusts, 2016; Ellwood-Lowe et al., 2022) and motivates our experimental design approach.

3 Simple Framework

We present in Appendix B a simple model with three goals. First, we lay out what a standard model—one that ignores any effect of financial strain on cognition or productivity—predicts in our experimental conditions. Second, we extend this model to include psychological elements, to provide one way to formalize what we mean by phrases such as "financial strain" and "hard to focus at work." Finally, we draw out the predictions of this more psychological model for our experimental treatments.

Our baseline model is a standard single-worker, infinite-horizon model where the worker makes inter-temporal consumption and work decisions. Inter-temporal choices include what to consume each period and whether to save or pay down debt. For simplicity and realism, we solve the case where the worker has positive debt level D_t at a high interest rate (greater than the discount rate), so the effective choice is between paying down debt and consuming. In the baseline model, the only choice at work is the decision of how hard to work (i.e., the choice of effort e); we do not explicitly model the labor supply decision of whether to work.

Central to our behavioral model is the notion of two kinds of inputs into production. We call one input e to denote "effortful" inputs, those controllable by the worker. These include physical components such as the speed of moving one's hands, which might traditionally be called effort, as well as psychological components such as the decision of how much attention to pay. We call the other input a to denote mental inputs that are beyond a worker's control

(i.e., "automatic"). Central to our hypothesis is the idea that some elements of focus are beyond the worker's control. We aim to capture this by assuming that the level of a might be lower or higher depending on the context; for example, a worker who is distracted would have lower a. Both a and e affect output $f(e, a)$.

In both the baseline and behavioral model, e is chosen optimally by the worker, assuming convex costs in e. In the baseline model, though, a is a fixed parameter ($\bar{a}$) that captures the worker's capacity for automatic attention, a worker characteristic like skill or human capital. In the behavioral model, however, a varies and depends on the extent of financial strain. We model financial strain as being higher when workers have more pressing needs for resources today. Practically, workers can have "pressing needs" for two reasons. First, when the marginal utility of consumption $u'(c_t)$ is high, resources are valuable because additional consumption has high value. Second, when debt D_t is high, resources are valuable because there is more debt to pay down and pressure from lenders to be released. Thus, in our model, strain increases with $u'(c_t)$ and D_t. Notice that strain is present-oriented; *current* marginal utility of consumption and debt levels dictate strain.

Once strain is defined in this way, our behavioral model allows us to analyze the case where the automatic input a is not a fixed trait of the worker but instead changes with context. Specifically, in the behavioral model, we capture decreases in financial strain in higher levels of automatic input a. Additionally, we assume that workers are naive with respect to the effect of strain on a; they do not take into account the effects of their choices on future strain, perhaps because they are not aware of such effects (e.g. Dean, 2024).

The essence of the experimental intervention—described in detail in Section 4—is that some workers are paid earlier than others, and they are told in advance about this upcoming payment 3 to 4 days before it occurs. In the model, the first period corresponds to the post-announcement period (when workers know their payment schedule but no payments have yet been disbursed) and the second period corresponds to the post-payment period (when treated workers have received a payment while control workers have not).

The baseline model (with no effect of strain on a) makes two predictions about the effect of early payment. First, paying workers early *reduces* their subsequent productivity post-payment: a (small) income effect from receiving money earlier (and thus paying down high-interest debt earlier) increases the net present value of lifetime earnings and thereby lowers the marginal utility of consumption—depressing effort and output. Second, while the productivity effect in the post-announcement period is ambiguously signed, it should be larger (more positive) than the effect in the post-payment period. The relevant mechanism in the post-announcement period works through discounting: the same payment paid earlier has (slightly) higher value and thus raises the return to effort exerted in the post-announcement period for workers who receive their income earlier.

To understand the predictions of the behavioral model—wherein financial strain reduces the capacity to focus (a)—we calculate the *additional* impact of early payment on productivity in this model that works through the automatic input channel, holding e constant. Compared to the baseline model, the predictions about productivity change in two ways. First, in the post-payment period, there is an incremental positive treatment effect on productivity. This is because workers use their early payment to cover important household items and pay down some debt—thus reducing their financial strain—which increases automatic input a and output. In the empirical section, we test for this mechanism directly through measurements of automatic input a (in addition to measurements of the overall effect on output). Second, since strain only falls once workers receive their payment, any additional treatment effect in the post-payment period should be larger (more positive) than the additional effect in the post-announcement period.

The behavioral model allows us to disentangle the effort effects from the financial strain effects. In particular, it highlights how these two effects differ between the announcement period from the post-payment period. The key assumption is that financial strain is reduced when payment arrives. Effort on the other hand should change as soon as the announcement is made. As a result, one pattern of treatment effects would clearly indicate a financial strain effect: (i) positive post-payment effects on productivity and (ii) smaller or non-existent post-announcement effects on productivity.[5]

4 Experimental Design

To enable our test, we utilize the worksite infrastructure developed by Breza, Kaur and Shamdasani (2018), wherein workers are hired in contract jobs during the agricultural lean season. Workers are employed full-time for two weeks in a small-scale manufacturing task: making disposable plates for restaurants. Given low employment opportunities in the lean season, this job is workers' main source of income not only during the two-week contract period but for the month. They are paid piece rates for output so that changes in output translate directly into changes in earnings. Workers can thus be expected to be highly motivated to be productive in this setting, especially given the financial constraints documented above.

4.1 Treatment: Variation in Cash-on-Hand

Our design manipulates financial strain using a naturalistic manipulation: changes in the timing of when wages are paid out. The treatment generates differences in cash-on-hand

[5]Failing to find such a pattern could be interpreted in two ways. First, if the effort responses are indeed small, then it would suggest little or no effect of financial strain. Second, however, it may simply indicate a setting in which the effort margin is large.

while holding other job features constant. This design therefore allows us to construct a test for whether being financially constrained in and of itself affects productivity.

Cash treatment. Figure II provides an overview of the timeline for a typical experimental round. Control workers receive all their accrued earnings at the end of the contract period (on workday 12). In contrast, treatment workers receive their earnings in two installments: an interim payment where they receive their accrued earnings to date—randomly varied to be on either workday 8 or 9—with the balance of their earnings paid at the end of the contract on day 12 (see Section 4.4 below for implementation details).

This interim payment is a substantial cash infusion, corresponding to what workers typically earned in the entire month before joining the study. Consequently, in the "post-pay" period—the days after the interim payment until the end of the contract—some workers are flush with cash while others are not. We examine worker output in this period to test whether cash receipt causes an immediate effect on productivity.

Announcement. The interim payment is not delivered as a surprise. When workers arrive on day 1, they are told that some workers may receive their earnings in two tranches rather than one, and that each worker's exact payment schedule will be announced on day 5. On the morning of day 5, each worker is told individually when he will receive his payment. The subsequent "announcement period" between days 5 and 8 enables us to test whether workers immediately react to news of their payment schedule, and more broadly whether we see any changes in productivity in anticipation of cash arrival.

Discussion. Under our design, treatment and control workers all face the same piece rate, and their earnings for work performed in the "post-pay" period will be received on the last day of the contract period. In addition, because we only change pay timing but not pay levels, overall compensation, and therefore wealth, is held fixed across workers.[6] This design is in contrast to manipulations that sizably increase total wealth, like cash transfer programs, which could substantively alter the motivation to work through purely neoclassical channels (i.e., changes in the preference for leisure versus effort), making it difficult to interpret effects on output. Our design avoids this challenge, providing a clean test of whether financial strain in and of itself has productivity effects.

4.2 Work Task and Outcomes

Work task. Workers produce disposable plates, made from stitching together leaves from sal trees (see Appendix Figure A.2). Such plates are a ubiquitous local product used, for example, in virtually all low-tier restaurants in the region. The standards for the plates are

[6]Possible interest rate savings from paying down loans 4 days early are small. Using an annualized interest rate from moneylenders in India of about 40% (Surendra, 2020) and our estimated treatment effect of Rs. 271 on loan payments, the treatment group would have saved less than Rs. 2 on interest payments—less than 1% of a worker's daily wage during the time of the experiment.

set by partnering contractors, and all output is sold to restaurants.

Workers are paid a flat base wage for attendance plus a piece rate per completed leaf plate. To qualify for payment, a plate is required to: (i) meet a minimum size requirement; (ii) have no holes or gaps so that it can hold food (e.g., curry) without leaks; (iii) have all leaf stalks covered by other leaves; and (iv) have the leaves that form the outer ring (perimeter) of the plate be placed on top of the other leaves that compose the inner section of the plate.

Making leaf plates is physically exacting, requiring repeated fine motor movement. It is also cognitively demanding: leaves come in irregular (oval) shapes and sizes, and these varying shapes must be stitched together to produce a circular plate. Since each additional leaf takes time to stitch, workers try to use as few leaves as possible. Making leaf plates therefore requires making and adhering to a plan. The consequences of failing to do so are clear when watching plates being made. A worker who has not thought things through might find partway through making a plate that the shape has started to veer from circular toward oblong, thus requiring him to undo stitches to detach the most recent leaves added to the plate and re-attach them with different positioning. Or, after joining together a series of leaves, a worker might find that a stem is visible or a small gap has appeared between leaves, leading the worker to patch it with another leaf on top.

When focus wanders, work suffers. Workers may need to use more leaves and stitches to compensate for the lack of strategic placement. They may need to undo errors by removing stitches in order to re-arrange leaves. Mental errors consequently come at a cost: they increase the time to produce each plate and thus reduce earnings.

Outcome: Output. Our main measure of output is the number of accepted leaf plates, measured at the hourly level. We focus on accepted leaf plates as these determine workers' payment, but we also measure rejected leaf plates. Workers quickly learned to meet the required standards such that over 97% of leaf plates were accepted overall and over 98% after the baseline period.

Outcome: Attentiveness index. We hypothesize that cash receipt affects workers' psychological state—easing the mental burdens indicated in Figure I and enabling workers to be more attentive at work. We directly test for positive evidence for such a channel by unpacking how workers produce their plates. Specifically, as part of collecting product quality indicators, we measure three unincentivized markers of attentiveness on each plate: (i) the number of "double holes"—the telltale sign that a worker removed a stitch from a plate in order to detach a leaf to undo a mistake; (ii) the number of leaves used; and (iii) the number of stitches used. A worker who has to undo fewer mistakes, or who makes a completed plate without using extra leaves or stitches to compensate for poor planning or mistakes can be expected to work faster—spending less time per plate. Workers were unaware that these dimensions of their output were measured.

We calculate the average number of leaves, stitches, and double holes per plate during each worker-hour slot, for a subset of hours in each experimental round. The three measures are normalized using the control group's production (mean and standard deviation) in the post-pay period and then averaged to create the attentiveness index. We reverse the scale so that higher values on the index correspond to improved attentiveness (i.e., fewer double holes, leaves, or stitches). We also create an indicator of "high attentiveness," defined as having an index value greater than the median, to show robustness in addition to the linear measure.

4.3 Additional Treatments

We augment our design with two additional sources of variation.

Piece-rate variation. In five supplementary experimental rounds without the interim-pay treatment, we vary piece rates for output (see Section 4.4). We adjust the base wage to hold overall earnings roughly constant across days. We use this variation to examine what happens to output when the marginal return to work has changed, but wealth and financial strain have not. Unlike our main cash-on-hand manipulation, this variation should produce no change in workers' level of mental burdens.

Priming. Our primary test relies on using real income variation. As a supplementary exercise, following previous work (e.g., Mani et al., 2013; Bartoš et al., 2021), we implement a priming intervention intended to direct workers' attention to their finances: on a randomly selected day, we ask workers how they would raise money to cover a large unexpected expense. We test the hypothesis that priming causes two competing effects: while bringing financial concerns top of mind could reduce output through a cognition effect, reminding workers about their financial needs could motivate them to work harder or focus, increasing output. We cross-randomize the priming intervention with the interim-pay treatment. Some workers are randomized to receive the priming treatment two days before the interim payment day, others two days after the interim payment day, and others not at all (see Appendix Figure A.3). Appendix A.2 describes the design in more detail and reports the results.

4.4 Implementation and Protocols

We conducted field activities during the main lean season (March to June) of 2017 and 2018 in Odisha, India, with piloting beginning in 2017. We ran 14 experimental rounds with 26-30 workers each across five worksites in four districts in Odisha. Our main sample includes 408 workers, drawn from 47 villages within daily commuting distance of the worksites. We lay out our protocols for a typical round below; deviations are documented in Appendix C.

Recruitment. A few days prior to the start of each round of the experiment, recruiters visited a set of new target villages and advertised the upcoming work opportunity through

door-to-door visits and fliers. Potential participants were informed about the location, work tasks, duration, and their potential compensation. Workers were eligible to sign up if they were aged between 18 and 55, fluent in Odiya (the local language), worked regularly as wage laborers, and were not migrants (i.e., present in their home village for at least 3 of the past 6 months). All workers were male due to cultural restrictions on women traveling outside the village for work. Since the number of interested workers exceeded the worksite capacity, we hired 30 randomly selected workers from the sign-up list for each given round. A pool of 5 backup participants was used to replace any workers who dropped out of the study during the first three days of a round (before treatment assignment was announced). We exclude 21 participants who dropped out in the first three days. Among the 408 workers who were enrolled when treatment status was announced, only 6 dropped out before the end of the study period—3 in the interim payment group and 3 in the control group. We include all 408 workers in the analysis, coding the attendance and output of attritors as zero.

Worksite setup. In a typical round, workers worked full-time at the worksite for 12 consecutive days. Hours matched the norms for casual wage work in the villages corresponding to each round. Work typically began at 8 am or 9 am, and ended between 2 pm and 5 pm, with 5 hours of work per day in the modal round.[7] Workers worked individually in their own personal work areas, where they also ate lunch, physically distanced from other workers; this limited the scope for interactions between workers to minimize output comparisons or social conversation at work.

Workers were told their daily output each day throughout the experiment, limiting any uncertainty about the outstanding payment amount. At the end of day 1, all workers were paid a flat wage of Rs. 250 (about US $4) as a training wage, to foster trust in the employer among workers. For the remaining days, workers were paid a base wage of Rs. 200 and a piece-rate wage of Rs. 3 per plate. The performance payment comprised about 20% of the overall payment. To encourage high attendance, workers were given a completion bonus (Rs. 300) if they attended all of days 6 through 11, paid out on the final day of the contract. This bonus limits potential extensive margin labor supply responses to the treatment and thus enables us to cleanly investigate our primary research question—whether workers' capacity to be productive is affected by their cash-on-hand—without (selective) attrition induced by absences confounding the analysis.

Payment schedule implementation. When workers were recruited in their villages, they were informed that they would receive a training payment at the end of day 1 and their remaining earnings on the final day of the contract. When they arrived at the worksite on

[7] In 9 rounds, the workday ended at 2 pm, when laborers in villages go home to have lunch and rest to avoid the afternoon heat. Five- to six-hour workdays are common for casual labor jobs in these areas, especially in the lean season due to elevated heat levels. The other rounds had different daily work schedules, e.g., from 9 to 5, based on local norms, and some rounds were shorter or longer than 12 days (see Appendix C).

day 1, they were informed that some workers may be paid in two tranches and that each worker would be informed of his exact payment schedule on day 5. On the morning of day 5 (the "announcement day"), workers were told as a group that each worker would learn his payment schedule that day, and after this, each worker was individually told his payment schedule by his manager.

To limit payday effects driven by present focus, as found in Kaur, Kremer and Mullainathan (2015), workers' output during the day of the interim payment itself did not affect how much they were paid on that day. For example, workers paid on the evening of day 8 received their earnings from days 2 to 7 only. While payments were made in private at the end of a given worker's payment day, all workers were aware that some payments had occurred at their worksite.

In this setting, when workers have a multi-day contract, they may receive their wages in a lump sum at the end of the contract period or in more frequent interim payments. Based on qualitative interviews, workers in our sample have experience with both types of arrangements, and there is not one clear preferred pay frequency among workers as a whole. To help make differences in pay frequency across workers feel more natural, we slightly staggered start times at the worksite on day 1 of each round, so workers arrived at different (randomly assigned) times. Workers' start times on day 1 were not correlated with their treatment assignment, but the heterogeneity in day 1 arrival times reduced the feeling that workers were part of one common cohort and provided context to justify why different workers may end up in different "batches." This terminology matches one that workers are used to in this local context. Contractors often source laborers on a rolling basis for a firm or project where job tasks or features (e.g., shift hours, responsibilities, pay dates) may differ across workers. In such situations, workers may get arbitrarily placed into a "batch" and their batch determines many features of their job.

Output measurement. At the end of each work hour, staff collected completed leaf plates from each worker, under the premise of clearing work areas. Plates were then counted in a private back room, away from workers. For a subset of days, staff also recorded the number of double holes, leaves, and stitches for every plate produced (the components of the attentiveness index). We had two staff members independently count output and the attentiveness measures, with any discrepancies reconciled by a supervisor through a third count, to reduce measurement error.

Randomization. In each experimental round, workers were randomly assigned to the interim payment (treatment) group or the control group.[8] Within each round, all workers were cross-randomized into Wave A or Wave B, which determined the specific timing of

[8]In most rounds, workers were divided evenly between the two groups. In rounds 1 to 3, the interim pay group was over-weighted in the randomization to comprise nearly 70% of the sample.

treatments. Among treatment workers, those in Wave A received their interim payment on day 8, while those in Wave B received theirs on day 9. Finally, workers were also cross-randomized into priming on one morning during the experiment, resulting in three mutually exclusive arms of the priming intervention: early priming (i.e., 2 days before their wave's interim payment), late priming (i.e., 2 days after their wave's interim payment), and no priming (see Appendix Figure A.3).

Piece-rate rounds. Implemented from February to April 2019, the supplementary rounds involved *only* piece-rate variation, i.e., none of the above treatments. Undertaking these rounds during the lean season ensured that economic conditions were similar to those during our main experimental rounds. Workers for the piece-rate rounds were redrawn from the main experimental sample, up to a year after the main rounds were conducted. This enabled us to hire experienced workers who knew how to make leaf plates from day 1, avoiding strong learning trends in the data. The sample of 150 workers in these extra rounds is balanced by treatment status (i.e., interim cash payment) in the main rounds, and is also representative in terms of baseline characteristics (Appendix Table A.1).

Workers were hired for seven days with piece rates changing across the last six days. On the first day, they received a flat wage of Rs. 250 with no piece-rate component. In the remaining six days, workers were paid a piece rate of Rs. 2, 3, or 4 in randomized order, with each rate lasting for two consecutive days. This order varied across workers within a round, so that on any given day, a third of workers each faced one of the three piece rates. The base wage was adjusted so that average daily earnings would be approximately similar (about Rs. 270 per day) for all three piece rates (see Appendix C for details). In addition, mirroring the main experimental rounds, workers received an attendance bonus of Rs. 200 if they attended all days, leading to a high attendance rate of 97% during these rounds. All payments were made on the final day (i.e., day 7).

Surveys and data collection. To maintain a natural work environment and to avoid influencing workers' attention through survey activities, we only collected a relatively small set of survey data. All workers completed a short baseline survey including basic demographics such as age, education, measures of income and wealth, and information about outstanding loans and financial worries. On the last day of each round, we conducted more intensive endline surveys. These collected information about financial worries as well as expenditure patterns and food consumption over the previous 3 to 4 days. Finally, we conducted a short survey on day 10 or 11 asking workers about what they thought about while working that day, as summarized in Appendix Figure A.3.[9]

[9]We include all our endline survey instruments in Online Appendix D. One of our pre-registered outcomes, life satisfaction, was not collected across rounds, so we are unable to examine impacts on this outcome.

5 Data and Empirical Strategy

5.1 Summary Stats, Heterogeneity in Wealth, and Balance

Table I presents summary statistics and baseline balance tests. A typical worker in our sample is about 40 years old. Virtually all workers are married (98%) and have children (89%). 75% of workers report casual daily labor as their primary source of earnings over the year, and the average worker found 9 days of paid wage work over the last month.

To compute a summary measure of baseline wealth and liquidity, we use the four binary variables at the bottom of Panel A: house quality (i.e., living in a non-mud house, constructed of durable material); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 in an emergency. The first measure captures wealth through the quality of the worker's housing; itis the quintessential measure used in a proxy means test to capture wealth. The last two variables reflect liquidity levels. We take a simple average of these four binaries to form a wealth index. When one of the measures is missing due to non-response (1.5% of the sample), the index is an average of the remaining three measures. Since we have multiple proxies for wealth, we report treatment effect heterogeneity by the wealth index as a whole. We examine effects using both the continuous wealth index and also a binary indicator that equals 1 if the worker's value of the wealth index is weakly greater than the median value across the sample of workers. In the appendix, we report heterogeneity by the house quality variable alone, since this is most likely to capture differences in underlying wealth levels across individuals.

The baseline characteristics do not statistically differ between the treatment and control groups overall (Table I Cols. 2 and 3), indicating a successful randomization procedure. We do not have baseline survey data for one worker due to an administrative oversight; analyses using this heterogeneity are therefore comprised of a sample of 407 workers (instead of 408).

5.2 Empirical Strategy

For our primary test of treatment effects of the cash infusion, we run regressions to estimate average treatment effects at the worker-hour level, using data from the announcement date onward:

$$\begin{aligned} y_{irdh} =& \beta(\text{Cash}_i \times \text{Post-Pay}_{ird}) + \gamma(\text{Cash}_i \times \text{Announcement period}_{ird}) \\ &+ \theta(\text{Post-Pay}_{ird}) + \mu(\text{Announcement period}_{ird}) + X'_{ir}\lambda + \delta_r + \varepsilon_{irdh} \end{aligned} \tag{1}$$

where y_{irdh} is the outcome of worker i in round-wave r on day d in hour h. Cash_i is a binary indicator for whether an individual is in the interim-pay treatment group. Post-Pay_{ird} is a binary indicator that equals 1 on the days after the interim payment was disbursed in the worker's wave. $\text{Announcement period}_{ird}$ equals 1 during the days after the payment

schedule was announced through the day the interim payment was disbursed, and equals 0 otherwise (see Figure II). Regressions control for round-wave (i.e., strata) fixed effects (δ_r). Finally, X'_{irdh} is a vector of baseline controls, chosen using the post-double-selection LASSO procedure developed by Belloni, Chernozhukov and Hansen (2014). We show robustness to alternate specifications, including both fewer and more detailed sets of controls, with the results virtually unchanged.

The key coefficient of interest is β, representing the average treatment effect of the interim payment (i.e., the difference between the treatment and control groups) in the days following the cash infusion. In addition, γ estimates the announcement effect—the extent to which the treatment and control group's behavior is different after workers are told their payment schedules, but before any money is paid out. We also examine treatment effect heterogeneity by baseline wealth levels, using the wealth index defined in Section 5.1.

For some supplementary analyses, such as effects on expenditures, self-reported focus during work, or breakfast measures, outcomes are collected only at endline. In these analyses, we run simple intent-to-treat regressions comparing the treatment and control groups:

$$y_{ir} = \beta Cash_i + X'_{ir}\lambda + \delta_r + \varepsilon_{ir} \tag{2}$$

where y_{ir} is the outcome of worker i in round-wave r, and all other covariates are as defined above. As above, in most cases, we select baseline controls using the post-double-selection LASSO procedure (Belloni, Chernozhukov and Hansen, 2014).[10]

6 Results

6.1 Effects on Financial Strain

For our design to be effective, the cash infusion must materially reduce financial strain. Before examining output effects, we first check whether it does so. By design, the interim payment is large enough to provide significant liquidity. On average it is over Rs. 1,400, corresponding to almost one month's typical wages during the lean season, given the intermittent nature of wage work at the time of our experiment.[11] We examine whether this indeed changes workers' expenditures and whether it translates into an impact on self-reported focus at work.

[10] In the post-double-selection LASSO procedure, the imposed regularization parameter is chosen in a data-driven way but, rather than through cross-validation, by making parametric assumptions. In some instances, our analyses necessarily have only one observation per worker and the sample size becomes small. In these cases, especially as the parametric assumptions made in choosing the regularization parameter need not hold in our data, we take a more standard approach: we simply control for baseline measures of the dependent variable (or a close proxy of it). Irrespective, we show that effects are similar under alternate control strategies.

[11] On average, workers had 8.6 days of paid wage work in the month preceding the experiment (Table I Col. 1).

Table II presents estimates of Intent-to-Treat regressions at the worker level on expenditures, comparing average expenditures in the 3 days following the interim cash payment among treatment vs. control workers. Panel A shows effects summed over the 3 days post-interim payment (showing estimates of equation (2)), while Panel B presents estimates separately for each day. After receiving the cash infusion, treatment workers immediately pay off loans and increase household expenditures—the two most common sources of financial stress cited by workers in our sample (Figure I Panel D). Within three days of cash receipt, treated workers increase loan payments by Rs. 271, a 287% increase relative to the control group mean (Table II Col. 1, $p < 0.001$). Treatment workers are 40 percentage points (222%) more likely to pay off any loans or credits (Col. 2, $p < 0.001$). The majority of these repayments are made on the very same evening as when the cash is disbursed; on the day of the interim payment, workers pay back an additional Rs. 169 in loans and credits (Panel B, Col. 1)—a 746% increase.

The cash infusion also increases household expenditures, such as food, clothing, soap, and fuel, by Rs. 150 or 40% on average (Panel A Col. 3, $p < 0.001$), and by Rs. 70 or 68% on the day of the interim payment (Panel B Col. 3, $p < 0.001$). Cols. 4 to 8 decompose household expenditures into major subcategories. We see significant effects on expenditures on food (25%, Col. 4), clothes (242%, Col. 5), and household essentials like soap, detergent, petrol, and diesel (172%, Col. 6). Given the effects on food, we consider potential impacts through nutrition channels in Section 7.2.

We find no detectable impacts on other spending categories—agricultural inputs, construction, transfers, and festivals—except for a marginally significant effect on festival expenditures ($p = 0.092$). We also find no treatment effects on purchases of durables (Appendix Table A.2 Col. 1). Finally, treated workers are also less likely to undertake expenditures on credit during this period, with about a 54% reduction in spending using credit (Appendix Table A.2 Col. 2, $p = 0.010$), consistent with an improved ability to cope with urgent cash needs post-interim payment. These findings, along with the loan repayments, suggest that on average, workers can borrow if needed but would prefer to hold less debt. In addition, treated workers are 9 percentage points more likely to lend money to other workers at the worksite in the days following the interim payment (Appendix Table A.2 Cols. 3 and 4, $p < 0.001$); while this suggests the presence of some spillovers, if anything, this should dampen the impact of our treatment by reducing the size of the first stage.

Despite the higher borrowing among control workers, treated workers spend more overall after cash receipt. In the three days following interim payments, treatment workers spend Rs. 371 or 65% more than control workers (Table II Col. 9, $p < 0.001$). In total, treated households spend Rs. 900 in the days following cash receipt, about two-thirds of the average interim payment. The majority of the total spending impact is concentrated in the first day,

with an increase of 140% (Panel B Col. 9, $p < 0.001$). These patterns indicate that the cash infusion has the potential to immediately reduce financial strain among treated workers. The ways in which it potentially does so differ across workers: paying off loans, meeting regular household expenditures, or having more cash-on-hand to finance shocks.

While these data tell us about expenditures, it would also be useful to see a direct impact on focus at work and worries. By construction, we do not have the ideal data for this. We chose not to ask workers daily questions on these topics both because we wanted to limit surveys until the end of each round (to maintain as much normalcy in the workplace as possible) and because we did not want these questions to interfere with the actual experiment (such as by serving as primes). Instead, two days after the interim payments are disbursed, we ask workers the following open-ended question at the end of one workday: "What were you thinking about while you were working today?" Workers can answer in any way they like. In the analysis, we exclude workers who were randomized to receive priming on the specific day this question was asked, to avoid confounding effects on worker thoughts.[12]

Figure III plots the results of this open-ended exercise. Workers who received the interim payment are 11.5 percentage points (15.5%) more likely to report feeling focused on the work task ($p = 0.032$). In addition, among the control group, about 60% of workers report thinking about worries related to finances or household expenses while making plates. This is mitigated by treatment: after receiving interim payments, workers are more likely to only report thinking about their work task or other topics outside of financial worries (13.7 percentage points, or 32.7%, $p = 0.044$).

To supplement this evidence, we also borrow from the approach of Shah et al. (2018) to test whether treatment changes the cognitive mindset of workers. As described in Section 2, we show workers a picture of a low-income individual with negative affect, and ask them to come up with possible reasons why the person may be feeling this way (Appendix Figure A.1). In response to this open-ended question, among the control group, almost all workers (92%) list financial worries as a possible reason for negative affect, but fewer (33%) list any other sort of reason. We examine whether the person's frame of mind allows them to contemplate any other potential reason outside of financial worries for negative affect. Consistent with our hypothesis, workers who receive the interim payment are 10 percentage points (31%) more likely to come up with reasons for negative affect other than financial worries (Appendix Table A.3 Cols. 1-2, $p<0.05$). Similarly, they are 9 percentage points (66%) more likely to come up with reasons that are more generally distinct from income or being poor, such as

[12]This question is asked as part of the "End-of-Day" survey, conducted two days after each wave's respective interim payment day. This coincides with the timing of the post-payment priming intervention on days 10 and 11 (see Appendix Figure A.3). Because workers who are primed that day are specifically told we expect them to think about their finances, we exclude these workers when examining treatment effects of the interim payments on this question.

that the person might be feeling ill (Cols. 3-4, $p<0.05$).[13]

While only suggestive, these patterns introduce the potential for the cash infusion to enable workers to be more effective while working. Ultimately, we rely on productivity impacts as the main test of our hypothesis—both due to its greater objectivity as a measure, and because of the richness in productivity data enabled by our data collection strategy.

6.2 Productivity Effects

In Table III, we test whether receiving the cash infusion alters worker productivity. We estimate average treatment effects on the number of accepted leaf plates using the approach outlined in Section 5.2. Col. 3 corresponds to the specification in equation (1).

In the days following the interim payment, treated workers increase output by 0.109 SD, corresponding to a 6.9% increase in output (Table III Col. 3, $p = 0.020$). In contrast, we see no evidence of a treatment effect during the announcement period: the estimated coefficient is 0.014 SD ($p = 0.685$). Moreover, we can reject that the effects on output during the announcement period and after the interim payment are the same ($p = 0.008$). This indicates that the treatment effects on productivity do not materialize once workers learn about the interim payment, but rather after they receive the cash in hand.

The effects on productivity are not driven by changes in the extensive margin. As intended by our protocols, average daily attendance is high (98.3%), with no treatment effects of the cash infusion on attendance (Appendix Table A.5 Col. 1). Similarly, there is no scope for treatment response in hours per day as work hours are fixed.[14] Consequently, the impacts in Table III reflect increases in actual productivity: how quickly workers produce plates in each hour. These results are robust to alternate empirical specifications in Appendix Tables A.6 and A.7. They are also robust to explicitly controlling the false discovery rate within each family of hypotheses (Appendix Table A.8 Panel C).

The productivity impacts are concentrated among poorer workers, who increase output by 0.204 SD (13.0%) following the cash infusion (Table III Col. 6, $p = 0.003$). In contrast, we cannot reject that there is no impact on the remaining workers ($p = 0.819$). We also continue to find no impact during the announcement period, even among the poorer workers.

[13] We cannot conduct a similar analysis for the richer person's picture since workers do not perceive them as having financial worries to begin with (see Section 2). In addition, we find no impact on self-reported happiness. However, in the psychology literature, happiness is a distinct concept from mechanisms that prevent focus such as worries or rumination. These two sets of concepts are often not even correlated with each other, and psychologists view them as disparate domains. Consistent with this, for example, individuals' level of baseline financial worries is not predictive of their level of happiness (Appendix Table A.4).

[14] After training, workers understand how to create plates and modify mistakes to prevent rejections. During the post-pay period, the average share of rejected plates is only 1.3% in the control group, and we find no significant impacts of the interim payment on this share (Appendix Table A.5 Col. 3). Note that our treatment effect on productivity is economically meaningful, especially when compared to the relatively low wage elasticity researchers have found in other real-effort experiments (DellaVigna et al., 2022).

If we estimate treatment effects separately for each value of the wealth index, the pattern of results remains similar: effects are concentrated among workers with below-median wealth (Appendix Figure A.4). These results are robust to instead using the standard proxy means test characteristic for wealth: the quality of the worker's housing stock (Appendix Table A.9 Col. 1). More generally, while the individual components of the wealth index tend to predict treatment effects, other demographic characteristics we collected at baseline have no predictive power for the results (Appendix Table A.10). For example, treatment effects do not depend on the number of children or years of education. Finally, we see some evidence of heterogeneity by baseline financial worries: treatment effects are concentrated among workers who report feeling worried at baseline (Appendix Table A.11). However, this analysis is underpowered, both because 86% of workers report being worried about their finances at baseline and because we did not collect this baseline variable in all rounds.[15]

There are two potentially complementary interpretations for the stronger impacts among poorer workers. First, these workers may have greater financial strain to start with, thus increasing the scope for our treatment to reduce strain. Alternatively, it is possible that both poorer and richer workers feel mentally burdened by financial strain—since in absolute terms all of them are poor—but the intervention is more meaningful for workers with fewer assets and liquidity since it is larger compared to their wealth. The fact that both richer and poorer workers report high and similar levels of baseline worries and have similar magnitudes of outstanding loans is consistent with this second interpretation (see Table I).

In Figure IV, we plot daily treatment effects of the cash infusion. Recall that treated workers receive their interim payments in the evening before going home for work on day 8 or 9. We stack these observations so that day 1 corresponds to the first day post-interim payment for workers and compare output differences to the baseline period.[16] Among poorer workers, treatment effects materialize immediately, the day after receiving the cash infusion: when workers return to work the following day, their output increases by 0.22 SD, matching the sharp overnight expenditure increase on loans and household necessities seen in Table II. These effects persist and even slightly increase for the remaining days of the contract period.

Finally, note that these effects capture changes in workers' total output since it is unlikely

[15] We do not find differential treatment effects among workers who report having loans that they are worried about, though the results are imprecisely estimated. Since this is one of many different causes for financial worries—and comprises only a fraction of total expenditures—this might not provide a sufficient signal of worries.

[16] Due to this stacking, we cannot show a full day-by-day event study that encompasses both the announcement period and the post-pay period, because these are different lengths and occur on different days across workers in the same round (based on workers' wave assignments) and also across rounds (due to different announcement period lengths across rounds). Thus, we stack the event study at payment day to cleanly and transparently show effects in the post period relative to the baseline. In Table VI, we show day-by-day treatment effects during the announcement period. As we discuss in detail in Section 7.1, we find no evidence of productivity changes immediately following the announcement.

that the treatment meaningfully affected paid or unpaid work outside of the experiment. In our particular context, after a day of wage work, workers do not tend to engage in secondary work activities, including self-employment and domestic duties (e.g., collecting firewood). For instance, using data from a similar population in the same regions of Odisha, India, Breza, Kaur and Shamdasani (2021) find that rural casual workers reported doing any secondary activities after a day of wage work on only 1.72% of days.

6.3 Attentiveness at Work

More detailed production measures, beyond total output, provide a window into *how* workers produce—into mental lapses during production. As described above, we combine three markers of attentional errors into an "attentiveness index" and a "high attentiveness" indicator.

Receiving the interim payment increases workers' attentiveness (Table IV). Across all workers, we find suggestive evidence of an increase in the attentiveness index of 0.077 SD (Col. 1, $p = 0.092$) and an increase in the high attentiveness indicator of 0.095 percentage point (Col. 2, $p = 0.001$). These findings are similar if we replace our attentiveness index, which averages across the component measures, with the first principal component of the three measures (Appendix Table A.12).

Mirroring the impacts of the interim payment on productivity, the effects on attentiveness are concentrated among poorer workers (Cols. 3-5). Among workers with below-median wealth, receiving a cash influx increases attentiveness by 0.17 SD (Col. 3, $p = 0.041$). In contrast, we cannot reject no change in attentiveness among richer workers. These heterogeneity results are similar if we instead examine heterogeneity using the proxy means test measure, house quality, as our wealth indicator (Appendix Table A.9 Cols. 2-3). Finally, again mirroring the impacts on productivity, we detect no treatment effects on attentiveness during the announcement period; the improvements in attentiveness only emerge once the money arrives in workers' hands.

These results indicate that while being flush with cash, poorer workers engage in better planning and leaf placement, resulting in fewer mistakes that have to be undone or patched. As described in Section 6.2, after training, workers rarely make plates that are rejected. Note that a plate that scores higher or lower on the attentiveness index is not inherently of different value: contractors and restaurants pay per usable (i.e., accepted) plate. Rather, the attentiveness index reflects the number of steps needed for a worker to get to a completed plate, with lower attentiveness increasing the number of steps and therefore time per plate.

We interpret these findings as suggesting that the productivity effects we observe are at least partly mediated through improvements in workers' cognitive engagement while working.[17] Workers increase their pace of work, reducing time per plate, but do so while simul-

[17] Consistent with the idea that improved attentiveness reflects improved cognition, we find a strong baseline

taneously *reducing* their rate of mistakes. Such attentional impacts are consistent with a range of potential psychological mechanisms that could operate by improving attentiveness at work, including cash-on-hand reducing worries and thus distractions during work, as well as stress, mental health, or happiness.

6.4 Impacts of Piece-Rate Variation

The interim payment increases workers' productivity and attentiveness. Is this happening because workers are simply more motivated? Or perhaps even more extremely, whenever a worker works harder, do both productivity and attentiveness increase? To better understand the relationship between effort and attentiveness, we examine the effect of experimentally varied piece rates in separate short experimental rounds (see Sections 4.3 and 4.4). Since we adjusted the base wage to hold overall earnings roughly constant across days, unlike our main cash infusion manipulation, this variation should not change workers' mental burdens. Thus, we can isolate the degree to which increased effort affects productivity and attentiveness.

Increasing piece rates raises productivity (Table V, Cols. 1-3). Each one-rupee increase in the piece rate increases output by 0.020 SD ($p = 0.042$), while a 1% increase in the piece rate leads to an output increase of 0.058 SD ($p = 0.038$). This moderate impact is consistent with studies in other contexts, which often find modest piece-rate elasticities in real-effort experiments (DellaVigna et al., 2022). We interpret the output changes due to piece-rate changes as an effort response, i.e., the extent to which output can be changed by conscious effort within the context of our particular task. In contrast, higher piece rates do not alter the attentiveness measures (Cols. 4-6). Across specifications, the point estimates are actually negative, though statistically insignificant. This may suggest an increase in mistakes when workers consciously are hurrying to make extra plates. We can reject that the output and attentiveness effects are the same: a test of equality of coefficients between Cols. 1 and 4 in Table V has a p-value of 0.001.[18]

correlation between workers' attentiveness index and their performance on an incentivized memory task, Corsi, a standard cognitive test in psychology (Appendix Table A.13). We undertook this test in the supplementary piece-rate rounds only to correlate cognitive function with attentiveness. Of course, this is a simple correlation and therefore only suggestive.

[18] In contrast, we cannot reject that the impact of interim payments is the same on output vs. attentiveness (p-values range from 0.556 to 0.778). One may still be concerned that, because productivity effect sizes are small, we may simply lack the power to detect attentiveness effects. The piece-rate effect on attentiveness is -0.013 SD, with a 95% confidence interval of [-0.0327, 0.0073] (Col. 4). In the main manipulation, the treatment effect on attentiveness is 71% the size of the treatment effect on productivity (0.109 SD vs. 0.077 SD). If productivity and attentiveness move together, then one may expect a piece-rate effect on attentiveness of $0.020 \times 0.71 = 0.0142$. This lies outside the above confidence interval (CI) and is 94% larger than the right-hand side of the CI. While this is not conclusive since attentiveness and productivity may not scale linearly, this back-of-the-envelope calculation suggests that power issues do not necessarily undermine our ability to detect effects. In addition, this does not shed light on whether attentiveness may respond to motivation at higher stakes or to incentives explicitly tied to the attentiveness measures.

6.5 Priming

As discussed above, some workers receive a priming intervention, varying timing to occur before or after the exogenous interim payments (see details in Appendix A.2). We find limited evidence for any effects in the one or two hours immediately after workers are primed—the period when priming interventions typically have their strongest effects (Appendix Table A.14 Cols. 1-4). Examining the entire day after priming, we see some suggestive but not statistically significant evidence for productivity effects among poorer workers (Appendix Table A.14 Cols. 5-6). In line with limited priming effects, the treatment effect of receiving the cash infusion is similar across the three priming conditions—no priming, priming before cash infusion, and priming after cash infusion (Appendix Table A.15).[19] Overall, the lack of evidence of priming effects is consistent with the broader debate around both the replicability of priming and how to understand its "first stage"—both treatment intensity, which can be non-monotonic in underlying worries, and what specific set of thoughts or pathways are triggered (e.g., Kahneman, 2012; Cesario, 2014; Banker et al., 2020; Sherman and Rivers, 2021). Rather than using priming to direct attention as a "treatment", directly using attention as an outcome variable—as we do in this paper—may constitute a useful design strategy for sidestepping some of these concerns.

7 Confounds and Supplementary Tests

7.1 Announcement Effects and Perceptions of the Employer

Because the interim payment is delivered by the employer, this raises potential concerns that the treatment could change workers' perceptions toward the employer—specifically stemming from fairness concerns or trust toward the firm.

Announcement effects, gift exchange, and fairness. If treated workers feel they have been given a gift, they might reciprocate by working harder; conversely, if control workers feel they have been treated unfairly, they may reduce effort. While fairness considerations are undoubtedly important in a range of settings, four pieces of evidence indicate they are unlikely to drive our observed treatment effects. First, the most straightforward fairness stories would not (necessarily) imply that the treatment effects should only arise for poorer workers. While ex post, one could argue that poorer workers value the "gift" more, it is

[19] Muralidharan et al. (2023) raise important interpretation and inference concerns regarding factorial designs such as ours; in particular highlighting that estimated treatment effects are the average of treatment effects in each cross-randomized condition. Our estimated treatment effects of the impacts of cash in Table III should be interpreted as weighted average of the treatment effects among individuals in the three priming conditions. Reassuringly, we find very similar point estimates of the treatment effects (0.129) in the no-priming group alone as in our main specification (0.111).

not obvious ex ante that richer workers—who also use the interim payment for immediate expenditures and debt—should not value it at all.

Second, fairness concerns would need to account for the effects on the attentiveness measures, which were collected unbeknownst to workers. When motivated by their own personal interest with higher piece rates, workers do not change their attentiveness; it is consequently unclear why they would then alter it when motivated by a desire to improve output for the employer. Moreover, these measures are unlikely to reflect an attempt to increase plate quality; treated workers spend *less* time per plate, speeding through faster to satisfy minimum standards to earn more money.

Third, under these alternative mechanisms, we would expect there to be some impact of the pay schedule announcement. Even if fairness concerns are more salient after payment is delivered, given the magnitude of our treatment effects post payment, one might expect at least *some* response (even if muted) when the news is delivered on day 5. However, in the results above, we consistently see no evidence of effects in the announcement period. In a more detailed test, Table VI Cols. 1-2 show difference-in-differences regressions comparing the output of the treatment group to that of the control group on the day after the announcement (Cash $\times$ 1 day post announcement) and the day after that (Cash $\times$ 2 days post announcement).[20] In contrast to a fairness concerns story, the announcement effect coefficients are not positive; rather, they are small and statistically insignificant. The upper bound on the 95% confidence interval for the effect immediately after the announcement is 0.055 SD (Col. 1). In contrast, the average treatment effect in the post-pay period is 0.110 SD (Col. 2). We can reject that this coefficient equals the announcement effects at the 1% level.

Fourth, we test whether the control group decreases effort *after* interim payments are delivered to treatment workers. Recall that we further randomized the treatment group to receive the interim payment on day 8 (Wave A) vs. day 9 (Wave B) (Appendix Figure A.5). If workers who are paid later than others feel treated unfairly, then on day 9, control group workers should also feel treated more unfairly relative to the Wave B treatment workers (who have not yet received cash in their pockets but will be paid that evening). However, we see no evidence that control workers work less hard than Wave B treatment workers on day 9. In Table VI Cols. 3-4, the coefficient showing the difference between Wave B treated workers on day 9 relative to control workers is close to zero and insignificant.[21]

[20] We focus on these first two days because not all rounds have longer announcement periods. The announcement is made on the morning of day 5. Workers walk or travel together between the worksites and their villages so that they have discussed each other's pay schedules by the time they return to work on day 6.

[21] Specifically, we add the triple interaction "Cash $\times$ Payment day $\times$ Wave B" in Cols. 3-4 of Table VI. Under this specification, the double interaction "Cash $\times$ Payment day" captures the payday effect for Wave A (on day 8). The triple interaction captures any *incremental* payday effect for Wave B (on day 9), i.e., the difference between the payday effect for Wave B vs. the payday effect for Wave A. Under the fairness confound, this triple interaction should be positive: control workers would be upset about having witnessed Wave A treatment workers be paid on the previous day and drop effort relative to the Wave B treatment

Related studies. Of course, finding a lack of effects from gift exchange or fairness does not detract from their potential relevance in other settings. Rather, we designed our experiment to mitigate the presence of these mechanisms to the extent possible. For example, our setup has several contrasting features with Breza et al. (2018), who find negative morale effects in the same cultural setting. Perhaps most importantly, there were no actual pay differences across workers. Differences in amenities, including features of payment aside from wage levels, are much less likely to trigger fairness violations in this setting relative to differences in wage levels (Kaur, 2019). Consistent with this, debriefs with workers indicate that pay frequency is just one of many job details and does not loom large relative to the "luck" of getting the job during the lean season, along with its associated amenities (steady work with competitive wages, learning a new task, being given lunch at the worksite, etc.). In addition, in contrast to Kaur, Kremer and Mullainathan (2015), our study does not allow us to test for payday effects due to present focus. This is because, in our experiment, workers' output on the day of the interim payment itself did not count toward their payment that evening.

Trust in the employer. An additional potential concern is that the interim payment could increase workers' trust in getting paid in the future. We include several operational features in our design to boost trust. For example, all workers are paid at the end of the first day—in accordance with what workers are told during recruiting—to build trust that we would pay when we promised. We also announced the worksite schedule in advance (e.g., payment schedules announced on day 5) and adhered to it meticulously to instill a feeling of predictability in the worksite. The worksites also operated in the area for months, providing a sense of reliability in the area. In addition, this explanation is inconsistent with the main pattern of results. It is unclear why trust should only increase among poorer workers, why it should affect attentiveness, or why it should lead workers to report feeling more focused at work. Given that higher trust in payment increases one's expected payment per output, such a story also requires a high piece-rate elasticity, in contrast to results in Table V.

We use two further tests to examine this story. First, we verify that we see no evidence for differential treatment effects in rounds that were run in later months at a given worksite—when presumably trust would be higher because the worksite would have built a local reputation for paying as promised (Appendix Table A.16).

Second, we again exploit the staggered timing of cash infusion among Wave A vs. B treatment workers. If workers increase output because they update their beliefs about the probability of payment, then we might expect Wave B treatment workers—who saw Wave A workers being paid—to also update their beliefs when they arrive at work on day 9. However,

workers (who have not yet been paid).

contrary to this story, in Table VI Cols. 3-4, the coefficient on the triple interaction—Cash × Payment Day × Wave B—is not positive; it is close to zero and insignificant. Perhaps more problematic for a trust story, when Wave A workers are paid on day 8 as promised, it is unclear why this should not boost *all* workers' confidence in being paid as expected.

7.2 Physiological Channels: Nutrition and Sleep

The traditional development literature has considered various non-psychological channels through which cash-on-hand may affect productivity. Among these, our design rules out the possibility that our effects operate through investments in traditional human capital (because of the time horizon) or physical capital (because all tools are provided at work). This leaves *physiological* channels, such as nutrition and sleep.

Nutrition. While the workers in our sample are poor, they are not at subsistence; for example, at baseline, 94% of our sample reported not missing any meals in the previous week. However, to the extent that the increased food expenditures affect nutrition, there are two categories of potential pathways for how this may impact productivity. First, according to the biological and medical literatures, one potential pathway—a change in worker's nutritional stock—is unlikely to produce effects overnight (e.g., Gómez-Pinilla, 2008). This is consistent with prior development work that indicates slower-moving or no effects of increased caloric intake on productivity (Schofield, 2020; Park and Kim, 2024).

The second, more plausible channel is short-run blood sugar increases for workers who would otherwise feel hungry at work. Once workers arrive at the worksite, there are no differences among them in food intake; there are no snacks from outside, and any food consumed is provided by us. Consequently, the primary way through which increased food purchases could generate biologically driven changes in productivity overnight is through breakfast consumption. We undertake two tests for such a story, shown in Table VII. First, using data from the expenditure survey, we find no evidence of increased breakfast consumption, including whether workers had breakfast, how much, and what they ate (Cols. 1-5). This appears to be because in this setting, breakfast consumption is fairly inelastic; almost all workers (98%) in the control group report eating breakfast, and almost everyone (94%) reports eating a particular rice dish that is common in the area. Second, if workers experience blood sugar spikes due to increased breakfast consumption, or if they feel more full from eating a larger dinner the night before, we would expect these effects to wear off by the end of the workday, especially as all workers are provided the same food in the afternoon. However, we find persistent (and perhaps increasing) impacts of the interim-pay treatment throughout the day, including the last couple of hours of the workday (Cols. 6-9).

Sleep. An additional physiological channel through which the cash infusion could have affected workers is via improved sleep. At endline, we asked workers to rate sleep quantity

and quality. Control workers report sleeping about 7 hours per night on average. We find no evidence for an increase in the number of hours of sleep (Appendix Table A.17 Col. 1), or self-reported sleep quality (Cols. 2-3). The estimated effects are small in magnitude and insignificant. Moreover, Bessone et al. (2021) do not find evidence of changes in worker productivity due to increased night sleep in a low-income sample in urban India, which may be related to the low quality of sleep in low-income contexts (Rao et al., 2021).

7.3 Mechanisms: Summary and Discussion

Our experiment is primarily designed to test whether providing workers with cash-on-hand impacts productivity. Our findings indicate that an improved ability to be attentive at work helps drive the productivity gains we see. While we rule out physiological channels such as nutrition and sleep, and some obvious confounds such as gift exchange, our study is not designed to pinpoint the specific psychological pathway from cash-on-hand to improved attentiveness and productivity. There are several such pathways. Reducing financial constraints could directly lower anxiety about one's expenses. Less directly, it could reduce fights with one's spouse or prevent feelings of guilt from seeing one's children cry for an item they want. Any of these could then divide attention and reduce the capacity to focus at work.

While the goal of our paper is not to differentiate between these channels, we can use our data to help gain some insight into their scope. Through three open-ended questions, we ask workers about sources of financial worries for themselves, what they were thinking about while at work, and potential sources of worries for others (as reported in Figure I, Figure III, and Appendix Table A.3, respectively). In each case, workers are asked to list as many sources of worry as they can think of. Marital conflict is rarely mentioned: less than 2% of the time among the control group in any of the questions. In contrast, most workers report feeling anxiety about and ruminating over their financial problems while at work. Anxiety about fulfilling needs for one's children or family is mentioned frequently; worries over other items requiring cash outlays, such as health issues and daughters' marriage, also appear.

Overall, according to workers' self-reports, workers' most top-of-mind worries while working are their financial concerns, thus providing scope for the treatment to alleviate them. Since our treatment directly impacted these concerns (Figure III and Table II), it is reasonable to assume that a reduction in financial concerns drives at least some of our results. At the same time, workers' responses also suggest the potential for additional psychological benefits of cash via dynamics in the household, all of which could lead to less rumination and divided attention at work. We do not attempt to disentangle these channels but rather view them as a bundle. They are interrelated and together constitute ways through which relieving financial strain can improve workers' ability to focus while making plates. In addition, we do not take a stance on the exact psychological mechanism through which changes in focus may

occur—such as attention, affect, or mental health.

8 Conclusion

We are only beginning to understand the psychological consequences of poverty. The early work has largely been on laboratory measures of cognition, self-reported well-being, mental health, or biomarkers such as stress (Mani et al. 2013; Chemin et al. 2013; Carvalho, Meier and Wang 2016; Haushofer and Shapiro 2016, 2018; Ridley et al. 2020). Evidence on economic field behaviors is a necessary next step to understand the implications for economic outcomes, and earnings are a particularly important outcome with widespread consequences. The impact of financial concerns on earnings could eventually change our thinking about impediments to escaping poverty and related policies. Though these lessons are down the road, requiring a great deal more empirical work, we suggest potential avenues.

First, the positive impact of early payment seems to say something about optimal payment frequency, specifically that more frequent payments (say, weekly rather than monthly) could be better. However, care should be taken in making such an inference because it omits another important consideration: worker self-control problems in consumption. When those are included, the analysis becomes more complex. Consider the following example. Suppose that a worker is paid monthly and also has rent due monthly. If that worker receives a weekly payment, self-control problems may lead them to save too little and at the end of the month they may not be able to make rent payments. Weekly payment may—when combined with lumpy consumption and imperfect self-control—create *more* financial strain. Workers in our context do not have such lumpy consumption needs, but they may in other contexts. Once there is a schedule of consumption needs, the optimal payment frequency will need to account for both the financial strain effects we document as well as the potential for self-control problems in consumption. Such a careful analysis might reveal an intuitive payment structure: payment frequency (and sizes) that matches the expenditure needs. More broadly, a focus on payment frequency alone might be too narrow; financial products that allow workers to move income to match expenses could be a more general solution and one that does not appear to be present in the market (Pew Charitable Trusts, 2016). Additionally, these issues raise important questions of market efficiency: what frictions, if any, prevent firms from providing these optimal payment contracts or offering these financial products?

Second, these effects may cause us to reconsider cash transfer programs in search of similar direct effects. For instance, Fink, Jack and Masiye (2020) document increases in on-farm labor supply and harvest output following liquidity drops among Zambian farmers; Banerjee et al. (2015) and Bandiera et al. (2017) find large and persistent impacts of bundled treatments to support the ultra-poor. Such impacts are often attributed to neoclassical explanations such as credit constraints (Matsuyama, 2011; Ghatak, 2015; Balboni et al., 2022). Our evidence

suggests that direct effects of changes in financial strain could contribute to the positive impacts of such interventions. Moreover, these programs may have broader social returns. Except for self-employed individuals, most workers are not able to internalize the returns of their productivity. Consequently, transfer programs could have supply-side multiplier effects via higher firm productivity, providing an additional rationale for subsidizing such programs.

Third, we might want to consider models that incorporate the effects we have found. For instance, our results could suggest a different interpretation of efficiency wages. Firms may voluntarily pay workers more not to enhance nutrition (Dasgupta and Ray, 1986), avoid moral hazard (Shapiro and Stiglitz, 1984), or improve worker selection (Weiss, 1980), but to enhance focus and productivity. Similarly, regulations that improve workers' financial well-being such as minimum wages could have additional productivity benefits for workers with high levels of financial strain (Coviello, Deserranno and Persico, 2022).

Finally, if poverty reduces productivity, it creates a mechanism that amplifies negative income or wealth shocks. Faced with a calamity, people would be less productive exactly when they are in greatest need of cash. These problems are particularly severe given that in most poor countries, individuals are especially reliant on labor earnings to smooth consumption and self-finance productive investment in their enterprises (Kochar, 1999). Accordingly, if poverty negatively affects productivity, then the benefits of reducing volatility (e.g., through stable employment or public workfare programs) or mitigating financial vulnerability (e.g., through credit access or unemployment insurance) could be larger than predicted in the traditional economics literature. More broadly, the psychological impact of poverty on productivity offers directions for new models of poverty traps, as exemplified by recent work by Sergeyev, Lian and Gorodnichenko (2023).

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Table I: Sample Characteristics and Tests for Baseline Balance

	Control Mean (1)	Coef. on Cash (2)	p-value (3)
Panel A. Demographics, Labor, and Wealth			
Age	39.188	-0.447	0.596
	[8.856]	(0.841)	
Years of education	4.721	-0.060	0.857
	[3.540]	(0.330)	
Can read newspaper in Odiya	0.630	0.021	0.670
	[0.484]	(0.048)	
Married	0.984	-0.012	0.468
	[0.127]	(0.016)	
Has any children	0.891	-0.038	0.262
	[0.313]	(0.034)	
Primarily daily laborer	0.751	-0.056	0.216
	[0.433]	(0.045)	
Days of paid work in past 7 days	1.884	-0.130	0.509
	[2.125]	(0.196)	
Days of paid work in past 30 days	8.602	0.098	0.889
	[6.307]	(0.701)	
House quality (durable house)	0.238	0.003	0.946
	[0.427]	(0.042)	
Owns farmland	0.568	0.012	0.788
	[0.497]	(0.046)	
No outstanding food loans	0.459	-0.006	0.902
	[0.500]	(0.051)	
Can get Rs. 1K in emergency	0.355	-0.034	0.458
	[0.480]	(0.046)	
Wealth index (continuous)	0.406	-0.006	0.809
	[0.246]	(0.024)	
Panel B. Financial Worries and Loans			
Worried about finances	0.883	-0.022	0.551
	[0.323]	(0.037)	
Worried about any loan	0.579	-0.031	0.513
	[0.495]	(0.048)	
Amount of loans worried about	14,625	-913	0.679
	[15,994]	(2,204)	
Has loans	0.683	0.027	0.550
	[0.467]	(0.045)	
Has moneylender loans	0.175	-0.021	0.572
	[0.381]	(0.037)	
Panel C. Baseline Attendance and Productivity			
Attendance	0.978	0.005	0.550
	[0.146]	(0.008)	
Hourly production	3.353	0.073	0.643
	[2.159]	(0.158)	
Hourly production (normalized)	1.398	0.030	0.643
	[0.900]	(0.066)	
Attentiveness index (continuous)	-0.053	0.000	0.999
	[0.783]	(0.052)	
N: workers (Control or Cash)	183	224	

Notes: This table reports baseline worker characteristics for the control group and tests for baseline differences between the control group and the interim-pay treatment group. Cols. 2 and 3 show the coefficient and the p-value of a regression at the worker level of each variable on a treatment indicator with round-wave (strata) fixed effects. For attendance, the regression is at the worker-day level, and for hourly production and the attentiveness index, the regression is at the worker-hour level. Standard deviations are reported in brackets and robust standard errors in parentheses. The wealth index is a simple average of four binary variables: house quality (i.e., living in a non-mud house, constructed of durable material); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 easily in case of an emergency. Hourly production is normalized by dividing by the control group's standard deviation in the post-pay period. To generate the attentiveness index, we average the normalized value of each of the three measures of attentiveness (number of double holes, leaves, and stitches per plate), with the scale reversed so that a higher value on the index corresponds to improved attentiveness.

Table II: Effects on Expenditures

	Loans and Credits		Household expenditures						Total expenditures
	Amount	Any Payment	Total	Food	Clothes	HH Essentials	Medical	Tobacco/ Alcohol	Amount
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
PANEL A: Overall Impacts									
Cash	270.77***	0.40***	149.95***	68.61***	34.58**	13.63***	13.18	-0.28	371.24***
	(53.79)	(0.04)	(39.00)	(24.42)	(16.88)	(5.07)	(12.29)	(4.56)	(67.74)
Control group mean	94.20	0.18	372.37	270.36	14.31	7.92	31.55	34.01	568.08
N: workers	402	402	402	402	402	402	402	402	402
PANEL B: Daily Impacts									
Cash × Day of payment	169.47***	0.17***	69.64***	49.48***	0.79	6.96**	3.73	2.76	205.19***
	(45.07)	(0.04)	(16.88)	(13.75)	(4.21)	(3.03)	(5.03)	(1.98)	(34.24)
Cash × 1 day post-pay	66.61**	0.13***	39.30*	18.01	9.45	3.84**	-0.61	-0.23	109.47***
	(26.37)	(0.03)	(21.59)	(15.15)	(7.06)	(1.79)	(7.43)	(1.75)	(37.26)
Cash × 2 days post-pay	39.07*	0.16***	46.20*	1.26	27.43*	3.19	11.33	-3.17	63.73
	(21.20)	(0.04)	(25.19)	(12.36)	(16.52)	(3.84)	(10.05)	(2.24)	(44.35)
Control group mean	32.55	0.07	128.65	93.40	4.94	2.74	10.90	11.75	196.26
Control group mean, day of payment	22.72	0.07	102.43	79.20	3.86	1.47	5.53	10.24	146.06
N: worker-days	1,160	1,160	1,160	1,160	1,160	1,160	1,160	1,160	1,160

Notes: This table tests for the impact of the interim-pay treatment on expenditures. The table compares average differences in expenditures in the 3 days following the interim payment among treatment vs. control workers.

- Panel A shows the overall impacts of the treatment using regressions at the worker level. "Cash" is a binary indicator for being in the interim-pay treatment group.
- Panel B shows the treatment effect on each day following the cash infusion with regressions at the worker-day level. "Day of payment" is the day on which the interim-pay treatment group received cash at the end of work, so workers were able to spend money that evening.
- The dependent variables in the first two columns are the total amount of payments toward loans or credits (Col. 1) and whether the participant made any such payments (Col. 2). "HH essentials" (Col. 6) include expenses on soap, detergent, other toiletries, petrol, and diesel. Total household expenditures (Col. 3) include the expenses in Cols. 4-8 as well as miscellaneous spending on children, education, electric bills, mobile recharge, and transportation fares. "Total expenditures" (Col. 9) include spending on agricultural inputs, construction, transfers, and festivals in addition to loans and household expenditures. The daily amounts of total expenditures are winsorized at the 99th percentile.
- Data are based on recall from a survey administered on the final day of each round. The regressions in Panel A control for round-wave (strata) fixed effects and those in Panel B control for round-wave-day fixed effects. In addition, all regressions control for the baseline covariates chosen using the Lasso post-double-selection procedure (Belloni, Chernozhukov and Hansen, 2014) in the regression in Panel B, Col. 9 of this table. Robust standard errors are reported in Panel A, and standard errors are clustered by worker in Panel B. *$p < .10$, **$p < .05$, ***$p < .01$.

Table III: Effects on Worker Productivity

	Hourly Production					
	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post-pay	0.097**	0.108**	0.109**	0.111**	0.220***	0.204***
	(0.047)	(0.047)	(0.047)	(0.047)	(0.079)	(0.069)
Cash × Post-pay × Higher wealth					-0.284**	-0.190**
					(0.144)	(0.093)
Cash × Announcement period	-0.002	0.014	0.014	0.012	0.013	0.039
	(0.035)	(0.035)	(0.035)	(0.035)	(0.072)	(0.061)
Cash × Announcement × Higher wealth					0.013	-0.039
					(0.135)	(0.081)
Linear baseline output	Y	Y	Y	Y	Y	Y
Quadratic baseline output	N	Y	Y	Y	Y	Y
Post-double selection lasso controls	N	N	Y	Y	Y	Y
Day FE and hour FE	N	N	N	Y	N	N
Round-wave FE	Y	Y	Y	Y	Y	Y
P-val: Cash × Post-pay = Cash × Announcement	0.006	0.008	0.008	0.007	0.000	0.000
Wealth index					Continuous	Binary
Coef: Cash × Post-pay + Cash × Post-pay × Wealth					-0.064	0.014
SE: Cash × Post-pay + Cash × Post-pay × Wealth					0.093	0.063
P-val: Cash × Post-pay + Cash × Post-pay × Wealth					0.489	0.819
N: worker-hours	17,441	17,441	17,441	17,441	17,381	17,381

Notes: This table tests for the impact of the interim-pay treatment on worker productivity. Regressions are at the worker-hour level. The sample includes all observations post announcement of the pay schedule.

- The dependent variable is the number of accepted leaf plates produced in a given worker-hour, normalized by dividing by the standard deviation of the control group in the post period. "Cash" is a binary indicator for whether an individual is in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment. "Announcement period" equals 1 in the period following the pay schedule announcement but prior to the interim payment.
- Cols. 1-4 present average treatment effects across workers. Col. 1 controls for the worker's linear baseline output, Col. 2 adds a control for quadratic baseline output. Col. 3 controls for the covariates chosen using the LASSO post-double-selection procedure (Belloni, Chernozhukov and Hansen, 2014). Col. 4 additionally adds day and hour fixed effects.
- Cols. 5-6 show heterogeneous treatment effects by wealth. Regressions correspond to the Panel A Col. 3 specification, but add interactions with a proxy for higher wealth. Col. 5 uses the continuous wealth index, which averages four binary measures: high house quality (i.e., living in a non-mud house); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 easily in case of an emergency. Col. 6 uses a binary indicator that equals 1 if the worker's wealth index is weakly greater than the sample median.
- All regressions include round-wave (strata) fixed effects. Standard errors are clustered by worker. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table IV: Effects on Attentiveness

	Attentiveness index (1)	High attentiveness (2)	Attentiveness index (3)	Attentiveness index (4)	High attentiveness (5)
Cash × Post-pay	0.077*	0.095***	0.170**	0.133**	0.122***
	(0.045)	(0.029)	(0.083)	(0.064)	(0.040)
Cash × Post-pay × Higher wealth			-0.243	-0.114	-0.056
			(0.177)	(0.089)	(0.054)
Cash × Announcement period	-0.001	0.027	0.043	0.022	0.043
	(0.043)	(0.026)	(0.086)	(0.063)	(0.039)
Cash × Announcement × Higher wealth			-0.098	-0.037	-0.027
			(0.178)	(0.087)	(0.053)
P-val: Cash × Post-pay = Cash × Announcement	0.050	0.010	0.014	0.015	0.019
Wealth index			Continuous	Binary	Binary
Coef: Cash × Post-pay + Cash × Post-pay × Wealth			-0.072	0.019	0.066
SE: Cash × Post-pay + Cash × Post-pay × Wealth			0.116	0.063	0.039
P-val: Cash × Post-pay + Cash × Post-pay × Wealth			0.534	0.765	0.092
N: worker-hours	13,020	13,020	12,982	12,982	12,982

Notes: This table tests for the impact of the interim-pay treatment on attentiveness. Regressions are at the worker-hour level. The sample includes all observations post announcement of the pay schedule.

- The attentiveness index is comprised of three proxies for attentiveness: the average number of leaves, stitches, and double holes (which signifies that a stitch was removed to correct a mistake) per plate during the production hour slot. The three measures are normalized using the control group's production (mean and standard deviation) in the post-pay period. We then take a simple average to create the attentiveness index, with the scale reversed (multiplied by -1) so that a higher value on the index corresponds to improved attentiveness. "High attentiveness" indicates that the index value is greater than the sample median.
- "Cash" refers to whether an individual is in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment. "Announcement period" equals 1 in the period following the pay schedule announcement but prior to the interim payment.
- Cols. 1-2 present average treatment effects across workers. Cols. 3-5 test for the heterogeneous treatment effects by wealth by adding interactions with a proxy for higher wealth. Col. 3 uses the continuous wealth index; Cols. 4-5 use a binary indicator that equals 1 if the worker's wealth index is weakly greater than the median. All regressions control for the covariates chosen using the LASSO post-double-selection procedure in the regression in Col. 1 in this table. All regressions also include round-wave (strata) fixed effects. Standard errors are clustered by worker. *$p < .10$, **$p < .05$, ***$p < .01$.

Table V: Piece-Rate Variation

	Hourly production			Attentiveness index			Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Piece rate	0.020**			-0.013			0.000		
	(0.010)			(0.010)			(0.006)		
Log(piece rate)		0.058**			-0.035			0.002	
		(0.028)			(0.029)			(0.017)	
Piece rate = Rs. 3			0.024			-0.004			0.014*
			(0.018)			(0.024)			(0.008)
Piece rate = Rs. 4			0.040**			-0.025			-0.000
			(0.020)			(0.020)			(0.012)
P-val: equality of coefficients									
Piece rate in (1) and (4)	0.001								
Log(piece rate) in (2) and (5)		0.001							
Piece rate = Rs. 3 in (3) and (6)			0.211						
Piece rate = Rs. 4 in (3) and (6)			0.001						
N: worker-hours	4,374	4,374	4,374	4,373	4,373	4,373			
N: worker-days							898	898	898

Notes: This table tests for the impact of changing piece rates on worker productivity and attentiveness. The observations come from supplementary rounds (without the interim-pay treatment) with 150 workers.

- The dependent variables are normalized hourly production (Cols. 1-3), the attentiveness index (Cols. 4-6), and daily attendance (Cols. 7-9). The production and attentiveness measures are normalized using the same control group mean and standard deviations as the measures in the main rounds.
- The piece-rate wage was randomized to be either Rs. 2, 3, or 4, so the omitted category in Cols. 3, 6, and 9 is a piece-rate wage of Rs. 2.
- On the first day of each piece-rate round, workers were paid a flat wage rather than a piece rate. The regressions in Cols. 1-6 use hourly observations after the first day, conditional on attendance. Cols. 1-3 and Cols. 7-9 control for the same covariate controls used in Col. 3 of Table III. Cols. 4-6 use the same controls used in Table IV. All regressions control for round fixed effects. Standard errors are clustered by worker. $*p < .10$, $**p < .05$, $***p < .01$.

Table VI: Fairness Concerns

	Hourly Production			
	(1)	(2)	(3)	(4)
Cash × 1 day post announcement	-0.015	-0.034		
	(0.036)	(0.039)		
Cash × 2 days post announcement	0.032	0.015		
	(0.036)	(0.038)		
Cash × Announcement period			0.021	0.000
			(0.031)	(0.034)
Cash × Payment day			0.078	0.067
			(0.059)	(0.059)
Cash × Payment day × Wave B			0.007	-0.006
			(0.091)	(0.092)
Cash × Post-pay		0.110**		0.109**
		(0.047)		(0.047)
Post-payment period	N	Y	N	Y
P-val: Cash × Post-pay = Cash × 1 day post announcement		0.009		
P-val: Cash × Post-pay = Cash × 2 days post announcement		0.029		
P-val: Cash × Post-pay = Cash × Announcement				0.005
N: worker-hours	9,651	17,441	9,651	17,441

Notes: This table tests for effects on productivity during the announcement period.

- "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group. "1 day post announcement" is an indicator that equals 1 on the day the pay announcement was made (i.e., corresponding to day 5, the day the announcement is made in the morning), and "2 days post announcement" is an indicator that equals 1 the day after that.
- "Payment day" is an indicator that equals 1 on the day when the interim payment occurred for a given worker's wave (i.e., day 8 for Wave A workers and day 9 for Wave B workers). Cash payments were made in the evening after work on these days, so "Cash × Payment day" captures effects during the workday before the evening payment was made to treatment workers.
- "Post-pay" is an indicator for the post-pay period for the worker's wave (after the interim-cash payments have been disbursed).
- Cols. 1 and 3 restrict the sample to exclude the post-pay period; the remaining columns include the full sample. Cols. 1-2 also include an indicator for 3+ days post announcement but before the interim payment interacted with Cash. All regressions include round-wave (strata) fixed effects and control for the same selected covariates used in Col. 3 of Table III. Standard errors are clustered by worker. *$p < .10$, **$p < .05$, ***$p < .01$.

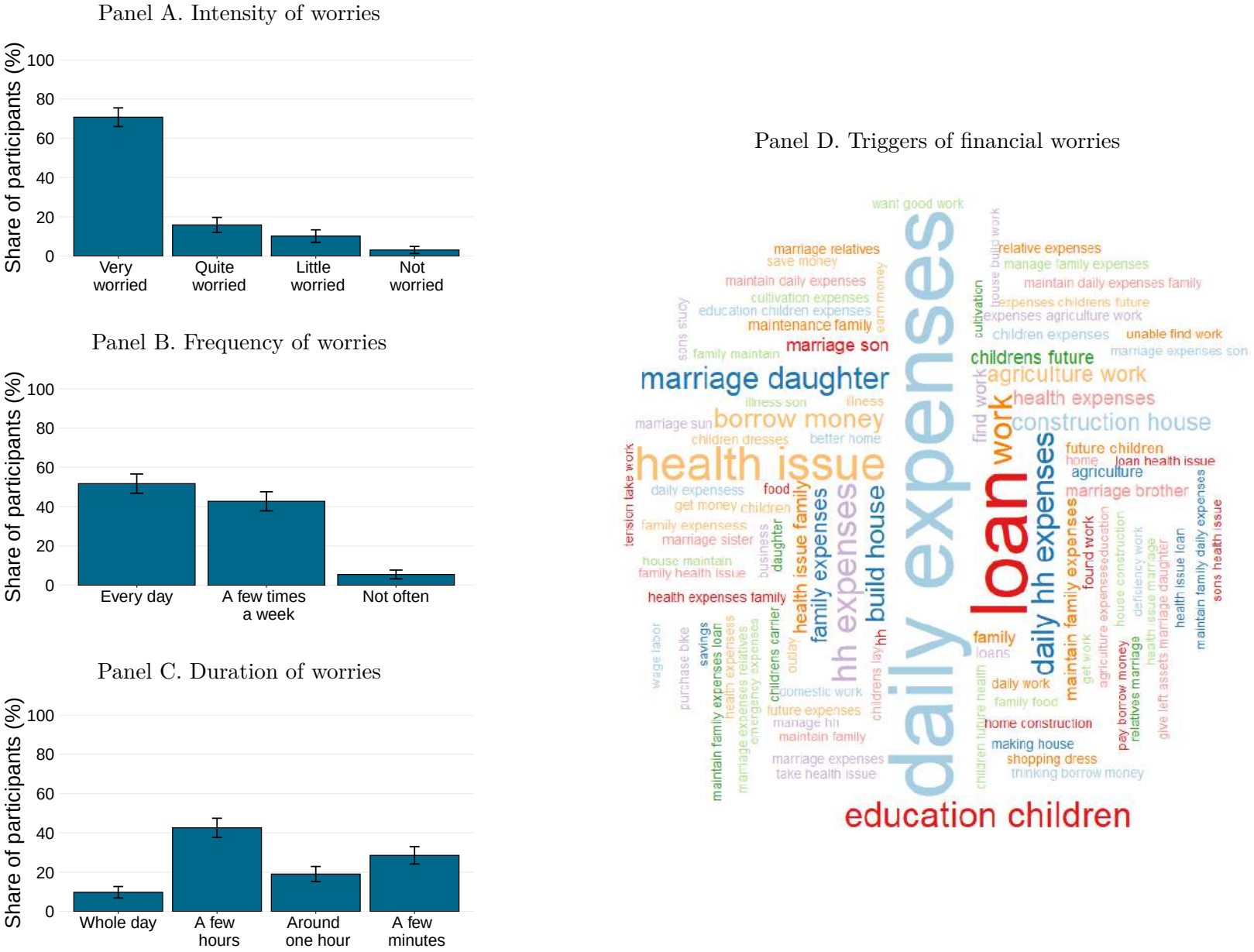
Table VII: Tests for Nutrition Channels

	Breakfast Measures (Post-pay Period)					Hourly Production			
	Had any breakfast	Ate rice	Amount of rice	Ate vegetables	Ate any other item				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cash	-0.007	-0.002	-4.048	-0.024	0.059				
	(0.013)	(0.025)	(7.223)	(0.042)	(0.044)				
Cash × Post-pay						0.060	0.173**	0.104**	0.083*
						(0.050)	(0.073)	(0.047)	(0.045)
Cash × Post-pay × Hour of day						0.014**	0.008		
						(0.007)	(0.010)		
Cash × Post-pay × Higher wealth							-0.204**		
							(0.103)		
Cash × Post-pay × Hour of day × Higher wealth							0.005		
							(0.013)		
Cash × Post-pay × Last 2 hours of day								0.013	
								(0.020)	
Cash × Post-pay × Last 1 hour of day									0.104***
									(0.026)
Control group mean	0.984	0.938	180.625	0.759	0.266				
N: workers	320	320	320	320	320				
Coef: cash effect + interaction								0.117	0.187
SE: cash effect + interaction								0.048	0.054
P-val: cash effect + interaction								0.016	0.001
N: worker-hours						17,441	17,381	17,441	17,441

Notes: This table tests whether improved nutrition can account for the treatment effects on productivity.

- Cols. 1-5 present worker-level regressions where the dependent variables are breakfast consumption measures averaged across the two mornings following the interim cash payment day for each wave. This time window corresponds to the same period examined for the impacts on expenditures in Table II. "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group. These regressions control for round-wave (strata) fixed effects and the same covariate controls as in Table II. Robust standard errors are reported.
- In Cols. 6-9, the dependent variable is normalized hourly production. "Post-pay" equals 1 on the days after interim payment. "Hour of day" is a linear control for the work hour within a production day. "Last 1 (2) hour(s) of day" is an indicator for the last one (two) production hours in a day (for rounds with 5-hour schedules) or post-lunch production (for rounds with 7-hour schedules). "Higher wealth" is an indicator that equals 1 if the worker's wealth index is weakly greater than the median. Regressions control for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

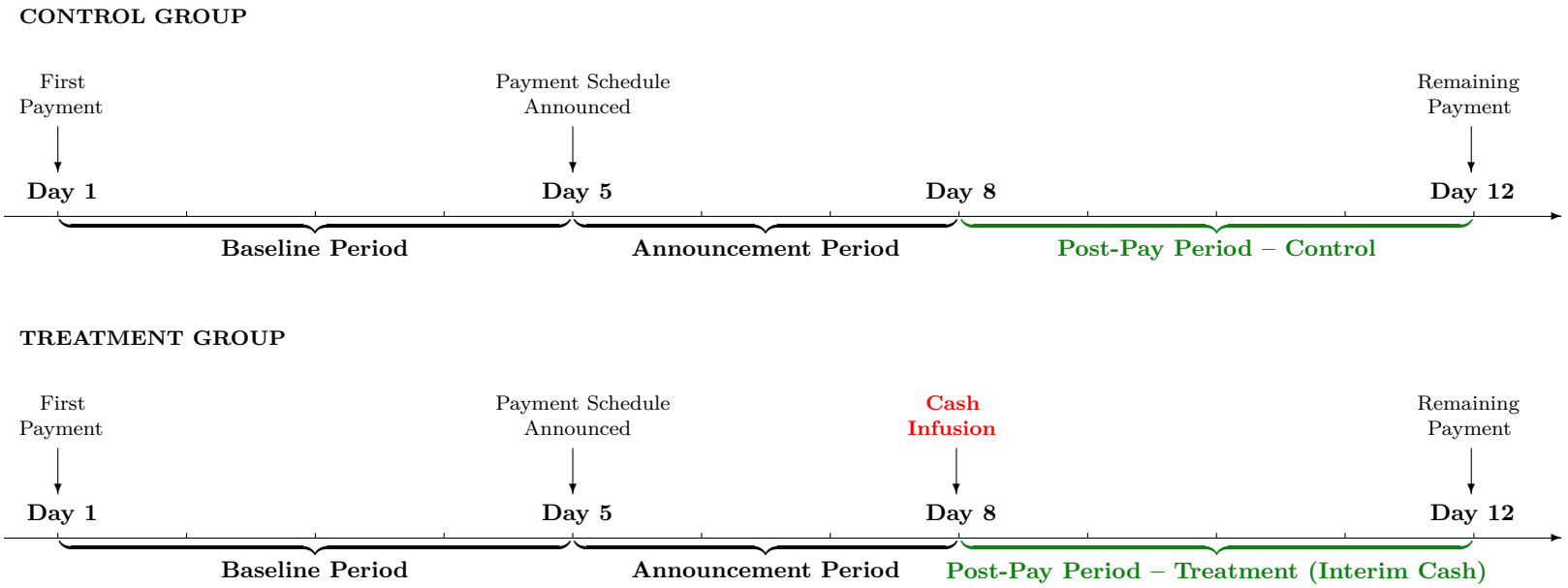
Figure I: Financial Concerns



Notes: This figure shows participants' responses to questions related to financial worries and stressors.

- In Panels A to C, we show answers to four questions: (A) "How worried are you about your finances?" ($N = 352$); (B) "How often do you worry about your finances?" ($N = 402$); and (C) "How long do you worry about finances every day?" ($N = 401$). Question (A) was asked during baseline, except in rounds 3 and 4. Questions (B) to (C) were asked during later surveys; we exclude those not present on those survey days.
- Panel D shows a word cloud representing answers to the question "What makes you worry about money issues?" ($N = 402$). Surveyors entered workers' responses in short phrases or sentences. The font size is proportional to the frequency of terms mentioned by participants. We visualize their frequency distribution after minimal processing, i.e., removing stop words and typos (e.g., Fellows, 2012).

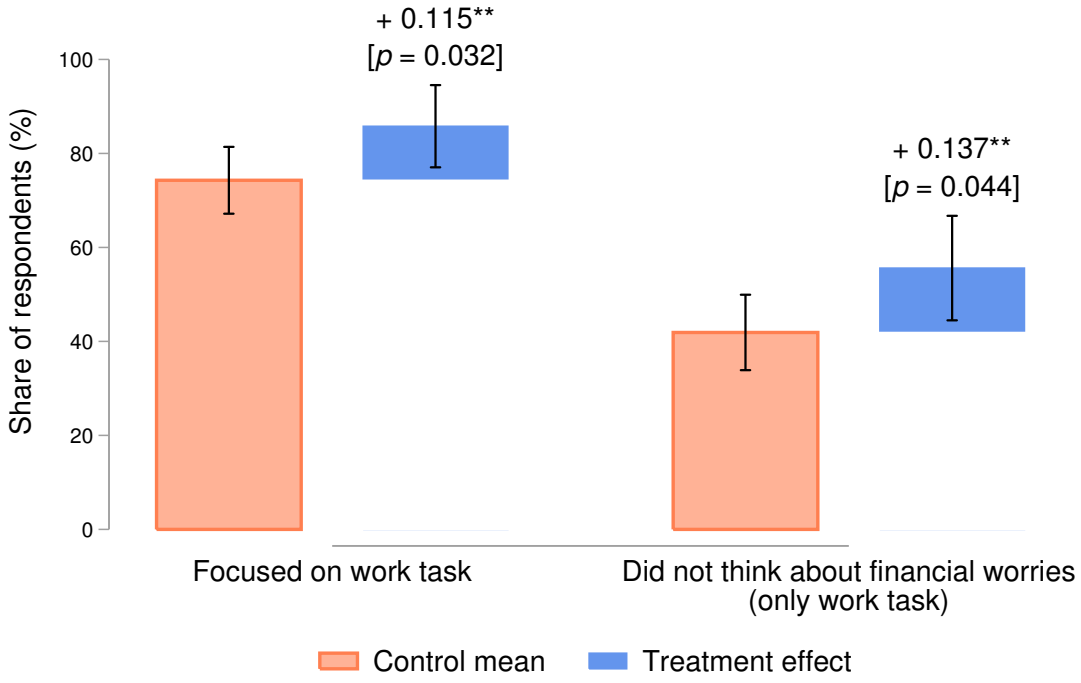
Figure II: Experimental Design



Notes: This figure shows the experimental design of the study.

- In the control group (upper timeline), workers were paid their training wage on day 1 and received the rest of their accrued earnings on day 12.
- In the treatment group (lower timeline), workers were paid their training wage on day 1. They then received an interim payment on day 8, comprised of their accrued earnings from days 2 to 7. They received the remainder of their accrued earnings on day 12.
- Within each round, all workers were cross-randomized to Wave A or Wave B. The payment schedule for Wave A workers is shown here. Wave B treatment workers were paid one day later, on day 9 (see Appendix Figure A.5 for detailed depiction).
- Within each of the treatment and control groups, workers were randomized to receive the priming intervention on day 6, day 10, or not at all for Wave A, and on days 7, 11, or not at all for Wave B.

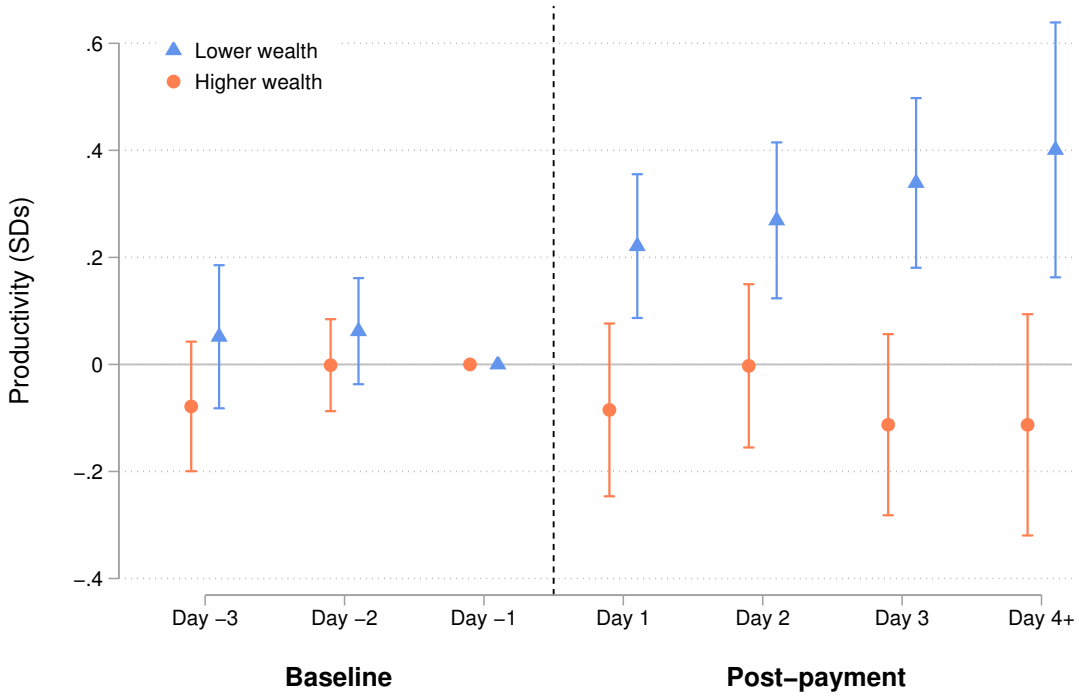
Figure III: Thoughts while Working



Notes: This figure tests for impacts of the interim payment on workers' self-reported thoughts while working.

- Answers were collected from an unprompted, open-ended question asked at the end of the workday, 2 days after the interim payment was disbursed in each wave: "What were you thinking about while you were working today?" Workers could list as many items as they wanted.
- "Focused on work task" equals 1 if the worker mentioned anything about thinking about work or the work task, and zero otherwise. "Did not think about financial worries" equals 1 if the worker did not report any thoughts related to worrying about finances (only the work task).
- The orange bars show the mean of each variable for the control group. The blue bars show the coefficient of a regression on the interim-pay treatment indicator. All regressions control for baseline proxies for financial worry: level of self-reported financial worry (collected in a subset of rounds), having a high-interest (i.e., moneylender) loan, number of loans the worker is worried about, and number of days of paid employment in the past month; variables with missing values are coded as zero and a dummy indicating the variable is missing is included in the regressions. Regressions also include round-wave (strata) fixed effects.
- The sample is 234 workers. This includes all workers except those who received priming on the same day this question was asked. 90% confidence intervals are shown. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Figure IV: Treatment Effects of Interim-Cash Payment on Worker Productivity



Notes: This figure plots the estimated effects of the interim payment on hourly output, comparing the treatment and control group, separately for workers with above and below median values of the wealth index.

- The x-axis indexes days so that Day 1 is the first day of the post-pay period (after the interim payment is disbursed to treatment workers in a given wave). Day -1 is the last day of the baseline period (before treatment status is announced), and is the omitted time category in the regression.
- The wealth index is an average of four binary measures: house quality (i.e., living in a non-mud house, constructed of durable material); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 easily in case of an emergency. "Higher wealth" is an indicator that equals 1 if the worker has an above-median value of the wealth index.
- Estimates are from a difference-in-differences regression on the full sample, with controls for worker and day fixed effects. The regression also includes controls to absorb the announcement period.
- Standard errors are clustered by worker. 90% confidence intervals are shown.

ONLINE APPENDIX:

DO FINANCIAL CONCERNS MAKE WORKERS LESS PRODUCTIVE?

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Contents

1 Appendix Figures and Tables

1.1 Appendix Tables

Table A.1: Sample Characteristics for Supplementary Piece-Rate Rounds

	Mean: No piece-rate rounds (1)	Coef: In piece-rate rounds (2)	P-value (3)
Panel A. Demographic Characteristics and Financial Worries			
Age	39.508	2.094	0.073*
	[8.692]	(1.165)	
Years of education	4.230	-0.179	0.706
	[3.556]	(0.473)	
Can read newspaper in Odiya	0.625	-0.052	0.420
	[0.485]	(0.064)	
Married	0.977	-0.000	0.992
	[0.151]	(0.021)	
Has any children	0.891	-0.019	0.725
	[0.313]	(0.053)	
Primarily daily laborer	0.739	0.005	0.931
	[0.440]	(0.061)	
Days of paid work in past 7 days	1.944	-0.109	0.711
	[1.979]	(0.294)	
Days of paid work in past 30 days	9.098	-0.817	0.407
	[6.448]	(0.985)	
Wealth index (continuous)	0.376	0.061	0.083*
	[0.266]	(0.035)	
Higher wealth (binary)	0.479	0.069	0.349
	[0.501]	(0.073)	
Worried about finances	0.857	0.036	0.476
	[0.351]	(0.051)	
Worried about any loan	0.599	0.047	0.490
	[0.491]	(0.067)	
Amount of loans worried about	16,315	1,470	0.654
	[18,498]	(3,275)	
Has loans	0.732	0.009	0.892
	[0.444]	(0.068)	
Has moneylender loans	0.202	-0.052	0.282
	[0.403]	(0.048)	
Panel B. Baseline Performance			
Hourly production	3.927	0.170	0.174
	[2.356]	(0.125)	
Attentiveness index (continuous)	0.024	0.127	0.155
	[0.767]	(0.089)	
Panel C. Treatment Probability			
Cash	0.591	-0.083	0.276
	[0.493]	(0.076)	
N: workers	257	150	

Notes: This table reports baseline worker characteristics for two worker groups: those who are only in the main rounds vs. those who are also included in the supplementary piece-rate rounds. Cols. 2 and 3 show the coefficient and the p-value of a regression at the worker-level of each variable on an indicator for being in supplementary rounds with round-wave (strata) fixed effects. For hourly production and the attentiveness index, the regression is at the worker-hour level. The remaining regressions are at the worker level. "Cash" is a binary indicator for being in the interim-pay treatment group. Standard deviations are reported in brackets and robust standard errors in parentheses. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

Table A.2: Effects on Expenditures, Borrowing, and Lending

	Expenditure on Durable Goods (1)	Expenditures Taken on Credit (2)	Borrowing at Worksite (3)	Lending at Worksite (4)
Cash	-5.61	-119.65**	-0.09***	0.09***
	(4.28)	(46.43)	(0.02)	(0.03)
Control group mean	6.83	202.93	0.09	0.02
N: workers	402	402	400	400

Notes: This table tests for the impact of the interim-pay treatment on expenditures on durable goods, expenditures taken on credit, as well as borrowing and lending at the worksite.

- "Cash" is a binary indicator for being assigned to the interim-pay treatment group. All regressions are at the worker level.
- Cols. 1-2 compare average differences in expenditures in the 3 days following the cash infusion among treatment vs. control workers. The dependent variable in Col. 1 is the amount of expenditures on durable goods. This includes spending on agricultural machinery (e.g., renting or buying tractors) and purchases of tools such as plows and hoes. The dependent variable in Col. 2 is the total amount of expenditures taken through loans or on credit with a shop. This is a subset of the total expenditures reported in Table II Col. 9.
- Cols. 3-4 compare average differences in the tendencies to borrow from or lend to other workers at the worksite, in the post-payment days of the contract period. In Col. 3 (4), the dependent variable equals 1 if the worker borrowed from (lent to) someone at the worksite, and 0 otherwise.
- These regressions control for round-wave (strata) fixed effects and the same covariate controls as in Table II. Robust standard errors are reported.

Regressions use survey responses from the end of the contract period. No baseline survey is available for these outcomes.

Table A.3: Effects on Top-of-Mind Worries

	Reasons other than worries		**Reasons other than worries or poverty**	
	(1)	(2)	(3)	(4)
Cash	0.102**	0.101**	0.092**	0.086**
	(0.049)	(0.050)	(0.039)	(0.039)
Control group mean	0.33	0.33	0.14	0.14
Baseline worries	N	Y	N	Y
N: workers	402	401	402	401

Notes: This table shows the impacts of the interim pay treatment on what worries workers ascribe to an anonymous person, as a way to gauge what is top of mind in their thoughts.

- Answers were collected from the exit survey on the last work day. Workers were shown a photo of a middle-aged man, Panel A of Appendix Figure A.1. They were then asked: "Could you guess how this person is feeling? Could you guess why this person is feeling that way?". Participants could list as many reasons as they wanted.
- The outcome variable in Cols. 1-2 is a binary indicator that equals 1 if workers come up with reasons for negative affect other than financial worries. Similarly, the outcome variable in Cols. 3-4 is a binary indicator that equals 1 if workers come up with reasons that are more generally distinct from income or being poor, such as the possibility that the person may be feeling ill.
- All regressions control for the same covariates as in Figure III: level of self-reported financial worry (collected in a subset of rounds), having a high-interest (i.e., moneylender) loan, number of loans the worker is worried about, and number of days of paid employment in the past month; variables with missing values are coded as zero and a dummy indicating the variable is missing is included in the regressions. Regressions also include round-wave (strata) fixed effects. Robust standard errors are reported.

Table A.4: Correlation between Financial Worries and Happiness

	Happiness scale	Very happy or happy	Very happy
	(1)	(2)	(3)
Worries scale	-0.038		
	(0.076)		
Very worried or worried		-0.042	
		(0.091)	
Very worried			-0.040
			(0.074)
Dependent variable mean	1.99	0.81	0.23
N: workers	159	159	159

Notes: This table shows the correlation between baseline level of financial worries and level of happiness.

- Financial worries answers were collected from the baseline survey, but happiness answers were collected from the exit survey (at endline). Consequently, we restrict this analysis to control group workers only. Happiness question asked: "How would you rate your happiness on a scale of 1 to 4 today?" (from 1 - "very happy" to 4 - "not at all happy"). Financial worries question asked: "How worried are you about your future finances?" (from 1 - "very worried" to 4 - "not worried").
- The outcome variable in Col. 1 is the continuous happiness scale from 1-4; in Col. 2 is an indicator for reporting "very happy" or "happy"; and in Col. 3 is an indicator for reporting "very happy". "Worries scale" is the continuous worries scale from 1-4; and the indicators for worries are defined analogously to the happiness indicators.
- The outcome means for the control group are reported in the table footer. All regressions control for round-wave (strata) fixed effects. Robust standard errors are reported.

Table A.5: Effects on Worker Productivity: Additional Outcomes

	Attendance	Number of hours worked in a day	Share of rejections	Total hourly production
	(1)	(2)	(3)	(4)
Cash × Post-pay	-0.003	-0.007	0.003	0.111**
	(0.014)	(0.007)	(0.002)	(0.047)
Control group mean	0.983	5.265	0.013	1.582
Include rejections				Y
N: worker-days	2,967	2,917		
N: worker-hours			17,033	17,441

Notes: This table tests for the impact of the interim-pay treatment on worker attendance and productivity using alternate sample restrictions and productivity measures.

- In Col. 1, the dependent variable is attendance, a binary indicator for whether the worker was present at the worksite on a given day. In Col. 2, the dependent variable is the number of hours worked in a day, calculated as the difference between work start time and end time, conditional on attendance.
- In Col. 3, the dependent variable is the share of rejections, which corresponds to the number of plates that did not meet quality standards (see Appendix Figure A.2) out of all the plates produced in the hour.
- Col. 4 corresponds to Col. 3 in Panel A, Table III, but the dependent variable is normalized total number of plates produced per hour including rejections. Total hourly production is normalized by dividing by the control group's standard deviation in the post-pay period.
- Regressions control for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.6: Effects on Worker Productivity — Robustness: Worker-level Regressions

	Hourly Production			
	(1)	(2)	(3)	(4)
Cash × Post-pay	0.082*	0.091**	0.093**	0.082*
	(0.044)	(0.043)	(0.044)	(0.044)
	[0.066]	[0.035]	[0.033]	[0.065]
Cash × Announcement period	0.005	0.014	0.015	0.005
	(0.031)	(0.031)	(0.031)	(0.032)
	[0.874]	[0.649]	[0.623]	[0.864]
P-val: Cash × Post-pay = Cash × Announcement	0.022	0.023	0.023	0.022
Baseline output	Y	Y	Y	N
Education	N	Y	Y	N
Experience	N	Y	Y	N
Marital status	N	Y	Y	N
Baseline worries controls	N	N	Y	N
Post-double selection lasso controls	N	N	N	Y
N: workers	408	407	407	408
N: worker-periods	787	785	785	787

Notes: This table tests for the impact of the interim-pay treatment using specifications that average worker output over the announcement and post-pay periods.

- All regressions use two observations per worker: one observation for the post-pay period, and one observation for the announcement period. The dependent variable is the worker's mean hourly normalized output in the given period. Note that in one short round (round 13), the interim payment schedule was not announced in advance, and so there is no announcement period; in this case, there is only one observation per worker.
- "Cash" is a binary indicator for being in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment. "Announcement period" equals 1 in the period following the pay schedule announcement but prior to the interim payment.
- Col. 1 regression controls for a quadratic of the individual's mean hourly output in the baseline period (i.e., pre-announcement period). Col. 2 regression adds controls for years of education, days of experience before the interim cash payment day, and marital status. Col. 3 regression adds controls related to financial worries from the baseline survey. Col. 4 controls for the covariates chosen using the LASSO post-double-selection procedure, the same ones used in Col. 3 of Table III. All regressions include round-wave (strata) fixed effects. Standard errors are clustered by worker and shown in parentheses. p-values are reported in brackets.

Table A.7: Effects on Worker Productivity — Robustness: Alternate Specifications

	Hourly Production				
	(1)	(2)	(3)	(4)	(5)
Cash × Post-pay	0.109**	0.108**	0.093**	0.093**	0.092**
	(0.047)	(0.047)	(0.036)	(0.036)	(0.038)
Cash × Announcement period	0.014	0.015	-0.004	-0.004	-0.004
	(0.035)	(0.035)	(0.028)	(0.028)	(0.028)
Priming controls	N	Y	Y	Y	Y
Exclude absent workers	N	N	Y	Y	Y
Answered baseline questions	N	N	N	Y	Y
Exclude primed workers	N	N	N	N	Y
P-val: Cash × Post-pay = Cash × Announcement	0.008	0.009	0.002	0.002	0.006
N: worker-hours	17,441	17,441	17,149	17,089	16,003

Notes: This table tests for robustness of the interim-pay treatment effects to alternate specifications.

- The specification in Col. 1 of this table corresponds to the exact specification in Col. 3 of Table III. The remaining regressions show robustness to alternate specifications. Standard errors are clustered by worker.
- Col. 1 regression controls for round-wave fixed effects and the same covariate controls as in Col. 3 of Table III. Col. 2 regression is similar but also includes priming controls, which include a dummy for all slots occurring after any priming intervention on that day, and its interaction with an indicator for whether a worker received a priming intervention.
- The regression in Col. 3 excludes observations from the days when a worker was absent. Col. 4 restricts the sample to the workers who answered the Baseline survey. Col. 5 additionally excludes observations from the days when a worker was primed.

Table A.8: Multiple Hypothesis Testing p-Value Corrections

Model	Variable	Coef	SE	p-val	Bonferroni p-val	Westfall-Young p-val	FDR q-val
PANEL A: Worries (Figure III)							
Left bar	Cash	0.115	0.053	0.032	0.063	0.041	0.047
Right bar	Cash	0.137	0.068	0.044	0.063	0.041	0.047
PANEL B: Expenditure (Table II)							
Col. 1	Cash	270.774	53.790	0.000	0.000	0.001	0.001
Col. 2	Cash	0.398	0.045	0.000	0.000	0.000	0.001
Col. 3	Cash	149.947	39.005	0.000	0.001	0.004	0.001
Col. 4	Cash	68.610	24.423	0.005	0.026	0.048	0.006
Col. 5	Cash	34.582	16.879	0.041	0.123	0.145	0.018
Col. 6	Cash	13.635	5.072	0.007	0.030	0.053	0.007
Col. 7	Cash	13.176	12.286	0.284	0.568	0.486	0.077
Col. 8	Cash	-0.284	4.564	0.950	0.950	0.940	0.268
Col. 9	Cash	371.335	67.744	0.000	0.000	0.000	0.001
PANEL C: Production (Table III)							
Col. 1	Cash × Post-pay	0.097	0.047	0.039	0.117	0.123	0.034
Col. 2	Cash × Post-pay	0.108	0.047	0.020	0.107	0.064	0.025
Col. 3	Cash × Post-pay	0.109	0.047	0.020	0.107	0.063	0.025
Col. 4	Cash × Post-pay	0.111	0.047	0.018	0.107	0.058	0.025
Col. 5	Cash × Post-pay	0.220	0.079	0.005	0.038	0.022	0.023
Col. 5	Cash × Post-pay × Higher wealth	-0.284	0.144	0.050	0.117	0.123	0.034
Col. 6	Cash × Post-pay	0.204	0.069	0.003	0.027	0.016	0.023
Col. 6	Cash × Post-pay × Higher wealth	-0.190	0.093	0.043	0.117	0.123	0.034
PANEL D: Attention (Table IV)							
Col. 1	Cash × Post-pay	0.077	0.045	0.092	0.368	0.317	0.091
Col. 2	Cash × Post-pay	0.095	0.029	0.001	0.008	0.018	0.009
Col. 3	Cash × Post-pay	0.170	0.083	0.041	0.225	0.197	0.067
Col. 3	Cash × Post-pay × Higher wealth	-0.243	0.177	0.170	0.511	0.363	0.128
Col. 4	Cash × Post-pay	0.133	0.064	0.037	0.225	0.197	0.067
Col. 4	Cash × Post-pay × Higher wealth	-0.114	0.089	0.199	0.511	0.363	0.129
Col. 5	Cash × Post-pay	0.122	0.040	0.002	0.017	0.027	0.009
Col. 5	Cash × Post-pay × Higher wealth	-0.056	0.054	0.296	0.511	0.363	0.173

Notes: This table shows p-values adjusted using the False Discovery Rate correction of Anderson (2008) and the Family-Wise Error Rate correction of Jones, Molitor, and Reif (2019). Corrections are done within each family of hypotheses, represented as a distinct panel in the table. The table continues to the next page.

Table A.8: Multiple Hypothesis Testing p-Value Corrections – Continued

Model	Variable	Coef	SE	p-val	Bonferroni p-val	Westfall-Young p-val	FDR q-val
PANEL E: Piece Rate on Production (Table V)							
Col. 1	Piece Rate	0.020	0.010	0.042	0.153	0.093	0.059
Col. 2	Log(Piece Rate)	0.058	0.028	0.038	0.153	0.090	0.059
Col. 3	Piece Rate = Rs. 3	0.024	0.018	0.187	0.187	0.213	0.059
Col. 3	Piece Rate = Rs. 4	0.040	0.020	0.042	0.153	0.093	0.059
PANEL F: Piece Rate on Attention (Table V)							
Col. 4	Piece Rate	-0.013	0.010	0.210	0.841	0.364	0.461
Col. 5	Log(Piece Rate)	-0.035	0.029	0.237	0.841	0.394	0.461
Col. 6	Piece Rate = Rs. 3	-0.004	0.024	0.866	0.866	0.869	0.461
Col. 6	Piece Rate = Rs. 4	-0.025	0.020	0.210	0.841	0.364	0.461
PANEL G: Piece Rate on Attendance (Table V)							
Col. 7	Piece Rate	0.000	0.006	1.000	1.000	1.000	1.000
Col. 8	Log(Piece Rate)	0.002	0.017	0.895	1.000	0.893	1.000
Col. 9	Piece Rate = Rs. 3	0.014	0.008	0.099	0.396	0.182	0.655
Col. 9	Piece Rate = Rs. 4	-0.000	0.012	1.000	1.000	1.000	1.000
PANEL H: Fairness Concerns (Table VI)							
Col. 1	Cash × 1 day post announcement	-0.015	0.036	0.668	1.000	0.977	1.000
Col. 1	Cash × 2 day post announcement	0.032	0.036	0.372	1.000	0.822	1.000
Col. 2	Cash × 1 day post announcement	-0.034	0.039	0.383	1.000	0.822	1.000
Col. 2	Cash × 2 day post announcement	0.015	0.038	0.703	1.000	0.977	1.000
Col. 2	Cash × Post-pay	0.110	0.047	0.019	0.225	0.112	0.138
Col. 3	Cash × Announcement period	0.021	0.031	0.497	1.000	0.907	1.000
Col. 3	Cash × Payment day	0.078	0.059	0.185	1.000	0.591	1.000
Col. 3	Cash × Payment day × Wave B	0.007	0.091	0.943	1.000	1.000	1.000
Col. 4	Cash × Announcement period	0.000	0.034	0.993	1.000	1.000	1.000
Col. 4	Cash × Payment day	0.067	0.059	0.259	1.000	0.718	1.000
Col. 4	Cash × Payment day × Wave B	-0.006	0.092	0.952	1.000	1.000	1.000
Col. 4	Cash × Post-pay	0.109	0.047	0.020	0.225	0.119	0.138
PANEL I: Nutrition Channel Breakfast Measures (Table VII)							
Col. 1	Cash	-0.007	0.013	0.604	1.000	0.952	1.000
Col. 2	Cash	-0.002	0.025	0.932	1.000	0.952	1.000
Col. 3	Cash	-4.048	7.223	0.576	1.000	0.952	1.000
Col. 4	Cash	-0.024	0.042	0.570	1.000	0.952	1.000
Col. 5	Cash	0.059	0.044	0.174	0.872	0.567	1.000
PANEL J: Nutrition Channel on Production (Table VII)							
Col. 1	Cash × Post-pay	0.060	0.050	0.225	0.902	0.565	0.148
Col. 1	Cash × Post-pay × Hour of day	0.014	0.007	0.043	0.302	0.226	0.095
Col. 2	Cash × Post-pay	0.173	0.073	0.019	0.167	0.113	0.092
Col. 2	Cash × Post-pay × Hour of day	0.008	0.010	0.390	1.000	0.697	0.243
Col. 2	Cash × Post-pay × Higher wealth	-0.204	0.103	0.048	0.302	0.229	0.095
Col. 2	Cash × Post-pay × Hour of day × Higher wealth	0.005	0.013	0.708	1.000	0.758	0.395
Col. 3	Cash × Post-pay	0.104	0.047	0.028	0.226	0.159	0.093
Col. 3	Cash × Post-pay × Last 2 hours of day	0.013	0.020	0.500	1.000	0.758	0.286
Col. 4	Cash × Post-pay	0.083	0.045	0.067	0.334	0.241	0.103
Col. 4	Cash × Post-pay × Last 1 hour of day	0.104	0.026	0.000	0.001	0.001	0.001

Notes: This table is continued from the previous page.

Table A.9: Treatment Effects — Heterogeneity by House Quality

	Hourly Production (1)	Attentiveness Index (2)	High Attentiveness (3)
Cash × Post-pay	0.142***	0.128***	0.116***
	(0.047)	(0.047)	(0.030)
Cash × Post-pay × House quality	-0.209***	-0.260***	-0.114**
	(0.066)	(0.079)	(0.047)
Coef: cash effect + interaction	-0.067	-0.132	0.003
SE: cash effect + interaction	0.062	0.079	0.048
P-val: cash effect + interaction	0.280	0.097	0.955
N: worker-hours	17,381	12,982	12,982

Notes: This table tests for the heterogeneous impact of the interim-pay treatment on worker productivity and attentiveness by house quality.

- "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment. "House quality" is a binary measure of house quality (i.e., living in a non-mud house, constructed of durable material).
- Regressions control for the covariate controls chosen using the LASSO post-double-selection procedure. The controls in Cols. 1-2 correspond to those used in Col. 3 of Table III and the controls in Cols. 3-4 are the same as those in Table IV. All regressions include round-wave (strata) fixed effects. Standard errors are clustered by worker.

Table A.10: Treatment Effects — Heterogeneity by Wealth, Financial Constraints, and Demographics

	Wealth		**Financial Constraints**		**Demographics**				
	Durable house	Owns land	No food loans	Can access emergency cash	Literacy	Education years	Age	Number of children	Any children
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cash × Post-pay	0.130***	0.179***	0.102**	0.145***	0.103*	0.143**	0.161	0.121*	0.088
	(0.046)	(0.059)	(0.051)	(0.055)	(0.061)	(0.060)	(0.139)	(0.064)	(0.092)
Cash × Post-pay	-0.210***	-0.121*	0.015	-0.122*	-0.030	-0.007	-0.002	-0.005	0.025
× Covariate	(0.066)	(0.064)	(0.062)	(0.062)	(0.067)	(0.008)	(0.003)	(0.019)	(0.085)
Coef: cash effect + interaction	-0.079	0.058	0.116	0.023	0.072	0.136	0.159	0.115	0.113
SE: cash effect + interaction	0.061	0.054	0.061	0.054	0.047	0.055	0.136	0.053	0.047
P-val: cash effect + interaction	0.197	0.285	0.057	0.672	0.126	0.014	0.243	0.030	0.017
N: worker-hours	17,165	17,329	17,381	17,209	17,165	17,381	17,225	17,321	17,321

Notes: This table tests for the heterogeneous impact of the interim-pay treatment on worker productivity. Regressions show heterogeneous impacts by different measures of wealth, financial constraints, and demographic characteristics.

- The dependent variable is normalized hourly production. "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment.
- In each column, the covariate in the interaction term is listed at the top of the column. The covariates in the first four columns are the components of the wealth index. They are binary indicators for house quality, i.e., living in a non-mud house, constructed of durable material (Col. 1); owning farmland (Col. 2); not having resorted to obtaining food or daily goods on credit from grocers and neighbors (Col. 3); and being able to come up with Rs. 1,000 in an emergency (Col. 4). The dependent variable in Col. 5 (Literacy) is a binary indicator for being able to read a newspaper in Odiya, and that in Col. 9 (Any children) is a binary indicator for having any children.
- Regressions control for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.11: Treatment Effects — Heterogeneity by Baselines Worries

	Hourly production		**Attentiveness index**	
	(1)	(2)	(3)	(4)
Cash × Post-pay	0.146***	0.238***	0.073	0.114*
	(0.054)	(0.071)	(0.053)	(0.068)
Cash × Post-pay × Not worried	-0.142	-0.100	-0.160	-0.143
	(0.106)	(0.112)	(0.120)	(0.123)
Cash × Post-pay × Higher wealth		-0.184*		-0.083
		(0.098)		(0.091)
Coef: cash effect + worry interaction	0.004	0.138	-0.087	-0.029
SE: cash effect + worry interaction	0.093	0.127	0.110	0.132
P-val: cash effect + worry interaction	0.963	0.276	0.431	0.825
Coef: cash effect + wealth interaction		0.053		0.031
SE: cash effect + wealth interaction		0.075		0.072
P-val: cash effect + wealth interaction		0.475		0.664
N: worker-hours	17,381	17,381	12,982	12,982

Notes: This table tests for the heterogeneous impact of the interim-pay treatment on worker productivity and attentiveness by baseline worries.

- "Cash" is a binary indicator for being in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment. "Not worried" is a binary indicator for reporting "little worried" or "not worried" to the following question in the Baseline survey: "How worried are you about your future finances?" (from 1 - "very worried" to 4 - "not worried"). "Higher wealth" is an indicator that equals 1 if the worker has an above-median value of the wealth index.
- Regressions control for the covariates chosen using the LASSO post-double-selection procedure. The controls for Cols. 1-2 correspond to those used in Col. 3 of Table III and the controls for Cols. 3-4 are the same as those in Table IV. All regressions include round-wave (strata) fixed effects. Standard errors are clustered by worker.

Table A.12: Effects on Attentiveness PCA Score

	Attentiveness PCA index (1)	PCA high attentiveness (2)	Attentiveness PCA index (3)	Attentiveness PCA index (4)	PCA high attentiveness (5)
Cash × Post-pay	0.132*	0.094***	0.296**	0.232**	0.122***
	(0.079)	(0.029)	(0.145)	(0.111)	(0.040)
Cash × Post-pay × Higher wealth			-0.425	-0.203	-0.058
			(0.307)	(0.154)	(0.054)
Cash × Announcement period	-0.003	0.027	0.073	0.038	0.044
	(0.074)	(0.026)	(0.149)	(0.109)	(0.039)
Cash × Announcement × Higher wealth			-0.172	-0.067	-0.028
			(0.308)	(0.151)	(0.054)
P-val: Cash × Post-pay = Cash × Announcement	0.049	0.012	0.014	0.014	0.022
Wealth index			Continuous	Binary	Binary
Coef: Cash × Post-pay + Cash × Post-pay × Wealth			-0.129	0.029	0.064
SE: Cash × Post-pay + Cash × Post-pay × Wealth			0.202	0.110	0.039
P-val: Cash × Post-pay + Cash × Post-pay × Wealth			0.523	0.789	0.102
N: worker-hours	13,020	13,020	12,982	12,982	12,982

Notes: This table tests for the impact of the interim-pay treatment on attentiveness, using an alternative measure of attentiveness.

- As with the attentiveness index, the principal component analysis (PCA) score is generated using the same three proxies for attentiveness: the average number of leaves, stitches, and double holes per plate during the production hour slot. The three measures are normalized using the control group's production (mean and standard deviation) in the post-pay period. We then perform a PCA using the covariance matrix of these variables and obtain the PCA score. The scale is reversed (multiplied by -1) so that a higher value of the score corresponds to improved attentiveness. "High attentiveness score" indicates that the PCA score value is greater than the sample median.
- The regression specifications correspond exactly to those in Table IV. Standard errors are clustered by worker.

Table A.13: Correlation Between Worker Productivity, Attentiveness, and Cognition

PANEL A: Main rounds—Productivity and Attentiveness

	Attentiveness index (1)	High attentiveness (2)	Number of leaves (3)	Number of stitches (4)	Number of double holes (5)
Hourly production	0.390***	0.186***	-0.847***	-5.743***	-0.645***
	(0.063)	(0.034)	(0.167)	(1.530)	(0.144)
N: workers	380	380	369	288	369

PANEL B: Supplementary rounds—Productivity, Attentiveness, and Cognition

	Attentiveness index (1)	High attentiveness (2)	CORSI performance (3)	Attentiveness index (4)	High attentiveness (5)
Hourly production	0.390***	0.239***	1.308***		
	(0.071)	(0.042)	(0.289)		
CORSI performance				0.044***	0.027***
				(0.015)	(0.010)
N: workers	150	150	145	145	145

Notes: This table shows the cross-sectional relationships between worker productivity, attentiveness, and cognition.

- Panel A shows the cross-sectional relationship between baseline (i.e., pre-announcement) productivity and attentiveness using the data from the main experiment sample. Data are from the rounds with baseline periods, i.e., rounds 1-13. Worker-level averages are calculated using observations from the last day of the baseline period (i.e., before treatment status is announced). The attentiveness index is comprised of three proxies for attentiveness: the average number of leaves, stitches, and double holes (which signify that a stitch was removed to correct a mistake) per plate during the production hour slot. The three measures are normalized using the control group's production (mean and standard deviation) in the post-pay period. We then take a simple average to create the attentiveness index, with the scale reversed (multiplied by -1) so that a higher value on the index corresponds to improved attentiveness. "High attentiveness" indicates that the index value is greater than the sample median. All regressions control for round-wave (strata) fixed effects. Robust standard errors are reported.
- Panel B shows the relationship between average productivity, attentiveness, and cognitive function using the data from the supplementary piece-rate rounds. Worker-level averages are calculated using observations after the first (training) day. Corsi performance is the worker's score on an incentivized memory test (Corsi Span Test, see a detailed description in Dean, Schilbach and Schofield, 2018). The average score was 9 out of 15 with a standard deviation of 2.4. All regressions control for round fixed effects. Robust standard errors are reported.

Table A.14: Effects of Priming

	Hourly Production					
	First hour after priming		Two hours after priming		All hours after priming	
	(1)	(2)	(3)	(4)	(5)	(6)
PANEL A: Overall priming impacts						
Post-priming	0.026	0.038	0.026	0.099	0.036	0.111
	(0.065)	(0.082)	(0.069)	(0.100)	(0.058)	(0.076)
Post-priming × Pre-pay	0.012	0.028	0.008	-0.059	0.000	-0.078
	(0.089)	(0.099)	(0.090)	(0.116)	(0.089)	(0.097)
Post-priming × Higher wealth		-0.025		-0.160		-0.173*
		(0.125)		(0.126)		(0.104)
Post-priming × Pre-pay × Higher wealth		-0.026		0.151		0.182
		(0.167)		(0.164)		(0.157)
N: worker-hours	17,441	17,381	17,441	17,381	17,441	17,381
PANEL B: Priming impacts before and after interim payment						
Post-priming (Day 10-11)	0.026	0.039	0.026	0.099	0.036	0.111
	(0.065)	(0.082)	(0.069)	(0.100)	(0.058)	(0.076)
Post-priming (Day 10-11) × Pre-pay	-0.046	0.028	-0.014	-0.019	-0.047	-0.041
	(0.088)	(0.115)	(0.088)	(0.124)	(0.083)	(0.112)
Post-priming (Day 6-7)	0.054	0.014	-0.009	-0.111*	0.053	-0.040
	(0.071)	(0.067)	(0.065)	(0.061)	(0.077)	(0.053)
Post-priming (Day 10-11) × Higher wealth		-0.026		-0.160		-0.173*
		(0.125)		(0.126)		(0.104)
Post-priming (Day 10-11) × Pre-pay × Higher wealth		-0.133		0.032		0.020
		(0.170)		(0.164)		(0.156)
Post-priming (Day 6-7) × Higher wealth		0.072		0.193		0.180
		(0.134)		(0.121)		(0.144)
N: worker-hours	17,441	17,381	17,441	17,381	17,441	17,381

Notes: This table shows the impact of the priming intervention on worker productivity.

- "Post-priming" is an indicator that equals 1 if the individual received the priming intervention earlier that day. Column sub-headings describe how many hours constitute the post-priming period. "Post-priming (Day 10-11)" refers to the post-priming periods that happened two days after the interim payment day, i.e., day 10 for Wave A and day 11 for Wave B. "Post-priming (Day 6-7)" similarly refers to the post-priming periods before the interim payment day.
- "Pre-pay" is an indicator that equals 1 if the worker has not (yet) received a cash infusion, i.e., on the days before the post-pay period for workers in the interim-pay treatment group, and on all days for those in the control group. "Higher wealth" is an indicator that equals 1 if the worker has an above-median value of the wealth index.
- All regressions include variables to account for the effects of the interim-pay treatment, i.e., an indicator for the post-pay period and its interaction with being in the interim-pay treatment group. Similarly, regressions include variables to account for the effects of the announcement. Regressions control for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.15: Effects of Cash across Priming Conditions

	Hourly Production	
	(1)	(2)
Cash × Post-pay	0.129**	0.268***
	(0.058)	(0.089)
Cash × Post-pay × Cash-poor priming	0.047	-0.150
	(0.105)	(0.105)
Cash × Post-pay × Cash-rich priming	-0.077	-0.077
	(0.064)	(0.094)
Cash × Announcement period	0.014	0.037
	(0.035)	(0.061)
Cash × Post-pay × Higher wealth		-0.280***
		(0.105)
Cash × Post-pay × Cash-poor priming × Higher wealth		0.401*
		(0.211)
Cash × Post-pay × Cash-rich priming × Higher wealth		-0.017
		(0.121)
Cash × Announcement × Higher wealth		-0.036
		(0.081)
Linear baseline output	Y	Y
Quadratic baseline output	Y	Y
Post-double selection lasso controls	Y	Y
Round-wave FE	Y	Y
Coef: (Cash × Post-pay) + (Cash × Post-pay × Cash-poor priming)	0.177	0.118
SE: (Cash × Post-pay) + (Cash × Post-pay × Cash-poor priming)	0.096	0.096
P-val: (Cash × Post-pay) + (Cash × Post-pay × Cash-poor priming)	0.067	0.218
Coef: (Cash × Post-pay) + (Cash × Post-pay × Cash-rich priming)	0.052	0.191
SE: (Cash × Post-pay) + (Cash × Post-pay × Cash-rich priming)	0.058	0.083
P-val: (Cash × Post-pay) + (Cash × Post-pay × Cash-rich priming)	0.372	0.021
N: worker-hours	17,441	17,381

Notes: This table tests for robustness of the interim pay treatment effects to priming conditions.

- The specifications in this table correspond to the specification in Col. 3 of Table III, but include additional covariates shown in the table. "Cash-poor priming" refers to those who received the priming before being paid, and "Cash-rich priming" refers to those who received the priming after being paid early. The omitted group is those who did not receive priming at all. "Higher wealth" is an indicator that equals 1 if the worker has an above-median value of the wealth index.

Table A.16: Trust: Effects in Later Rounds

	Hourly Production			
	Number of prior rounds (continuous)		Any prior round in worksite (binary)	
	(1)	(2)	(3)	(4)
Cash × Post-pay	0.081		0.089	
	(0.074)		(0.064)	
Cash × Post-pay × Prior rounds in worksite	0.011	0.014	0.026	0.003
	(0.024)	(0.024)	(0.077)	(0.074)
Interactions with number of total rounds in worksite	Y	N	Y	N
Interactions with worksite ID fixed effects	N	Y	N	Y
N: worker-hours	17,441	17,441	17,441	17,441

Notes: This table tests for the heterogeneous impact of the interim-pay treatment on worker productivity by whether prior rounds have been conducted in a given worksite (providing scope for the worksite to build a local reputation for reliability in the area).

- The dependent variable is normalized hourly production. In each column, the covariate in the interaction term is listed at the top of the column. "Number of prior rounds" is a continuous variable describing how many prior rounds have occurred in the worksite. "Any prior round in worksite" is an indicator that equals 1 if any prior round has been conducted in the worksite.
- "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group. "Post-pay" equals 1 on the days after interim payment.
- Cols. 1 and 3 include interactions of the total number of rounds conducted in a given worksite with Cash and Cash × Post-pay. Cols. 2 and 4 instead include interactions of worksite ID with Cash and Cash × Post-pay, so that effects are identified off within-worksite variation in how many rounds have been conducted over time. As a result, the Cash × Post-pay coefficient is not identified and therefore not reported.
- Regressions control for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.17: Effects on Reported Sleep Quantity and Quality

	Hours of sleep (1)	Sleep quality scale (2)	Had a good sleep (3)
Cash	-0.062	-0.056	-0.047
	(0.164)	(0.061)	(0.043)
Control group mean	6.90	2.76	0.82
N: workers	400	400	400

Notes: This table tests for the impact of the interim-pay treatment on self-reported sleep quantity and quality.

- Answers were collected from the exit survey on the last work day. Workers were asked: "How many hours did you sleep last night?" and "How well did you sleep last night?" (from 1 - "Did not have a good sleep" to 3 - "Had a good sleep").
- The outcome variable in Col. 1 is the number of hours of sleep; in Col. 2 is the sleep quality scale from 1-3; and in Col. 3 is a binary indicator for reporting "Had a good sleep." "Cash" is a binary indicator for whether the individual is in the interim-pay treatment group.
- All regressions control for the same covariates as in Figure III: level of self-reported financial worry (collected in a subset of rounds), having a high-interest (i.e., moneylender) loan, number of loans the worker is worried about, and number of days of paid employment in the past month; variables with missing values are coded as zero and a dummy indicating the variable is missing is included in the regressions. Regressions also include round-wave (strata) fixed effects. Robust standard errors are reported.

1.2 Appendix Figures

Figure A.1: Top-of-Mind Pictures

Panel A

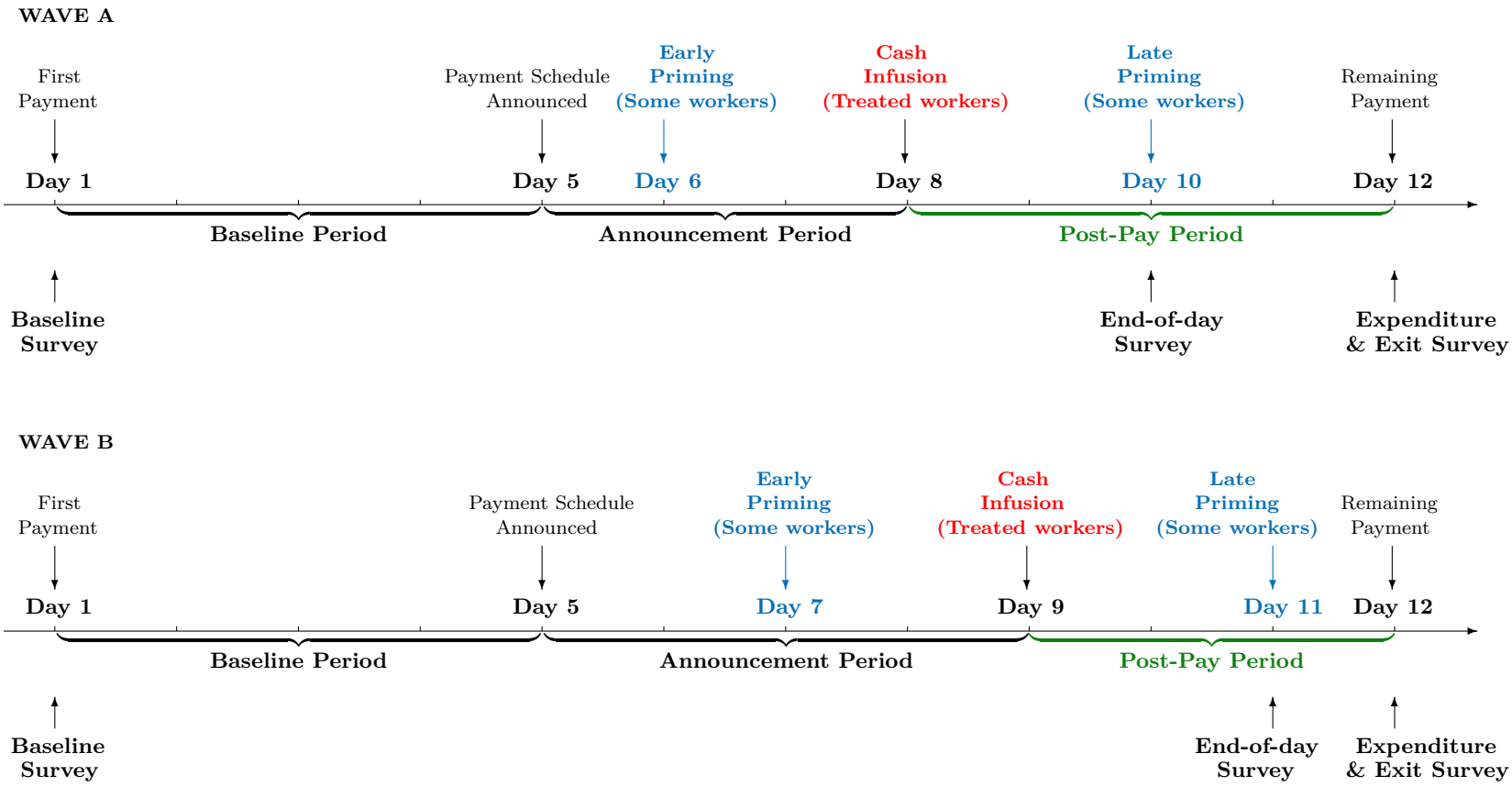
Panel B

Notes: This figure contains the photos that accompanied the top-of-mind questions in the exit survey. Workers were first shown the picture in Panel A and asked, "Could you guess how this person is feeling?" They were then asked the open-ended question, "Could you guess why this person is feeling that way?" and could say as many things as they wanted; surveyors then coded these according to some predetermined categories in recording responses. Workers were then shown the picture in Panel B and asked the same questions regarding this photo.

Figure A.2: Leaf Plate

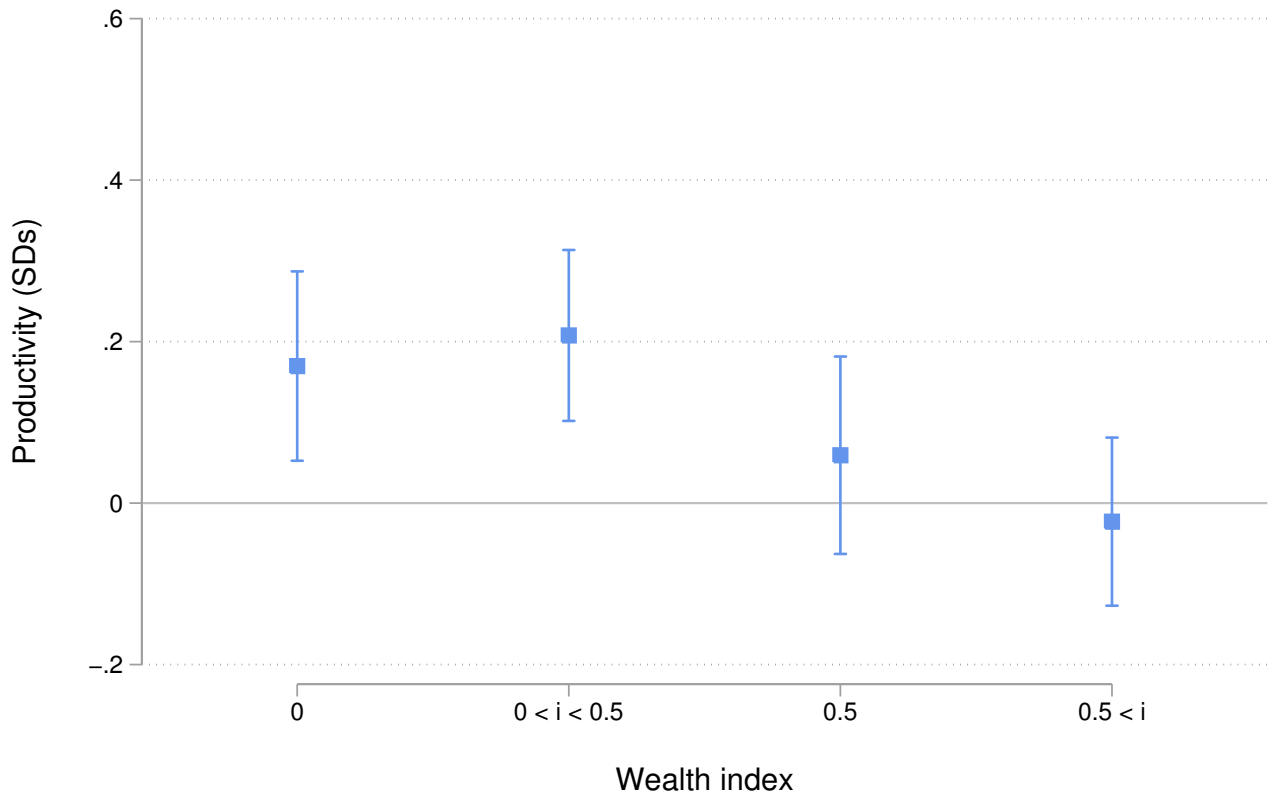
Notes: This figure shows a sal tree leaf plate akin to the ones produced by workers in the experiment. In accordance with quality standards set by partnering contractors, leaf plates were required to (i) meet a minimum size requirement, (ii) have no gaping holes, (iii) have all leafstalks (petioles) covered by other leaves, and (iv) have the leaves that form the outer ring (perimeter) of the plate be placed on top of the other leaves that compose the inner section of the plate. This ensures that all the side edges of the leaves forming the outer ring are clearly visible.

Figure A.3: Experimental Design – Timeline including Priming and Surveys



Notes: This figure is a more detailed version of Figure A.5. This figure additionally shows the timing of the priming interventions and surveys, while combining the interim-pay treatment and control groups within each wave. Workers were randomized into Wave A and Wave B. Wave B is identical to Wave A, except that the priming intervention, interim payment, and end-of-day survey happen one day later in this wave. The activities conducted with all workers are shown in black, the interim payment interventions for treated workers are shown in red, and the priming interventions with randomly selected subsets of workers are shown in blue. All workers answer the baseline survey on day 1, and the expenditure and exit surveys on day 12. In Wave A, the interim-pay treatment group receives the interim payment on the evening of day 8. All Wave A workers are randomized to be primed on day 6, day 10, or not at all, and they answer end-of-day survey on the evening of day 10. In Wave B, the interim-pay treatment group receives the interim payment on the evening of day 9. All Wave B workers are randomized to be primed on day 7, day 11, or not at all, and they answer the end-of-day survey on the evening of day 11.

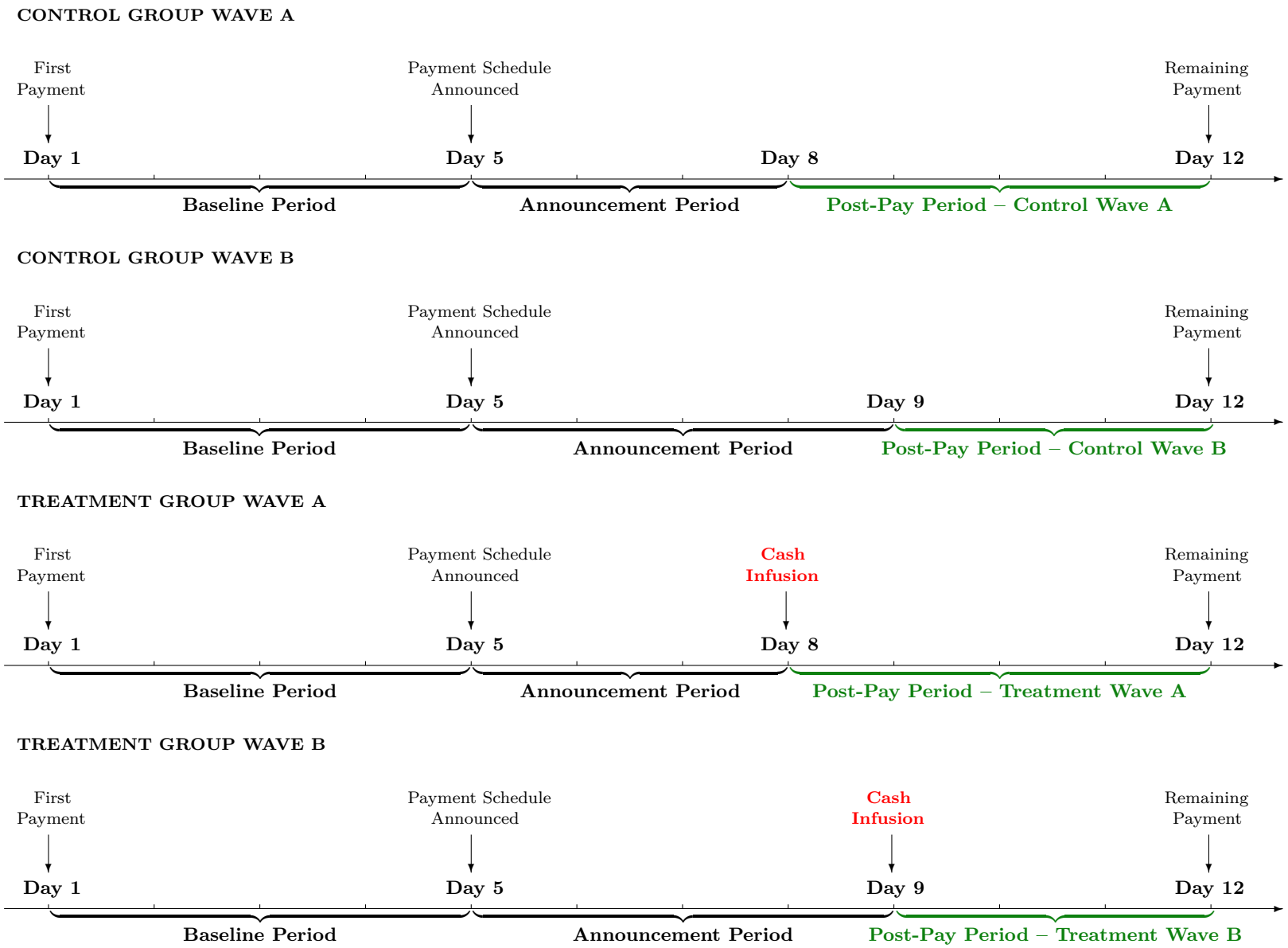
Figure A.4: Treatment Effects on Worker Productivity by Wealth Level



Notes: This figure plots the estimated effects of the interim payment on output separately for different values of the wealth index.

- The x-axis indexes the quartiles of the wealth index. Lower values of the index indicate lower wealth.
- The wealth index is an average of four binary measures: house quality (i.e., living in a non-mud house, constructed of durable material); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 easily in case of an emergency. When one of the measures is missing due to non-response (1.5% of the sample), the index averages the remaining three measures.
- Estimates are from a single regression that interacts a dummy for being in the interim-pay treatment group with each of the quartiles of the wealth index variable. The regression controls for round-wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III.
- Standard errors are clustered by worker. 90% confidence intervals are shown.

Figure A.5: Experimental Design – Detailed Timeline



Notes: This figure is a more detailed version of Figure II. The interim-pay treatment and control groups are each randomized into Wave A and Wave B. Wave B is identical to Wave A, except that the priming and interim payment interventions happen with a one-day lag for these workers. In Wave A, the interim-pay treatment group receives the interim payment on the evening of day 8, and all workers in Wave A are randomized to be primed on day 6, day 10, or not at all. In Wave B, the interim-pay treatment group receives the interim payment on the evening of day 9, and all workers in Wave B are randomized to be primed on day 7, day 11, or not at all.

1.3 Priming

Design. Our primary test uses variation in real income to examine the impact of financial strain on productivity, and we use attention as an outcome variable to examine channels. The priming approach instead uses attention as a "treatment," by directing attention to financial constraints. Psychologists have recently raised concerns about the reliability and replicability of priming (e.g., Kahneman, 2012; Chivers, 2019; Sherman and Rivers, 2021). However, for completeness, we follow prior work (e.g., Mani et al., 2013; Bartoš et al., 2021) and implement a priming intervention intended to direct workers' attention to their finances. During this intervention, surveyors tell workers a story about a fictional worker's financial strain and then conduct a survey asking them to list all their loans, employment opportunities, and discuss their finances. This 30-minute discussion takes place in the morning as part of a financial planning exercise. Before returning to work, we ask workers how they would raise the money to cover an unexpected, large expense. Workers are asked to think about this question so that their answers can be discussed at the end of the day with the same surveyor. The "priming" manipulation itself resembles a detailed finances survey—a common activity in household surveys. Priming interventions are viewed as not creating new thoughts, but rather giving cues to bring already existing associations top of mind. Because of the short-livedness of priming interventions—sometimes on the order of minutes (e.g., Molden, 2014; Wentura and Rothermund, 2014)—we examine effects in varying time windows immediately post-priming.

We test the hypothesis that priming causes two competing effects: while bringing financial concerns top of mind could reduce output through a cognition effect, reminding workers about their financial needs could motivate them to work harder or focus, increasing output.[1] We thus cross-randomize the priming intervention with the interim-pay treatment. Some workers are randomized to receive the priming treatment two days before the interim payment day, others two days after the interim payment day, and others not at all (see Appendix Figure A.3). We use this variation to test whether priming more negatively affects productivity among cash-poor workers (those who received the priming before being paid) compared to its impact on cash-rich workers (those who received the priming after being paid early).

Results. Priming interventions usually have their strongest effects immediately after the prime is delivered (e.g., Shanks et al., 2013). However, we find limited evidence for any effects in the one or two hours immediately after workers are primed (Appendix Table A.14 Cols. 1-4), both across the sample as a whole or among the poorer workers. Examining effects over the entire day after priming, we see some suggestive evidence of effects on productivity (Appendix Table A.14 Cols. 5-6). Consistent with our prediction, priming has a more negative impact when workers are cash poor (before receiving a cash infusion) relative to when they are cash rich (after the interim payment), but this difference is not statistically significant. For example, among workers with below-median wealth, output is 0.078 SD lower when priming is delivered when they are cash-poor versus cash-rich

[1] The prior literature has only examined the negative cognition effect, because the outcomes in prior work were laboratory measures of cognition, providing no scope to examine a positive motivational effect wherein working harder and earning more would help one solve the financial concerns that are now top of mind.

(Col. 6, Panel A, p=0.418).[2]

We see some suggestive evidence for a potential motivational effect of priming. Workers who receive priming after the interim payment raise output by 0.036 SD on average (Col. 5, Panel A, p=0.542) and by 0.111 SD among poorer workers (Col. 6, Panel A, p=0.148). This is consistent with the idea that focusing workers' attention on their finances could increase motivation,since effort at work can directly help overcome the problems being primed, resembling reminder effects (Karlan et al., 2016). Prior work has only focused on the potential negative effects of priming, in part because the measured outcomes (laboratory measures of cognition) are thought not to be too sensitive to motivation. In contrast, with real-world work productivity, motivation could play a large role so that the overall effect of priming is ambiguous. Finally, we do not observe any detectable effects of priming on the day after it occurs.

Overall, these priming effects are only suggestive. The ambiguity of our findings is consistent with the broader debate around how to understand the "first stage" of priming treatments—both treatment intensity, which can be non-monotonic in underlying worries, and what specific set of thoughts or pathways are triggered (e.g., Shah, Mullainathan and Shafir, 2012; Cesario, 2014; Banker, Bhanot and Deshpande, 2020). Using attention as an outcome variable, as we do in this paper, may constitute a useful design strategy for sidestepping some of these concerns.

[2]These patterns are similar if we test for the effects of priming in the second half of the work contract (i.e., days 10-11), comparing those who had received the interim payment versus those who had not (shown in the first two rows of Appendix Table A.14 Panel B.

2 A Simple Framework

Consider a worker who lives for infinitely many periods $t = 1, 2, \ldots$. In each period, the worker chooses how much to consume c_t (and thus how much to save s_t). In periods during the experiment, the worker also chooses how much "effortful" input e_t to provide at work. This includes physical components such as the speed of moving one's hands that might be traditionally called effort, as well as psychological components such as the decision of how much attention to pay.[3] The first two periods are the two adjacent experimental periods: (i) the post-announcement period ($t = 1$) and (ii) the post-pay period ($t = 2$).

Workers maximize their total discounted consumption utility $u(c)$ net effort costs $g(e)$ across all periods. We assume that the consumption utility and effort costs are separable. Consumption utility is increasing and concave ($u'(c) \geq 0, u''(c) \leq 0$) and additively separable across periods. Effort costs are increasing and convex in effort ($g'(e) \geq 0, g''(e) \geq 0$).

Output $f(e, a)$ is increasing in both effortful input e and automatic input a (i.e., $\frac{\partial f}{\partial e} \geq 0, \frac{\partial f}{\partial a} \geq 0$), which reflects the fact that productivity increases in response to both higher effortful input (e.g., working faster or trying harder to pay attention) and higher automatic input (e.g., the capacity to pay more attention). We also assume that output is concave in effortful input ($\frac{\partial^2 f}{\partial e^2} \leq 0$). For simplicity, we assume effort and attentiveness are complements in production ($\frac{\partial^2 f}{\partial a \partial e} > 0$).

Each period, workers consume out of their total earnings, which consist of output in the experimental study $y = f(e, a)$ and constant outside per-period income w. Workers discount across periods by factor $\delta \leq 1$. If workers save some of their earnings in period τ, they receive $(1 + r)$ in period $\tau + 1$ for each unit of earnings they saved. Similarly, if workers borrow in period τ, interest accrues so that the total amount owed is $(1 + r)^j$ in period $\tau + j$ for each unit they borrowed.

We index workers by their treatment group $g \in \{T, C\}$. Workers in the control group are paid at the end of period 2 (for their total output in the two periods). Workers in the treatment group receive an interim payment at the end of period 1 (for their output in period 1) and are paid again at the end of period 2 (for their output in period 2). We can generate predictions for the direction and relative size of the treatment effect in each period, defined to be the difference in output between a treatment and control group worker:

$$TE_t := y_{t,T} - y_{t,C} \qquad t \in \{1, 2\} \tag{1}$$

[3] Effortful input may capture the worker's decisions to work faster, shorten their break time between plates, quicken their actions for making plates, or try harder to pay attention. Note that we hold labor supply constant by the design of the experiment, as measured by the number of hours or days worked.

2.1 Baseline model: No effect of financial strain on attentiveness

First, we consider a baseline version of the model in which the level of automatic input that enters the production function is held fixed at some level $a = \bar{a}$. Thus, production only depends on effortful inputs e. To simplify notation, we suppress the attentiveness argument of output and write $f(e) := f(e, \bar{a})$. We relax this assumption in Section 2.2 below.

In each period, workers choose consumption and effort to maximize their lifetime utility:

$$\max_{e_1, e_2, \{c_t\}_{t=1}^{\infty}} \quad \sum_{t=1}^{\infty} \delta^{t-1} \Big[u(c_t) - g(e_t) \Big] \tag{2}$$

Budget constraints. Due to the different payment timing, income for the workers in the control and treatment groups differ in periods $t = 2$ (post-pay) and $t = 3$ (post-experiment). Note that the timing of the periods is such that workers only receive payments for their output in the period(s) *after* the period in which they worked, i.e., payment for period t is only available for consumption in period $t + 1$ at the earliest.

- Both groups of workers face the same budget constraint in period 1, in which workers come in with their per-period income and any pre-existing amount of assets s_0 (which could be savings if $s_0 \geq 0$ or debt if $s_0 < 0$); in this particular setting, we think of this variable as debt for most individuals in the sample).

$$c_{1,g} + s_{1,g} = w + s_0 \quad \forall g \in \{C, T\} \tag{3}$$

- In each subsequent period, workers choose consumption c_t and savings s_t, which must sum to their available resources in that period. Each period, workers' available total income consists of fixed, per-period outside payment w, the prior periods' savings s_{t-1} with interest accrued, as well as payment for output from prior periods y_{t-j} depending on the payment schedule for that worker's experimental group.

- Since the control group receives all payments from the experiment at the end of the study, their per-period budget constraints in periods 2 and 3 are:

$$c_{2,C} + s_{2,C} = w + (1 + r)s_{1,C} \tag{4}$$

$$c_{3,C} + s_{3,C} = w + (1 + r)s_{2,C} + y_{1,C} + y_{2,C} \tag{5}$$

- In contrast, the treatment group is paid at the end of both periods 1 and 2, so the

per-period budget constraints in periods 2 and 3 are:

$$c_{2,T} + s_{2,T} = w + (1+r)s_{1,T} + y_{1,T} \tag{6}$$

$$c_{3,T} + s_{3,T} = w + (1+r)s_{2,T} + y_{2,T} \tag{7}$$

- In all remaining time periods, workers in both groups face the same budget constraint:

$$c_{t,g} + s_{t,g} = w + (1+r)s_{t-1,g} \quad \forall g \in \{C,T\}, \forall t \geq 4 \tag{8}$$

First-order conditions. For both groups of workers, we can write down the first-order conditions that characterize the *intertemporal* optimal consumption/savings decisions, a standard Euler equation:

$$u'(c_{t,g}) = \delta(1+r)u'(c_{t+1,g}) \tag{9}$$

We can also write down the first order conditions to characterize the *intratemporal* optimal level of effort in each of the experiment periods for each group of workers:

$$[e_{1,T}] : g'(e_{1,T}) = \frac{u'(c_{1,T})}{(1+r)} f'(e_{1,T}) \tag{10}$$

$$[e_{1,C}] : g'(e_{1,C}) = \frac{u'(c_{1,C})}{(1+r)^2} f'(e_{1,C}) \tag{11}$$

$$[e_{2,T}] : g'(e_{2,T}) = \frac{u'(c_{2,T})}{(1+r)} f'(e_{2,T}) \tag{12}$$

$$[e_{2,C}] : g'(e_{2,C}) = \frac{u'(c_{2,C})}{(1+r)} f'(e_{2,C}) \tag{13}$$

The conditions for the treatment and control group are nearly identical, with two exceptions. First, the control group receives their payments only at the end of period 2 (rather than at the end of period 1), leading to a difference of $\frac{1}{1+r}$ between Equations (10) and (11). Second, the level of consumption in a given time period may differ between treatment and control group workers due to the differing lifetime budget constraints described above.

Predictions. We can use these optimality conditions and the budget constraints to make two key predictions of this baseline model for the treatment effects in period t (TE_t):

(1) **Prediction 1: ($\text{TE}_2 < 0$).** *The treatment group will have lower output than the control group in period 2 (post-pay period).* The first prediction of the baseline model is that the treatment effect in period 2 is negative. Workers in the treatment group produce less than workers in the control group. This is because the marginal utility of

consumption for the treatment group is lower due to their higher lifetime earnings due to interest accrued (or averted) based on being paid earlier, putting them on a higher level consumption path. Given the lower marginal utility of consumption, workers in the treatment group will exert less effort and thus produce less output in period 2.[4] We expect this effect to be quantitatively small based on the results of the piece-rate experiment, which increased returns to effort for workers roughly by a factor of two but only induced a 1% change in effort. Within the lens of this model, this 1% change should be an upper bound on the effect of effort on output, unless utility is so concave (or consumption moves so drastically) that the marginal utility of the treatment group is twice that of the control group as a result of the earlier payment.

(2) **Prediction 2 ($\mathbf{TE}_1 > \mathbf{TE}_2$):** *The difference in output between the treatment and control groups will be more positive in period 1 (post-announcement) than in period 2 (post-pay period).* The second prediction of the baseline model is that the treatment effect in period 1 is more positive than the treatment effect in period 2. In both periods, there is a negative effect of treatment on output due to higher lifetime earnings for the treatment group, described in prediction 1. However, in period 1, there is an additional offsetting positive effect: workers in the treatment group exert relatively more effort because the marginal benefit of consumption is diminished by a factor of $\frac{1}{1+r}$ for the control group due to delayed payment. Taken together, this implies that the predicted sign of the treatment effect in period 1 is ambiguous, but the treatment effect in period 1 is more positive than the treatment effect in period 2.[5] Again, given the low impact of piece-rate variation on output, we expect this effect to be quantitatively small.

The empirical results from our experiment are clearly at odds with these two key predictions, thus rejecting the baseline model based solely on effortful input, which encompasses all dimensions of input to the production function under conscious control of the worker.

[4] To see this, we first note that the Euler equation (equation 9) is identical for both groups, which implies that consumption *growth* rates are also identical. Next, we pin down the *level* of each group's consumption path and show that the treatment group has higher total discounted lifetime earnings by calculating the lifetime budget constraints for each group using the expressions for savings in each period outlined in equations (3) through (8). Since the treatment group has higher lifetime earnings but identical initial assets and consumption growth as the control group, we know that consumption in each period will be higher for treatment group workers. Hence $c_{2,T} < c_{2,C}$ which implies the expression on right-hand side of equation 12 is smaller than that of equation 13 due to the concavity of $u(c)$. As a result, the optimal level of effort chosen will be lower for the treatment group than the control group ($e_{2,T} < e_{2<C}$) which implies lower output in the treatment group and a negative treatment effect.

[5] The mechanics of the negative effect of treatment on output is described in prediction 1. To understand the mechanics of the offsetting positive effect, we can compare (10) and (11): the marginal utility of the control group is discounted by an additional factor of $\frac{1}{1+r}$ due to forgone interest accrued on savings (or debt) relative to the treatment group. This corresponds to a positive treatment effect absent other differences. Since there are two competing effects with opposite signs in the post-announcement period, the theoretical prediction for the sign of TE_2 is ambiguous.

2.2 Augmented model with financial strain attentiveness channel

Suppose now that output is a function of not only effortful input that is consciously chosen by the worker, but also some involuntary productive input that we refer to as automatic input a. For example, we could think of automatic input as a multiplier that magnifies each unit of effort chosen by the worker, generating higher levels of output per unit of effort e at higher levels of automatic input a. It can be thought of as a measure reflecting the worker's capacity to translate their effort into performing their work with more care and/or fewer mistakes.

Let automatic input a_t be a function of the extent to which the worker is financially constrained. To capture the dependence of a on financial strain, we model a as a function of two measures of financial constraint. First, a is decreasing with the marginal utility of consumption $u'(c)$, which reflects the idea that people facing more acute consumption constraints have higher financial strain, which in turn decreases their level of attentiveness. Second, a is decreasing with the level of debt D, which captures the idea that financial strain is not only a function of consumption flows but also sensitive to the level of outstanding debt. Debt each period evolves according to how much individuals save: $D_t = D_{t-1} - s_t$. We model automatic input in each period as a function of both these variables: $a_t = a(u'(c_t), D_t)$.[6] We assume that workers do not account for the benefits of higher future levels of automatic input when making current-period decisions to work. This may occur, for example, because they are not aware of such effects as in **?**.[7]

Predictions. The optimization problem for choosing effort in each period is unchanged from before (as captured by (10) through (13)). We consider the effect of automatic input by looking at the partial derivative of output with respect to a (which we assumed was positive):

$$\frac{\partial f}{\partial a}(e, a) > 0 \tag{14}$$

Holding effort fixed (i.e., only considering the independent partial effect of automatic input), the model then makes two predictions for the TEs in the two experiment periods:[8]

[6] The timing of actions within each period is such that agents first make consumption-savings decisions and then make effort-output decisions. Both factors impacting automatic input $u'(c_t)$ and D_t are determined before the worker exerts effort to produce output in period t.

[7] Accounting for this channel of future benefit would increase the perceived returns to effort in the earlier periods, which predicts a larger treatment effect on output in the post-announcement period ($t = 1$). If workers internalized the productivity benefits of higher levels of automatic input, they would pay off debt and/or consume in anticipation of the later benefits of higher output. However, the empirical results suggest this is unlikely to be the case: empirically there is no difference in output between workers in treatment and control in the post-announcement period, suggesting workers do not anticipate the future benefits of higher automatic input from working more in the current period.

[8] In the previous section, we analyzed the effects of treatment on output via effort, holding the level of automatic input fixed. In this section, we analyze the first-order effects of automatic input on output by considering the partial derivatives of output with respect to a – this analysis takes an "all else equal" interpretation and thus implicitly holds effort fixed. To consider the total *first-order* effect of treatment on output through both channels, we can sum the two partial effects of effort input and automatic input.

(3) **Prediction 3 ($\mathbf{TE}_2 > 0$).** *The treatment effect through the automatic attention channel will be positive in period 2 (post-pay period), holding constant the effort channel.* In period 2, the treatment workers make decisions after having received a large lump-sum payment for their output in the previous period. The consumption-savings decisions of workers in the treatment group impact their levels of automatic input through two channels – both of which impact automatic input in the same direction. First, the treatment workers' consumption levels are still slightly higher due to the same aforementioned income effect (see Prediction 4 for more detailed discussion), which decreases financial strain. Second, the treatment workers save the rest of their lump-sum payment, which should substantially decreases their debt level D relative to the control group. As a result, the period 2 treatment effect from changes in a via altered financial strain should be positive.

$$f(e_2, a(u'(c_{2,C}), D_{2,C})) = y_{2,C} < y_{2,T} = f(e_2, a(u'(c_{2,T})), D_{2,T})$$

(4) **Prediction 4 ($\mathbf{TE}_2 > \mathbf{TE}_1$).** *The treatment effect through the automatic attention channel will be smaller in period 1 (post-announcement) than in period 2 (post-pay), holding constant the effort channel.* In period 1, after the announcement about payment schedules, workers in the treatment group will slightly adjust their consumption levels $c_{1,T}$ in period 1 due to having slightly higher net present value lifetime income than the control group (details the same as in previous section). As a result, workers in the treatment group face two competing changes on levels of automatic input, relative to the control group. First, their consumption level slightly increases, which decreases financial strain through decreasing $u'(c)$. Second, their debt level slightly increases since they are increasing consumption without having any more cash in hand than the control group, which slightly increases their financial strain through increasing D. In net, the effect of the announcement on output is ambiguous because the two factors impacting automatic input move in opposite directions. In practice, we expect output for treatment and control groups to be approximately equal because they have the same amount of cash-on-hand and these effects are likely to be small given the small lifetime income effect of the earlier payment. As a result, workers likely choose similar levels of consumption and debt in period 1 and thus similar levels of automatic input.

$$f(e_1, a(u'(c_{1,C}), D_{1,C})) = y_{1,C} \approx y_{1,T} = f(e_1, a(u'(c_{1,T}), D_{1,T}))$$

Taken together with the effects discussed in Prediction 3, this yields the prediction that the treatment effect in period 2 is larger than the treatment effect in period 1. In

summary, the sign on the difference in output between treatment and control workers in period 1 is ambiguous due to competing effects of consumption and debt levels on the level of automatic input – but the magnitude of the difference is likely to be small. However, the treatment group should unambiguously produce more output than the control group in period 2 because both channels impacting automatic input work in the same direction. As a result, the treatment effect in the post-pay period should not only be positive but also larger than the effect in the post-announcement period.

2.3 Empirical tests of the full model with both input channels

We can predict the first-order effects of treatment on output through both channels of automatic and effortful input by considering effects of both channels and summing them together. When considering each of the two partial effects separately, we end up with diverging predictions for the treatment effects: both the predicted sign of the treatment effect in the post-pay period as well as the relative sizes of the treatment effects in the post-pay and post-announcement periods are opposites. In particular, the effort-only channel would suggest a negative treatment effect in the post-pay period (prediction 1) while the automatic-only channel would suggest a positive additional effect (prediction 3). Furthermore, the effort-only channel would suggest a larger (i.e., more positive) effect in the post-announcement than post-pay period (prediction 2) while the automatic-only channel would suggest an additional larger (more positive) effect in the post-pay than post-announcement period (prediction 4).

Taken together, the augmented model suggests that the signs of the resulting net treatment effects depend on the relative magnitudes of the two partial effects. But notice that the only way for there to be a positive productivity effect in post-pay period is in the model augmented with financial strain and automatic mental inputs. Similarly, the only way for there to be a more positive effect in post-pay period than in the post-announcement period is in the augmented model. Thus, the empirical results support the hypothesis that the automatic input channel is important: the treatment effect in the post-pay period is large, significant, and positive ($TE_2 > 0$) and larger than the insignificant effect in the post-announcement period ($TE_2 > TE_1 \approx 0$), suggesting the automatic-input channels dominates the effortful input channel and is economically relevant in our setting.

Heterogeneity by baseline wealth. What does this model predict about heterogeneous treatment effects with respect to financial strain? For exposition, we focus only on heterogeneity in financial strain as captured by consumption levels. Similar derivations would follow for strain captured by debt levels. Assuming that automatic inputs $a(u'(c))$ are convex in marginal utility, the model predicts that the effect of the early payment on output will be

largest for the poorest workers.[9] To see this, consider the treatment effect in period 2:

$$TE_2 = f(e_2, a(u'(c_{1,T}))) - f(e_2, a(u'(c_{1,C}))) \geq 0$$

When a is concave in consumption, the effects of treatment on both a and output will be higher for workers with lower baseline consumption levels. In other words, the output of the poorest workers will be the most responsive to treatment:

$$\begin{aligned}\frac{\partial TE_2}{\partial w} &= \frac{\partial f}{\partial a} \cdot \frac{\partial a(x)}{\partial x}|_{x=u'(c_{1,T})} - \frac{\partial f}{\partial a} \cdot \frac{\partial a(x)}{\partial x}|_{x=u'(c_{1,C})} \\ &= \frac{\partial f}{\partial a}\left[\frac{\partial a(x)}{\partial x}|_{x=u'(c_{1,T})} - \frac{\partial a(x)}{\partial x}|_{x=u'(c_{1,C})}\right]\end{aligned}$$

[9]Note that a being convex in marginal utility is equivalent to a being concave in consumption levels. This shape arises when a marginal increase in consumption improves attentiveness *more* at lower levels of consumption. This is likely a reasonable assumption in this context: if lower a—in our context, attentiveness—is caused by financial strain, increasing consumption for workers with the highest baseline consumption levels may affect their a (attentiveness) less, since they are less constrained to begin with.

3 Protocols Appendix

This appendix provides additional detail on the study protocols.

Standard round timing. The standard schedule refers to the 12-day, 5-hour work schedule with a base rate of Rs. 200 and a piece rate of Rs. 3 per plate, implemented for rounds 4 to 12 of the study. In those rounds, the payment schedule was announced at the beginning of day 5. Within each round, the treatment and control groups were each divided into two Wave A and Wave B:

- For Wave A treatment workers, the interim payment happened at the end of the day 8. For those assigned to receive either early or late priming in Wave A, priming sessions were conducted on day 6 or 10.
- For Wave B treatment workers, the interim payment occurred on day 9. For Wave B treatment and control workers who were assigned to receive priming, priming sessions were randomized to occur a day later than Wave A, on day 7 or 11.
- For the interim-pay treatment, workers received wages earned up to one day before the payday, i.e., payment lag was one day.
- Attentiveness measures were collected on days 4 and 6-11.

Any deviations from this standard schedule is described below and are summarized in Panel A of Appendix Table A.18.

Deviations. There were several deviations from the standard schedule:

- Rounds 1-3, which were conducted in March-June of 2017, had several deviations from the standard schedule and wage rates, which were later finalized and then implemented during March-June of 2018. During these rounds, each workday contained 7 hours of work and a lunch break, rather than 5 continuous hours of work without lunch. Both types of workday schedules are common in the local region. Some workers expressed their preferences for shorter work days due to hot weather, so the daily schedules were updated in 2018. Workers with the 5-hour schedules still received a snack at the end of each day. Attentiveness measures were collected on days 4, 6, and 7-10 for Wave A, and 4, 6, and 8-11 for Wave B.
- In rounds 1-3, workers who were randomly assigned to not receive priming interventions instead participated in control interventions. They listened to a story about a famous

lake or a sports player and discussed their pastime activities. When the workday was shortened to 5 hours, we discontinued this due to operational and time constraints.

- The later rounds (rounds 12-14) were shortened to avoid running the experiment into the transplanting season. Round 12 follows the standard schedule but is shortened by one day. Its schedule is equivalent to skipping day 5 and having the announcement of the payment schedule on day 6.

- Rounds 13-14 were shorted to 6 days. The payment schedule was not separately announced during round 13, but was announced on day 2 in round 14. To make the size of the interim payments comparable to the other rounds, the interim-pay treatment group's initial payment included a bonus of Rs. 200 in addition to all wages earned up to the payment day (i.e., including the first day's wage). The control group received this bonus on the last day, along with other payments. Workers also received an attendance bonus of Rs. 200 if they missed none of the last five workdays. Attentiveness measures were collected on all days after day 1.

- While most rounds had consecutive work days, some rounds had one-day breaks in the first half of the rounds due to local events and religious festivals. Specifically, there were one-day breaks after day 5 in round 2, after day 2 of round 3, and after day 3 of round 12.

Randomization weights. In rounds 1 to 3, the interim-pay treatment group were overweighted in the randomization to comprise nearly 70% of the sample. Starting with round 4, the sizes of the control group and the interim-pay treatment group were approximately equal. Conditional on interim-pay treatment status, the sizes of groups that receive a priming intervention on day 6 vs. day 10 vs. not at all, was randomized to be 2:2:1.

Attentiveness measures. The attentiveness index measure was not included in our pre-registry due to an oversight. However, we did intend to collect these measures ex-ante: for a subset of days in each round, we collected attentiveness measures for every single plate that was produced. This involved significant operational cost and burden, but was collected due to our intention to use these measures as a proxy for attentiveness. Moreover, the components of the attentiveness index are the only three measures we collected in this guise. The number of double holes and leaves was collected in all rounds, and the number of stitches was collected from round 4 onwards. In each round, these measures were collected on the day before announcement (i.e., workday 4) and then each day starting two days before interim payments began until the penultimate day of the contract period (i.e., workdays 6-11).

Supplementary piece-rate rounds. In the supplementary piece-rate rounds (conducted after the 14 main experimental rounds had been completed), there was no variation in the payment schedule: all workers were paid all their post-training earnings on the final day. During these rounds, we induced random variation in piece rates across days. As in the main experimental rounds, workers received a flat wage of Rs. 250 with no piece-rate component on the first day. In the remaining six days, workers were paid a piece rate of Rs. 2, 3, and 4. Each workers received each of the three piece rates for two consecutive days, with the order of piece rates randomized across workers. The base wage was adjusted so that average daily earnings would be approximately similar for all three piece rates. To do this, we calibrated the base wage based on workers' average productivity during the main rounds. The base wage rates for each round are described in Panel B of Appendix Table A.18.

Table A.18: Schedule and Wage Summary

PANEL A: Main Rounds Schedule and Wage

	Round 1	Round 2	Round 3	**Round 4-12**	Round 13-14
Total days	12	11	12	**12**∗	6
Work hours per day	7	7	7	**5**	5
Baseline survey	Day 1	Day 2	Day 2	**Day 1**	Day 1
Schedule announcement	Day 5	Day 5	Day 5	**Day 5**∗	Day 2†
First priming session	Day 7/8	Day 7/8	Day 8/9	**Day 6/7**	Day 3/4
Early-Pay Treatment	Day 8/9	Day 8/9	Day 9/10	**Day 8/9**	Day 3/4
Second priming session	Day 10/11	Day 10/11	Day 11/12	**Day 10/11**	Day 5/6
Endline survey	Day 11-12	Day 11	Day 12	**Day 12**	Day 6
First day flat wage	230	250	250	**250**	250
Base wage	200	180	175	**200**	200
Piece-rate wage	2	3	3	**3**	3
Attendance bonus	350	350	350	**300**	400‡
Payment lag	2 days	2 days	2 days	**1 day**	0 day

PANEL B: Supplementary Rounds Wage

	Round 15	Round 16	Round 17	Round 18	Round 19
Base wage when piece-rate = 2	230	240	230	240	220
Base wage when piece-rate = 3	215	220	205	220	200
Base wage when piece-rate = 4	200	200	180	200	180

Notes: This table shows key features of the different experimental rounds. Panel A shows information for the main rounds, while Panel B shows information for the supplementary piece-rate rounds.

∗ Round 4-11 all involved 12 days. Round 12 followed the standard schedule but is shorter by one day. Its schedule was equivalent to skipping day 5 and having the schedule announcement on day 6.

† Payment schedule was announced on day 2 in round 14. However, in round 13, payment schedule was never separately announced.

‡ In rounds 13-14, everyone received a bonus of Rs. 200 (which was combined with the interim-pay treatment for the Interim Pay Group), and the attendance bonus was Rs. 200. Hence the total amount of bonus was Rs. 400.

4 Survey Instruments Appendix

This appendix provides the instruments for the 3 endline survey modules.

End-of-day Survey

I1. Participant ID: | _ | _ | _ |	I2. Participant Name:	I3. Date: | _ _ | _ _| _ _ _ _|
I10. Round ID: | _ | _ | _ | _ |	I8. Start time: | _ | _ |:| _ | _ |	I9. End time: | _ | _ |:| _ | _ |
I7. Surveyor ID: | _ | _ |	I11. Type: _____	I12. Treatment : |_|_|

End-of-day Survey

Now we would like you to ask a few more questions about your experience here, and your opinions.

Priming Effect

1.	(a) What were you thinking about while you were working today? ***(Note to surveyors**: Give examples, DON'T read out options. Can mark more than one.)*	0. [ ] Nothing 1. [ ] Household-related worries 2. [ ] Finances-related worries 3. [ ] Task related -98. [ ] Others Specify: _____
	(b) Were you thinking about any worries or finances while working?	1. [ ] Yes 2. [ ] No
	(c) What were you thinking about? *[can mark multiple]*	1. [ ] Agriculture tasks 2. [ ] Finding work 3. [ ] Meeting expenses 4. [ ] Loans 5. [ ] Construction/maintenance of house 6. [ ] Daughter's marriage 7. [ ] Children's education 8. [ ] Health issues -98. [ ] Others
		Specify: _____
2.	***[Surveyor: Ask only if they did priming story]*** (a) You heard a story and had a conversation about your financial situation. Right after this activity, when you started working again, do you feel like you were able to focus more on the work and work better? Or did it make you less focused?	1. [ ] More focused 2. [ ] Less focused 3. [ ] Same → **Skip to 3**
	(b) Why?	1. [ ] Activity motivated me to work harder/earn more money 2. [ ] Felt distracted because I was thinking about finances -98. [ ] Others
		Specify:_____

	(c) ***[If they were less focused]*** How long do you feel like you were less focused?	1. [ ] Less than 1 hour 2. [ ] 1-2 hours 3. [ ] All day -98. [ ] Others Specify:_____
	(d) ***[If they were less focused]*** Did you try to make more plates and catch up later?	1. [ ] Yes, but I could not focus 2. [ ] Yes, and I did catch up 3. [ ] No, I did not try to make more plates -98. [ ] Others Specify:_____
3.	***[Surveyor: Ask only if they are a part of W1, W4, W1b, W4b]*** (a) You heard the story about Bhibuti a few days back. Did you discuss this story with people at the worksite?	1. [ ] Yes 2. [ ] No
	(b) When?	1. [ ] At the worksite 2. [ ] On the way to the village after work 3. [ ] In the village -98. [ ] Other Specify:_____
	[Surveyor: Ask only if they are a part of W2, W5, W2b, W5b] (c) You heard a story about Bhibuti today. Have you heard this story before today?	1. [ ] Yes 2. [ ] No
	(d) When?	1. [ ] At the worksite 2. [ ] On the way to the village after work 3. [ ] In the village -98. [ ] Other Specify:_____

Expenditure Survey

SECTION A: SURVEY INFORMATION

A.1	Interviewer Code	|__|__|	A.2	Round ID	|__|__|
A.3	Date of Interview	__/__/__	A.4	PID	|__|__|
A.5	Interview End Time	|__|__|	A.6	Interview Start Time	|__|__|
A.7	Worksite ID	|__|__|	A.8	Day of study	|__|__|
A.9	District Name: Prefill		A.10	Block Name: Prefill	

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

Survey intro: *Hello. Thank you for completing the training program here. I hope you enjoyed working here.*

We are trying to understand the various finances and expenses of people like you in this area. For this reason, we will ask you some questions about your expenses in the past few days and about the expenditure you plan on making in the near future. The survey will take about 30 minutes to complete.

Please try to answer the questions as honestly and accurately as possible. Remember: The answers are only for study purposes and will be kept strictly confidential, i.e. we will not share them with anyone else. Moreover, your answers to any of the questions will ***not*** *affect your compensation or any other future benefits from us in any way.*

SECTION B: HOUSEHOLD EXPENDITURES

I would like to ask you today about your spending in the last four days: what you used your money for and how much you spent on the different items. I would also like to ask you how you plan on spending your wage payments. Please let me now start with some basic questions about your expenses.

B1.	**(a) [Pre-filled]** This respondent was paid:	1. [ ] 4 days ago 2. [ ] 3 days ago 3. [ ] Not yet → ***Skip to B1 (c)***
	(b) [Pre-filled] How much he was paid:	Rs. _____
	(c) You were paid *[time in B1(a)]* the amount of *[amount in B1(b)]*. Is this correct?	1. [ ] Yes 2. [ ] No Reason: _____

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

SECTION C: EXPENDITURE RECALL

Now I would like to ask you for more details on your expenditures in the last four days.

*C1. Please tell me about the items you purchased **yesterday** and how much you spent.*
[Surveyor: For the main categories, fill in 0 if they did not spent money, -66 if they do not handle expense for this, -77 if they do not remember, -88 if they do not know how others have spent money, -99 for other reasons]

C.1.1 S. No	**C.1.2 Categories**	**C.1.3 Total Expenditure by household / Personal consumption**	**C.1.4 How much was spent on credit or by taking a new loan**	**C.1.5 Did you consume the item on this day?**
1.	**Food**			
1.1	Rice			
1.2	Potatoes and onions			
1.2.1	Fruits and vegetables (excluding potatoes and onions)			
1.3	Cheap non-vegetarian: fish, chicken skin, eggs, etc.			
1.4	Expensive non-vegetarian: chicken, mutton, etc.			
1.5	Lentils			
1.6	Oil			
1.7	Others			
1.8	Others			
2.	**Tobacco and Intoxicants** ***[Only ask for personal consumption]***			
2.1	Tobacco: bidi, chewing tobacco			
2.2	Alcohol			
2.3	Marijuana			
2.4	Others:			
3.	**Loans and credit**			

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

3.1	Paying off store credit			
3.2	Paying off institutional loan or interest			
3.3	Paying off private loan or interest			
3.4	Lending to another person			
3.5	Others:			
4.	**Medical expenses**			
4.1	Doctor's fee			
4.2	Hospital charges			
4.3	Medicine			
4.4	Others:			
5.	**Agricultural Inputs:**			
5.1	Heavy inputs: tractor, bullocks, etc.			
5.2	Fertilizers			
5.3	Seeds			
5.5	Wages for hired laborers			
5.4	Others:			
-98.	**Others:**			
-98.1				
-98.2				
-98.3				

C 1.6	(a) Breakfast	1. [ ] Yes 2. [ ] No → *Skip to C2*		
	(b) What did you have for breakfast?	Item	Quantity	Unit
		1. |_|_| 2. |_|_| 3. |_|_| 4. |_|_| 5. Others: _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

*C2. Please tell me about the items you purchased **2 days ago** and how much you spent.*
[Surveyor: For the main categories, fill in 0 if they did not spent money, -66 if they do not handle expense for this, -77 if they do not remember, -88 if they do not know how others have spent money, -99 for other reasons]

C.2.1 S. No	C.2.2 Categories	C.2.3 Total Expenditure by household / Personal consumption	C.2.4 How much was spent on credit or by taking a new loan	C.2.5 Did you consume the item on this day?
1.	**Food**			
1.1	Rice			
1.2	Potatoes and onions			
1.2.1	Fruits and vegetables (excluding potatoes and onions)			
1.3	Cheap non-vegetarian: fish, chicken skin, eggs, etc.			
1.4	Expensive non-vegetarian: chicken, mutton, etc.			
1.5	Lentils			
1.6	Oil			
1.7	Others			
1.8	Others			
2.	**Tobacco and Intoxicants** ***[Only ask for personal consumption]***			
2.1	Tobacco: bidi, chewing tobacco			
2.2	Alcohol			
2.3	Marijuana			
2.4	Others:			
3.	**Loans and credit**			
3.1	Paying off store credit			
3.2	Paying off institutional loan or interest			
3.3	Paying off private loan or interest			
3.4	Lending to another person			

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

3.5	Others:			
4.	**Medical expenses**			
4.1	Doctor's fee			
4.2	Hospital charges			
4.3	Medicine			
4.4	Others:			
5.	**Agricultural Inputs:**			
5.1	Heavy inputs: tractor, bullocks, etc.			
5.2	Fertilizers			
5.3	Seeds			
5.5	Wages for hired laborers			
5.4	Others:			
-98.	**Others:**			
-98.1				
-98.2				
-98.3				

C 2.6	(a) Breakfast	1. [ ] Yes 2. [ ] No → *C6*		
	(b) What did you have for breakfast?	Item	Quantity	Unit
		1. |_|_| 2. |_|_| 3. |_|_| 4. |_|_| 5. Others: _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

*C3. Please tell me about the items you purchased **3 days ago** and how much you spent.*
[If this person was paid 3 days ago:] This is the day you received the cash payment.
[If this person was paid 4 days ago:] This is one day after you receive the cash payment.

[Surveyor: For the main categories, fill in 0 if they did not spent money, -66 if they do not handle expense for this, -77 if they do not remember, -88 if they do not know how others have spent money, -99 for other reasons]

C.3.1 S. No	C.3.2 Categories	C.3.3 Total Expenditure by household / Personal consumption	C.3.4 How much was spent on credit or by taking a new loan	C.3.5 Did you consume the item on this day?
1.	**Food**			
1.1	Rice			
1.2	Potatoes and onions			
1.2.1	Fruits and vegetables (excluding onions and potatoes)			
1.3	Cheap non-vegetarian: fish, chicken skin, eggs, etc.			
1.4	Expensive non-vegetarian: chicken, mutton, etc.			
1.5	Lentils			
1.6	Oil			
1.7	Others			
1.8	Others			
2.	**Tobacco and Intoxicants** ***[Only ask for personal consumption]***			
2.1	Tobacco: bidi, chewing tobacco			
2.2	Alcohol			
2.3	Marijuana			
2.4	Others:			
3.	**Loans and credit**			
3.1	Paying off store credit			

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

3.2	Paying off institutional loan or interest			
3.3	Paying off private loan or interest			
3.4	Lending to another person			
3.5	Others:			
4.	**Medical expenses**			
4.1	Doctor's fee			
4.2	Hospital charges			
4.3	Medicine			
4.4	Others:			
5.	**Agricultural Inputs:**			
5.1	Heavy inputs: tractor, bullocks, etc.			
5.2	Fertilizers			
5.3	Seeds			
5.5	Wages for hired laborers			
5.4	Others:			
-98.	**Others:**			
-98.1				
-98.2				
-98.3				

C 3.6	(a) Breakfast	1. [ ] Yes 2. [ ] No → *Skip to C4*		
	(b) What did you have for breakfast?	Item	Quantity	Unit
		1. |_|_| 2. |_|_| 3. |_|_| 4. |_|_| 5. Others: _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

*C4. Please tell me about the items you purchased **4 days ago** and how much you spent.*
[If this person was paid 3 days ago:] This is one day before you received the cash payment.
[If this person was paid 4 days ago:] This is the day you receive the cash payment.

[Surveyor: For the main categories, fill in 0 if they did not spent money, -66 if they do not handle expense for this, -77 if they do not remember, -88 if they do not know how others have spent money, -99 for other reasons]

C.4.1 S. No	C.4.2 Categories	C.4.3 Total Expenditure by household / Personal consumption	C.4.4 How much was spent on credit or by taking a new loan	C.4.5 Did you consume the item on this day?
1.	**Food**			
1.1	Rice			
1.2	Fruits and vegetables			
1.2.1	Potatoes and onions			
1.3	Cheap non-vegetarian: fish, chicken skin, eggs, etc.			
1.4	Expensive non-vegetarian: chicken, mutton, etc.			
1.5	Lentils			
1.6	Oil			
1.7	Others			
1.8	Others			
2.	**Tobacco and Intoxicants** ***[Only ask for personal consumption]***			
2.1	Tobacco: bidi, chewing tobacco			
2.2	Alcohol			
2.3	Marijuana			
2.4	Others:			
3.	**Loans and credit**			
3.1	Paying off store credit			
3.2	Paying off institutional loan or interest			

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

3.3	Paying off private loan or interest			
3.4	Lending to another person			
3.5	Others:			
4.	**Medical expenses**			
4.1	Doctor's fee			
4.2	Hospital charges			
4.3	Medicine			
4.4	Others:			
5.	**Agricultural Inputs:**			
5.1	Heavy inputs: tractor, bullocks, etc.			
5.2	Fertilizers			
5.3	Seeds			
5.5	Wages for hired laborers			
5.4	Others:			
-98.	**Others:**			
-98.1				
-98.2				
-98.3				

C 4.6	(a) Breakfast	1. [ ] Yes 2. [ ] No → *Skip to D*		
	(b) What did you have for breakfast?	Item	Quantity	Unit
		1. |_|_| 2. |_|_| 3. |_|_| 4. |_|_| 5. Others: _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____	1. _____ 2. _____ 3. _____ 4. _____ 5. _____

Survey ID |0|2| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

Expenditure Survey

SECTION D: EXPENDITURE PLANNING

Now I would like to ask you about any recent loans among the participants here and how you plan to spend your money in the near future.

D1.	(a) In the last 5 days, have you loaned/borrowed any money to/from someone who is currently coming to this worksite?	1. [ ] Loaned 2. [ ] Borrowed 3. [ ] No→ ***Skip to D.2***
	(b) If yes, could you tell us the name of that person?	Name: _____ [Filled in by supervisor] PID: _____ [Filled in by supervisor] wave: _____
	(c) How much money did that person loan/borrow from you?	Amount: Rs. _____

D2.	Do you have any pressing need or plans for spending your money in the next 7 days? (*Note to Surveyor: This question relates to any expenditure the respondent may have planned in the next seven days in total)*					
	(a) Food	(b) Tobacco and intoxicants	(c) Loans and credit	(d) Medical expenses	(e) Agricultural inputs	(f) Other

Expenditure Survey

	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________
	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________
	Categories: ______________ Rs. _____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________
	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________
	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________	Categories: ______________ Rs. ____________

Expenditure Survey

Code 15	
1	Rice
2	Fruits
3	Vegetables
4	Biscuits
5	Sweets
6	Lentils
7	Fish
8	Chicken skin
9	Eggs
10	Chicken
11	Mutton
12	Fried Snacks
13	Other Packaged food
14	Curd
15	Others

Exit Survey

SECTION A: PERSONAL IDENTIFICATION

1.	Interview date	_ _ / _ _ / _ _
2.	Surveyor ID	| _ | _ |
3.	Start_Time	|__|__|
4.	End_Time	|__|__|
5.	Worksite ID	| _ | _ |
6.	Round ID	| _ | _ |
7.	Worker Name: _____	
8.	PID	| _ | _ | _ |
9.	Village Name: _____	
10.	Wave	_____

SECTION B: HAPPINESS

B1	How would you rate your happiness on a scale of 1 to 4 today?	1. [ ] Very happy 2. [ ] Happy 3. [ ] Not very happy 4. [ ] Not at all happy -98. [ ] Don‘t know

SECTION C: TOP-OF-MIND

[Annotation for data users (added to the instrument for clarification):

Surveyors asked each of the 4 questions in C1 in an open-ended way. For C1b and C1d, the response options reflect categories based on the most frequent answers provided during pilot surveys. Surveyors marked all the relevant options based on the respondents' freeform answers and also wrote out any answers that did not correspond exactly to the existing options. In no case did surveyors ever prompt respondents with the specific answer categories listed in the survey form.]

C1	(a) Could you take a look at this picture? Could you guess how this person is feeling? *[surveyor: show picture A; do not read options]*	1. [ ] Happy 2. [ ] Sad 3. [ ] Worried/anxious -98. [ ] Others
		Specify: _____
	(b) Could you guess why this person is feeling that way? There is no correct answer. *[surveyor: do not read options; can mark multiple options]*	1. [ ] Person is poor 2. [ ] Person is worried about money/job 3. [ ] Person is worried about food expenses/lack of food 4. [ ] Person is worried about other expenses 5. [ ] Person is feeling sick/weak -98. [ ] Others *[data users: see the annotation above.]*
		Specify: _____
	(c) Could you take a look at this picture? Could you guess how this person is feeling? *[surveyor: show picture B; do not read options]*	1. [ ] Happy 2. [ ] Sad 3. [ ] Worried/anxious -98. [ ] Others

		Specify: _____
	(d) Could you guess why this person is feeling that way? There is no correct answer. [*surveyor: do not read options; can mark multiple options]*	1. [ ] Person is rich or has enough money 2. [ ] Person has a good job 3. [ ] Person is worried about jobs / has no work 4. [ ] Person is well educated -98. [ ] Others *[data users: see the annotation above.]*
		Specify: _____
C2	Who do you think spend more time worrying about money issues? The rich or the poor?	1. [ ] The rich 2. [ ] It depends 3. [ ] The poor -98. [ ] Do not wish to answer / Don't know
C3	(a) When are you more worried about money issues or finding enough work?	1. [ ] In the lean season 2. [ ] In the peak season 3. [ ] About the same -98. [ ] Do not wish to answer/Don't know
	(b) Which of the following best describes how often you think about money issues?	1. [ ] Always on my mind 2. [ ] Not all the time, but they often come to my mind everyday 3. [ ] They come to my mind a few times a week 4. [ ] I don't think about it often -98. [ ] Do not wish to answer/Don't know
C4	(a) When you think about money issues, how long do you spend thinking about it?	1. [ ] A whole day 2. [ ] A few hours 3. [ ] An hour or less, but longer than a few minutes 4. [ ] A few minutes -98. [ ] Do not wish to answer/Don't know
	(b) What makes you think about money issues?	Specify: _____
C5	(a) How many hours did you sleep last night?	_____ hours
	(b) How well did you sleep last night?	1. [ ] Had a good sleep 2. [ ] Had an average sleep

		3. [ ] Did not have a good sleep -98. [ ] Do not wish to answer/Don't know

(Picture A)

(Picture B)

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