Do Financial Concerns Make Workers Less Productive?*

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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 SDs), 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.

Keywords: financial strain, financial worries, psychology of poverty, attention
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1 Introduction

People, especially the poor, frequently find themselves having too little cash on hand to meet necessary expenses. This can happen because of unexpected shocks, whether to income (e.g., job loss) or expenses (e.g., a medical emergency). In addition, the evidence indicates that cash shortfalls also routinely arise from predictable fluctuations in income or expense. However they arise, these shortfalls often have meaningful effects on consumption and well-being. For example, studies have found that people consume fewer calories the further away they are from their last paycheck, food stamp payment, or harvest payment (e.g., Shapiro, 2005; Fink et al., 2020).

A growing literature argues that such shortfalls—the feeling of being financially constrained—does not just result in less consumption but has broader psychological consequences.[1] Specifically, the inability to meet expenses can create mental burdens, such as anxiety, worry, stress, or sadness. Mental costs can persist even if individuals know that expected income will arrive in the near future. For example, the share of Americans who feel financially insecure rises steadily over the course of the month (as cash-on-hand dwindles), and then drops sharply by 53% at the start of the next month when paychecks arrive (Pew Charitable Trusts, 2016). We refer to the mental burdens created by periods of cash shortfalls as *financial strain*. Recent work argues that financial strain can adversely impact how people think, reason, and choose (Haushofer and Fehr, 2014; Mullainathan and Shafir, 2013). As a result, this literature posits that feeling financially strained itself could have economic consequences. One important and unexplored consequence could be in the workplace: if workers are mentally burdened, might their work suffer?

In this paper, we focus on one category of mechanisms by which financial strain might reduce worker productivity: strained workers may find it harder to focus on the task at hand. For example, they may be distracted directly by worries about money, or less directly, by thoughts about increased tension within the household or feelings of guilt from seeing one's children cry for an item they want. All of these reasons for mental distraction could reduce productivity. Our empirical work investigates a hypothesis directly implied by this reasoning: alleviating workers' momentary financial strain could increase productivity by improving the capacity to focus at work.

We test this hypothesis using a field experiment with 408 male workers in rural Odisha, India. The experiment takes place during the lean season, when there is little agricultural work and people work instead as casual laborers in other sectors. Such jobs are intermittent and typically of short duration, ranging from one day to a couple of weeks (Kaur, 2019; Breza et al., 2021). We partner with local contractors to employ workers in such a contract job,

[1] E.g., Mullainathan and Shafir (2013); Chemin et al. (2013); Haushofer and Fehr (2014); Haushofer and Shapiro (2016, 2018); Green et al. (2016); Lichand and Mani (2019); Ridley et al. (2020).

making disposable plates for restaurants, for two weeks during the lean season. Workers are paid piece rates, and so 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 (Figure I, Panel A). The two most commonly reported sources of worries are daily expenses and loans (Panel E), 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. Self-reports suggest that workers bring their 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 (Panel D). This creates scope for financial strain to potentially impact worker productivity.

Our financial strain manipulation is motivated by evidence that receiving money reduces financial strain, even when the payment is fully anticipated (Pew Charitable Trusts, 2016; Ellwood-Lowe et al., 2022). For example, Mani et al. (2013) find no evidence of cognitive improvements in the post-harvest period until the expected cash actually arrives in farmers' pockets.[2] More generally, financial strain may not be fully alleviated until the cash is on hand. Consistent with this, in qualitative interviews, workers 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. This suggests that for liquidity-constrained workers, the actual arrival of predictable income—enabling them to buy essentials for family, pay off loans that weigh on the mind, or have a liquidity buffer if needed—could meaningfully alleviate financial strain.

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 paid on the final day. Consequently, for a four-day window, treatment workers have received a large cash infusion while control workers have not. We examine how treatment and control workers differ during this window. In addition, we exploit the timing of pay schedule

[2] Once the harvest is delivered to the sugar mill, farmers know exactly how much income they will receive. Mani et al. (2013) explicitly test for effects after harvest but before the cash arrives vs. after it arrives; they only find cognitive improvements in the latter case.

announcement, along with supplementary design features, to examine potential confounds like fairness concerns.

This design eases financial strain while holding constant both the incentive to work and wealth. In contrast, manipulations that increase wealth (like cash transfer programs) could affect the motivation to work through purely neoclassical channels (i.e., changes in the preference for leisure versus effort), making it difficult to interpret effects. By avoiding this challenge, our design provides a cleaner test of whether financial strain in and of itself affects productivity. This is a necessary first step toward understanding total effects in policy settings.

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 when the cash is disbursed. 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 indicate that the treatment reduced financial strain and potentially improved focus—creating the potential for it to impact productivity.

When we examine actual output, we find that the treatment significantly boosts productivity. The day after receiving a cash infusion, treated workers increase output by 0.109 standard deviations (SDs), or 6.9%, relative to the control group (p=0.020). These gains persist throughout the work day and for the remaining days of the contract period. In addition, they are concentrated among workers who are poorer at baseline, measured both by having fewer assets and less liquidity. The interim payment increases productivity for these poorer workers by 0.204 SDs (p=0.003).[3] Because work hours are fixed and attendance is high (98.3%), these output increases reflect improvements in actual productivity: how quickly workers produce plates in each hour.

These findings tell us treatment workers became more productive, but our data let us go further: we can also measure changes in *how* they worked. To produce a leaf plate, irregularly sized leaves must be assembled together to form a clean circle. Doing so efficiently requires planning and focus: to think through how the leaves fit together and to make sure each

[3] 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 that fact that the magnitude of the interim payment is relatively more meaningful for poorer workers.

stitch is in line with that plan. Otherwise, there will be more work: if a plate becomes irregularly shaped, either stitches have to be removed or additional leaves will be needed to compensate—all of which raises the time per plate and reduces the amount a worker can make in a day. Each finished leaf plate contains traces of how attentive a worker was in making it—the number of leaves or stitches used, or pairs of holes that indicate where mistaken stitches were removed—which we measure unbeknownst to workers. The cash infusion not only increases total plates produced, it also appears to improve planning and focus. After treatment workers receive their interim payment, such "attentional lapses" decline by 0.08 SDs (p=0.092). As with the productivity results, these effects are concentrated among the poorer workers, whose attentional lapses fall by 0.13 SDs (p=0.037). These reductions also persist across the remaining days of the contract period.[4]

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 and effort mechanically increase attentiveness? To test this, we experimentally vary the piece rate between Rs. 2 to Rs. 4, adjusting the base wage to hold overall earnings constant. Each one-rupee increase in the piece rate raises output by 0.020 SDs. However, this is not accompanied by any discernible change in attentional lapses: the estimated coefficient is essentially zero and significantly different from the output effect (p=0.001). In other words, motivated workers do exert more effort but are no more attentive. These facts together suggest that the productivity effects are mediated, at least in part, through workers' inability to focus on the task at hand, consistent with financial strain reducing attentiveness.[5] They suggest a model of worker productivity where attentiveness and effort can operate independently: interventions that increase effort (such as the piece rate) need not increase attentiveness, and increases in attentiveness can happen without changes in motivation. In fact, the reduction in attentional lapses caused by the interim payment is particularly striking given that workers are working *faster*: more cash-on-hand increases pace while simultaneously reducing the rate of mistakes.

Could this pattern of results be explained by mechanisms other than the psychological benefits of relieving financial strain? We examine two potential sets of confounds. Because interim payments were made by the employer, the treatment may have affected workers' perceptions of trust or fairness—generating, for example, gift exchange. We use two direct tests to examine such possibilities. First, workers were told their payment schedules 3 to 4 days in advance of the interim payments; in contrast to the predictions of a basic gift exchange

[4]This offers an example of testing for the effects of financial strain by using attention as an outcome variable, versus a treatment as in priming manipulations. To mimic the previous literature, we also tested a priming intervention, with mixed results—highlighting the difficulty in reliably undertaking such manipulations—matching a growing concern in psychology about the reliability of priming (Kahneman, 2012; Chivers, 2019; Sherman and Rivers, 2021).

[5]Though our goal is not to isolate any particular psychological mechanisms, several—including worries, stress, or sadness—have the property of involuntarily interfering with workers' attention while at work.

or fairness model, we find no effects of the announcement.[6] Second, within the treatment group, we randomized the exact day of the interim payment, so that some treatment workers received the cash infusion on day t and others on day $t+1$. Using the variation from this staggered timing, we find no evidence that control workers decrease effort from a sense of unfairness after seeing others receive an interim payment. Moreover, such stories are not obviously consistent with the pattern of our main results: effects being concentrated among the more financially constrained workers and the effects on attentiveness.[7]

A second potential confound is the possibility that our results are due to workers making a productivity-enhancing investment through better nutrition. Work in health and related fields indicates that nutritional changes require longer horizons to translate to productivity effects (e.g., Gómez-Pinilla, 2008; Schofield, 2014), whereas our results manifest overnight.[8] In addition, we directly measure workers' breakfast intake (they are provided the same food after arriving to work, e.g., lunch) and find it is unaffected by the interim payment. In addition, treatment effects do not decline by the end of each workday (when recent calorie intake is the same across treatment and control workers). Consequently, while plausible ex ante, confounds from fairness or nutritional channels do not appear to explain our results.

This paper contributes to the growing literature on the psychological impacts of economic conditions (Haushofer and Fehr, 2014; Schilbach et al., 2016). One set of studies has focused on effects on happiness 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 et al., 2015; Carvalho et al., 2016; Bartos et al., 2021; Ong et al., 2019; Fehr et al., 2020; Lichand and Mani, 2019).[9] Building on these studies, recent work examines whether priming

[6]This is not driven by a lack of credibility around the announcement. For example, we find no evidence for announcement effects in later rounds in a given worksite (when the worksite would have built a greater reputation in the area for paying when promised). Similarly, we find no change in treatment effects of the interim payment across subsequent rounds, helping rule out a story based on trust in the employer.

[7]If interim payment constitutes a "gift," basic gift exchange models may not a priori have predicted that we should see effects only for poorer workers. Similarly, if effects stem from workers being motivated to work harder for the employer, it is unclear how this would generate a sharp increase in attentiveness, which was measured unbeknownst to workers and does not respond to higher motivation or effort from higher piece rates. This lack of evidence for fairness concerns is consistent with prior work documenting that, in this specific setting, there are fairness norms with respect to wage *levels* (which we held fixed during the experiment) but limited evidence for strong norms around other aspects of the pay structure (e.g., Kaur, 2019). In other similar low-income settings, workers have expressed a preference for larger lump sum payments rather than smaller, more frequent ones (Casaburi and Macchiavello, 2019; Brune et al., 2021). Consequently, it is unclear whether workers themselves would have viewed interim payments as preferable.

[8]In addition, 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—limiting the potential scope for large productivity gains from simply increasing calories.

[9]Carvalho et al. (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

individuals on their finances during an academic test can affect test performance, or alter demand for an educational intervention (Duquennois, 2022; Lichand et al., 2021).

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 effect on productivity unclear. This motivational channel is absent when looking at outcomes like paper-and-pencil cognitive tests. 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. Moreover, we find that such impacts can occur even when the receipt of cash is expected. This pattern matches previous findings on psychological effects, and 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. Evidence that asset transfers increase labor supply suggests broader potential relevance of the effects we document (Banerjee et al., 2020).[10] Our results are complementary because 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 channels suggests additional pathways through which alleviating financial constraints could increase earnings among the poor.

We do not take a stance on the specific psychological mechanism, such as worry, anxiety, affect, etc., 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

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.

[10]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).

pay structures or consumption smoothing technologies—presents interesting directions for further research.

2 Context and Simple Framework

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 et al., 2021). 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), consistent with findings in other studies in rural India (e.g., Muralidharan et al., 2016; Breza et al., 2021). Contract jobs may pay wages daily, at interim intervals, or as a lump sum at the end of the contract period. Combined with intermittent employment, this leads to both low and variable income in lean months, the time of our experiment. Consequently, workers report high levels of financial constraints, especially among those who are dependent on wage labor for their primary earnings (i.e., who own little or no farmland).

2.1 Financial Concerns

In our sample, 71% of workers report outstanding loans at baseline (Table I, Panel B). Nearly 50% have outstanding credits with local shops for basic household consumption, consistent with difficulties in meeting basic daily expenditures.[11] Overall, 68% of workers say they would have difficulty coming up with Rs. 1,000 (i.e., 4 days of 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 cash-on-hand and difficulty in financially coping with shocks in a range of contexts, including in the U.S. and in developing countries (Lusardi et al., 2011; Morduch and Schneider, 2017; Collins et al., 2009).

These financial burdens are reflected in high levels of worries. In Figure I, we depict workers' self-reports of how thoughts about finances interact with their daily lives.[12] 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

[11]Specifically, among the 54% with outstanding credits, 84% have credits with shopkeepers. The remaining have credits with neighbors, former employers, etc.

[12]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., Fresco et al., 2002; Zebb and Beck, 1998).

about finances at least once per day, and almost all reporting 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 did 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.III 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. The results of this exploratory exercise illustrates what workers view as most important. In interpreting the emotional states of others, they reflexively turn to financial concerns first.

Perhaps most relevant for 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 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. Such motivational data are of course only suggestive; they do not necessarily indicate that financial concerns alter productivity. However, they provide a glimpse into how frequently financial burdens rise top of mind while individuals are working.

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 (Pew Charitable Trusts, 2016; Mani et al., 2013; Ellwood-Lowe et al., 2022) and motivates our experimental design approach.

2.2 A Simple Framework

We present in Appendix B a simple model with three goals. First, we lay out what a standard model—i.e. one that ignores any effect of financial strain—predicts in our experimental conditions. Second, we extend this model to include psychological elements, so as to provide one formalism for 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 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 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. We call the other input a to denote mental inputs that are beyond a worker's control ("automatic"). Central to our hypothesis is the idea that some elements of focus are beyond the worker's control. We capture that here 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, set to $\bar{a}$, the worker's capacity for automatic attention; in the baseline case, then, it is 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 need 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 D_t is high, resources are valuable because there is more (high interest rate) debt to pay down, and pressure from lenders to be released. So in our model, strain increases with $u'(c_t)$ and D_t: workers are more strained when there are high returns to consumption or bigger debts to pay down. 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: workers are less able to provide mental inputs when they are financially strained. 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 as in Dean (2020).

The essence of the experimental intervention—described in detail in Section 3—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 increases net present value of lifetime earnings and thereby lowers marginal utility of consumption—thus 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 treatment group 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, including behavioral effects of financial strain changes the predictions about the productivity effect 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. We are able to provide direct empirical measurements of automatic input a (in addition to measurements of the overall effect on output). Second, the additional treatment effect in the post-announcement period should be smaller (less positive) than the additional effect in post-payment period.

The behavioral model nests the baseline model and allows for analysis of the total effect of treatment on productivity, which will include both effort effects as well as productivity effects from decreased strain. In the post-payment period, the effort effects are negative and go in the opposite direction of the positive automatic input effects. Furthermore, the effort effects predict a more positive effect in the post-announcement period than the post-payment period, which go in the opposite direction of the automatic input effects. As a result, one pattern of treatment effects would clearly indicate the existence and relative importance of a financial strain effect: (i) positive post-payment effects on productivity and (ii) smaller or non-existent post-announcement effects on productivity. What about the converse? Suppose we did not find positive post-payment effects. This 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 an experimental design in which the effort margin is large.

3 Experimental Design

Our primary aim is to test for a direct impact of financial constraints on worker productivity. To enable this, 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 the intermittent nature of employment, 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.

3.1 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 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 3.4 below for implementation details).

This interim payment is a substantial cash infusion, corresponding to what workers typically earned in the month before joining the study.[13] 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 there is an immediate effect of cash receipt 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 in a few days. In the morning of workday 5, each worker is told individually when he will receive his payment (see Section 3.4 for details). The subsequent "announcement period" between days 5 to 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. We use this for supplementary analyses—for example, as one of our tests for potential confounds such as fairness concerns and gift exchange. In addition, we combine this with variation in when the interim payment arrived (day 8 vs. 9) to rule out confounds such as trust in the employer (see Section 6).

Discussion. Our design alters workers' financial constraints while holding fixed the incentive to work. Regardless of whether workers received an interim payment on day d, when they arrive to work on day $d+1$, the factors that determine their effort level in a standard model are unchanged. Specifically, they face the same piece rate, and their earnings for work on day $d+1$ 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.[14]

This design is in contrast to manipulations that increase total wealth, like cash transfer programs, which could 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 cleaner test of our specific mechanism

[13] This is primarily due to low employment rates in the lean season (see Section 2 and Table I).

[14] The treatment could have a modest effect on wealth levels since treatment workers may save some interest by paying back loans up to four days early. However, the magnitude of such effects is small, and any resulting effects on productivity would bias our results *against* finding treatment effects of the early-pay treatment. For example, using a recent estimate of annualized interest rates from moneylenders in India of about 40% from Surendra (2020) and the fact that the treatment group reports paying back Rs. 271 more, the treatment group may have saved less than Rs. 2 due to receiving the money four days earlier, corresponding to less than 1% of a worker's daily wage during the time of the experiment. Given that sizable piece rate changes lead to only small impacts on productivity, the magnitude of any effects of this limited wealth change on effort and productivity is negligible. In addition, if anything, we should expect this effect to decrease the treatment group's marginal benefit of exerting effort and thus slightly *lower* the treatment group's effort and productivity. See also the discussion in Section 2.2.

of interest: whether financial strain in and of itself has productivity effects. Understanding whether this mechanism exists is a necessary first step to interpret total effects of wealth transfers in policy settings.

Our test only has power to detect effects if receiving money reduces financial strain, even when the payment is fully anticipated. In our experimental setting, treatment workers know the interim payment is coming, and both treatment and control workers know they will receive their earnings by the end of the contract period. As discussed in Section 2, prior work provides evidence that despite such knowledge, receiving cash itself can still have an incremental effect on financial strain. This effect will drive any treatment impacts we observe. To the extent that receiving the job itself also reduces mental burdens, our treatment effects may only capture a share of the total impact of receiving an income boost.

Finally, while our experimental manipulation uses changes in pay frequency, the goal of our experiment is not to provide insight into optimal pay structure. Our manipulation should not be interpreted as a general test for optimal pay frequency for two reasons. First, whether more frequent payments constitute a large change in liquidity will vary depending on the context.[15] Second, other factors, such as lining up income payments with lumpy expenditures, can affect both worker welfare and productivity, and would therefore be an important input into optimal frequency. Rather, in our experiment, the interim payments are simply an effective tool to induce a large cash infusion among liquidity-constrained workers in our particular setting. This allows us to construct a clean test for whether financial strain affects worker productivity in a high-stakes setting—a possibility for which there is currently scant empirical evidence. If such effects do exist, then this would provide impetus to consider this mechanism (among others) in pay structure and policy design.

3.2 Work Task and Outcomes

Work task. Workers produce disposable plates, made from stitching together leaves from sal trees (Appendix Figure A.I). 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 set by partnering contractors, and all output was sold to restaurants.

Workers are paid a flat base wage for attendance plus a piece rate per completed leaf plate that satisfied the quality standards developed by contractors. To qualify for payment, a leaf plate is required to: (i) meet a minimum size requirement; (ii) have no holes or gaps so that it could hold food (e.g., curry) without leaks; (iii) have all leaf stalks covered by other

[15] For example, the relative liquidity boost from being paid daily or monthly would be different in long-run employment—indeed, having such stable employment would limit the likelihood that a worker faces large financial strain in the first place (e.g., Morduch and Schneider, 2017). Moreover, in our context, long-term stable employment is largely not possible for the low-income rural workers who comprise our sample—precisely the reason why a cash infusion has a meaningful impact in our experiment.

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—stitching plates requires repeated fine motor movement. It is also cognitively demanding. The process begins with leaves that come in irregular (oval) shapes and sizes, and each leaf is different. These varying shapes must be stitched together so as to produce a circular plate. And 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 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. Given the high acceptance rates, using the completed number of leaf plates yields nearly identical results.

Outcome: Attentiveness index. We hypothesize that cash receipt affects workers' psychological state, easing the mental burdens indicated in Figure I and potentially 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: (a) 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;[16] (b) the number of leaves used; and (c) 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.

We collected these three measures for a subset of hours in each experimental round.[17]

[16]When a stitch is removed from the plate, it leaves two holes (one at each end of the stitch), indicating that a stitch was undone so that the leaf could be removed and re-positioned.

[17]The attentiveness index measure was not included in our pre-registry due to an oversight. However, we

Workers were unaware that these dimensions of their output were measured. We normalize these measures and combine them into an "attentiveness index," reversing the scale so that higher values on the index correspond to improved attentiveness (i.e., fewer double holes, leaves, or stitches). Specifically, we calculate the average number of leaves, stitches, and double holes per plate during each worker-hour slot. 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 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.

If we find that being flush with cash improves attentiveness—leading to fewer mistakes and more efficient production—this would be consistent with improved focus at work. However, this would not distinguish between various psychological mechanisms that could give rise to such improvements, for example, worrying, mind wandering, stress, or affect. Rather, it would indicate that the mechanism at play operates by improving attentiveness at work.

3.3 Additional Treatments

We augment our design with two additional sources of variation.

Piece-rate variation. In five supplementary experimental rounds, we vary piece rates for output, without the interim pay treatment (see Section 3.4 for details). 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.

The piece-rate variation uncovers the extent to which output can be changed by *conscious* effort—when workers are motivated to work harder through increased marginal returns to effort—within the context of our particular task. We also measure the effects of increased piece rates on attentiveness. This allows us to test whether workers increase their focus when they are more motivated, in this case by a piece rate. In contrast, psychological mechanisms (e.g., worry) are at least partly beyond a worker's control: A worker who is more motivated may not be able to simply choose to worry less and thus be more focused. Finally, by comparing the impacts on output and attentiveness, we can examine whether both measures

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).

exhibit an inherent correlation or whether one can change without the other.

Priming. Our primary test relies on using real income variation. As a supplementary exercise, following previous work (e.g., Mani et al., 2013; Bartos et al., 2021), we 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 answer 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.[18] 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.II). 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).

3.4 Implementation and Protocols

We conducted field activities during the main lean season (March through June) of 2017 and 2018 in Odisha, India, with piloting beginning in 2017. We ran 14 experimental rounds with about 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 five worksites.[19] We lay out our protocols for a typical round below; deviations from these protocols are documented in Appendix C.

Recruitment. A few days prior to the start of a new round of experiment, recruiters

[18] 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.

[19] This number excludes 21 participants who dropped out in the first four days before the payment schedules (i.e., treatment status) were announced. Each round had 26 to 30 workers each.

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 norms that restrict women traveling outside the village for work. Since the number of interested workers exceeded the worksite capacity in each experimental round, we hired 30 randomly selected workers from the sign-up list for that round. In addition, 5 back-up participants replaced any participants who dropped out of the study during the first three days of a round (before treatment assignment was announced). Among the 408 workers who enrolled in the study, only 6 dropped out before the end of the employment contract—3 in the interim payment group and 3 in the control group. We include all 408 workers in the analysis.

Worksite setup. In a typical round, workers worked full-time at the worksite for 12 consecutive days.[20] 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.[21] 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 in order to minimize workers' ability to compare output with each other or engage in 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, with the goal to foster trust in the worksite among workers.[22] 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.[23]

[20]It is common for short contract jobs to require attendance on consecutive days.

[21]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 also Appendix C).

[22]While larger or additional early payments would have been desirable to foster further trust, they would have eased financial constraints among all workers, thus limiting the potential for the experimental variation to create meaningful differences in financial constraints.

[23]When considering the extensive (labor supply) margin, other forces come into play. While ex ante the

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 receive the rest of their earnings on the final day of the contract. When they arrived at the worksite on 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 et al. (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.[24] 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.

Moreover, given that receiving a well-paying two-week contract job was the salient major event for workers, the details of being told there may be pay frequency differences was minor in comparison to the "luck" of getting the job itself. Debriefs with workers after piloting indicated that pay frequency did not loom large in their minds in the overall picture of what having a two-week contract job entailed—for example, being fortunate enough to receive steady work with competitive wages, learning a new task, or being given lunch at the worksite.

extensive margin effect is ambiguous, recent research argues that the total effects could be even larger due to a positive labor supply response (Banerjee et al., 2020). Our goal is not to characterize the overall policy response from a cash drop, but to construct a clean test for a direct and immediate effect of cash on productivity.

[24]These interviews suggest that pay frequency varies substantively across casual jobs, and does not seem correlated with pay levels, amenities, or other features.

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 minimize measurement error.

Randomization. In each experimental round, workers were randomly assigned to the interim payment (treatment) group or the control group.[25] Within each round, treatment and control workers were cross-randomized into Wave A or Wave B, which determined the specific timing of 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: cash-poor priming (i.e., 2 days before their wave's interim payment), cash-rich priming (i.e., 2 days after their wave's interim payment), or no priming.

Piece-rate rounds. Implemented in 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.I).

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).

[25]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.

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 last 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. Appendix Figure A.II summarizes the timing of when surveys were conducted. We include all our endline survey instruments in Online Appendix D.[26] The surveys were translated from English to Odiya and piloted by local field staff to ensure appropriate phrasing for the local context, and then back-translated into English by an independent translator to ensure accuracy.

4 Data and Empirical Strategy

4.1 Summary Stats, Heterogeneity in Wealth, and Balance

Table I presents summary statistics and baseline balance tests. Column 1 shows means and standard deviations for all control group workers. A typical worker in our sample is about 40 years old. Virtually all workers are married (98%) and have children (88%). 72% 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, which captures wealth through the quality of the worker's housing, is the quintessential measure that would be 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 addition, in the appendix, we report heterogeneity by the

[26]One of our pre-registered outcomes, life satisfaction, was not collected across rounds, and so we are unable to examine impacts on this.

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).

4.2 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 is disbursed in the worker's wave. $\text{Announcement period}_{ird}$ equals 1 during the days after the payment schedule was announced until the interim payment was disbursed, and equals 0 otherwise (see Figure II). Regressions control for round times 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 et al. (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 4.1.

In addition, 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 with each other:

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

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 et al., 2014).[27] β provides an estimate of the average treatment effect of the interim payment relative to the control group.

5 Results: Impacts of Cash Infusion

5.1 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.[28] We examine whether this indeed changes workers' expenditures and whether it translates into an impact on self-reported focus at work.

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 regression 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, $p<0.001$). Columns 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,

[27] This procedure has the benefit of being automated, but we note that these benefits hold asymptotically. The challenge in using LASSO lies in the choice of the regularization parameter. 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. Rate results show that, when the assumptions needed hold, it provides excellent way to choose controls in larger samples. So, following a recent convention, we largely rely on it for choice of controls. However, in a very few 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.

[28] The typical worker had 8.6 days of paying wage work in the month preceding the experiment (Table I).

petrol and diesel (172%, Col. 6). Given the effects on food, we consider potential impacts through nutrition channels in Section 6.2.

Other potential spending categories include agricultural inputs, construction, transfers, and festivals; we find no detectable impacts on these categories, 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.II, 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.II 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 have the ability to borrow if needed but would prefer to hold less debt. In addition, treated workers are 9 percentage points more likely to lend money to control workers in the days following the interim payment (Appendix Table A.II Col. 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. Summing across all expenditures, 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. 951 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 differs 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. Unfortunately, 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 (in order 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, in order to avoid confounding effects on worker thoughts.[29]

[29]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.II). Because workers who are primed that day are specifically told we expect

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.III). 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 p.p. (30%) more likely to come up with reasons for negative affect other than financial worries (Appendix Table A.III, Cols. 1-2, p=0.038-0.045). Similarly, they are 9 p.p. (62%) more likely to 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 (Cols. 3-4, p=0.018-0.027).[30]

While only suggestive, these patterns introduce the potential for the cash infusion to enable workers to be more effective while working. Ultimately, however, we rely on examining impacts on productivity 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.

5.2 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 4.2. Column 3 corresponds to the specification in equation (1).

In the days following the interim payment, treated workers increase output by 0.109 SDs,

them to think about their finances, we exclude these workers when examining treatment effects of the interim payments on this question.

[30]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 are not predictive of their level of happiness (Appendix Table A.IV).

corresponding to a 6.9% increase in output (Col. 3, p=0.020). In contrast, we see no evidence for a treatment effect during the announcement period: the estimated coefficient is 0.014 SDs (p=0.685). Moreover, we can reject that the effect 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.V, Col. 1). Similarly, there is no scope for treatment response in hours per day as work hours are fixed.[31] Consequently, the impacts in Table III reflect increases in actual productivity: how quickly workers produce plates in each hour. We show that these results are robust to alternate empirical specifications in Appendix Tables A.VI and A.VII. In addition, these findings are robust to explicitly controlling the false discovery rate within each family of hypotheses (Appendix Table A.VIII, Panel C).

The productivity impacts are concentrated among poorer workers, who increase output by 0.204 SDs (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.

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.IV). In addition, the 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.IX, 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.X). For example, treatment effects do not depend on household composition (i.e., the number of children) or years of education. Finally, we see some evidence for heterogeneity by baseline financial worries: treatment effects are concentrated among workers who report feeling worried at baseline (Appendix Table A.XI). However, this analysis is under-powered, 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.[32]

[31] 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.V, 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).

[32] 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 may not provide sufficient signal on worries.

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 potentially consistent with this second interpretation (see Table I). Finally, while the heterogeneous effects by wealth are consistent with and support our hypothesis, we view the average treatment effects as the core of our analysis.

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 the interim payment for workers, and compare output differences to the baseline period.[33] The figure indicates that, 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 SDs. These effects persist and even slightly increase for the remaining days of the contract period. This pattern matches the sharp overnight expenditure increase on loans and household necessities seen in Table II above.

Finally, note that these effects capture changes in workers' total output, since it is unlikely 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 et al. (2021) find that rural casual workers reported doing any secondary activities after a day of wage work on only 1.72% of days.

5.3 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 variable.

[33]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 6.1, we find no evidence of productivity changes immediately following the announcement.

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 SDs (Col. 1, p=0.092) and an increase in the high attentiveness indicator of 0.095 percentage points (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.XII).

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 SDs (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.IX, 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 5.2, 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 amount 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.[34] Workers increase their pace of work, reducing time per plate, but do so while simultaneously *reducing* their rate of mistakes. Such attentional impacts are consistent with a potential range of psychological mechanisms—including cash-on-hand reducing worries and thus distractions during work, or stress, mental health, or happiness—which could operate by improving attentiveness at work.

5.4 Impacts of Piece-Rate Variation

The interim payment increases workers' productivity and attentiveness. Perhaps this is happening because workers are simply more motivated? Or perhaps even more extremely,

[34]Potentially consistent with the idea that improved attentiveness reflects improved cognition, we find a strong baseline correlation between workers' attentiveness index and their performance on an incentivized memory task, Corsi, a standard cognitive test in psychology (Appendix Table A.XIII). We undertook this test in the supplementary piece rate rounds only in order to correlate cognitive function with attentiveness. Of course, this is a simple correlation and therefore only suggestive.

whenever a worker works harder, 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 3.3 and 3.4). 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.

We estimate impacts in Table V. Increasing piece rates increases productivity (Cols. 1-3). Each one-rupee increase in the piece rate increases output by 0.020 SDs (p=0.042), while a 1% increase in the piece rate leads to an output increase of 0.058 SDs (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 Columns 1 and 3 in Table V has a p-value of 0.001.[35]

Taken together, these results are consistent with the idea that the capacity to focus is not fully in a worker's control. Consequently, a worker who is more motivated by the piece rate may not be able to simply ignore mental burdens and have greater focus and engage in better planning, or cognition. Motivated by this evidence, our simple framework in Section 2.2 models production as a function of two kinds of inputs into production: an effortful one that is fully under worker's control, and an automatic one that is beyond the worker's control. These findings suggest that piece rates and cash infusions boost productivity through distinct channels—and as such the two can have very different properties and magnitudes.

[35]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 70% the size of the treatment effect on productivity (0.11 SD vs. 0.077 SD). If productivity and attentiveness move together, then we may have expected a piece rate effect on attentiveness of $0.020 \times 0.7 = 0.014$. This lies outside the above confidence interval (CI), and is 92% larger than the right-hand side of the CI. While this is not conclusive, since attentiveness and productivity may not scale linearly with each other, as a back-of-the-envelope calculation, it 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 Confounds and Supplementary Tests

6.1 Announcement Effects and Perceptions of the Employer

Our intervention is designed to manipulate cash-on-hand. However, since the manipulation is delivered by the employer—in the form of the interim payment—this raises potential concerns that the treatment could change workers' perceptions towards the employer. In this subsection, we examine two sets of potential concerns in particular: gift exchange or fairness concerns, and trust toward the employer.

Announcement effects, gift exchange, and fairness. One possible explanation for the above results is based in notions of fairness. If treated workers feel they have been given a gift ("gift exchange"), they might reciprocate by working harder; conversely, if control workers feel they have been treated unfairly, they may reciprocate by working less hard (e.g., Gneezy and List, 2006; Fehr et al., 2009; Cohn et al., 2014; Breza et al., 2018). Previous work in this specific setting indicates that while there are strong fairness norms with respect to wage *levels* (which were the same across workers), norms over amenities such as other aspects of the pay structure are weaker (e.g., Kaur, 2019). Consistent with this, our debriefs with workers indicate that pay frequency is just one of many details about the job, and does not loom large in workers' minds relative to the "luck" of getting the job itself, and the various other amenities associated with the job (steady work with competitive wages, learning a new task, being given lunch at the worksite, etc.).

However, despite this, given the strong expenditure responses we observe, it may of course be possible that pay frequency could evoke fairness considerations. While such considerations are undoubtedly important in a range of settings, four pieces of evidence indicate that these mechanisms 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. The "gift" is the same across all treated workers, so given the strong response of poorer workers, fairness concerns might have suggested all respond to some extent. While ex post one can adjust models to explain this pattern (by arguing poorer workers value the "gift" more), it is not obvious ex ante that richer workers should not value it at all.

Second, fairness concerns would need to account for the attentiveness results. Even when motivated for their own personal interest with higher piece rates, workers do not change their attentiveness (Section 5.4). Given this, it is unclear why they would then alter their attentiveness when motivated by a desire to improve output for the employer. In addition, recall that workers are not even aware that any such measures were being collected, making a strategic reason for altering these dimensions less likely. Moving beyond our specific attentiveness measures, workers do not appear to be trying to produce higher-quality plates. In fact, after the cash infusion, treated workers spend *less* time per plate, speeding through faster to earn more money. If treatment workers were somehow reciprocating by trying to increase quality, one may expect this time per plate to go up rather than down.

Third, under these alternate mechanisms, we would expect there to be some impact immediately following the pay schedule announcement on day 5. In other words, as soon as treatment workers learn they will be treated "well" or control workers learn they will be treated "unfairly," there should be some change in their behavior. Even if one thought fairness concerns may be more salient after payment is actually 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, throughout the tables in the paper, we consistently see no evidence of effects in the announcement period. In a more detailed test for announcement effects, 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 post the announcement (Cash $\times$ 1 day post announcement) and the day after that (Cash $\times$ 2 days post announcement).[36,37] Under gift exchange or fairness concerns, we would expect positive announcement effect coefficients. However, they are small and statistically indistinguishable from zero. 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 0.11 SD. We can reject that this coefficient equals the announcement effects at the 1% level (Col. 2).

Fourth, one possible concern is that, for some reason, fairness concerns only kick in once payments are actually delivered. Again, this may not be what one may expect ex ante under standard fairness models, but one could perhaps construct this prediction by adding features such as salience. As a fourth piece of evidence, we test whether the control group decreases effort *after* the interim payments are delivered to treatment workers. To test for this, we exploit a feature of our randomization. Recall that we randomized the treatment group into two subgroups: interim payment on day 8 (Wave A) vs. day 9 (Wave B), as illustrated in the more complete timeline in Appendix Figure A.V. When workers arrive to work on day

[36] One potential concern with this test is that workers may simply not have trusted the announcement. However, if workers trust that the employer will pay them enough to come to work each day, it is unclear why they should not trust the announcement over what day they will get paid. In addition, in Appendix Table A.XIV, we document that announcement effects are not larger if a worksite has been operational in a given area for a longer time. In other words, we see no evidence for positive announcement effects even in later rounds when worksites should have had a local reputation for paying as promised.

[37] 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 schedules by the time they return to work on day 6. Consequently, even if workers must learn the specific schedules of others, we would expect effects on day 2 post announcement (i.e., day 6).

9, Wave A treatment workers have already been paid, and Wave B treatment workers are going to be paid that evening. If workers who are paid later than others feel treated unfairly, then the control group workers should also feel treated more unfairly relative to the Wave B treatment workers.

This generates the following implication: on day 9, we should observe a differential drop in the control group's output relative to the Wave B treatment group. In other words, if strong feelings of unfairness are what drive our treatment effects, then we should see at least some manifestation of this in how hard control workers work on day 9 relative to the Wave B treatment workers who are about to be paid that day. We look for such effects in Table VI, Cols. (3)-(4), and fail to find evidence in support of such a pattern: the coefficient showing the difference between Wave B treated workers on day 9 relative to control workers is insignificant and actually negative in sign ("Cash × Payment day × Wave B" coefficient).[38] Consequently, we see no evidence that control workers decrease effort after seeing interim payments made at the worksite.

Finally, although we did not collect direct data on workers' demand for the different payment regimes, evidence from other settings suggests that at least some workers prefer more infrequent payments as a method of commitment savings (Casaburi and Macchiavello, 2019; Brune et al., 2021), such that the direction of overall preference would be a priori unclear. This may especially be true if workers are not aware of the extent to which relieving mental burdens could affect their productivity (e.g., Dean, 2020).

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). In addition, we set the reference point so that any shocks were positive (avoiding negative reciprocity or loss aversion effects). Moreover, as discussed in Section 3.4,

[38]Specifically, we add the triple interaction "Cash × Payment day × Wave B" in Columns 3-4 of Table VI. Under this specification, the double interaction "Cash × 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 workers (who have not yet been paid). However, the coefficient on the triple interaction is negative (though imprecise), inconsistent with the idea that fairness concerns drive the large treatment effects we see. In Column 3, we add heterogeneity by wealth, and still do not see evidence supporting the fairness story—the triple interaction term is still not positive when looking only at the poorer workers.

our protocols assign workers to "batches" to mitigate comparison effects across workers; this leverages the approach in Breza et al. (2018), which constructed reference groups within the workplace to limit comparisons to workers in other groups. The worksites also kept workers socially distanced to reduce reference group effects.

In addition, in contrast to Kaur et al. (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. Moreover, note that self-control problems in effort cannot account for our treatment effects in the post payment period. Any output produced in the post-pay period is paid at the end of the contract period for both treatment and control workers; in other words, the temporal distance between when effort is exerted and its gains are realized are the same across treatments. Consequently, if the interim payments affect effort in the post-pay period, this must come from a mechanism other than present-focus in effort.

Trust in the employer. An additional potential concern is that the interim payment could increase workers' trust in getting paid in the future. This would change the implied piece rate they actually expect to receive, thereby potentially increasing effort and output. 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 had a worksite schedule that we announced 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. Despite these efforts, one may be concerned about some residual trust issues.

However, this explanation is also inconsistent with the main pattern of our results. It is unclear why trust should only increase among poorer workers, why it should affect attentiveness (since it would be in the worker's interest to make fewer costly mistakes to begin with), or why it should lead workers to report feeling more focused at work. Such a story would also suggest a high rate of responsiveness to increases in the piece rate, a premise that is not supported by the results in Table V.

We use two further tests to examine this story. First, we use the feature that we ran many experimental rounds in each worksite. In Appendix Table A.XIV, we verify that we see no evidence for differential treatment effects in later rounds—when presumably trust would be higher because the worksite would have built a local reputation for paying as promised.

Second, we again exploit the staggered timing of cash infusion among Wave A vs. Wave B treatment workers. If workers increase output because they update beliefs about probability of payment, then we might expect the Wave B treatment workers—who saw Wave A workers being paid—to also update their beliefs when they arrive at work on day 9. So they too should increase their output on their payday even before actually being paid in the evening. This would suggest that the coefficient on the triple interaction—Cash × Payment Day × Wave B—should be positive in Table VI, Cols. 3-4. However, this coefficient is insignificant and even negative in magnitude. Perhaps more problematic for a trust explanation, when Wave A workers are paid on day 8 as promised, it is unclear why this should not boost *all* workers' confidence that the employer pays out as expected. In other words, it is unclear why seeing some people paid as promised should generate differential changes in relative trust between treatment and control groups.

6.2 Nutrition and Sleep

The primary goal of our experiment is to examine whether cash-on-hand has a direct and immediate impact on worker productivity, operating outside of the traditional investment-based channels discussed in the literature such as human capital, physical capital, or nutrition. By construction, our design rules out human or physical capital changes.[39] In this section, we discuss whether changes in *physical* well-being (i.e., nutrition or sleep) could plausibly drive our finding of overnight increases in productivity.

Nutrition. A long literature in development economics has hypothesized the potential for nutrition to affect productivity (e.g., Dasgupta and Ray, 1986). We find some evidence that workers increase their food expenditures following the cash infusion (Table II), as discussed in Section 5.1. There are two categories of potential pathways: (i) a change in workers' underlying nutritional stock and (ii) short-run blood sugar increases from increased food intake for workers who would otherwise feel hungry at work.

The first pathway—biological changes for malnourished workers—is unlikely given the immediate nature of productivity effects in our setting. According to the biological and medical literatures, such changes cannot occur overnight (e.g., Gómez-Pinilla, 2008). For example, consistent with slower-moving effects, Schofield (2014) finds evidence of increased earnings among workers only starting a week after increasing their caloric intake. In addition, 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. This lessens the scope for a severe caloric deficit based explanation for the sizable productivity gains we observe. Consistent with this, for example, recent work by Park and Kim (2021) finds no impact of increased caloric intake on worker productivity.

The second channel is more plausible ex ante, since it occurs through immediate and short-run changes. In our setting, immediate energy increases from eating would need to occur via

[39] The time horizon does not allow for human capital investments, and there was no scope for workers to bring any implements or physical capital to the worksite.

breakfast intake, leading treated workers to arrive to the worksite with fuller stomachs. This is because once workers arrive at the worksite, there are no differences among them in food intake. In the rounds with worksites open past 2 pm, all workers are provided the exact same lunch at their work stations, and in other rounds, they are provided the same snacks at the end of each day. Snacking while working is not something that occurs in our context. Workers eat at their workstations during the organized breaks in which we provide lunch or snacks. They do not bring snacks from home and are not allowed to go off site during the work day. In addition, any differences in dinner would not biologically alter energy levels 12 to 18 hours later. 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, the expenditure survey collects measures of daily breakfast consumption for the days following the interim payment day. We find no evidence of increased breakfast on any of the dimensions of our survey, including whether workers had breakfast, how much, and what they ate (Cols. 1 to 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, thus leaving not much room at the extensive margin, 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 work day, especially as all workers are provided the same food in the afternoon. However, we find persistent impacts of the interim pay treatment throughout the day, including the last couple of hours or the workday, i.e., 5 to 7 hours after eating breakfast (Cols. 6 to 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.XV, 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).

6.3 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.III, 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, the worries that are most often top of mind while working are their financial concerns, thus providing considerable scope for the treatment to alleviate them. Since our treatment directly impacted these concerns (i.e., the treatment effects on thoughts while working in Figure I, and effects on expenditures in 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—or whether the change in focus is conscious or beyond the worker's direct awareness; any of these could plausibly affect attentiveness at work. Probing the specific forces that link financial strain to attentiveness is an interesting direction for further psychological work.

6.4 Priming

Our design uses variation in real income to examine the impact of financial strain on productivity. In contrast, several previous studies have instead used priming manipulations to trigger financial worries. While we use attention as an outcome variable to examine channels, the priming approach instead uses it 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, as a supplementary exercise, we follow prior work by also undertaking a priming exercise, and cross-cut it with the cash infusion treatment (see Section 3.3). As discussed above, our main test examines the relative difference in priming effects when workers are cash-rich vs. cash-poor (due to the exogenous interim payments).

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.XVI, 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 for effects on productivity (Appendix Table A.XVI, 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 (Col. 6, Panel A, p=0.418).[40]

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 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 et al., 2012; Cesario, 2014; Banker et al., 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.

[40]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.XVI, Panel B.

7 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; Carvalho et al., 2016; Ridley et al., 2020; Chemin et al., 2013; Haushofer and Shapiro, 2016, 2018). Evidence on economic field behaviors is a necessary next step to understand what implications these have for economic outcomes. Earnings is one such outcome and a particularly important one since its consequences are widespread. As such, an impact of financial concerns on earnings could eventually change our thinking about the impediments to escaping poverty and related policies. Though these lessons are down the road, requiring a great deal more empirical work, we suggest several potential avenues.

First, the positive impact of early payment seems to say something about optimal payment frequency, specifically that more frequent payment (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 in other contexts they may. Once there is a schedule of consumption needs, the optimal payment frequency will need to account both for the financial strain effects we document as well as the potential for self control problems in consumption. It is possible that such a careful analysis might reveal intuitive payment structure: payment frequency (and sizes) that matches the cadence of expenditure needs. More broadly, a focus on payment frequency alone might be too narrow: financial products that allow workers to appropriately move income to match expenses could be a more general solution and one which does not appear to be present in the market (Pew Charitable Trusts, 2016). Additionally, these issues raise important questions of market (and even Coasian) efficiency: what frictions, if any, are present that prevent firms from providing these optimal payment contracts or offering these financial products?

Second, these effects may cause us to reconsider conditional and unconditional cash transfer programs. Given our findings, it seems worth revisiting other contexts in search of similar direct effects. For instance, Fink et al. (2020) document increases in on-farm labor supply and harvest output following liquidity drops to 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). Our evidence suggests that direct effects of changes in financial strain could contribute to 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 own 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 more specifically models of worker output that incorporate the effects we have found. For instance, our results could suggest a different interpretation of efficiency wages. Firms may be compelled to 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 at work. 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 et al., 2021). Disentangling the exact mechanism for our effect could prove fruitful to better understand the nature of these relationships.

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 especially 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.

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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	4.694	-0.044	0.892
	[3.468]	(0.325)	
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 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 towards 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 expenses (Col. 3) include the expenses in Col. 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 times wave (strata) fixed effects and those in Panel B control for round times wave times day fixed effects. In addition, all regressions control for the baseline covariates chosen using the Lasso post-double-selection procedure (Belloni et al., 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.

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 interim payment.
- Cols. 1-4 presents 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 et al., 2014). Col. 4 additionally adds day and hour fixed effects.
- Cols. 5-6 shows 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.

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 in order 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 interim payment.
- Cols. 1-2 presents average treatment effects across workers. Cols. 3-5 tests 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 uses 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.

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.

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).
- Col. 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.

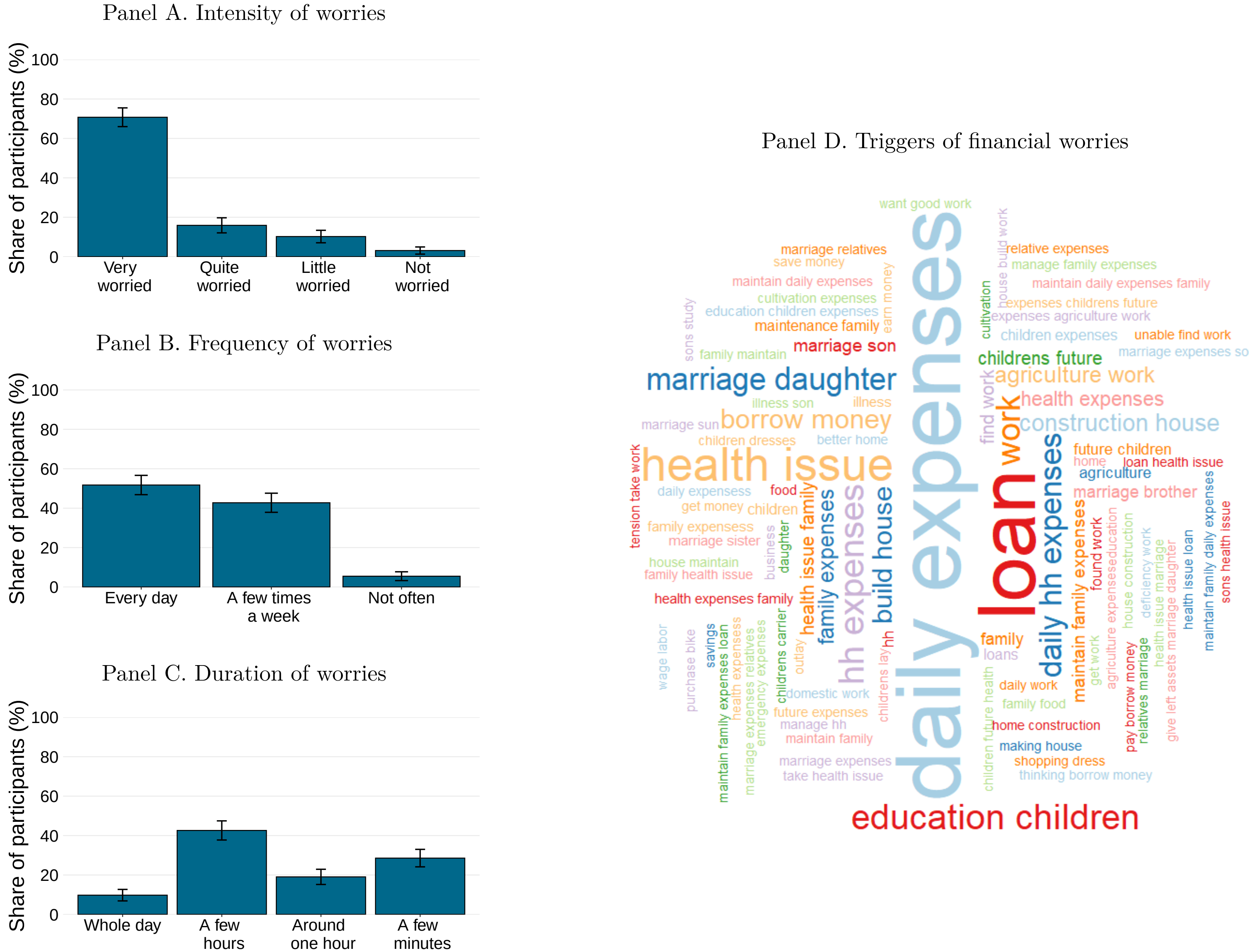
Table VII: Tests for Nutrition Channels

	Breakfast Measures (Post-pay Period)					**Hourly Production**			
	Had any break-fast	Ate rice	Amount of rice	Ate veg-etables	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 times 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 times wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

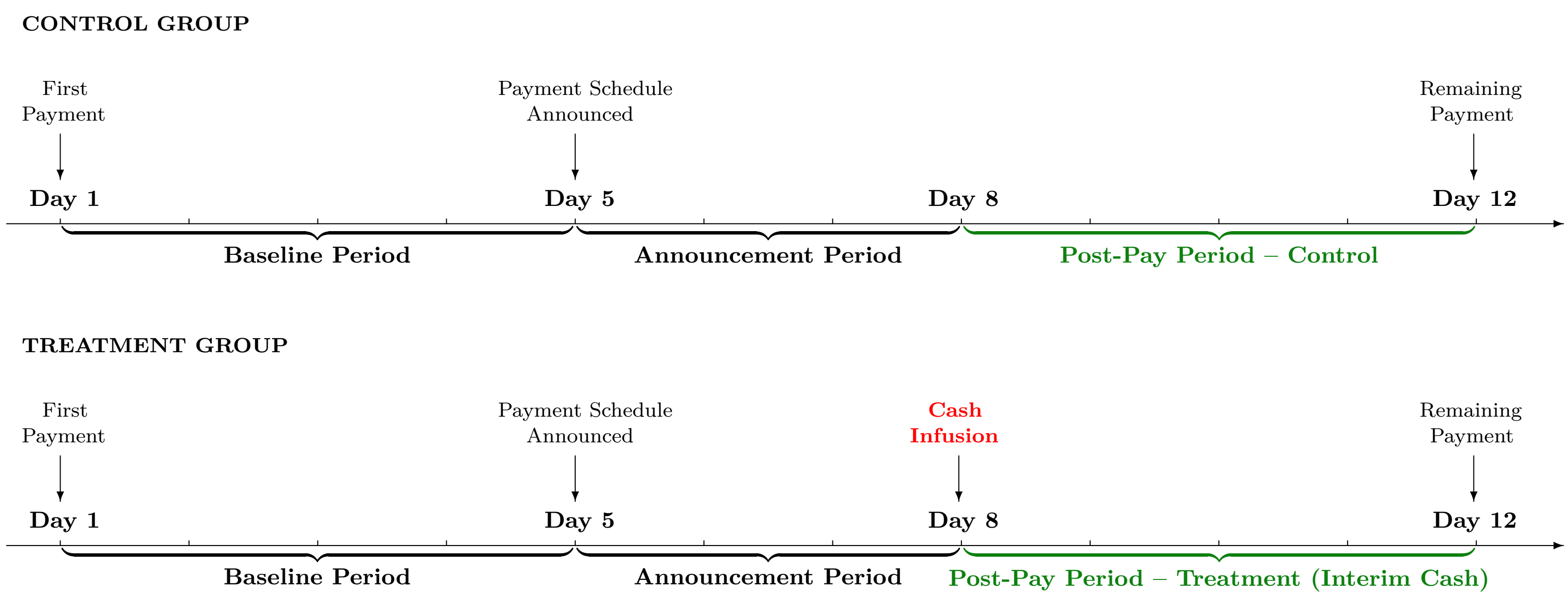
Figure I: Financial Concerns



Notes: This figure shows the participants' responses to question 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?" ($N = 400$); and (C) "How long do you worry about finances every day?" ($N = 399$). Question (A) was asked during baseline, except in rounds 3 and 4. Questions (B) to (C) were asked during later surveys, and 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).

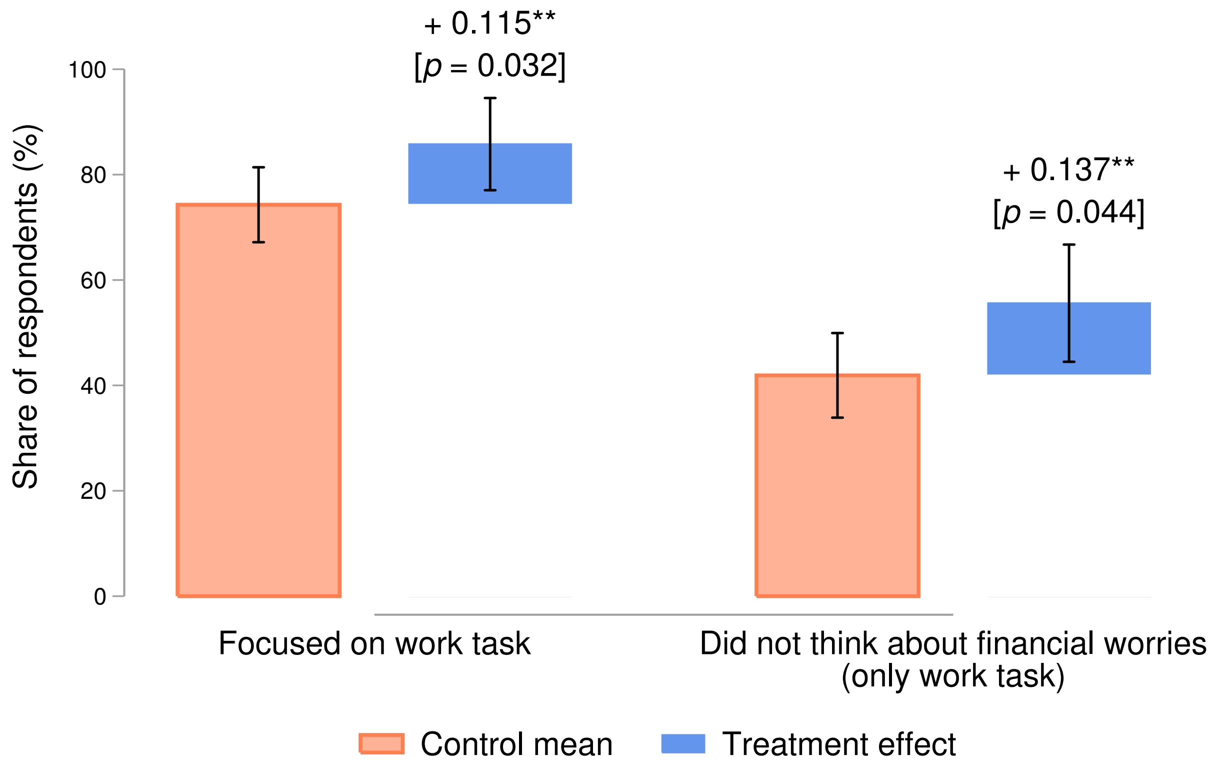
Figure II: Experimental Design



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 on 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.V 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.

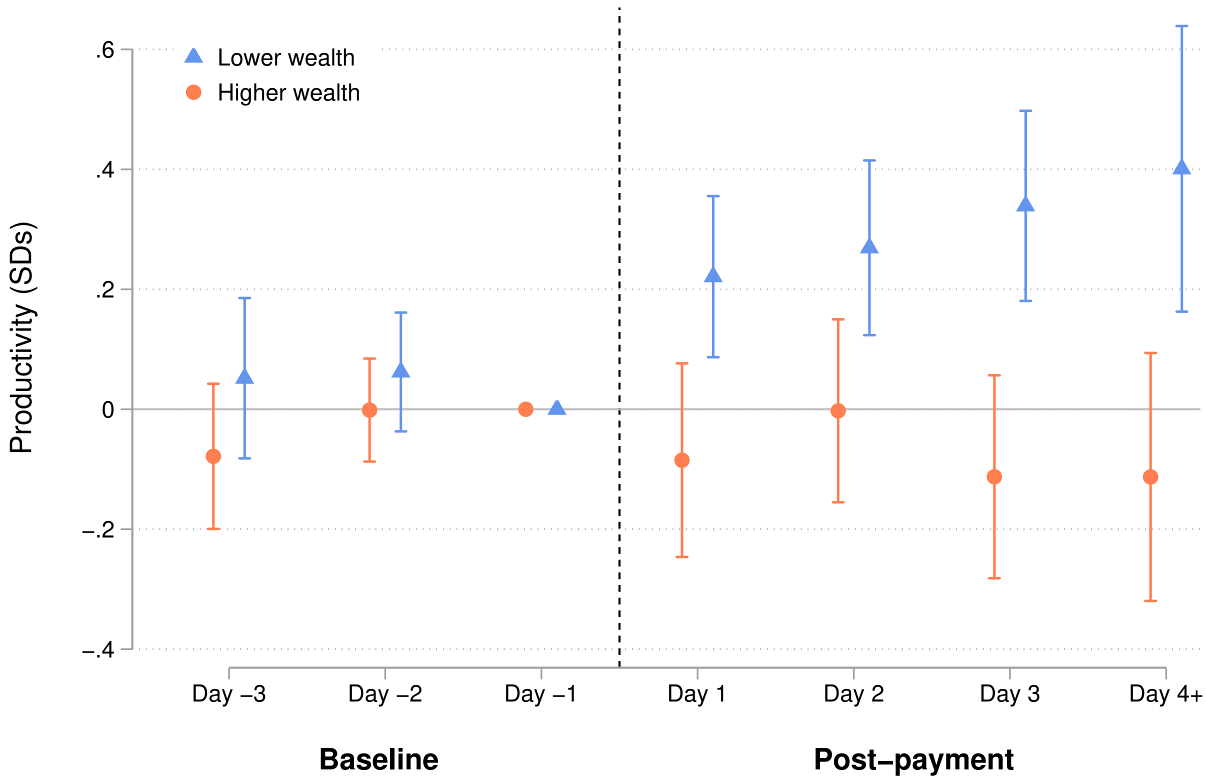
Figure III: Thoughts while Working



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 payment 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.

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



Notes: This figure plots estimated effects of the interim payment on 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 diference-in-differences regression on the full sample, with controls for worker and day fixed effects, and controls to absorb the announcement period.
- Standard errors are clustered by worker. 90% confidence intervals are shown.

A Online Appendix

A.1 Appendix Figures and Tables

Table A.I: 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	4.218	-0.211	0.647
	[3.524]	(0.460)	
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.

Table A.II: 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 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 ploughs 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 final 5 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 times 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.III: Reactions to Picture

	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 tests whether the interim payment changes 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.III. They were then asked: "Could you guess how this person is feeling? Could you guess why this person is feeling that way?". They could list as many reasons as they wanted.
- The outcome variable in Col. 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 Col. 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.IV: 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 times wave (strata) fixed effects. Robust standard errors are reported.

Table A.V: 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 worker was present at the work site 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.I) 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 times wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.VI: 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: 1 observation for the post-pay period, and 1 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 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 covariate controls 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.VII: 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 times 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 actually 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.VIII: 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 tables continues to the next page.

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: Announcement Effects (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.IX: 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.X: 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 × Covariate	-0.210***	-0.121*	0.015	-0.122*	-0.030	-0.007	-0.002	-0.005	0.025
	(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 owning farmland (Col. 1); house quality, i.e., living in a non-mud house, constructed of durable material (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 times wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.XI: 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.XII: 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.XIII: 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 signifies that a stitch was removed in order 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 worker's score on an incentivized memory test (Corsi Span Test, see a detailed description in Dean et al. (2018)). The average score was 9 out of 15 with standard deviation of 2.4. All regressions control for round fixed effects. Robust standard errors are reported.

Table A.XIV: 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 times wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

Table A.XV: Effects on Sleep 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	1.24	0.82
N: workers	400	400	400

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

Table A.XVI: 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

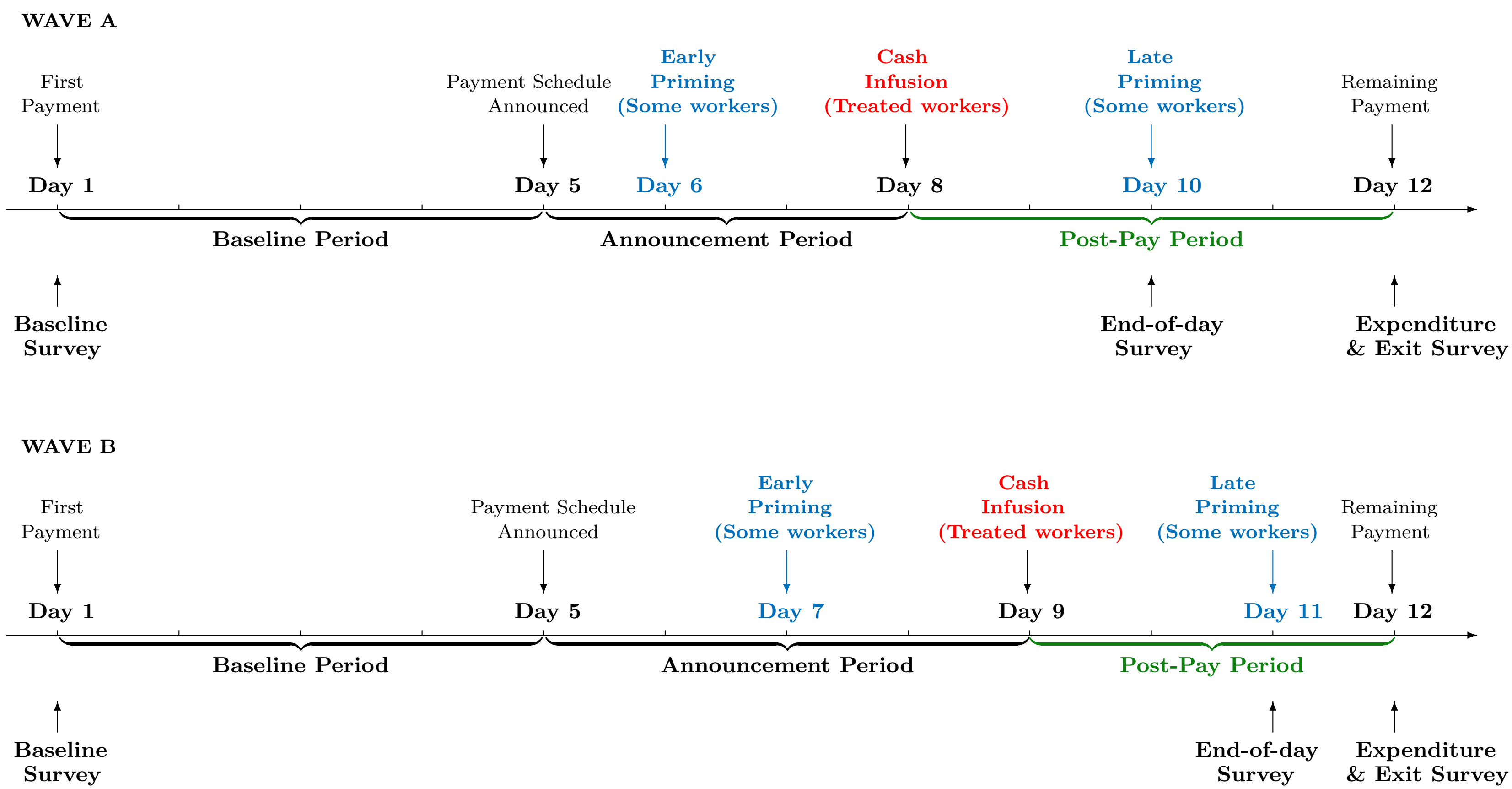
Notes: This table shows the impact of the priming intervention on worker productivity.

- "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. 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. 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 times wave (strata) fixed effects and the same covariate controls as in Col. 3 of Table III. Standard errors are clustered by worker.

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



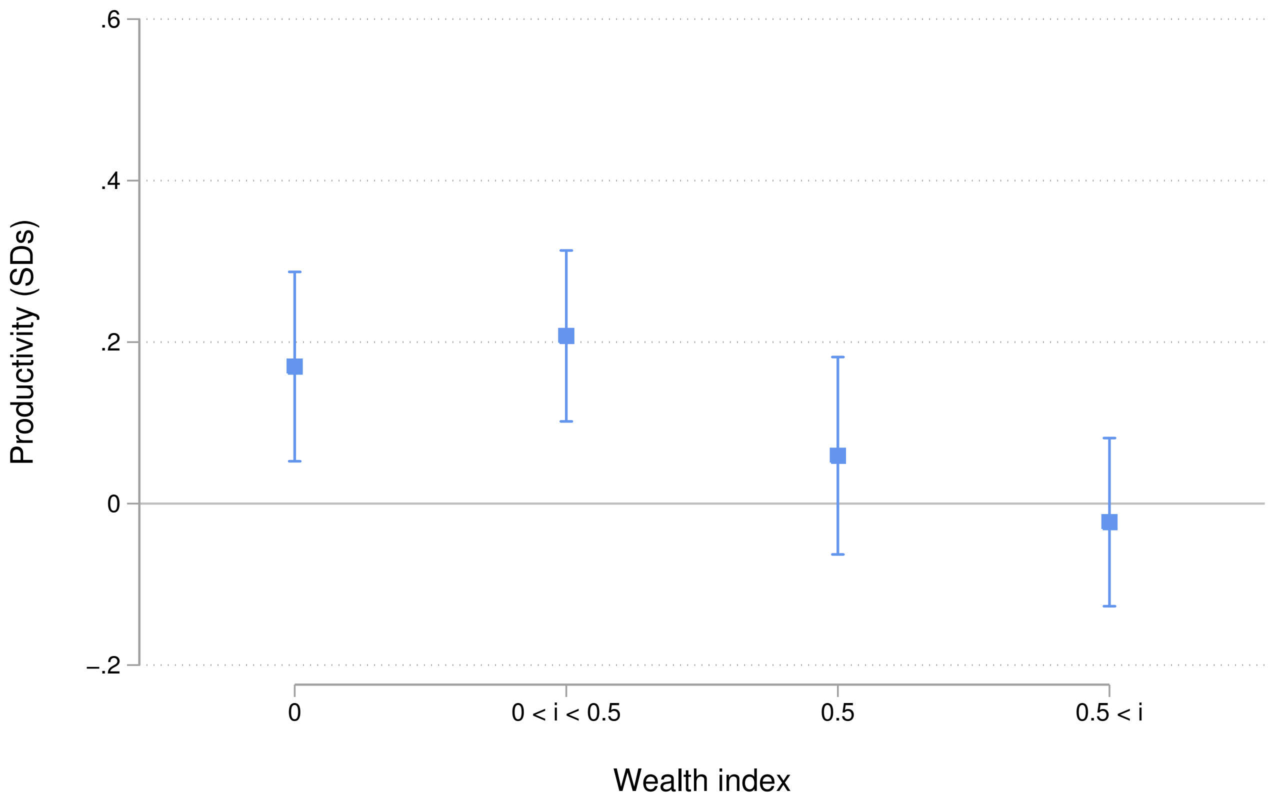
Notes: This figure is a more detailed version of Figure A.V. This figure additionally shows the timing of the priming interventions and surveys, while combining the interim pay treatment and control groups within each wave. Workers were randomized into Wave A and Wave B. Wave B is identical to Wave A, except for 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 Baseline survey on day 1, and Expenditure and Exit surveys on day 12. In Wave A, the interim pay treatment group receives the interim payment on the evening on 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 on day 9. All Wave B workers are randomized to be primed on day 7, day 11, or not at all, and they answer End-of-day survey on the evening of day 11.

Figure A.III: Top of Mind Pictures

Panel A　　　　Panel B

Notes: This figure contains the photos that accompanied the top-of-mind questions in 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 pre-determined categories in recording responses. Workers were then shown the picture in Panel B and asked the same questions regarding this photo.

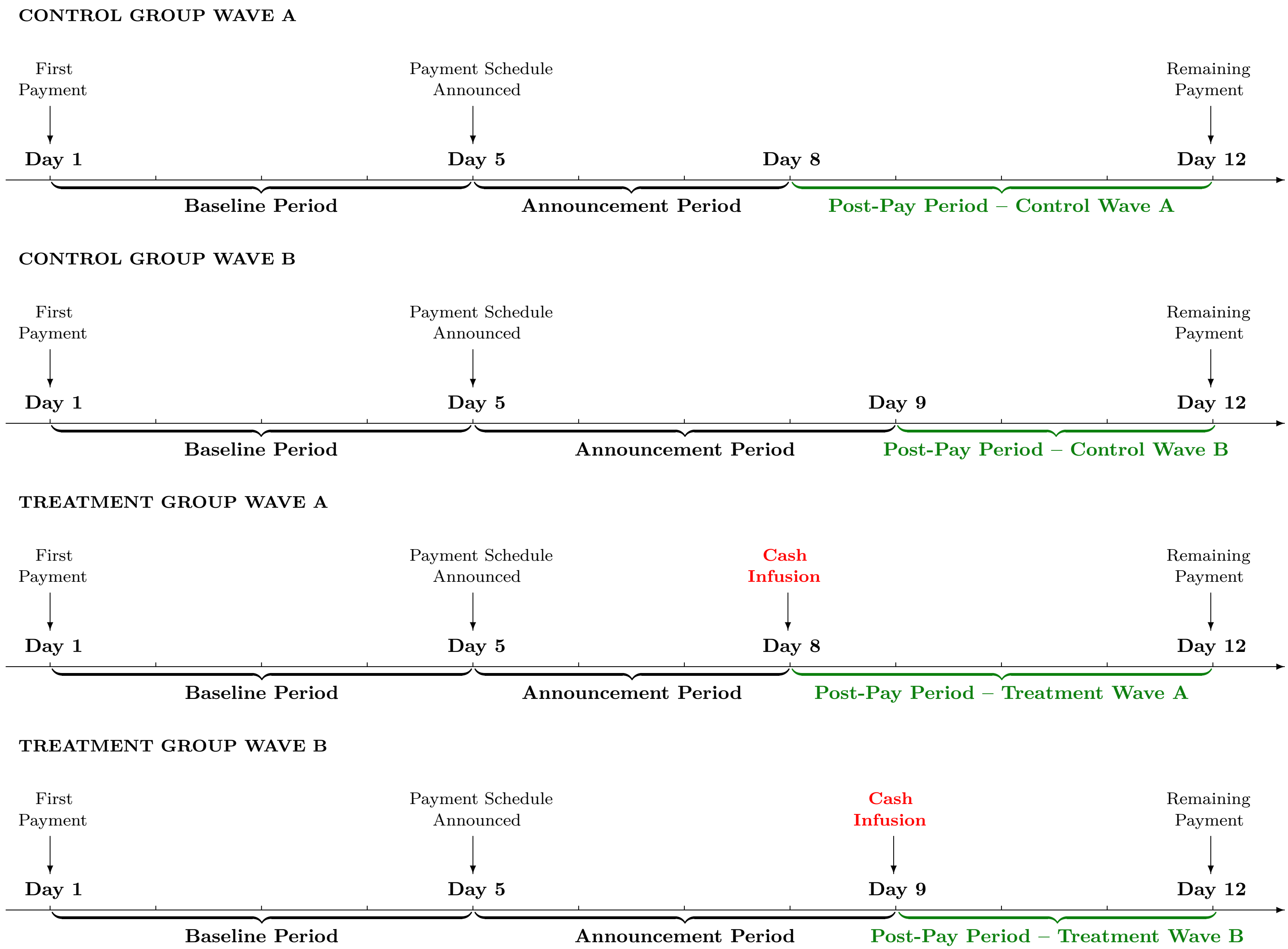
Figure A.IV: Treatment Effects on Worker Productivity by Wealth Level



Notes: This figure plots 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 is an average of 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 times 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.V: 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 for 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.

B 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 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.[41] 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, 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). ($\frac{\partial f}{\partial e} \geq 0, \frac{\partial f}{\partial a} \geq 0$) 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 of time 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 time period, defined to be the difference in output between a treatment and control group worker:

$$TE_t := y_{t,T} - y_{t,C} \qquad t \in \{1, 2\} \tag{3}$$

[41] 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.

B.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 B.2 below.

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

$$\max_{e_1, e_2, \{c_t\}_{t=1}^{\infty}} \quad \sum_{t=1}^{\infty} \delta^{t-1} \Big[u(c_t) - g(e_t) \Big] \tag{4}$$

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).

$$c_{1,g} + s_{1,g} = w + s_0 \quad \forall g \in \{C, T\} \tag{5}$$

- In each subsequent period, workers choose consumption c_t and savings s_t levels, 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.[42]

- 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:

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

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

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

[42]For exposition, we refer to s_t as savings, but the workers do not necessarily have to choose $s_t > 0$ and in fact can take on debt with $s_t < 0$.

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

$$c_{2,T} + s_{2,T} = w + (1+r)s_{1,T} + y_{1,T} \tag{8}$$

$$c_{3,T} + s_{3,T} = w + (1+r)s_{2,T} + y_{2,T} \tag{9}$$

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

$$c_{t,g} + s_{t,g} = w + (1+r)s_{t-1,g} \quad \forall g \in \{C, T\}, \forall t \geq 4 \tag{10}$$

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:

$$u'(c_{t,g}) = \delta(1+r)u'(c_{t+1,g}) \tag{11}$$

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:

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

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

$$[e_{2,T}] : g'(e_{2,T}) = \frac{u'(c_{2,T})}{(1+r)} f'(e_{2,T}) \tag{14}$$

$$[e_{2,C}] : g'(e_{2,C}) = \frac{u'(c_{2,C})}{(1+r)} f'(e_{2,C}) \tag{15}$$

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 (12) and (13). 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:

(1) **Prediction 1: ($\mathbf{TE}_2 < 0$).** *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.[43] 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 treatment effect in period 1 is more positive than the TE in period 2. In both periods, there is a negative effect of treatment on output due to the 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.[44] 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 that are under conscious control of the

[43]To see this, we first note that the Euler equation (equation 11) 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 (5) - (10). 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 14 is smaller than that of equation 15 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 leads to lower output and a negative treatment effect.

[44]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 (12) and (13): 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.

worker.

B.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 that reflects the capacity of the worker 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 culmination 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: $a_t = a(u'(c_t), D_t)$.[45] 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 Dean (2020).[46]

Predictions. The optimization problem for choosing effort in each period is unchanged from before (as captured by (12) through (15)). We consider the effect of automatic input by looking at the partial derivative of output with respect to a (which we assumed was positive):

$$\frac{\partial f}{\partial a}(e, a) > 0 \tag{16}$$

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:[47]

[45] 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.

[46] 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.

[47] In the previous section, we analyzed the effects of treatment on output via effort, holding the level

(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 treatment effect in period 2 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 that impact automatic input move in opposite directions. In practice, we expect output for treatment and control groups should 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.

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

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.

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.

B.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. 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.[48] 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:

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

[48]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.

C 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.XVII.

Deviations. There were several deviations from the standard schedule:

- Rounds 1-3, which were conducted in March-June of 2017, had a number of 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 work day 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 in order 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. In order 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.

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.XVII.

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.

Table A.XVII: 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.

D 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.

<u>Priming Effect</u>

<table>
<tr><td rowspan="4">1.</td><td>(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.)</td><td>0. [ ] Nothing
1. [ ] Household-related worries
2. [ ] Finances-related worries
3. [ ] Task related
-98. [ ] Others
Specify: _______________</td></tr>
<tr><td>(b) Were you thinking about any worries or finances while working?</td><td>1. [ ] Yes
2. [ ] No</td></tr>
<tr><td rowspan="2">(c) What were you thinking about? [can mark multiple]</td><td>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</td></tr>
<tr><td>Specify: _______________</td></tr>
<tr><td rowspan="3">2.</td><td>[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?</td><td>1. [ ] More focused
2. [ ] Less focused
3. [ ] Same → Skip to 3</td></tr>
<tr><td rowspan="2">(b) Why?</td><td>1. [ ] Activity motivated me to work harder/earn more money
2. [ ] Felt distracted because I was thinking about finances
-98. [ ] Others</td></tr>
<tr><td>Specify:________________</td></tr>
</table>

	(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	

Expendituure 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. ________

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			

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. ____________________

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			

Expenditure Survey

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			

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				

<table>
<tr><td rowspan="3">C 4.6</td><td>(a) Breakfast</td><td colspan="3">1. [ ] Yes

2. [ ] No → Skip to D</td></tr>
<tr><td rowspan="2">(b) What did you have for breakfast?</td><td>Item</td><td>Quantity</td><td>Unit</td></tr>
<tr><td>1. |_|_|
2. |_|_|
3. |_|_|
4. |_|_|
5. Others: ____________________</td><td>1. ___________________
2. ___________________
3. ___________________
4. ___________________
5. ___________________</td><td>1. ___________________
2. ___________________
3. ___________________
4. ___________________
5. ___________________</td></tr>
</table>

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

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

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. ____________

Survey ID |_0_|_2_| Round ID |__|__| Workside ID: |__|__| Worker ID: |__|__| Version No: |__|

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**

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

Introduction: This is a 10-step ladder. When you are not worried at all, you will indicate towards the bottom rung of the ladder. Likewise, you will indicate towards the top rung of the ladder depending on how worried you are. You can choose a new step for every question that we ask you. I will present you with a number of situations that one may face. Please tell me how worried you are about each situation on a scale of 1-10, where 1 is not worried at all and 10 is very worried.

Note to surveyor: Show the respondents the 10-step ladder, the ladder is numbered on the right side of the steps. When a respondent indicates towards a step, note down the corresponding number. The ladder picture needs to be given to the respondent for the duration of this part of the survey.

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.]

<table>
<tr><td rowspan="3">C1</td><td rowspan="2">(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]</td><td>1. [ ] Happy
2. [ ] Sad
3. [ ] Worried/anxious
-98. [ ] Others</td></tr>
<tr><td>Specify: __________________</td></tr>
<tr><td>(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]</td><td>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.]</td></tr>
</table>

		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)