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Supreet Kaur
Sendhil Mullainathan
Suanna Oh
Frank Schilbach

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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 7.1% (0.12 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.

Supreet Kaur
Department of Economics
University of California, Berkeley
Evans Hall
Berkeley, CA 94720
and NBER
supreet@berkeley.edu

Sendhil Mullainathan
Booth School of Business
University of Chicago
5807 South Woodlawn Avenue
Chicago, IL 60637
and NBER
Sendhil.Mullainathan@chicagobooth.edu

Suanna Oh
Paris School of Economics
6th floor, office 38
48 boulevard Jourdan
75014 Paris
France
suanna.oh@psemail.eu

Frank Schilbach
MIT Department of Economics, E52-560
The Morris and Sophie Chang Building
77 Massachusetts Avenue
Cambridge, MA 02139
and NBER
fschilb@mit.edu

1 Introduction

People, especially the poor, frequently experience cash shortfalls. These may arise from unexpected shocks to income or expenses, such as job loss or a medical emergency. The evidence suggests, though, that they can also arise even from income or expense fluctuations that are entirely predictable. 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., 2018).

A growing literature argues that feeling financially constrained has psychological consequences beyond its direct effects on consumption.[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). A person burdened with financial worries may, for example, find it hard to focus and thereby not think as clearly or make poor decisions. As a result, this literature posits that feeling financially strained itself could have economic consequences.

One particularly important consequence could be on productivity. Workers whose mental resources are taxed by financial strain may be less productive—for example, because their mind might wander, leading them to work more distractedly or make more errors. Reduced productivity could in turn create additional economic consequences such as lower earnings. This paper investigates whether alleviating workers' momentary financial strain makes them more productive, in part because they focus better.

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., 2020). We partner with local contractors to employ workers in such a contract job, making disposable plates for restaurants, for two weeks during the lean season. Workers are paid piece rates, 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,

[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).

the stakes for productivity are large and 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 that low liquidity creates a feeling of vulnerability, 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 carry these 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., 2020). 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, worries may not be fully alleviated until the cash can actually be spent. 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 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

[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.

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 begin by gauging 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 0.115 standard deviations (SDs), or 7.1%, relative to the control group (p=0.047). 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 0.215 SDs (p=0.007).[3]

These findings tell us treatment workers became more productive, but our data lets 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 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

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

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=0.031). As with the productivity results, these effects are concentrated among the poorer workers, whose attentional lapses fall by 0.23 SDs (p=0.008). 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 0.018 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=0.004). 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 a mechanism not fully under the control of workers—consistent with psychological channels such as worry.[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 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

[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; Molden, 2014).

[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.

[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.

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; Green et al., 2016). 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., 2018; Ong et al., 2019; Fehr et al., 2020; Lichand and Mani, 2019). Building on these studies, recent work examines whether priming individuals on their finances during an academic test can affect test performance, or alter demand for an educational intervention (Duquennois, 2019; Lichand et al., 2021).[9]

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

[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]Duquennois (2019) finds that low-income students perform worse on math questions if they are phrased in monetary terms rather than non-monetary terms. Lichand et al. (2021) show that a priming intervention decreases poor parents' demand for an SMS program that provides motivational text messages to parents around children's schooling.

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 suggest additional pathways through which alleviating financial constraints could increase earnings among the poor.

Note that 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 lose focus 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.

We do not seek to translate these results into policy prescriptions, such as on optimal pay frequency. Our design uses the timing of payments solely as a tool to test for the effects of financial strain. Optimal pay frequency may depend on other considerations such as lining up payments with lumpy expenditures like rent or the availability of savings options (Casaburi and Macchiavello, 2019; Brune et al., 2021). Rather, the goal of our paper is to provide a clean proof of concept for whether productivity effects can occur in a high-stakes setting where workers' behavior determines their income. We find that a relatively modest manipulation of financial strain produces meaningful effects on productivity. The magnitude of our findings suggests that examining the productivity implications of broader interventions—for example, different pay structures or consumption smoothing technologies—presents interesting directions for further research.

[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., 2020).

2 Context: Financial Concerns

We undertake our study with low-income workers engaged in small-scale manufacturing in Odisha, India. In this area, laborers work in agriculture during peak planting and harvesting periods, which comprise about 4 to 6 months of the year. In the remaining lean agricultural months, they typically seek short-term contract employment in non-agricultural jobs, such as manufacturing and construction. These jobs are of short duration—with the modal job lasting one day, and lengths typically ranging from one day to a couple weeks (Breza et al., 2020). 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). Such low lean season employment rates are consistent with those found in other studies in rural India (e.g. Muralidharan et al., 2016; Breza et al., 2020). Contract jobs may pay wages daily, at interim intervals, or as a lump sum at the end of the contract period. This, combined with intermittent employment, 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).

In our sample, 71 percent of workers report outstanding loans at baseline (Table I, Panel B). Nearly 50% have outstanding credits with local shops for basic household consumption, indicating 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 coming up with the liquidity to cope 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 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,

[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).

we depict workers' responses to an open-ended qualitative question asking them what makes them think of financial issues; the figure aggregates raw text responses in a word cloud, where larger text denotes phrases that appear more frequently.[13] The results indicate that the struggle to meet daily expenses and pay off loans looms large.

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. 53% of workers report thinking about their finances—indicating that, on a given day, one out of two workers is 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 such worries 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., 2020) and motivates our experimental design approach.

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

[13]Surveyors entered workers' responses in short phrases or sentences. We visualize their frequency distribution without processing, i.e. without forming unigrams or bigrams (e.g. Fellows, 2012).

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.[14] 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

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

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.[15]

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 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 power 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.[16] 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.

[15]The treatment could have a modest effect on wealth levels due to reduced interest: workers could save some interest by paying back loans up to four days early. The presence of such wealth effects, which we quantify below, does not alter the core interpretation of our design.

[16]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.

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 leaves; and (iv) have the inner center parts placed underneath the outer rings of the plates.

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;[17] (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.[18] 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).[19] 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 pieces of variation.

Piece-rate variation. In five supplementary experimental rounds, we vary piece rates for output, without the interim payment 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

[17]When a stitch is removed from the plate, it leaves 2 holes (one at each end of the stitch), indicating that a stitch was undone so that the leaf could be removed and re-positioned.

[18]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).

[19]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 created the attentiveness index.

comparing the impacts on output and attentiveness, we can examine whether both measures 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., 2018), 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.[20] 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. We use this variation to test our hypothesis that priming would more negatively affect 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.[21] We lay out our protocols for a typical round below; deviations from these protocols are documented in Appendix A.2.

[20]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.

[21]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.

Recruitment. A few days prior to the start of a new round of experiment, recruiters visited a set of new target villages and advertised the upcoming work opportunity through door-to-door visits and fliers. Potential participants were informed about the location, work tasks, duration, and their potential compensation. Workers were eligible to sign up if they were aged between 18 and 55, fluent in Odiya (the local language), worked regularly as wage laborers, and were not migrants (i.e. present in their home village for at least 3 of the past 6 months). All workers were male due to cultural 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).

Worksite setup. In a typical round, workers worked full-time at the worksite for 12 consecutive days.[22] 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.[23] 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.[24] 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.[25]

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

[23]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. 5 to 6 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 A.2).

[24]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.

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

Payment schedule implementation. When workers were recruited, they expected to receive a training payment at the end of day 1, and receive the rest of their earnings on the final day of the contract. In the morning of day 5 (the "announcement day"), workers were reminded as a group that each worker would learn his payment schedule that day, and after this, each worker was told his individual 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 (Casaburi and Macchiavello, 2019; Brune et al., 2021).[26] 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.

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

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

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.[27] 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.

Survey data. 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.[28] 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 earlier that day.

Piece-rate rounds. Implemented in February to April 2019, the supplementary rounds involved *only* piece-rate variation, i.e. none of the above treatments. Workers for these rounds were redrawn from the main experimental sample, up to a year after the main rounds were conducted. Re-hiring these workers during the lean season ensured that our estimates are representative of those for our main experimental sample. It also 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 151 workers in these extra rounds is balanced by treatment status (i.e. interim cash payment) in the main rounds.

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

[27] 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.

[28] For the same reason, we also did not collect attention measures using cognitive tests as described in Dean et al. (2018). The most effective versions of the tests are computerized (e.g. the Psychomotor Vigilance Tasks), which would have been a highly unusual event for most of the workers in our sample who were largely unfamiliar with computers.

(about Rs. 270 per day) for all three piece rates (see Appendix A.2 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.

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 (99%) and have children (90%). 75% of workers report casual daily labor as their primary source of earnings over the year, and the average worker found 9 days of paid wage work over the last month.

To compute a summary measure of baseline wealth and liquidity, we use the four binary variables at the bottom of Panel A: house quality (i.e. living in a non-mud house, constructed of durable material); owning farmland; not having resorted to obtaining food or daily goods on credit from grocers and neighbors; and being able to come up with Rs. 1,000 in an emergency. The first measure, 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. Since we have multiple proxies for wealth, we report treatment effect heterogeneity by the wealth index as a whole. In addition, in the appendix, we report heterogeneity by the house quality variable alone—since this is most likely to capture differences in underlying wealth levels across individuals.

The baseline characteristics do not statistically differ between the treatment and control groups overall (Table I, Cols. 2 and 3), indicating a successful randomization procedure.[29]

4.2 Empirical Strategy

For our primary test of treatment effects of the cash infusion, we run difference-in-differences regressions using panel data at the worker-hour level:

$$y_{irdh} = \beta(\text{Cash}\times\text{Post-Pay}_{ird}) + \gamma(\text{Cash}\times\text{Post-Announce}_{ird}) + \theta_i + \sigma_d + \delta_{rh} + X'_{irdh}\lambda + \varepsilon_{irdh} \quad (1)$$

where y_{irdh} is the outcome of worker i in round r on day d in hour h. Cash$\times$Post-Pay$_{ird}$ is an indicator that equals 1 if a worker has received the interim cash payment, corresponding to the treatment group's post-pay period in Figure II. Cash$\times$Post-Announce$_{ird}$ is an indicator that equals 1 for the treatment group during the days after the payment schedule was announced

[29]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).

until the interim payment was disbursed, and equals zero otherwise, corresponding to the announcement period for the treatment group in Figure II. Regressions control for worker (θ_i), day (σ_d), and round times hour-of-the-day (δ_{rh}) fixed effects. Finally, X'_{irdh} is a vector of supplementary controls, including whether a worker received the priming intervention that day. We show robustness to alternate specifications, including both fewer and more detailed sets of fixed effects, with the results virtually unchanged.[30]

The key coefficient of interest is β, representing the treatment effect of the interim payment on worker output. Specifically, it estimates the difference in output between the treatment and the control groups in the post-pay period, relative to their difference in the baseline period (i.e. before payment schedules were announced). 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. These effects are estimated relative to the baseline period, the omitted time category in the regressions.

We also examine treatment effect heterogeneity by baseline wealth levels, using the wealth index defined in Section 4.1. We examine effects using both the continuous index measure and a binary split (i.e. above vs. below median).

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.[31] 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, while Panel B presents estimates separately for each day. After receiv-

[30]Because waves A and B receive the payment on different days (e.g. day 8 vs day 9) within a round, we also include Post dummies in regressions to absorb level effects for completeness (since these would not be fully absorbed by the day fixed effects in the regressions). The Post-announce control is an indicator that equals 1 during the days after schedule announcement and before the wave's cash infusion (i.e. the Announcement period), and the Post-pay control is an indicator that equals 1 during the days after the wave's cash infusion. In addition, we include controls for whether the production hour allotted to the worker was shorter than the full hour (e.g. if the worker was primed or administered the endline survey during that hour). We also show robustness to 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.

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

ing 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 E). Within three days of cash receipt, treated workers increase loan payments by Rs. 276, a 293% 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. 171 in loans and credits (Panel B, Col. 1)—a 753% increase.

The cash infusion also increases household expenditures, such as food, clothing, soap, and fuel, by Rs. 157 or 42% on average (Panel A Col. 3, $p<0.001$), and by Rs. 72 or 70% 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 (26%, Col. 4), clothes (249%, Col. 5), and household essentials like soap, detergent, petrol and diesel (169%, Col. 6).[32] 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 ($p=0.090$). Summing across all expenditures, in the three days following interim payments, treatment workers spend Rs. 383 or 67% more than control workers (Col. 9, $p<0.001$). This indicates that overall, treated households spend Rs. 951 in the days following cash receipt, about two-thirds of the total interim payment. Workers go out the very same evening after receiving the cash infusion to make purchases and settle debts for their household: the majority of the total spending impact is concentrated on this first day, with an increase of 106% (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.

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 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, listing as many items as

[32]There are additional "miscellaneous" household expenditures that are included in the total amount in Col. 3. Consistent with previous findings (Evans and Popova, 2017), we do not find any evidence of changes in reported expenses on tobacco and alcohol. However, baseline expenditures in this study population are much lower than found in other parts of India (Schilbach, 2019), possibly reflecting non-priced (e.g. home-made) alcohol consumption or reporting error.

they want without surveyor guidance. In the analysis, we restrict the sample to workers who did not receive any priming during the experiment in order to avoid confounding effects on worker thoughts.[33] The sample size for these outcomes is notably smaller than for our main analyses, but still offers useful suggestive evidence.

Figure III plots the results of this open-ended exercise. Workers who received the interim payment are 14.0 percentage points (18.9%) more likely to report feeling focused on the work task (p=0.039). In addition, they are 15.8 percentage points (29.6%) less likely to say they had thought about their finances or household worries while making plates (p=0.094). 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 0.115 SDs, corresponding to a 7.1% increase in output (Col. 3, p=0.047). These effects 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.I, Col. 1).[34] Consequently, the treatment effects in Table III reflect increases in actual productivity: how quickly workers produce plates in each hour.

The productivity impacts are concentrated among poorer workers, who increase output by 0.215 SDs (13.3%) following the cash infusion (Table III, Col. 6, p=0.007). In contrast, we cannot reject that there is no impact on the remaining workers (p=0.965).[35] These results are similar if we instead examine heterogeneity using the standard proxy means test characteristic for wealth—the quality of the worker's housing stock (i.e. living in a non-mud

[33]This question is asked 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. Results are similar if we only exclude workers who were primed on the same day as this survey question was asked.

[34]Similarly, there is no scope for treatment response in hours per day as work hours are fixed. In addition, 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.I, Col. 2). 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., 2019).

[35]Appendix Table A.III shows these results are robust to a variety of alternate specifications—changing controls has almost no impact on the estimated effects.

house, constructed of durable material) (Appendix Table A.II, Col. 1).

There are two potentially complementary interpretations for the stronger impacts among poorer workers. First, these workers might have more loans and worries about finances to start with, thus increasing the scope for our treatment to reduce such concerns. 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).[36] 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.[37] 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.[38] 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. (2020) find that rural casual workers reported doing any secondary activities after a day of wage work on only 1.72% of days.

[36]Because 86% of workers report being worried about their finances at baseline, we do not have the power to look at heterogeneity by self-reported baseline worries.

[37]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.

[38]We show regression estimates in Appendix Table A.IV, Columns 1 to 3. Of course, we cannot make claims about whether treatment effects would persist over a longer time horizon.

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.

The interim payment increases workers' attentiveness (Table IV). Across all workers, we find suggestive evidence of an increase in the attentiveness index (by 0.080 to 0.087 SDs, Cols. 1 and 2, p=0.031 and 0.157, respectively) and an increase in the high attentiveness indicator (by 7.7 to 9.7 percentage points, Cols. 5 and 6, p=0.002 and 0.011, respectively). Mirroring the impacts of the cash infusion on productivity, these impacts are concentrated among poorer workers (Cols. 3-4, 7-8). Among workers with below-median wealth, receiving a cash influx increases attentiveness by 0.23 SDs (Col. 4, p=0.008). In contrast, we cannot reject no change in attentiveness among richer workers. These heterogeneity results are similar if we instead examine heterogeneity using house quality as our wealth measure (Appendix Table A.II, Cols. 2-3). Finally, again mirroring the impacts on productivity, the effects on attentiveness also persist over the remaining duration of the contract period (Appendix Table A.IV).

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. Consequently, 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.[39]

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.[40] 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.

[39]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.

[40]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.V). 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.

5.4 Piece-Rate Variation: Effort vs. Attentiveness

We see that interim payment increases workers' productivity and attentiveness. Perhaps this is happening because workers are simply more motivated; or perhaps even more extremely, whenever a worker works harder both productivity and attentiveness increase.

To understand this possibility, note that workers could change how they make leaf plates in two ways. They could increase the pace at which they work: do all the steps required to complete a leaf plate more quickly (e.g. by moving their hands faster), or move more quickly from one leaf plate to the next. Alternatively, they could be more careful—on each plate, planning and focusing, to ensure that fewer errors have to be undone. This suggests two distinct production inputs: (i) effort and (ii) attentiveness. The concern then might be that more motivated workers generally increase both effort and attentiveness. If this were the case, then we should find that other forms of motivation operate similarly.

To study this, we examine the effect of experimentally varied piece rates (see Sections 3.3 and 3.4). Recall that in these rounds, we adjusted the base wage to hold overall earnings roughly constant across days. Consequently, unlike our main cash infusion manipulation, this variation should not change workers' mental burdens. This lets us examine the degree to which motivation itself increases both productivity and attentiveness. We estimate impacts in Table V. Increasing piece rates modestly but statistically significantly increase productivity (Cols. 1-2). Each 1 rupee increase in the piece rate increases output by 0.018 SDs (p=0.026). This moderate impact is consistent with studies in other contexts, which often find modest piece-rate elasticities in real-effort experiments (DellaVigna et al., 2019). 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. 3-4). The estimates are close to zero and insignificant. These patterns are similar even when examining effects for poorer workers only (Appendix Table A.VI).

Finally, 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 0.004. This indicates the two measures do not exhibit an inherent mechanical correlation: output can change without any change in attentiveness, suggesting that the attentiveness effects of the cash infusion in Section 5.3 are not simply a byproduct of the productivity effects due to increased effort.[41] Rather, they reflect a change in *how* workers produce plates.

[41] While attentiveness and output are not mechanically correlated, we do see suggestive evidence in the observational data that the types of workers who exhibit better attentiveness tend to have higher output at baseline (Appendix Table A.V); of course, this is only a correlation. In addition, note that, as in the main experiment, these results are not confounded by changes in the extensive margin of attendance, which averaged 97% in the piece rate rounds, and therefore capture total impacts on plate production per hour (Appendix Table A.VI, Col. 5).

These results are consistent with the idea that channels such as worry are not fully in a worker's control. Consequently, a worker who is more motivated by the piece rate may not be able to simply worry less and engage in better focus, planning, or cognition. This potentially suggests a simple reduced-form model of our findings, one in which productivity depends on both effort and attentiveness. Effort is chosen and responsive to motivators like piece rates, whereas attentiveness is less under direct control. Financial strain, via mechanisms such as worry, may reduce attentiveness and therefore productivity in a way that is not fully in a worker's control. Similarly, being motivated by a higher piece rate does not allow a worker, for example, to simply worry less. While speculative, this interpretation matches the findings above. 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.

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 workers feel they have been given a gift ("gift exchange"), they might reciprocate by working harder; conversely, if they feel they have been treated unfairly, they may reciprocate by working less hard.[42] Our priors around whether pay frequency would trigger such concerns were informed by previous work in this specific setting: while there are strong fairness norms with respect to wage *levels* (which we held fixed during the experiment), there is limited evidence for strong norms around other aspects of the pay structure (e.g. Kaur, 2019; Breza et al., 2018). 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.). In addition, as discussed in Section 3.4, our protocols assign workers to "batches" to limit comparison effects across workers.

However, despite this, given the strong expenditure responses we observed from interim

[42] See, e.g., Charness and Kuhn (2011); Gneezy and List (2006); Fehr et al. (2009); Kube et al. (2012); Cohn et al. (2015); Jayaraman et al. (2016); Esteves-Sorenson (2017); DellaVigna et al. (2019); Card et al. (2012); Kube et al. (2012); Breza et al. (2018).

payments, it may of course be possible that pay frequency could evoke fairness or gift exchange 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, it is not clear that the most straightforward fairness stories predict our results: that effects are concentrated among poorer workers. The gift is the same across all treated workers, so fairness concerns might have suggested all respond to some extent. While ex post one can adjust models to explain this pattern (perhaps 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.[43]

We test for announcement effects in Table VI.[44] Columns 1 and 2 display 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).[45] Under gift exchange or fairness concerns,

[43]Recall that workers are told on day 1 that some will be paid earlier than others, and that schedules will be announced on day 5. On the morning of day 5, workers are again reminded they will be told their payment schedules that day, after which each worker is told his schedule.

[44]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.VIII, 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.

[45]We focus on these first two days because not all rounds have longer announcement periods. The an-

we would expect the announcement effect coefficients to be positive. However, they are small and statistically indistinguishable from zero, in contrast with the 0.115 SD average effect in the post-payment period (Col. 2). In Column 3, we examine heterogeneity by wealth, where the first two rows provide estimates for workers with lower wealth. Even among this subset of workers, we do not see discernible announcement effects. In contrast, recall from Table III that the estimated treatment effect of the interim cash payment on output for poorer workers is 0.22 SDs.

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 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.II. When workers arrive to work on day 9, Wave A treatment workers have already been paid, and Wave B treatment workers are going to be paid that evening (and so presumably should not have strong feelings of unfairness). If control workers felt treated unfairly and reduced effort on day 9—and this is what causes the large treatment effects we see post payment—then we should be able to detect this as a differential drop in the control group's output relative to Wave B treatment on day 9. We look for such effects in Appendix Table A.VII, 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).[46] Consequently, we see no evidence that control workers decrease effort after seeing interim payments made at the worksite.

Finally, while 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

nouncement 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).

[46]Specifically, we add the triple interaction "Cash × Payment day × Wave B" in Column 1 of Appendix Table A.VII. 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.

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, given that, in this setting, fairness norms over pay levels are stronger than norms over amenities (Kaur, 2019), we designed our study to ensure that there were no actual payment differences across workers (conditional on performance). In addition, we set the reference point so that any shocks were positive (avoiding negative reciprocity or loss aversion effects). The worksites also kept workers socially distanced to mitigate reference group effects.[47]

As Table VI indicates, we also do not find sharp payday effects (i.e. increases in output on the day that workers will be paid), in contrast to Kaur et al. (2015). Recall that in our experiment, workers' output on the day of the interim payment itself did not count toward their payment that evening—limiting the scope for present focus to produce payday effects.[48]

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

[47]In Breza et al. (2018), workers primarily compared themselves to those in the same production unit (3 workers who sat together, ate lunch and took breaks together, etc.), but did not compare themselves to others in the worksite with whom there was little direct interaction. In our setting, the salience of coworkers as a reference group is likely to lie somewhere in between these two extremes.

[48]In addition, 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.

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.VIII, 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—in Appendix Table A.VII should be positive. 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 Investment Channels: Nutrition

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.[49] In this section, we discuss whether changes in nutrition could plausibly drive our finding of overnight increases in productivity.

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.

[49]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.

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 on the impacts of increased food intake, 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.

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 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 between 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. Beyond these, there is no room for snacking since it is difficult for workers to purchase snacks outside given the rural location of the job sites and the structure of the workday (workers 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 only 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 percent) in the control group report eating breakfast, thus leaving not much room at the extensive margin, and almost everyone (94 percent) reports eating a particular rice dish that is common in the area.

Second, we would expect any impacts of blood sugar spikes due to increased breakfast consumption to wear off by the end of the work day. 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).

6.3 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; Doyen et al., 2012; Harris et al., 2013; Shanks et al., 2013). 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.IX, 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 (Cols. 5-6). Consistent with our prediction, priming has a more negative impact when workers are cash poor (before the interim payment) relative to when they are cash rich (after the interim payment), but this difference is not statistically significant.[50] We also do not observe 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. Using attention as an outcome variable, as we do in this paper, may constitute a useful design strategy for sidestepping some of these concerns.

7 Conclusion

We are only beginning to understand the psychological consequences of poverty on economic outcomes. 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

[50] Among poorer workers, priming lowers productivity by 0.114 SD when it is delivered when workers are cash-poor relative to when they are cash-rich (Post-priming × Pre-pay coefficient, Appendix Table A.IX, Col. 6, $p = 0.129$). In addition, workers who receive priming after the interim payment raise output by 0.114 SD among poorer workers (Post-priming coefficient, Col. 6, $p = 0.073$)—in line with a positive motivation effect for cash-rich workers. Specifically, 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. With productivity, however, motivation could play a large role so that the overall effect of priming is ambiguous.

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, our experiment narrowly created payment variation to test the effects of financial strain. As constructed, it does not tell us much about optimal pay frequency. Future experiments that explicitly vary payment structure are needed to understand optimal pay frequency. Our findings, though, give us extra reasons to run such experiments. The timing of payment does not just have consequences for consumption but also for productivity and earnings, raising the importance of understanding how to properly structure payment.

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. (2018) 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.

Supreet Kaur: UC Berkeley and NBER (supreet@berkeley.edu)
Sendhil Mullainathan: University of Chicago and NBER (Sendhil.Mullainathan@chicagobooth.edu)
Suanna Oh: Paris School of Economics (suanna.oh@psemail.eu)
Frank Schilbach: MIT and NBER (fschilb@mit.edu).

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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	39.19	-0.51	0.54
	[8.86]	(0.83)	
Years of education	4.69	-0.04	0.89
	[3.47]	(0.32)	
Can read newspaper in Odiya	0.63	0.02	0.66
	[0.48]	(0.05)	
Married	0.98	-0.01	0.42
	[0.13]	(0.02)	
Has any children	0.89	-0.04	0.29
	[0.31]	(0.03)	
Primarily daily laborer	0.75	-0.05	0.27
	[0.43]	(0.05)	
Days of paid work in past 7 days	1.88	-0.16	0.43
	[2.12]	(0.20)	
Days of paid work in past 30 days	8.60	0.10	0.88
	[6.31]	(0.69)	
House quality (durable house)	0.24	0.00	0.92
	[0.43]	(0.04)	
Owns farmland	0.57	0.01	0.76
	[0.50]	(0.05)	
No outstanding food loans	0.46	-0.00	0.98
	[0.50]	(0.05)	
Can get Rs. 1K in emergency	0.36	-0.04	0.45
	[0.48]	(0.05)	
Wealth index (continuous)	0.41	-0.00	0.87
	[0.25]	(0.02)	
Panel B. Financial Worries and Loans			
Very worried about finances	0.69	0.03	0.57
	[0.46]	(0.05)	
Very worried about any loan	0.41	-0.02	0.71
	[0.49]	(0.05)	
Amount of loans very worried about	6,574	96	0.95
	[12,645]	(1,412)	
Has loans	0.68	0.03	0.57
	[0.47]	(0.04)	
Has moneylender loans	0.17	-0.02	0.60
	[0.38]	(0.04)	
Panel C. Baseline Attendance and Productivity			
Attendance	0.98	0.00	0.56
	[0.15]	(0.01)	
Hourly production	-0.18	0.02	0.74
	[0.90]	(0.07)	
Attentiveness index (continuous)	-0.05	-0.00	0.99
	[0.78]	(0.05)	
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 fixed effects. For attendance, the regression is at the worker-day level with round and day fixed effects. For hourly production and attentiveness index, the regression is at the worker-hour level and with round, day, and round times hour-of-the-day fixed effects. Standard deviations are reported in brackets and robust standard errors in parentheses. All variables are either baseline characteristics or averages of participants' performance measures *before* the cash infusion treatment happens. To generate the wealth index, we take a simple average of the preceding 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. To generate the attentiveness index, we normalize each of the three measures of attentiveness (i.e. number of double holes, leaves, or stitches) using the control group's post-pay performance for each measure and take a simple average, 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	275.81***	0.40***	156.86***	70.51***	35.15**	13.38***	15.93	0.66	383.01***
	(53.01)	(0.04)	(38.53)	(23.97)	(16.57)	(4.97)	(12.26)	(4.57)	(67.09)
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	171.13***	0.17***	71.92***	50.12***	0.96	6.88**	4.64	3.07	209.08***
	(44.96)	(0.04)	(16.80)	(13.66)	(4.18)	(3.01)	(5.22)	(1.94)	(34.47)
Cash × 1 day post-pay	68.27***	0.13***	41.58*	18.64	9.62	3.76**	0.30	0.09	113.37***
	(26.18)	(0.03)	(21.35)	(15.04)	(7.01)	(1.77)	(7.31)	(1.84)	(36.81)
Cash × 2 days post-pay	40.94*	0.16***	48.76*	1.97	27.63*	3.09	12.36	-2.81	68.11
	(20.94)	(0.04)	(25.22)	(12.29)	(16.41)	(3.81)	(10.02)	(2.22)	(44.16)
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 presents the impact of the interim-pay treatment on expenditure patterns. Regressions are at the worker level, comparing average differences in expenditures in the 3 days following the cash infusion among treatment vs. control workers.

- Panel A shows the overall impacts of the treatment. “Cash” refers to whether an individual is in the interim-payment treatment group.
- Panel B shows the treatment effect on each day following the cash infusion. “Day of payment” is the day on which the interim-payment treatment group received cash at the end of work schedule, 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” 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.

All regressions use survey responses from the end of the contract period. No baseline survey is available for these outcomes. All regressions in Panel A control for round times wave fixed effects and those in Panel B control for round times wave times day fixed effects. Robust standard errors are reported.

Table III: Effects on Worker Productivity

	Hourly Production					
	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post-pay	0.102***	0.115**	0.115**	0.114**	0.253***	0.215***
	(0.037)	(0.058)	(0.058)	(0.058)	(0.092)	(0.079)
Cash × Post-pay × Higher wealth					-0.365**	-0.211*
					(0.175)	(0.117)
Announcement controls	N	Y	Y	Y	Y	Y
Priming controls	N	N	Y	Y	Y	Y
Answered baseline questions	N	N	N	Y	Y	Y
Wealth index					Continuous	Binary
P-val: cash effect + interaction						0.965
N: worker-hours	22,849	22,849	22,849	22,789	22,789	22,789

Notes: This table shows the impact of the interim-payment treatment on worker productivity.

- Cols. 1-4 presents average treatment effects across workers. The dependent variable is the normalized version of hourly number of accepted leaf plates. "Cash" refers to whether an individual is in the interim-payment treatment group. "Post-pay" indicates the period after cash infusion. The specification in Col. 3 corresponds to regression equation 1 in Section 4.2. Col. 2 estimates this equation without priming controls, and Col. 1 also removes the announcement controls, so that both the Baseline and Announcement periods are pooled as the omitted (baseline) time category. Col. 4 restricts the sample to the workers who answered the baseline survey (i.e. for whom we have data on wealth), and therefore omits 1 worker from the sample. All regressions control for worker, day, and round times hour-of-the-day fixed effects, as well as dummies for shorter production slot lengths. Announcement controls include an indicator that equals 1 on the days during the announcement period, and also its interaction with "Cash". Standard errors are clustered by worker.
- Cols. 5-6 shows heterogeneous treatment effects by wealth. Regressions correspond to the Panel A Col. 4 specification, but add interactions with a proxy for higher wealth. Column headings indicate which proxy for wealth is used in regressions. The continuous wealth index is a simple average of the 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. The binary measure is an indicator that equals 1 if the worker's wealth index is weakly greater than the median.

Table IV: Effects on Attentiveness

	Attentiveness Index				High Attentiveness			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cash × Post-pay	0.080**	0.087	0.274**	0.229***	0.077***	0.097**	0.200***	0.186***
	(0.037)	(0.061)	(0.107)	(0.086)	(0.025)	(0.038)	(0.070)	(0.055)
Cash × Post-pay × Higher wealth			-0.493**	-0.287**			-0.276**	-0.185**
			(0.226)	(0.122)			(0.139)	(0.075)
Announcement controls	N	Y	Y	Y	N	Y	Y	Y
Wealth index			Continuous	Binary			Continuous	Binary
P-val: cash effect + interaction				0.513				0.985
N: worker-hours	15,265	15,265	15,227	15,227	15,265	15,265	15,227	15,227

Notes: This table shows the impact of the interim-payment treatment on attentiveness.

- The attentiveness index is an average of three proxies for attentiveness: the average number of leaves, stitches, and double holes (each of 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.
- The dependent variable is the continuous attentiveness index in Cols. 1 through 4, and the high attentiveness indicator variable in Cols. 5 through 8. "Cash" is a binary indicator that equals 1 if an individual is in the interim-payment treatment group. "Post-pay" indicates the period after cash infusion.
- The wealth index is a simple average of four binary proxies reflecting workers' assets and liquidity (see Section 4.1 and Table III). Cols. 3 and 7 examine heterogeneity using the continuous index measure. Cols. 4 and 8 examine heterogeneity using binary indicator that equals 1 if the worker's wealth index is weakly greater than the median..
- Regressions in Cols. 2-4 and 6-8 include standard controls: worker, day, and round times hour-of-the-day fixed effects, as well as announcement controls, priming controls, and dummies for shorter production slot lengths. Announcement controls include an indicator that equals 1 on the days during the announcement period, and also its interaction with "Cash". Regressions in Cols. 1 and 5 exclude announcement controls so that both the Baseline and Announcement periods are pooled as the omitted (baseline) time category. Standard errors are clustered by worker.

Table V: Piece Rate Variation

	Hourly Production		Attentiveness Index	
	(1)	(2)	(3)	(4)
Piece rate	0.018**		-0.008	
	(0.008)		(0.009)	
Piece rate = Rs. 3		0.021		0.002
		(0.015)		(0.023)
Piece rate = Rs. 4		0.036**		-0.015
		(0.016)		(0.019)
P-val: equality of coefficients				
Piece rate in (1) and (3)	0.004			
Piece rate = Rs. 3 in (2) and (4)		0.345		
Piece rate = Rs. 4 in (2) and (4)		0.004		
N: worker-hours	4,374	4,374	4,373	4,373

Notes: This table shows the impact of changing piece-rates on worker productivity and attentiveness. The observations come from supplementary rounds (without the interim-pay treatment) with 151 workers.

- The dependent variables are normalized hourly production in Cols. 1-2 and the attentiveness index in Cols. 3-4. The attentiveness index is an average of three normalized proxies for attentiveness: the average number of leaves, stitches, and double holes (which signify that a stitch was removed in order to correct mistake) per plate during the production hour slot. 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. 2 and 4 is a piece-rate wage of Rs. 2.
- All regressions control for individual, day, and round times hour-of-the-day fixed effects, and also include indicators for shorter production slot lengths. Standard errors are clustered by worker.

Table VI: Announcement Effects

	Hourly Production		
	(1)	(2)	(3)
Cash × 1 day post announcement	-0.031	-0.028	-0.059
	(0.035)	(0.034)	(0.044)
Cash × 2 days post announcement	0.005	0.010	0.031
	(0.045)	(0.044)	(0.045)
Cash × 1 day post announcement × Higher wealth			0.053
			(0.059)
Cash × 2 days post announcement × Higher wealth			-0.044
			(0.050)
Cash × Payment day	0.069	0.073	0.072
	(0.056)	(0.056)	(0.056)
Cash × Post-pay		0.115**	0.111*
		(0.058)	(0.058)
Post-payment period	No	Yes	Yes
N: worker-hours	15207	22849	22789

Notes: This table shows productivity in the treatment and control group after announcement.

- "Cash" is an indicator that equals 1 if an individual is in the interim-payment 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.
- All regressions also include an indicator for 3+ days post announcement (which absorbs any additional announcement days for rounds with longer announcement periods, and equals 1 for days that were 3 days or more post announcement, but before the payment day) interacted with Cash. "Post-pay" is an indicator for the Post-payment period for the worker's wave (after the interim cash payments have been disbursed). "Higher wealth" is an indicator that equals 1 if the worker's wealth index is weakly greater than the median..
- Col. 1 restricts the sample to exclude the post-payment period; the remaining columns include the full sample. All regressions include worker, day, and round times hour-of-the-day fixed effects, priming controls, and dummies for shorter production slot lengths. Standard errors are clustered by worker.

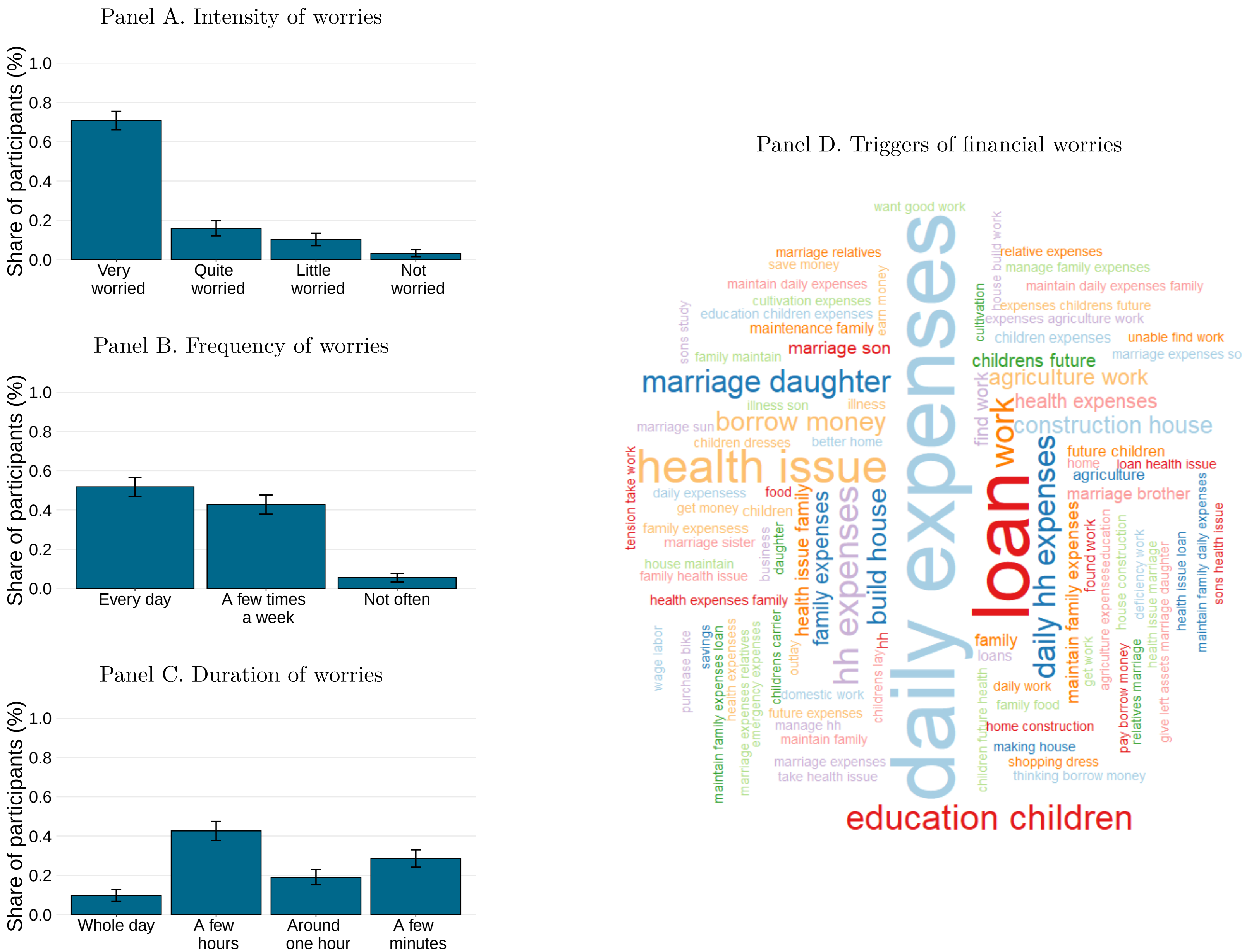
Table VII: Tests for Nutrition Channels

	Breakfast Measures					Hourly Production			
	Had any breakfast	Ate rice	Amount of rice	Ate vegetables	Ate any other item				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Cash	-0.007	-0.002	-3.889	-0.021	0.057				
	(0.013)	(0.025)	(7.154)	(0.041)	(0.043)				
Cash × Post-pay						0.109*	0.263***	0.112*	0.112*
						(0.065)	(0.092)	(0.060)	(0.058)
Cash × Post-pay × Hour of day						0.003	-0.011		
						(0.007)	(0.012)		
Cash × Post-pay × Higher wealth							-0.301**		
							(0.133)		
Cash × Post-pay × Hour of day × Higher wealth							0.023		
							(0.015)		
Cash × Post-pay × Last 2 hours of day								0.004	
								(0.024)	
Cash × Post-pay × Last 1 hour of day									0.019
									(0.032)
Control group mean	0.98	0.94	180.63	0.76	0.27				
N: workers	320	320	320	320	320				
p-value: cash effect + interaction								0.04	0.05
N: worker-hours						22,849	22,789	22,849	22,849

Notes: This table shows the impact of the interim-payment treatment on breakfast consumption following the cash infusion (Cols. 1-5) and on worker productivity changes throughout the day in the post-payment period (Cols. 6-9).

- Cols. 1-5 present worker-level regressions where the dependent variables are breakfast measures averaged across the 2 mornings following the interim cash payment day for each wave. "Cash" is an indicator that equals 1 if an individual is in the interim-payment treatment group. Regressions control for round fixed effects and an indicator for shorter rounds. Robust standard errors are reported.
- In Cols. 6-9, the dependent variable is normalized hourly output. "Post-pay" is an indicator that equals 1 on the days during the Post-payment period for the worker's wave. "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 in Cols. 6-9 include controls for worker, day, and round times hour-of-the-day fixed effects, as well as announcement controls, priming controls, and dummies for shorter production slot lengths. Standard errors are clustered by worker.

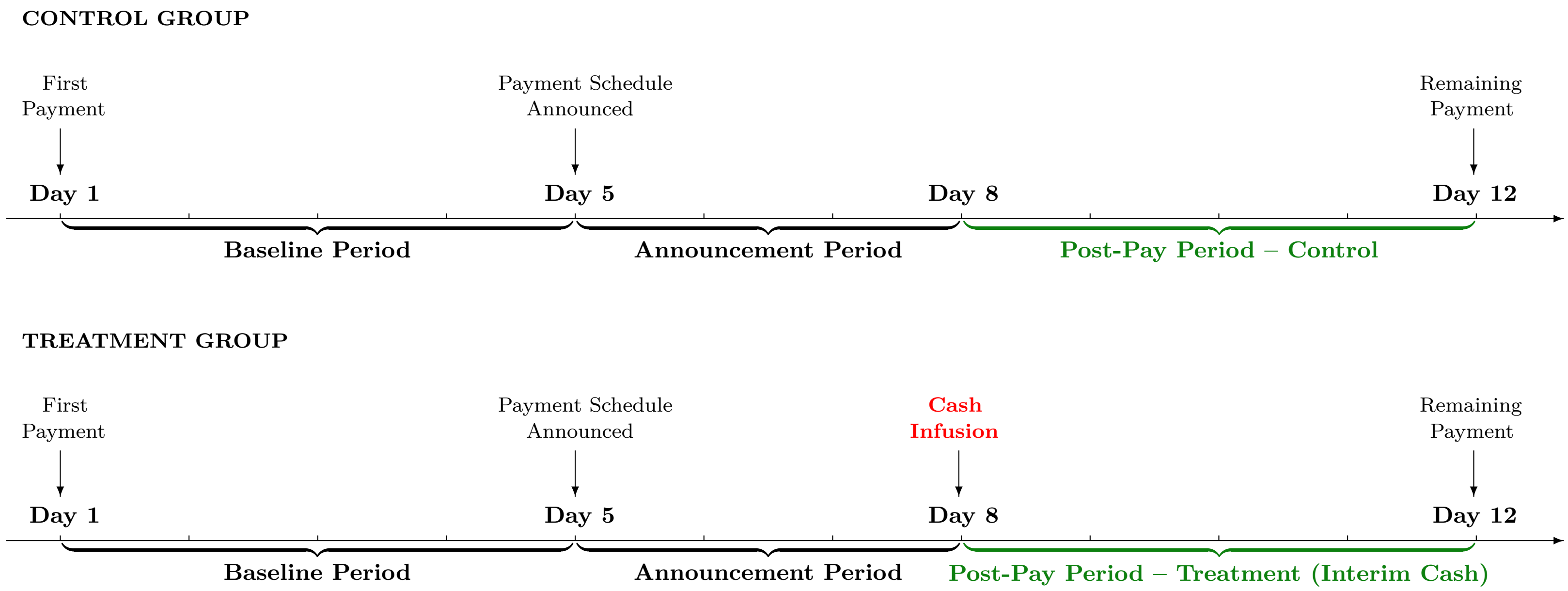
Figure I: Financial Concerns



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, so excludes those who were not present on those survey days.
- In Panel D, we show a word cloud representing answers to the question "What makes you worry about money issues?" ($N = 402$). The font size is proportional to the frequency of terms mentioned by participants.

Figure II: Experimental Design



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

- The lower part of the figure shows the (interim payment) treatment group. The upper part of the figure shows the corresponding control group.
- In the control group (upper part of the figure), 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 part of the figure), 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.II 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 shows impacts of the cash infusion 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?"
- The outcome variable for the first two bars equals 1 if the worker mentioned anything about thinking about work or the work task, and zero otherwise. The outcome variable for the last two bars equals 1 if the worker mentioned anything relating to thinking or worrying about finances. Workers could list as many items as they wanted, so that the dependent variables are not mutually exclusive.
- The orange bars show mean of each variable for the control group. The blue bars show the coefficient of a regression on the treatment indicator. All regressions control for baseline education, number of children, number of paid work days in the past month, baseline level of self-reported financial worry, and round times wave fixed effects.
- The sample is restricted to the 151 workers who did not receive the priming treatment.

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-payment period (after cash 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. See Figure II for definitions of the Baseline and Post-payment periods.
- 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 a above-median level of wealth by this index measure.
- The regression controls for shorter production slot lengths, announcement, and priming, and control for worker, day, and round times hour-of-the-day fixed effects. Announcement controls include an indicator that equals 1 during the days after schedule announcement and before cash infusion, and also its interaction with being in the interim-pay treatment group. Announcement controls are interacted with an indicator for higher wealth. Priming controls 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 on that day.
- Standard errors are clustered at the individual level. 95% confidence intervals are shown.

A Online Appendix

A.1 Appendix Figures and Tables

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

	Attendance (1)	Share of Rejections (2)	Hourly Production (3)
Cash × Post-pay	-0.012	0.009	0.118**
	(0.016)	(0.006)	(0.057)
Control group mean	0.983	0.013	0.031
Include rejections			Y
N: worker-days	4,039		
N: worker-hours		22,722	22,849

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

- In Col. 1, the dependent variable is attendance, a day-level variable indicating whether worker was present at the work site. Regressions control for worker and day fixed effects, as well as announcement controls.
- In Col. 2, 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. 3 corresponds to Col. 4 in Panel A, Table III, but the dependent variable is normalized hourly production including rejections. The total number of plates produced per hour (including rejections) is normalized using the control group's total production (mean and standard deviation) in the post-pay period.
- In Cols. 2-3, regressions include our standard set of controls: worker, day, and round times hour-of-the-day fixed effects, as well as announcement controls, priming controls, and dummies for shorter production slot lengths. Standard errors in all columns are clustered by worker.

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

	Hourly Production (1)	Attentiveness Index (2)	High Attentiveness (3)
Cash × Post-pay	0.139**	0.155**	0.142***
	(0.070)	(0.071)	(0.043)
Cash × Post-pay × House quality	-0.133	-0.333**	-0.220**
	(0.110)	(0.141)	(0.089)
P-val: cash effect + interaction	0.945	0.146	0.321
N: worker-hours	22,789	15,227	15,227

Notes: This table shows the heterogeneous impact of the interim-payment treatment on worker productivity by house quality. Regressions correspond to the Table III Col. 6 specification, but use the binary measure of house quality (i.e. living in a non-mud house, constructed of durable material). Standard errors are clustered by worker.

Table A.III: Effects on Worker Productivity: Robustness Checks

	Hourly Production					
	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post-pay	0.250***	0.205**	0.212***	0.215***	0.215***	0.239***
	(0.082)	(0.080)	(0.079)	(0.079)	(0.079)	(0.085)
Cash × Post-pay × Higher wealth	-0.259**	-0.191	-0.207*	-0.211*	-0.211*	-0.272**
	(0.131)	(0.117)	(0.117)	(0.117)	(0.117)	(0.125)
Announcement controls	Y	Y	Y	Y	Y	Y
Priming controls	N	N	N	N	Y	Y
Include absent workers	N	N	N	N	N	Y
Fixed effects included	Round	Worker	Worker, day	Worker, day, round-hour	Worker, day, round-hour	Worker, day, round-hour
N: worker-hours	22,789	22,789	22,789	22,789	22,789	23,217

Notes: This table shows robustness of the interim-payment treatment effects to alternate specifications.

- Col. 5 of this table corresponds to the exact specification in Col. 6 of Table III. The remaining regressions show robustness to alternate specifications. Standard errors are clustered by worker in all regressions.
- "Cash" refers to whether an individual is in the interim-payment treatment group. "Post-pay" indicates the period after cash infusion. "Higher wealth" is an indicator that equals 1 if the worker's wealth index is weakly greater than the median.
- In Col. 1, regressions include announcement controls, round fixed effects, and dummies for shorter production slot lengths. Regressions in Cols. 2-4 are similar to the one in Col. 1, but successively includes additional controls for worker, day, and round times hour-of-the-day fixed effects. The regression in Col. 5 additionally includes priming controls.
- The regression in Col. 6 adds in worker-days when a worker was absent, with output coded as zero for that day. The specification in this column is otherwise the same as that in Col. 5.

Table A.IV: Persistence of Effects of Interim Pay Treatment

	Hourly Production			**Attentiveness Index**			**High Attentiveness**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash × 1 day post-pay	0.130**	0.225**	0.203**	0.107*	0.286***	0.251***	0.178***
	(0.065)	(0.095)	(0.083)	(0.061)	(0.108)	(0.085)	(0.056)
Cash × 2 days post-pay	0.152**	0.276***	0.238***	0.086	0.280***	0.226***	0.220***
	(0.060)	(0.098)	(0.085)	(0.064)	(0.108)	(0.087)	(0.057)
Cash × 3+ days post-pay	0.066	0.252***	0.203**	0.023	0.203	0.163	0.099
	(0.061)	(0.097)	(0.081)	(0.083)	(0.141)	(0.114)	(0.070)
Cash × 1 day post-pay × Higher wealth		-0.250	-0.152		-0.475**	-0.293**	-0.164**
		(0.177)	(0.122)		(0.221)	(0.121)	(0.077)
Cash × 2 days post-pay × Higher wealth		-0.331*	-0.180		-0.515**	-0.282**	-0.203***
		(0.184)	(0.123)		(0.229)	(0.124)	(0.076)
Cash × 3+ days post-pay × Higher wealth		-0.481**	-0.286**		-0.458	-0.271*	-0.174*
		(0.200)	(0.121)		(0.316)	(0.156)	(0.094)
Wealth index		Continuous	Binary		Continuous	Binary	Binary
N: worker-hours	22,849	22,789	22,789	15,265	15,227	15,227	15,227

Notes: This table shows treatment effects of the interim-pay treatment separately for each day of the Post-pay period.

- The dependent variables are normalized hourly production (Cols. 1-3), the attentiveness index (Cols. 4-6), and a dummy for whether the attentiveness index is above the median (Col. 7).
- "1 day (2 or 3+ days) post-pay" is a dummy capturing whether the participant received payment 1 day ago (2 or 3+ days ago). We interact these variables with the continuous wealth index (Cols. 2 and 5) and the higher wealth binary indicator (Cols. 3, 6, and 7).
- All regressions include our standard set of controls: worker, day, and round times hour-of-the-day fixed effects, as well as announcement controls, priming controls, and dummies for shorter production slot lengths. Standard errors are clustered by worker.

Table A.V: 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	0.422***	0.190***	-0.337***	-0.728***	-0.519***
	(0.072)	(0.039)	(0.089)	(0.186)	(0.115)
N: workers	340	340	340	259	340

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	0.384***	0.236***	1.289***		
	(0.070)	(0.041)	(0.284)		
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 productivity and attentiveness using baseline (i.e. pre-announcement) observations from the main experiment sample. Data are from the longer rounds with full baseline periods, i.e. rounds 1-12. Worker-level averages are calculated using observations prior to announcement. The attentiveness index is an average of three normalized proxies for attentiveness: the average number of leaves, stitches, and double holes (which signify that a stitch was removed in order to correct mistake) per plate during the production hour slot. The scale is reversed so that a higher value on the index corresponds to improved attentiveness (i.e. fewer double holes, leaves, or stitches). High attentiveness indicates that the index value is greater than the sample median.
- Panel B shows the relationship between 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. Standard errors are clustered by worker.

Table A.VI: Piece-Rate Variation: Heterogeneity by Wealth

	Hourly Production		**Attentiveness Index**	
	(1)	(2)	(3)	(4)
Piece rate	0.017		-0.011	
	(0.013)		(0.014)	
Piece rate = Rs. 3		0.040*		0.039
		(0.022)		(0.039)
Piece rate = Rs. 4		0.035		-0.023
		(0.026)		(0.028)
Piece rate × Higher wealth	-0.000		0.005	
	(0.017)		(0.019)	
Piece rate = Rs. 3 × Higher wealth		-0.034		-0.065
		(0.029)		(0.049)
Piece rate = Rs. 4 × Higher wealth		-0.000		0.011
		(0.033)		(0.038)
Include absent workers	N	N	N	N
P-val: equality of coefficients				
Piece rate in (1) and (3)	0.056			
Piece rate = Rs. 3 in (2) and (4)		0.995		
Piece rate = Rs. 4 in (2) and (4)		0.054		
Piece rate × Higher wealth in (1) and (3)	0.772			
Piece rate = Rs. 3 × Higher wealth in (2) and (4)		0.473		
Piece rate = Rs. 4 × Higher wealth in (2) and (4)		0.765		
N: worker-hours	4,344	4,344	4,343	4,343

Notes: This table shows the impact of changing piece-rates on work outcomes. The table closely follows Table V and additionally includes interactions with the wealth index.

- The dependent variables are normalized hourly production in Cols. 1-2 and the attentiveness index in Cols. 3-4. The production and attentiveness measures are worker-hour level variables, normalized using the same control group mean and standard deviations as the measures in the main rounds.
- "Higher wealth" is a binary indicator that equals 1 if the worker's wealth index is weakly greater than the median. All regressions control for individual, day, and round times hour-of-the-day fixed effects, and also include indicators for shorter production slot lengths. Standard errors are clustered by worker.

Table A.VII: Fairness: Changes in Control Group Output after Wave-A Payment

	Hourly Production		
	(1)	(2)	(3)
Cash × Announcement period	0.006	0.008	0.007
	(0.041)	(0.041)	(0.041)
Cash × Payment day	0.099	0.099	0.119*
	(0.070)	(0.064)	(0.063)
Cash × Payment day × Wave B	-0.067	-0.041	-0.070
	(0.071)	(0.057)	(0.079)
Cash × Payment day × Higher wealth			-0.002
			(0.077)
Cash × Payment day × Wave B × Higher wealth			0.020
			(0.113)
Cash × Post-pay		0.116**	0.115**
		(0.058)	(0.058)
Post-payment period	No	Yes	Yes
N: worker-hours	15207	22849	22789

Notes: This table shows productivity in the treatment and control group on the payment day (before payment in the evening) across waves.

- "Cash" is an indicator that equals 1 if an individual is in the interim-payment treatment group. "Announcement period" is an indicator that equals 1 for all days during the announcement period (post announcement, before pay), *excluding* the payment day. "Wave B" is a dummy indicating that participants are assigned to Wave B (see Section 3.4 and Appendix Figure A.II for details). "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-payment period for the worker's wave (after the interim cash payments have been disbursed). "Higher wealth" is a binary indicator that equals 1 if the worker's wealth index is weakly greater than the median.
- Col. 1 restricts the sample to exclude the post-payment period; the remaining columns include the full sample. All regressions include worker, day, and round times hour-of-the-day fixed effects, priming controls, and dummies for shorter production slot lengths. Standard errors are clustered by worker.

Table A.VIII: 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	0.224*		0.172	
	(0.129)		(0.116)	
Cash × Post-pay × Prior rounds in worksite	-0.021	-0.018	0.046	0.018
	(0.040)	(0.040)	(0.132)	(0.130)
Cash × Announcement period	0.055	0.048	-0.009	-0.012
	(0.070)	(0.071)	(0.049)	(0.049)
Cash × Announcement period × Prior rounds in worksite	-0.025	-0.024	0.030	0.031
	(0.024)	(0.024)	(0.080)	(0.082)
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	22,849	22,849	22,849	22,849

Notes: This table tests for treatment effect heterogeneity 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).

- "Number of prior rounds" is a continuous variable describing how many prior rounds have occurred in the worksite, and is the interaction term in Cols. 1-2. "Prior round in worksite" is an indicator that equals 1 if any prior round has been conducted in the worksite, and is the interaction term in Cols. 3-4.
- "Cash" is an indicator that equals 1 if an individual is in the interim-payment treatment group. "Announcement period" is an indicator that equals 1 for all days during the announcement period (post announcement, before interim payment day for the worker's wave), excluding the payment day. "Post-pay" is an indicator for the Post-payment period for the worker's wave (after the interim cash payments have been disbursed).
- 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.
- All regressions include worker, day, and round times hour-of-the-day fixed effects, priming controls, and dummies for shorter production slot lengths. Standard errors are clustered by worker.

Table A.IX: Effects of Priming

	Hourly Production					
	First hour after priming		Two hours after priming		All hours after priming	
	(1)	(2)	(3)	(4)	(5)	(6)
Post-priming	0.056	0.016	0.060	0.079	0.078*	0.114*
	(0.055)	(0.075)	(0.059)	(0.090)	(0.045)	(0.064)
Post-priming × Pre-pay	-0.030	0.028	-0.036	-0.062	-0.055	-0.114
	(0.066)	(0.085)	(0.068)	(0.099)	(0.057)	(0.075)
Post-priming × Higher wealth		0.093		-0.045		-0.086
		(0.110)		(0.111)		(0.086)
Post-priming × Pre-pay × Higher wealth		-0.129		0.055		0.127
		(0.133)		(0.130)		(0.109)
N: worker-hours	22,849	22,789	22,849	22,789	22,849	22,789

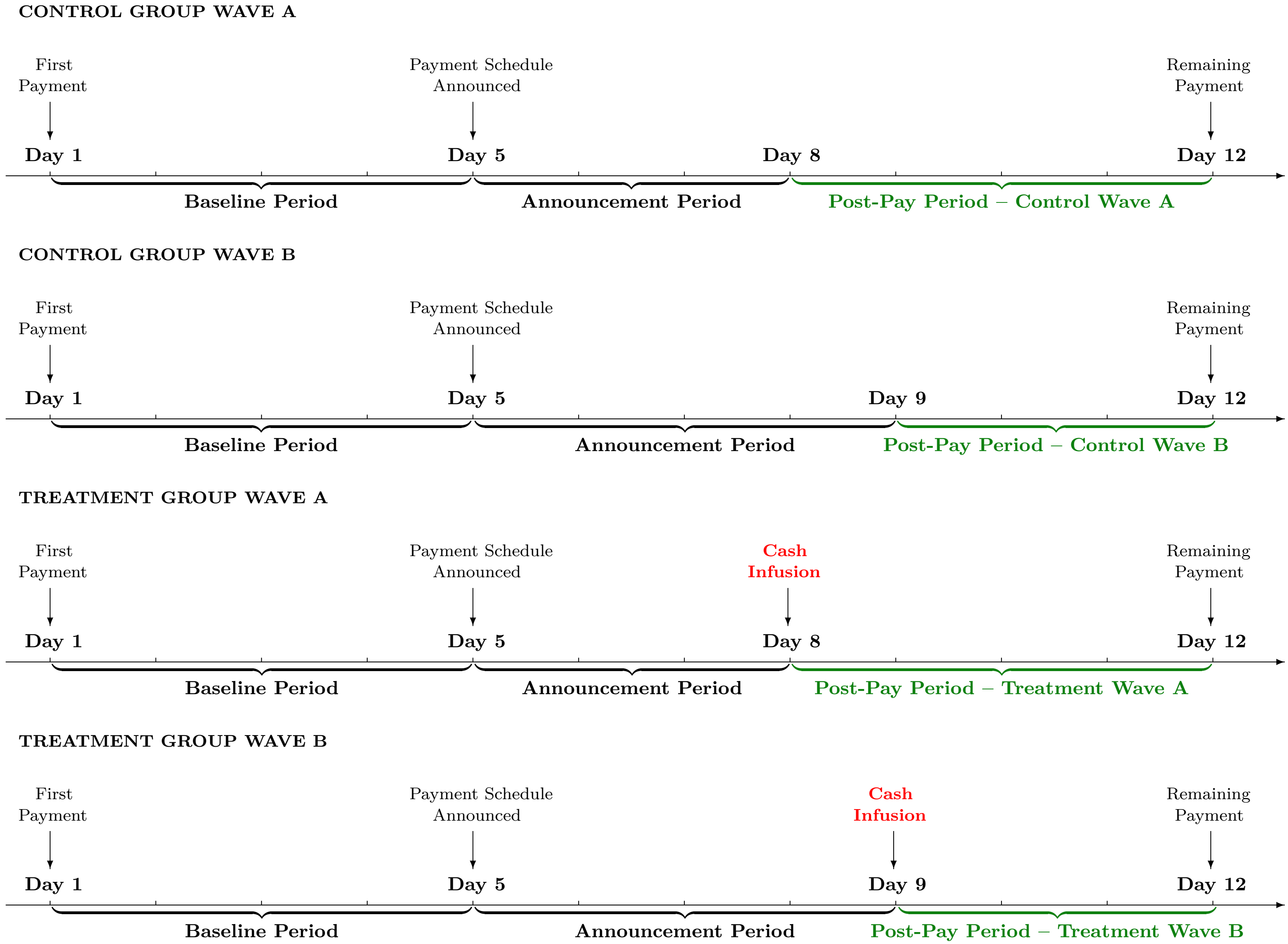
Notes: This table shows the impact of the priming intervention on worker productivity.

- "Post-priming" is an indicator that equals 1 if the individual received the priming intervention earlier that day. Column sub-headings show how many hours after the priming intervention constitute the post-priming period. "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-payment period for workers in the interim-payment treatment group, and on all days for those in the control group.
- "Higher wealth" is an indicator that equals 1 if the worker's wealth index is weakly greater than the median.
- All regressions include variables to account for the effects of the interim-payment treatment, i.e. an indicator for the post-payment period and its interaction with being in the interim-payment treatment group. Regressions also include controls for worker, day, and round times hour-of-the-day fixed effects, as well as announcement controls, priming controls, and dummies for shorter production slot lengths. 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. The backside of the plate is shown. 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 inner center parts placed underneath the outer rings of the plates.

Figure A.II: Experimental Design – Detailed Timeline



Notes: This figure is a more detailed version of Figure II. The interim payment 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 happened 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.

A.2 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.X.

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.
- 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 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.X.

Randomization weights. In rounds 1 to 3, the interim-pay group were over-weighted in the randomization to comprise nearly 70% of the sample. Starting with round 4, the sizes of the control group and the interim-pay 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.X: 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.