Do Financial Concerns Make Workers Less Productive?*

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Abstract

Mental burdens such as worry, stress, or sadness can make it harder to work productively. As a result, when a person is poorer, they will be less productive, even holding constant human or physical capital. We empirically test for this possibility using a field experiment with piece-rate manufacturing workers in India. We randomize the timing of income receipt, so that on any day some workers have more cash-on-hand than others. On cash-rich days, average worker productivity increases by 0.12 standard deviations; this effect is concentrated among workers who were poorer to begin with (0.22 standard deviation increase). On these days, workers have fewer attentional lapses during production – an effect also concentrated among poorer workers – suggesting improved cognition after earnings receipt. In contrast, mechanisms such as gift exchange, trust, and nutrition cannot explain the increased productivity. At a minimum, our results require a theory where cash-richness at a point in time directly affects worker productivity.

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1 Introduction

Earning less, almost by definition, is what makes someone poor. The causality, however, might also run in the other direction: being poor itself could perversely lower the capacity to earn. A long literature has studied various reasons why: the poor can afford less physical investments that are complementary to their earnings (such as machines or fertilizer); they have fewer means for acquiring education (schooling or training); and they are less able to invest in their own health (nutrition, bed nets, or vaccines).[1] Put simply, the poor lack the resources to make physical and human capital investments that would make them more productive.

Recent research suggests another reason why poverty might reduce productivity. Financial burdens can become mental burdens—such as through worry, stress, or sadness—and these burdens might make a person less productive at work.[2] Survey data from low-income workers in rural India offer a sense as to why one might hypothesize mental burdens could potentially affect productivity. Workers in our data are strapped for cash, which leads to a great deal of worrying (Figure 1).[3] The vast majority (85%) report being worried or very worried about their finances. Importantly, these worries appear to be top of mind at work. When asked at the end of a workday about thoughts during work that day, a striking 50% of workers mention thinking about their finances, in an open-ended question without prompts. While of course only suggestive, these data motivate our hypothesis that financial strain may impact earnings capacity.

We design our study within a context that is well-suited to studying the impact of financial strain on worker productivity. First, workers in our study area face severe financial constraints, which are particularly binding during lean months, when we conduct our experiment. Second, workers are paid piece rates for production—tying their productivity directly to their full-time earnings during our two-week experiment. Third, workers are employed in a cognitively challenging production task, where attentiveness and planning can boost output—providing scope for psychological forces to translate into productivity changes. Finally, we can precisely measure output, as well as observe proxies for attentional errors in production—enabling us to not only cleanly measure productivity effects, but also assess potential pathways through which such effects may arise.

Our experimental design focuses on manipulating immediate financial constraints rather than overall wealth. We aim to reduce mental burdens by increasing workers' available cash-

[1] A large literature provides evidence of the importance of these channels, including Miguel and Kremer (2004); Dupas (2011); Dupas and Miguel (2017).

[2] A growing literature investigates these channels, including Haushofer and Fehr (2014); Haushofer and Shapiro (2016); Mani et al. (2013); Carvalho et al. (2016); Lichand and Mani (2019); Oswald et al. (2015); Fehr et al. (2019).

[3] In our context, for example, 71% of workers have outstanding loans and report only 8.6 days of wage work in a month. Such low wage employment rates are ubiquitous in developing country settings (e.g. Muralidharan et al., 2016; Beegle and Christiaensen, 2019; Breza et al., 2020).

on-hand. Specifically, we experimentally vary the timing of income receipt, so that some workers receive their earnings to date sooner than other workers. As a result, on a subset of days during our two-week contract period, some workers have more cash-on-hand than others. These early payments were sizable, equivalent to almost one month's worth of typical labor earnings in the lean season, and thus meaningfully eased financial constraints. For example, workers were 40 percentage points (225%) more likely to pay back loans to moneylenders and other debtors within 3 days of early cash receipt. Consequently, this variation boosts the amount of money in workers' pockets and reduces debt overhang, while holding piece rates and overall wealth constant. The timing of our design means that we see the immediate effects of changing cash-on-hand; this enables us to distinguish psychological channels from those examined in the traditional literature—where improvements to worker productivity arise because money enables workers to invest in productive human or physical capital. In addition, through design features such as the timing of when payment schedules were announced, we can examine other confounding explanations such as gift exchange. We implement this design with 408 workers in Odisha, India.

Easing workers' cash constraints made them more productive. Starting the day after the early payment, worker productivity increased by 0.12 standard deviations relative to the control group on average. These productivity increases were sustained throughout the work day and for the remaining days of the treatment period. The effects were concentrated among poorer workers, as measured by an index of wealth measures such as land ownership and housing quality. Early payment increased worker productivity for poorer workers by about 0.22 standard deviations. These impacts are remarkable given the relatively minor experimental variation in payment schedules and the low wage elasticity of productivity in real-effort settings such as our worksite (DellaVigna et al., 2019).

By examining *how* workers produce, we can provide a window into what is happening inside the worker's mind. Making a leaf plate is not simply a rote activity: care and attention can be used to improve productivity. Leaves must be stitched to form a circle with no holes or gaps, yet each individual leaf is uniquely sized. Workers could mindlessly use plenty of leaves and stitches and undo any errors that arise by removing stitches, but that increases the time per plate. Care and attention can reduce time per plate and increase earnings. The leaf plates contain traces of such attentiveness—number of leaves, stitches, pairs of holes from removed stitches—which we measured unbeknownst to workers. By examining the impact of early payment on these "attentional lapses", we can see if workers' mental approach to work changed. Indeed, the early payment reduced attentional lapses by 0.09 standard deviations. Analogous to the overall productivity effect, poorer workers showed a bigger effect, 0.26 standard deviations.

What we view as two findings—increased productivity and reduced attentional lapses—may be only one if whenever workers were more productive they were also more focused.

To test for this possibility, we experimentally varied the piece rate for a subset of workers. Changing piece rate from Rs. 2 to Rs. 3 and 4 increased productivity by 0.021 and 0.036 standard deviations, respectively. However, the piece-rate increases produced no change in attentional lapses. These facts together suggest that the impact of cash-on-hand is mediated, at least in part, through a psychological mechanism not fully under control of workers. We do not, however, single out any of the particular mechanisms that interfere with attentiveness at work – including worries, stress, or sadness.

Our experiment also allows us to rule out two sets of alternative explanations for the impact of early payment on productivity, as discussed in detail in Section 4. First, a set of confounds concerns altered beliefs or perceptions toward the employer, such as reciprocity and trust, are unlikely to explain the observed effects. For example, workers were informed of their payment schedules several days before the early cash payments; this in itself had no impact on productivity—inconsistent with basic gift exchange or fairness models.[4] Second, we address the possibility of productivity-enhancing investments by workers due to the early payment. Through the design of the experiment, we can rule out investments in physical capital and other longer-run investments such as training. Inconsistent with a nutrition channel, the treatment effects last throughout the work day and we find no impacts of the treatment on caloric intake based on detailed measures of breakfast consumption.

Finally, motivated by the prior psychological literature (Mani et al., 2013), we cross-randomized a priming intervention: some workers were asked in the morning of one workday to recount their outstanding loans and think about how they would come up with a large sum in an emergency. Our (pre-registered) hypothesis was that the prime would more negatively impact productivity for workers who were randomized to be cash-poor when receiving the prime. We find suggestive but not statistically significant evidence of such a differential effect. However, the main effect of the prime intriguingly is positive, which could have various explanations.[5] For example, the effects appear concentrated on those with outstanding loans, suggesting we may have primed loans rather than poverty as a whole; and similarly, the wording of the prime may itself have served as an encouragement to work harder. Combined with the previous findings, these results suggest that rather than using attention as a treatment (through primes) to uncover mechanisms, it might be more effective to use attention as an outcome.

We view the primary contribution of our paper as establishing a direct relationship between cash-on-hand and worker productivity, adding a psychological channel to traditional

[4]This result suggests that the experience of receiving cash itself was crucial for causing our observed productivity effects, rather than changes in expected future payment streams, consistent with findings by Mani et al. (2013).

[5]The evidence of increased productivity of priming may relate to recent evidence that increased focus on money due to scarcity also produces some benefits: Shah et al. (2015) show that the poor are less affected by framing effects; and Fehr et al. (2019) find that scarcity reduces the endowment effect.

theories of how income could affect productivity and earnings (e.g. Dasgupta and Ray, 1986). Our results are consistent with findings that anti-poverty programs can boost earnings (e.g. Banerjee et al., 2015; Bandiera et al., 2017). While such studies by design involve traditional physical and human capital channels, our study is constructed to eliminate the relevance of these channels to isolate psychological channels. Our findings suggest that psychological pathways could also have the potential to contribute to the impacts of anti-poverty programs. This evidence is consistent with work showing that transfer programs affect psychological outcomes such as stress or mental health (Haushofer and Shapiro, 2016; Ridley et al., 2020). Our results provide a proof of concept and an impetus to explore these possibilities more broadly in future work.

Our study contributes to the nascent but growing literature on the psychology of poverty. Existing work has examined a number of channels through which poverty might impact earnings and decision-making, such as stress (Chemin et al., 2013; Haushofer and Fehr, 2014) or cognitive function (Mullainathan and Shafir, 2013). This work has largely focused on examining this relationship using laboratory task measures of cognition, decision-making, and preferences (Mani et al., 2013; Carvalho et al., 2016; Ong et al., 2019; Fehr et al., 2019; Bartos et al., 2018).[6] Complementary to Banerjee et al. (2020), our findings lend credence to the view that psychological mechanisms have the potential to affect economic field behaviors. Further, we provide some positive evidence of one underlying channel, attention.

The evidence in this paper is also relevant for the literature on poverty traps. Existing work has largely focused on neoclassical channels such as capital market imperfections (Kraay and McKenzie, 2014; Ghatak, 2015; Balboni et al., 2020). Our evidence suggests that psychological factors, specifically, attention could play a role in creating poverty traps, as hypothesized in Banerjee and Mullainathan (2008).

Finally, our evidence speaks to the debate on cash transfers to the poor. Recent work in developing countries shows evidence against the hypothesis that cash transfers or other welfare programs lead the poor to reduce labor supply (Alzua et al., 2013; Banerjee et al., 2017; Bosch and Schady, 2019).[7] Closest to our work, Banerjee et al. (2020) find that cash transfers can even increase recipients' willingness to supply labor. Our evidence suggests an additional effect. Not only might cash transfers, social safety programs, and other policies not have negative labor supply effects. If such programs reduce financial strain among the poor, they might even improve productivity.

The remaining parts of this paper are organized as follows. Section 2 provides background information on the study and its experimental design. Section 3 discusses the impact of early payments on workers' expenditure patterns, their work performance, and attentional errors.

[6] See Kremer et al. (2019) for review.

[7] In contrast, evidence from the United States and other developed countries shows that large reductions in labor supply in response to increased eligibility for and/or generosity of welfare programs. See, for instance, Hoynes (1997); Moffitt (2002).

Section 4 discusses potential confounds and additional evidence from the priming intervention. Section 5 concludes.

2 Experimental Design

2.1 Approximating the Ideal Experiment

The goal of our experiment is to test for real-world productivity impacts of mental burdens caused by financial strain. An ideal experiment to test this hypothesis includes the following features. First, it involves a task that allows for precise measurement of worker output, so as to be able to incentivize workers to be productive and to measure the causal impacts of different interventions in a real work environment. Second, it features a cognitively demanding task, thus providing scope for psychological mechanisms to influence worker productivity. Third, to focus on potential impacts on workers' capacity to be productive, the ideal design holds work hours constant. Fourth, it creates exogenous variation in financial strain while keeping constant overall wealth and worker incentives. Fifth, to isolate psychological channels, the ideal design shuts down any impacts of potential investments in physical or human capital, and/or tests for the empirical relevance of such channels. Finally, the ideal design has the potential to provide evidence of psychological mechanisms that mediate the effect of financial strain on worker productivity.

Using the infrastructure developed by Breza, Kaur and Shamdasani (2018), we implement a randomized field experiment in rural Odisha, India, with the goal of closely matching the ideal design. First, in our experiment, piece-rate workers completed a small-scale manufacturing task for which we collected precise measures of hourly productivity for two weeks. Second, we chose a production task—assembling leaf plates—that is relatively cognitively demanding yet representative of small-scale manufacturing tasks completed by low-income workers in rural India. Third, to isolate productivity effects from potential labor supply responses, we offered an attendance bonus to workers which ensures close to full attendance on the key days of interest. Fourth, since ethical considerations prohibit experimental increases in financial strain, we instead reduced financial strain among some workers who were severely financially strained. To enable us to meaningfully reduce financial strain, we hired workers in the lean season, at a time when the majority of them faced tight financial constraints and were burdened by significant worries about their finances. Fifth, by design of our experiment, physical or human capital investments such as buying machines or receiving training were not relevant in our setting. Moreover, we explicitly test for the remaining investment channel of increased nutritional intake by carefully tracking food consumption prior to work. Finally, we collected markers of workers' attentional lapses during work, as described in detail below. This approach maintains a natural work environment and complements other experimental

designs that produce more data but could create experimental artifacts in attention, such as detailed survey tasks, measures of cognitive function, or physical samples such as saliva tests.

To induce experimental variation in financial strain, we induced anticipated payday variation—rather than randomizing unconditional cash transfers across workers—because it features several advantages. First, it is more realistic than randomized cash drops in this setting since unconditional cash transfers are less common in our study area than in sub-Saharan Africa or other parts of the world. Second, anticipated payday variation is commonplace in developing and developed countries, thus boosting the external validity of our study (Shapiro, 2005; Kaminski et al., 2014). Third, the payday variation holds workers' wealth (approximately) constant, thus limiting the potential for effort or labor supply responses due to wealth or income effects.[8]

However, the current design also entails some drawbacks. First, the empirical test comparing workers across treatment groups around the early payday is only powerful if the psychological effects (primarily) ensue once workers actually received their payments, as opposed to when they only anticipate receipt of payment. Second, since the employer (our staff) informed workers of and implemented the payment variation across workers, the Early-Pay Treatment features an ancillary component of potential changes in workers' relationship with their employer, including gift giving, fairness, and trust in the employer paying workers as promised. As a result we must address these issues as potential confounders, which we do in Section 4.

2.2 Recruitment, Sample, and Ethical Considerations

Our key experimental variation is to alleviate workers' financial constraints by paying some workers earlier than others. For this intervention to effectively reduce financial strain, we required a sample that already faced significant financial burdens. We thus recruited our sample during the lean season (between March and June of 2017 and 2018), when the majority of workers were expected to experience tight financial circumstances and worries about their finances.

Recruitment. We recruited our study subjects from rural villages in Odisha, where a large number of villagers are engaged in daily wage labor. The study focused exclusively on male workers since it is more culturally appropriate for them to take jobs outside of their village for an extended period of time. A few days prior to the start of a new round of experiment, recruiters visited the target villages and advertised the upcoming work opportunity through door-to-door visits and fliers. Potential study participants were informed about the

[8]There are two reasons for (expected) wealth differences across treatment groups. First, workers who were paid early might have saved some interest by paying back loans or credits following the early payment. Second, productivity differences due to the early payment translate into differences in worker pay by the end of the experiment.

location, the tasks that they would be asked to do, the duration of the study, and their potential compensation.[9] Since usually the number of interested villagers exceeded the worksite capacity per experimental round, we randomly selected approximately 30 participants from the sign-up list to be invited to participate in the study. In addition, 5 back-up participants were selected to replace any participants who dropped out of the study during the first three days of a round (i.e. before treatment assignment).

Eligibility. The day before the experimental round, recruiters revisited the villages so that the interested villagers could sign up to participate in the study. During the sign-up process, recruiters used a number of screening questions to determine eligibility. Male workers meeting the following criteria were eligible to participate in the study: (i) aged between 18 and 55, (ii) fluent in Odiya (the local language), (iii) regularly working as daily wage laborers, (iv) having been present in their home villages for more than half of the time during the preceding 6 months, and (v) no prior experience producing leaf plates. In addition, the recruiters verified that potential participants were willing to come to work for the entire duration of the round.

Sample. The main experiment sample comprises of 408 male workers from 14 experimental rounds with about 30 workers each.[10] Tables A1 and A2 provide baseline characteristics of our sample. A typical worker in our sample was about 40 years old, had 4 to 5 years of education, and was primarily employed in daily wage labor. Daily attendance was high at 97.7 percent during the baseline period, and 98.3 percent post announcement, resulting in a main production dataset of 4,039 worker-days and 3,963 non-absent worker-days, with 5 to 7 hourly output measures per day.

Ethical Considerations. Our experiment was reviewed and approved by the MIT and Columbia IRB Committees and the IFMR Human Subjects Committee (the local IRB in India). We obtained informed consent from all participants prior to joining the study. Each participant was free to stop their participation at any time, and/or to not answer any of the survey questions at no penalty. The experiment itself offered a valuable opportunity—two weeks of paid work—during the lean season when jobs were scarce. The control group was paid at the end of their work period, which was typical for seasonal contract jobs in the study area. We believe that the two treatments constitute minor changes compared to the experience of the control group. First, relative to the Control Group, the Early-Pay Treatment arguably improves workers' financial situation by paying them early and thus reducing their financial strain. Second, closely following previous work (Mani et al., 2013), the priming treatment

[9] Workers were also provided with the contact information of the recruiting staff for any questions.

[10] This number excludes 21 participants who dropped out in the first four days before the payment schedules were announced.

induces thoughts about finances. Importantly, this treatment is not designed to create new thoughts but rather bring existing worries top of mind.

2.3 Measuring Worker Productivity and Attentiveness

As part of the field experiment, we employed 408 male workers over the course of 14 rounds with about 26 to 30 workers each. In the majority of the rounds, workers worked for 12 consecutive days and 5 working hours per day.[11] Such seasonal contract jobs are common during the agricultural lean season. Since the jobs at the study worksites were the primary source of earnings for workers and a regular job from their perspective, this setting provides an ideal environment to study the effects of financial strain in real-world environment with high stakes.

Work task. Workers produced disposable plates by stitching together sal tree leaves, as depicted in Figure 2. This task is relatively cognitively demanding as it requires considerable attention to stitch together the leaves in a way that satisfies the required quality standards set by partnering contractors whom we sold the output to. To qualify for payment, a leaf plate was required to (i) meet a minimum size requirement, (ii) have no gaping holes, (iii) have all leaf stalks covered by other leaves, and (iv) have the inner center parts placed underneath the outer rings of the plates. Workers were paid a flat base wage for attendance plus a piece rate per completed leaf plate that satisfied the quality standards, as described in detail below.

Output measures. We collected hourly measures of output. At the end of each work hour, staff collected completed leaf plates from each worker. Workers were allowed to continue and complete unfinished leaf plates in the subsequent hour. The main measure of output in our study is the number of completed leaf plates, which can be further divided into rejected and accepted output. 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. Our primary empirical analysis uses accepted leaf plates. Given the high acceptance rates, using the completed number of leaf plates yields nearly identical results.[12] In addition, for a subset of days around the early payday, we collected the three measures of attentiveness described next.

Attentional errors. Making leaf plates is not only physically straining, the combination of repetition and need for carefulness also make it cognitively demanding. Each plate must be round, of a certain size, leaves must overlap sufficiently along all internal edges so that there are no holes or gaps, and there must be enough stitches so that the plate can hold

[11] Nine rounds had the standard schedule. The other rounds were shorter in length or had different daily work schedules. These changes are described in detail in Appendix A.2.

[12] See Appendix Table A4. We find no significant impact of the early payment on the fraction of plates that were rejected (column 3).

food without breaking. Because leaves come in irregular shapes and sizes, accomplishing this requires attentiveness and planning. Increased effort can undo poor planning: workers can simply use plenty of leaves (so there are no gaps), plenty of stitches (so the plate will not break), and undo errors by removing stitches in order to re-arrange leaves (in case a gap arises or the shape becomes oblong). However, each of these margins takes time, slowing down the amount of time per plate. In contrast, through cognitive planning and vigilance, workers can increase their production speed and earnings by strategically placing leaves—cutting down on the number of leaves or stitches needed—and by avoiding errors in placement so that mistakes do not have to be undone.

Measuring attentiveness. To examine whether any potential productivity effects were mediated through changes in attentiveness, we collected three unincentivized markers of attentiveness for each plate: (a) the number of leaves, (b) the number of stitches, and (c) the number of "double holes"—the telltale sign that a worker had to undo a stitch from a plate after discovering a mistake. We collected these three measures for a subset of hours in each experimental round, without workers being aware that these dimensions were being measured. We normalize these measures and combine them into an "Attentiveness Index", where higher attentiveness corresponds to fewer attentional errors. We also create an indicator of "High Attentiveness", defined as having an index value greater than the median.

2.4 Financial Constraints and Worries at Baseline

Our key experimental variation is to alleviate workers' financial constraints by paying some workers earlier than others. For this intervention to effectively reduce financial constraints, we required a sample that was financially constrained to start with. We thus recruited our sample during the lean season (between March and June of 2017 and 2018), when the majority of workers faced tight financial constraints and were burdened by significant worries about their finances.

Financial strain. The majority of workers in our sample were severely financially constrained (Appendix Table A1). Fewer than 60 percent of workers own farmland, and over 70 percent of workers live in houses that contain mud structures, indicating low wealth. As to be expected during the lean season, workers reported low numbers of days worked. On average, workers reported working fewer than 2 of the last 7 days and fewer than 9 days of the last 30 days prior to joining the study. Unsurprising given the limited available income sources, workers also report severe financial constraints. Over 50 percent have outstanding credits for food and basic household consumption, and over 60 percent report having difficulties coming

up with Rs. 1,000 (about US $15) in case of an emergency.[13] 71 percent of workers reported outstanding loans, including 17 percent of workers indicating loans from moneylenders charging high interest rates, suggesting their lack of access to other sources of credit.

Worries. Consistent with tight financial constraints and significant debt, workers report high levels of worries about their finances (Figure 1). When asked how concerned they were about their (future) finances, 86 percent of workers indicated any worries and 70 percent reported being very worried about their finances. The majority of workers worries at least a few times a week, and about half of them worry every day. When worries arrive, they linger in workers' minds for at least several hours for the majority of the sample. Worries are triggered by a range of financial issues, including daily expenses, health issues, children's education and marriage, as well as loans.

Heterogeneity. While all workers in our sample are fairly poor by absolute standards, there is considerable heterogeneity in workers' wealth and financial well-being. To capture this heterogeneity, we create two measures of wealth. First, we create a wealth index, which is the simple average of four binary measures: whether the individual reported owning farmland, living in a non-mud house, not having outstanding credits, and being able to come up with Rs. 1,000 easily in case of an emergency. Second, we define workers with high wealth as those whose wealth index is greater than or equal to the median.

2.5 Inducing Variation in Financial Constraints

To create and examine an exogenously-induced reduction in financial constraints, we varied the timing of earnings payout across workers while holding constant piece rates and flat base payments. This experimental variation thus altered workers' short-run financial constraints while holding their overall wealth approximately constant.

Common features across workers. In the main experimental rounds, we employed each worker for 12 days at their worksite from 9 am to 2 pm.[14] All payments occurred at the end of workdays. Workers were informed of their output for each day throughout the experiment, limiting any uncertainty about the outstanding payment amount. At the end of day 1, all workers were paid out a flat wage of Rs. 250 (about US $4) as a training wage, with the goal to foster trust in the worksite among workers.[15] For the remaining days, workers

[13] Among those with outstanding credits, 84 percent has credits with shopkeepers. The remaining workers have credits with neighbors, employers, etc.

[14] Four rounds were shorter in duration, with two rounds of 11 days and two rounds of 6 days. The first three rounds involved 9 hours of work (9 am to 5 pm or 8 am to 4 pm) with one hour lunch break in the middle. The schedule and details for each round are described in Appendix A.2.

[15] While larger or additional early payments would have been desirable to foster further trust, they would

were paid a base wage of Rs. 200 and a piece-rate wage of Rs. 3 per plate.[16] Since cash-on-hand may impact the type of jobs workers engage in, or the number of days or hours worked (Banerjee et al., 2020; Balboni et al., 2020), workers were given a completion bonus (Rs. 300) if they attended all of days 6 through 11 (paid out at the end of the study). This completion bonus effectively shuts down any potential labor supply responses to the treatment.[17]

Payment schedule variation. The key experimental variation of our experiment is paying some workers earlier than others, as shown in Figure 3. On day 1, we informed all workers that everyone would be paid for their work by day 12, but the payment schedules would differ across workers, with some workers receiving part of the payments earlier than others. In the morning of day 5, workers were given full information about their individual payment schedules. About half of the workers in each round were randomly assigned to the Control Group and were told that they would be paid on day 12 (as promised on day 1). Moreover, they were informed that some workers at their worksite would be paid on an earlier day.[18]

In contrast, workers in the Early-Pay Group were told in the morning of day 5 that they would be paid on day 8 of the experiment for their wages earned by the *previous* day and the remaining amount on day 12. That is, workers in the Early-Pay Group received payments at the end of day 8 for the work from days 2 to 7. Importantly, workers' output during the day of the early payment itself did not affect how much they were paid on that day. This feature of the study was designed to limit payday effects driven by present focus as found in (Kaur et al., 2015). While payments were made in private at the end of each day, all workers were aware of payments when they occurred at their worksite.

We induced additional variation in payments *within* the Early-Pay Group by randomizing half of these workers in each round to have a delayed schedule. We use this variation to examine alternative explanations for the impact of the Early-Pay Treatment, as described in more detail in Section 4. These workers—whom we refer to as Early-Pay Group II and Control Group II—had the same schedules as the other groups until after the announcement on day 5, but had the rest of the schedules delayed by one day. That is, they received priming treatments on day 7 or 11 and received the early payment on day 9, if eligible. For the majority of analysis, we do not differentiate these groups and simply treat them as being in

have eased financial constraints among all workers, thus limiting the potential for the experimental variation to create meaningful differences in financial constraints.

[16]Since workers produced about 21 leaf plates per day, the incentive payment comprised about 20 percent of the overall payment. Any deviations from these wage rates are discussed in Appendix A.2.

[17]Indeed, attendance in the Early-Pay Group is not statistically different compared to the Control Group (Appendix Table A4 Column 4).

[18]In rounds 1 to 3, the Early-Pay Group were over-weighed in the randomization to comprise nearly 70% of the sample. Starting with round 4, the sizes of the Control Group and the Early-Pay Group were approximately equal. The schedule announcement was made on different days in three of the rounds that are shorter in length, as also described in Appendix A.2.

the Early-Pay Group or the Control Group.

Priming. Following the previous literature (Mani et al., 2013), we also implemented a priming intervention intended to direct workers' attention to their finances. During this intervention, surveyors told workers about another (fictional) worker's financial strain and then asked them individually about their own finances and loans. This discussion lasted about 30 minutes and took place during the first hour of work. Before returning back to work, again following Mani et al. (2013), workers were asked if they were to cover an unexpected large expense, how they would raise the money. Workers were asked to think carefully about this question so that their answers could be discussed at the end of the day with the same surveyors.

Some workers received the priming treatment on day 6 of the study, others on day 10 of the study, and others not at all.[19] This variation was intended to test the (pre-registered) 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), while also allowing to estimate the overall effect of priming by comparing all primed workers to those who were not primed at all.

Balance checks. The baseline characteristics do not statistically differ between the Early-Pay Group and the Control Group (Appendix Table A1 and A2). Comparing worker characteristics between the two groups, we find no statistically significant differences, indicating a successful randomization procedure. We cannot reject a joint orthogonality test between the two treatment groups (p=0.60). Similarly, we find little evidence of baseline imbalances for the priming treatment.

2.6 Supplementary Survey and Production Data

We collected two other pieces of data to complement the leaf plate production data described above.

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.[20] All study participants completed a short baseline survey that included basic demographics

[19] Conditional on Early-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.

[20] 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 the workers in our sample who were largely unfamiliar with computers.

such as age, education, and measures of income and wealth, and information about outstanding loans and financial worries. We use these variables to establish baseline balance across treatment groups and to consider heterogeneous treatment effects. Conducted on the last day of the study, the endline survey collected information about (i) recent major expenses and financial worries, (ii) participants' expenditure patterns over the last 3 to 4 days, (iii) participants' breakfast items and amounts consumed over the last 3 to 4 days.[21] Finally, we conducted a short survey on day 10 asking workers about what they thought about while working earlier that day.

Piece-rate variation. We supplement our main production rounds with five additional short rounds that do not include randomization of the above treatment conditions but instead involve a random variation in piece rates across days. We use the piece-rate variation (i) to estimate the piece rate elasticity of output to benchmark the impacts of the above treatments, (ii) to investigate whether there is a mechanical relationship between attentional errors and productivity, and (iii) to examine how attentional errors and productivity are correlated with a measure of cognitive function. These supplementary rounds, implemented in March and April 2019, involved a total of 151 workers who each worked for seven days. These workers were redrawn from the main experimental sample, one year after the main rounds were conducted.

Workers were hired for seven days with piece rates changing across the last six 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, or 4 in a randomized order, with each rate lasting for two consecutive days. The base wage was adjusted so that average daily earnings would be approximately similar (about Rs. 270 per day) for all three piece rates.[22] In addition, mirroring the main experimental rounds, workers received an attendance bonus of Rs. 200 if they attended all days. Finally, on their last day of these rounds, workers completed an incentivized memory test on computers (Corsi Span Test, see a detailed description in Dean (2018)), which provides a measure of cognitive function that we can correlate with the measures of productivity and attentiveness.

[21] Respondents recalled their expenditures and breakfast consumption over the previous four days in rounds 4 to 12, and over the previous three days in the remaining rounds. In round 1, we conducted the endline and expenditure surveys on the last day with the Early-Pay Group II and the Control Group II, and on the second-to-last day with the rest. In the remaining rounds, the surveys were conducted on the last day with everyone. Participants recalled breakfast consumption only in rounds 4 to 14.

[22] The detailed payment schedule is described in Appendix A.2.

3 The Impacts of Early Payment

3.1 Empirical Framework

We estimate the treatment effects of the early payment in two main ways. First, we compare **worker-level outcomes** such as expenditures during the experimental period between the Control Group and the Early-Pay Group. We estimate the regression

$$y_i = \beta \text{Cash}_i + \gamma X_i + \varepsilon_i, \tag{1}$$

where Cash_i is a dummy capturing whether participant i was assigned to the Early-Pay Group. X_i is a vector of controls including a dummy for having a shorter experimental period and round indicators. The coefficient of interest is β, which captures the average treatment effect (ATE) of the Early-Pay Treatment on worker-level outcomes.

Second, we examine how **worker-hour-level outcomes** such as production and attentiveness change in the experimental period with respect to the baseline period, depending on the Early-Pay Treatment.[23] For these variables we estimate the ATE of being paid early, β, with the regression

$$y_{its} = \beta \text{Cash}_i \cdot \text{Post}_{it} + \gamma X_{its} + \varepsilon_{its}, \tag{2}$$

where y_{its} is worker i's outcome on day t in production hour s. Post_{it} (or Post-payment_{it}) is a dummy corresponding to the experimental period shown in Figure 3, i.e. days after the Early-Pay Group receives their lump-sum payment.[24] X_{its} is a vector of controls. The standard set of controls account for the effects of announcement, production disruption, priming intervention, and shorter slot lengths.[25] Regressions also control for worker, day, and round times hour of the day fixed effects.[26] β captures the outcome difference between the Early-Pay Group and the Control Group after the early payment, minus the difference between the two groups *before* the announcement of the payment schedule.

Baseline wealth is an important source of treatment effect heterogeneity, so we also estimate models in which we interact the main treatment variables with wealth measures from

[23] Most production slots lasted one hour, but a few lasted, by design, 20 or 30 minutes. Some outcomes such as production are multiplied by 2 or 3 to adjust for the shorter slot lengths.

[24] The experimental period is day 9-12 for those with the standard schedule. It is day 10-11 for those with a delayed schedule (Control Group II and Early-Pay Group II), and is defined similarly for those with shorter rounds.

[25] Announcement controls include a dummy for the announcement period and its interactions with Cash_i. Disruption controls include a dummy for any production slot on the last day that was interrupted (e.g. due to the endline survey) and its interaction with Cash_i. 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. Controls for shorter slot lengths include dummies for 20-minute slots and 30-minute slots.

[26] Day here refers to day within the experiment, not calendar date. Hour of the day refers to actual time in the day.

the baseline survey. As discussed above, we define "Wealth index$_i$" as an average of four binary measures of wealth, and "High wealth$_i$" as having wealth index values greater than or equal to the median. In the regressions that explore wealth heterogeneity, we also interact the standard control variables with the relevant wealth measure.

3.2 Expenditures

The early payments provided meaningful amounts of liquidity to workers. These payments comprised almost one month's typical wages during the lean season (over Rs. 1,400 on average), given that typical workers worked 8.6 days of wage paying work in the month preceding the experiment. The majority (71%) of workers had outstanding loans at baseline, with a median amount of Rs. 11,000 of debt in our study population. Accordingly, the early payment relieved some of the pressure from indebtedness, but did not eliminate debt for most workers.

Consistent with most workers in the sample facing tight credit constraints, the Early-Pay Treatment significantly altered workers' expenditure patterns (Table 1, Panel A). Most notably, workers used their increased cash-on-hand primarily for paying back loans and credits. Within the first three days of being paid, workers were 40 percentage points more likely to pay back money toward any loan or credit, a large increase relative to the Control Group, in which 18 percent paid back any loans or credits (column 1). This increase corresponding to an additional Rs. 270 of repaid loans and credits, a substantial increase of over 250 percent relative to the control group mean of Rs. 94 (column 2).

The early payment also changed workers' other expenditure patterns. Food expenditures increased food expenditures by Rs. 67 relative to a Control Group mean of Rs. 270 (column 3).[27] These estimates indicate a need to consider potential impacts through nutrition channels, which we discuss in Section 4. Overall, workers reported increasing their expenditures by Rs. 370 following the early payment, a 65 percent relative to the Control Group (column 4).

The Early-Pay Treatment appears to have disproportionately affected expenditure patterns among relatively poorer workers in our sample (Table 1, Panel B). Using the wealth index variable, we find that this heterogeneity is particularly pronounced for the likelihood of repaying loans and credits (column 1). Workers with the lowest index scores are 53 percentage points more likely to make any payments towards loans and credits compared to similarly poor workers without the early payment, whereas this difference is 19 percentage points for workers with the highest wealth score.[28] We find some suggestive but not statistically significant evidence of heterogeneous impacts by wealth for repayment amounts, food, and overall

[27] Consistent with previous findings (Evans and Popova, 2017), we do not find any evidence of changes in reported expenses on tobacco and alcohol, though baseline expenditures in this study population are much lower than found in other parts of India (Schilbach, 2019).

[28] The results are similar, but less pronounced, when dividing the workers at the median, as shown in Appendix Table A3.

expenditures, though statistical power is limited to consider heterogeneity in those dimensions (columns 2 to 4).

3.3 Productivity Impacts

The Early-Pay Treatment increased worker productivity (Table 2 Panel A). Workers who had received the early payment produced an additional 0.26 leaf plates per hour compared to the Control Group (column 1), a 6.8 percent increase relative to the Control Group mean of 3.84 leaf plates. Similarly, using log hourly production, we find a 3.2 percent increase in productivity (p=0.065, column 2). In normalized terms, these effects correspond to a 0.111 to 0.124 standard deviations (columns 3 to 5). This treatment effect is economically meaningful, especially when compared to the relatively low wage elasticity that we find in this setting (see Section 3.4) and that other researchers have found in real-effort experiments (DellaVigna et al., 2019).

The productivity impacts tend to concentrate among poorer workers, as suggested by heterogeneous treatment effects with respect to baseline wealth (Table 2 Panel B). For each of these indicator variables, we observe sizable impacts of the Early-Pay Treatment on productivity among poorer workers compared to the remaining workers in the study (columns 1 through 4). While none of the interaction coefficients for the individual wealth proxies is statistically significant, the interaction coefficients across wealth proxies are consistently negative. Aggregating these measures into indices, we find a treatment effect of 0.22 to 0.25 standard deviations for the poorest workers of the sample, and marginally significantly lower (close to zero) impacts for the richer half of the sample (columns 5 and 6).

There are two potentially complementary interpretations for the stronger impacts among poorer workers. First, the poor might be experiencing more financial strain (e.g. loans, worries about finances) to start with, thus providing more opportunities for the greater impact of any given intervention that might reduce such strain. Alternatively, most workers in the sample were financially strained, but the intervention was more meaningful for poorer workers since it was larger compared to their wealth.

The impacts on worker productivity proved persistent over several days and throughout the day. Worker productivity in the Early-Pay Groups increased on each of the days following the early payment until the end of the contract period (Table 4, column 1). Similarly, the patterns of heterogeneous treatment effects with respect to wealth persisted for each of these days (Table 4, columns 2 to 3). Finally, the productivity impacts occurred throughout the day, including the last two hours of the workday (see Table 6, discussed below).

Our experiment was designed to capture potential impacts of increased cash-on-hand on worker productivity while keeping the type of work as well as days and hours worked constant. The completion bonus induced a high overall attendance (98.1 percent), thus limiting the

extent of any potential labor-supply response to the experimental variation in payments. Indeed, we find only minimal and statistically insignificant impacts of the early payment on the number of days worked (Appendix Table A4, column 4). Moreover, by the design of the experiment, there was no treatment response in hours per day given that work hours were fixed.

It is also unlikely that the Early-Pay 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 region of Odisha, India, Breza et al. (2020) find that only 1.72% of casual workers reported doing any secondary activities after work.

3.4 The Role of Attentiveness

So far, we have considered the impacts of our treatment leaf plate production, i.e. the output of worker's efforts. By examining *how* workers produce, we can provide a window into what is happening inside the worker's mind. As described above, we collected three markers of attentional errors, which we combine into an "Attentiveness Index" and a "High Attentiveness" indicator variable.

The Early-Pay Treatment increased our measures of workers' attentiveness, especially among the poorer half of the sample (Table 3). We find suggestive evidence of an increase in the Attentiveness Index (by 0.086 SDs, column 1) and a statistically significant increase in the High Attentiveness indicator (by 0.096 SDs, column 2). Mirroring the impacts of the Early-Pay Treatment on productivity, we find robust evidence that these impacts are concentrated among poorer workers in the sample (columns 3 to 6).[29] Attentiveness among those workers increased by 0.20 to 0.26 standard deviations while we find no such effects among the relatively richer workers in the sample.

The observed impacts of the Early-Pay Treatment on our measures of attentiveness suggest that reduced attentional errors are one contributing mechanism of the observed treatment effects on worker productivity. Such attentional impacts could be explained by cash-on-hand reducing worries and thus distractions during work hours, as hypothesized by Mullainathan and Shafir (2013). However, this evidence could also potentially be consistent with other psychological channels such as stress, mental health, sleep, happiness, or motivation, that operate in the same way, i.e that are concentrated among the poor and mediated through attentiveness.

While worker productivity and attentiveness are positively correlated, this relationship is

[29]The results are qualitatively similar when we use individual attentiveness measures or a different combined measure such as the Anderson index, as shown in Appendix Table A5.

not mechanical.[30] That is, increased attentiveness is not the only way to increase to produce more leaf plates per hour. Workers can also increase productivity by doing any of the tasks required to complete a leaf plate more quickly (e.g. by moving their hands faster or by taking fewer short breaks between leaf plates). In other words, there are two distinct production inputs: (i) attentiveness; and (ii) mechanical speed. While the latter is largely a function of effort, the former might not be fully under workers' control and/or awareness.

To benchmark the effects in our experiment, and to better understand the relationship between these production inputs and output, we implemented five supplementary rounds with 151 workers in total. In the rounds, increasing piece rates modestly but significantly increased productivity (columns 1 to 3). However, higher piece-rates did not lead to an improvement in the attentiveness measures (columns 4 and 5). One interpretation of this result is that increasing piece rates induced a modest increase in worker effort, which in turn increased mechanical speed and thus output per hour.[31] In contrast, the impacts of early payments in our main experimental rounds are too large to be plausibly explained by such effort responses. Instead, they appear to be at least in part be explained by increased attentiveness. Given that attentiveness did not change in response to piece rates, this suggests that attentiveness is not (fully) under workers' control, i.e. it cannot simply be changed in increased effort.

4 Potential Confounds

4.1 Ancillary Components of Bundled Treatment

Since the employers administered the Early-Pay Treatment, two ancillary components could have contributed to the observed treatment effects on productivity. First, the Early-Pay Treatment may have changed workers' feelings toward their employer, which could have affected their work performance via gift-exchange or fairness concerns. Second, the Early-Pay Treatment could have specifically impacted workers' trust in their employer.

Gift exchange and fairness. Being paid early might have caused the early payment group to feel more positive about their employer. Conversely, the Control Group might have felt unfairly treated by their employer. Several pieces of evidence contradict the hypothesis that such effects caused significant impacts on worker productivity.

First, the literature on gift exchange at the workplace has largely found no and/or short-lived effects, especially in field settings (Gneezy and List, 2006; DellaVigna et al., 2019; Esteves-Sorenson, 2017; de Ree et al., 2018; Gilricht et al., 2016).

[30] Appendix Table A7 shows the positive correlation between productivity and attentiveness measures.

[31] Modest effort and productivity responses in piece-rate experiments are typical in real-effort experiments, as discussed in detail in DellaVigna et al. (2019).

Second, if gift exchange or fairness concerns were important considerations in this setting, we would expect there to be some measurable impacts immediately following the announcement. However, we do not find any evidence of positive announcement effects on productivity on day 5 and/or day 6 of the study. The estimates in Table 5 show no evidence of significant announcement effects, neither when considering the entire announcement period (columns 1 through 3) nor the announcement day only (i.e. on day 5; column 4) nor the two work days following the announcement (column 5).

One might explain the absence of such effects by the fact that there were no actual payment differences across workers, unlike in Breza et al. (2018). Moreover, while we did not collect data of workers' demand for the different payment regimes, evidence from other settings suggests that at least some workers prefer more infrequent payments as a method of commitment savings (Casaburi and Macchiavello, 2019), such that the direction of any potential effects was a priori unclear.

Finally, any explanation involving gift exchange and fairness would also need to address why the effects would concentrate among poorer workers, which might be possible ex post but not what one might expect ex ante.

Trust in the employer. The Early-Pay Treatment could have increased workers' trust in their employers' assurances of future payments. Such increased trust would have increased workers' perceived expected piece rate—the perceived probability of being paid times the piece rate—and thus potentially increasing both effort and output. Several reasons lead us to believe that the observed treatment effects are not explained by such considerations.

First, we designed the payment on day 1 so as to build workers' trust in their employer. Second, as described above, some workers in the Early-Pay Group were paid on day 8; others were paid on day 9 (at the same worksite). If trust in the employer were a major concern among workers, then we would expect workers who were to be paid on day 9 to display an increase in trust towards their employer following the payment of day 8 payees. However, we found no impacts on day 9 for workers who were going to be paid later that day compared to the Control Group (Table 5 column 6).

4.2 Investment Channels

In general, cash-on-hand can have a variety of impacts on worker productivity, ranging from physical capital (e.g. machines, fertilizer) to human capital (e.g. training, schooling) and health investments (e.g. bed nets, nutrition). By design, the results of our experiment cannot be explained via effects of investments in physical or human capital since there was no scope for workers to bring any of their own physical capital to the worksite. Moreover, any human-capital investments would have taken much longer to come to fruition than the horizon of the

experiment.

Nutrition. A long literature in development economics considers the impact of nutrition on worker productivity (Dasgupta and Ray, 1986). We find some evidence of workers increasing their food expenditures following the early payment (Table 1), as discussed in Section 3.2. However, meaningful short-run impacts of the Early-Pay Treatment on worker productivity via nutritional channels are unlikely in our setting. We consider two categories of potential pathways.

A first potential channel could be biological changes for malnourished workers due to increased food intake. However, according to the biological and medical literatures on the impacts of increased food intake, such changes do not occur overnight. Consistent with this view, Schofield (2014) finds evidence of increased earnings among workers only starting a week after increasing their caloric intake.

A second potential channel could occur via potential impacts of increased breakfast intake due to blood-sugar spikes. We find clear evidence against such effects. We collected direct measures of breakfast consumption following the early payday. 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 (columns 3 through 7 of Table 6). A possible explanation for this lack of impacts on breakfast consumption patterns appears to be that almost all workers (98 percent) in the Control Group reported eating breakfast (thus leaving not much room at the extensive margin), and almost everyone (94 percent) reported eating a particular rice dish that is common in the area (often involving vegetables).

Moreover, 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 Early-Pay Treatment throughout the day, including the last couple of hours or the workday, i.e. 5 to 7 hours after eating breakfast (columns 1 and 2 of Table 6).

4.3 Priming Treatment

In addition to considering the impact of the Early-Pay Treatment, we also investigate the role of attention by investigating potential impacts of directly focusing workers' attention on their financial situation. To do so, we implemented a priming intervention that was cross-randomized to the Early-Pay Treatment.

Directing workers' attention can have two potentially opposing effects. First, since attention is limited, drawing workers' attention to their finances might divert valuable attentional resources from the work task and thus reduce worker productivity. Second, however, focusing workers' attention on their finances might raise workers' perceived marginal value of a dollar. Such impacts, resembling reminder effects in Karlan et al. (2016), might increase worker effort

and thus increase worker productivity. Importantly, previous work on scarcity—such as Mani et al. (2013)—had only limited scope for a positive channel.

To test for differential effect before vs. after payment, the priming treatment was randomized to be conducted before or after cash payment. Some workers received the priming intervention on day 6 (i.e. before any early payments occurred), others received it on day 10 (i.e. after some of those workers had received early payments), and others received no intervention. This design allows us to test whether potentially stronger impacts of induced financial worries on cognition before the early payment caused differential effects of the intervention for workers who had been paid compared to workers who had not been paid.

In our setting, the overall impact of the priming intervention on worker productivity is positive (column 1 of Table 7). After receiving the priming intervention during the first hour of work, hourly production increase by 0.045 standard deviations compared to the remaining study sample. This result suggests that the motivational effect of focusing workers' attention on their finances is stronger than adverse effects of diverting attentional resources away from work.

The positive effects are entirely concentrated among workers who have already been paid. When workers were cash-poor (i.e. before their first major payment), we found no evidence of any (positive or negative) impact of the priming treatment on worker productivity, suggesting that the two opposing effects described above cancel each other out. In contrast, after workers were paid, the salience intervention increased worker productivity by up to 0.081 standard deviations (column 2 of Table 7).

The heterogeneous treatment effects (columns 3 and 4 of Table 7) highlight the difficulties in targeting salience interventions precisely. Our intention was to target poverty with the priming intervention, but it appears that we may have instead made loans more salient. This interpretation may explain the lack of heterogeneous treatment effects with respect to wealth and the clear evidence of heterogeneous impacts with respect to loans that workers are highly worried about. More generally, it is difficult to raise the salience of only one particular issue of interest.[32]

Overall, we find a complicated set of results of the impact of the priming treatment on worker productivity, which highlights the caution warranted with salience or priming interventions, as also pointed out by Kahneman (2012).

[32]Moreover, priming interventions are likely to be non-monotonic (e.g. in baseline wealth or worries). For instance, on the one hand, one can only bring worries top of mind if they exist, e.g. impacts of making financial strain might be larger for people with more severe financial strain. On the other hand, the underlying concerns may already be top of mind for people who are very strained, leading to smaller treatment effects for people with more severe financial strain.

5 Conclusion

This paper tests for a direct relationship between financial constraints and productivity. We provide evidence that even relatively minor improvements in workers' financial situations can have relatively large impacts on their productivity. When workers have less cash-on-hand, they produce fewer plates, make more errors per plate, and earn less in total. This evidence suggests that financial constraints by themselves may be detrimental for earnings, beyond potential impacts through investments in complementary inputs, human capital, or health. We also provide some evidence that attention is one mediating mechanism. We find that relaxing workers' financial constraints also reduced attentional errors.

Given the impacts, it seems worth revisiting other contexts in search of similar direct effects. For instance, Fink et al. (2018) document large seasonal variation in earnings among farmers in Zambia. 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. Our evidence suggests that direct effects of changes in financial strain may have contributed to the observed impacts in these settings.

Finally, our findings may have some implications for policy. The direct impact of financial strain on worker productivity is a parameter of interest for various policies, including unemployment insurance, basic income, or conditional and unconditional cash transfers. Importantly, the observed direct effects of reducing financial strain may occur *in addition* to any investment effects economists usually consider.

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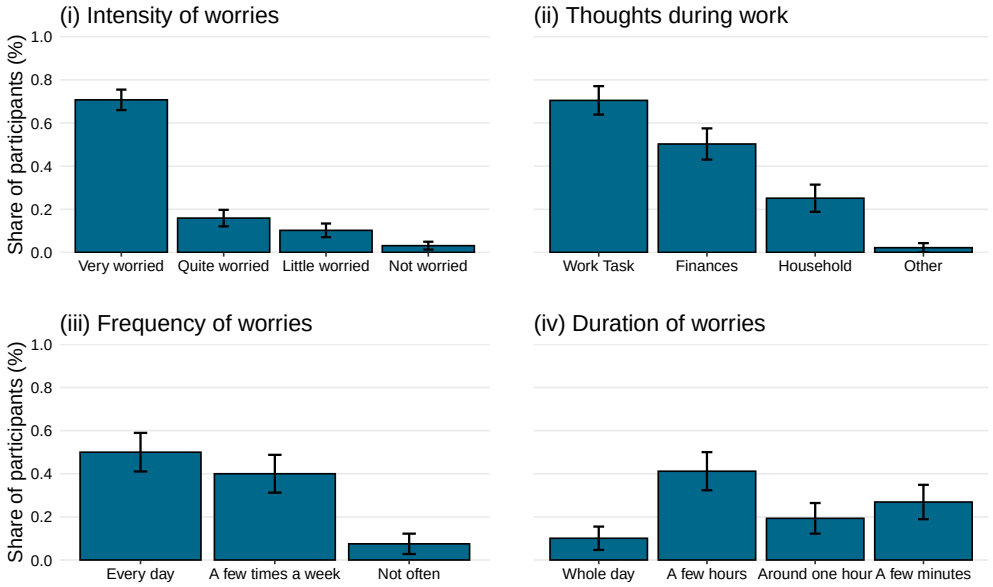
6 Tables and Figures

Figure 1: Financial Concerns

(a) Characterizing worries about finances



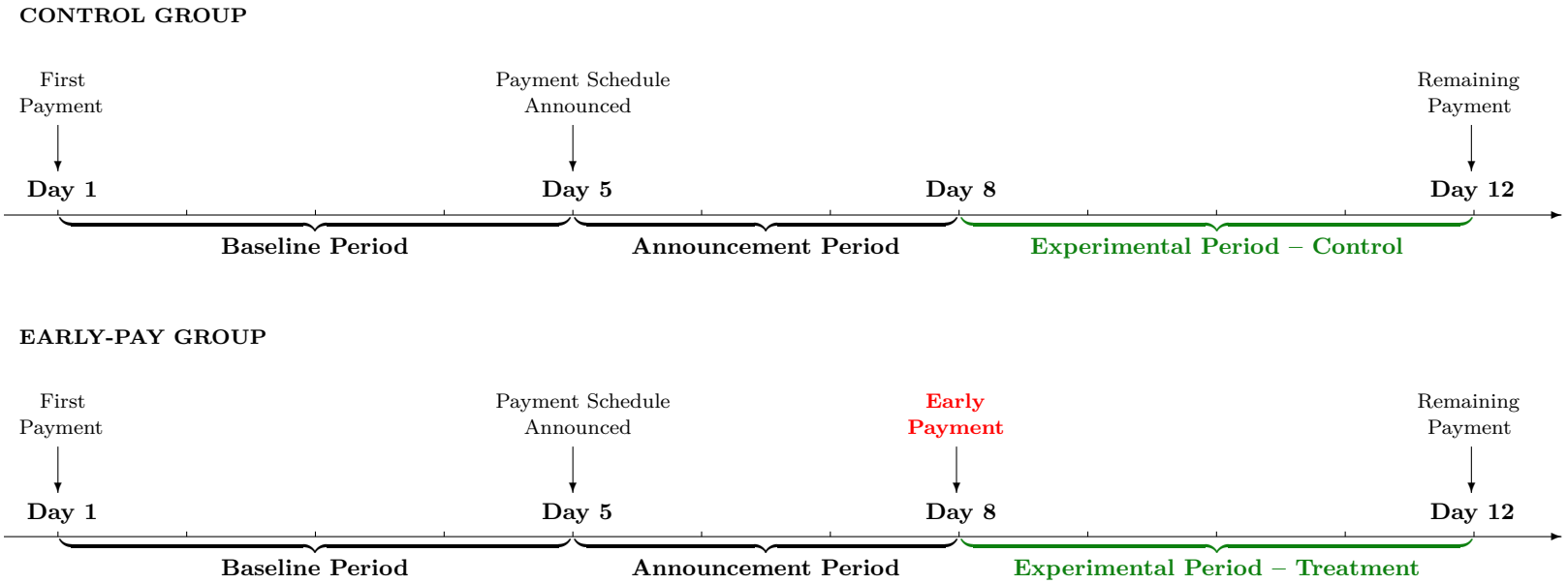
(b) Triggers of financial thoughts

Notes: This figure shows the participants' responses to question related to financial worries and stressors. In Panel (a) we ask four questions: (i) "How worried are you about your finances?" ($N = 352$); (ii) "What were you thinking about while working?" (unprompted question) ($N = 183$) (iii) "How often do you worry about your finances?" ($N = 120$); (iv) "How long do you worry about finances every day?" ($N = 119$). Question (i) was asked during baseline. Questions (ii)-(iv) were asked after treatment, so we exclude the participants in the early payment groups and who received the priming intervention on the day of the survey. Missing values are included in the denominator of the share of participants variables.
In Panel (b) we show a word cloud representing the answer to the question "What makes you think about money issues?" ($N = 408$). The question was asked after treatment, but the whole sample is included. The font size is proportional to the frequency the term was mentioned by participants.

Figure 2: Leaf Plate

Notes: This figure shows a sal tree leaf plate akin to the ones produced as part of the experiment. In accordance with quality standards set by partnering contractors, leaf plates were required to (i) meet a minimum size requirement, (ii) have no gaping holes, (iii) have all leafstalks (petioles) covered by other leaves, and (iv) have the inner center parts placed underneath the outer rings of the plates.

Figure 3: Experimental Design



Notes: This figure shows the experimental design of the study. In the Control Group (upper part of the figure), workers were paid on days 1 and 12. In the Early-Pay Group (lower part of the figure), workers were paid on days 1, 12, and additionally on day 8. Within each group, workers were randomized to receive the priming intervention on day 6, day 10, or not at all. In addition, within each round, half of workers were randomized to have a delayed schedule after announcement, i.e. the priming and early payment interventions happened with a one-day lag for these workers.

Table 1: Impact of Early Payment on Expenditure Patterns

PANEL A: Overall Impacts

	Loans and Credits		Expenditures	
	Any Payment (1)	Amount (2)	Food (3)	All Expenses (4)
Cash	0.40***	270.39***	67.44***	370.39***
	(0.04)	(51.59)	(24.15)	(68.18)
Control group mean	0.18	94.20	269.94	572.54
N: workers	402	402	402	402

PANEL B: Heterogeneous Treatment Effects

	Loans and Credits		Expenditures	
	Any Payment (1)	Amount (2)	Food (3)	All Expenses (4)
Cash	0.53***	311.83***	67.02*	488.20***
	(0.08)	(70.36)	(38.42)	(116.14)
Cash × Wealth index	-0.34**	-105.74	-0.92	-301.52
	(0.16)	(141.00)	(84.74)	(250.14)
P-val: cash effect + interaction	0.080	0.052	0.269	0.274
N: workers	399	399	399	399

Notes: This table shows the impact of the early-payment treatment on expenditure patterns in the three days following the early payments. Panel A shows the overall impacts of the treatment. "Cash" refers to whether an individual was part of one of the two early-payment groups. The dependent variables in the first two columns are whether the participant made any repayments of loans or credits (column 1) and the overall amount of such payments (column 2). Panel B shows heterogeneous treatment effects by wealth. The wealth index is a simple average of four binary wealth measures: whether the individual reported owning farmland, living in a non-mud house, not having outstanding credits, and being able to come up with Rs. 1,000 easily in case of an emergency. All regressions control for round fixed effects and an indicator for shorter experimental periods. Standard errors are clustered by worker.

Table 2: The Impact of Early Payment on Worker Productivity

PANEL A: Overall Impacts

	Hourly Production Measure				
	Raw	Log	Normalized		
	(1)	(2)	(3)	(4)	(5)
Cash × Post	0.263***	0.032*	0.111***	0.124**	0.124**
	(0.091)	(0.017)	(0.039)	(0.059)	(0.060)
Control group mean	3.843	1.222	-0.000	-0.000	-0.000
Announcement controls	N	N	N	Y	Y
Answered wealth questions	N	N	N	N	Y
N: worker-hours	22,849	22,621	22,849	22,849	22,656

PANEL B: Heterogeneous Treatment Effects

	Wealth Proxies				**Combined Measures**	
	Owns farmland	Non-mud house	No credits	Get 1k for emergency	Wealth index	High wealth
	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post	0.228***	0.127	0.147**	0.189***	0.254***	0.224***
	(0.079)	(0.077)	(0.072)	(0.071)	(0.095)	(0.082)
Cash × Post × Wealth	-0.178	-0.055	-0.130	-0.143	-0.349*	-0.212*
	(0.116)	(0.114)	(0.113)	(0.123)	(0.181)	(0.120)
P-val: cash effect + interaction	0.557	0.383	0.842	0.642	0.442	0.888
N: worker-hours	22,656	22,656	22,656	22,656	22,656	22,656

Notes: This table shows the impact of the Early-Payment Treatment on worker productivity. Standard errors are clustered by worker. Panel A shows the overall impacts of the treatment. "Cash" refers to whether an individual is in the Early-Pay Group. "Post" indicates the experimental period, i.e. period after the Early-Pay Group receives their lump-sum payment. Unless indicated otherwise, regressions include the standard set of controls that account for the effects of announcement, production disruption, priming intervention, and shorter slot lengths, as described in Section 3.1. Regressions also control for worker, day, and round times hour of the day fixed effects. Panel B shows heterogeneous treatment effects by wealth. The wealth index is a simple average of the four binary wealth proxies (column 5). High wealth indicates that the worker's wealth index value is greater or equal to the median (column 6).

Table 3: The Impact of Early Payment on Attentiveness

	Attentiveness Measures					
	Attentiveness index (1)	High attentiveness (2)	Attentiveness index (3)	High attentiveness (4)	Attentiveness index (5)	High attentiveness (6)
Cash × Post	0.086	0.096**	0.262***	0.225***	0.227***	0.203***
	(0.062)	(0.038)	(0.092)	(0.061)	(0.078)	(0.050)
Cash × Post × Wealth index			-0.480**	-0.331***		
			(0.204)	(0.128)		
Cash × Post × High wealth					-0.287**	-0.205***
					(0.117)	(0.072)
P-val: cash effect + interaction			0.136	0.221	0.495	0.956
N: worker-hours	15,265	15,265	15,159	15,159	15,159	15,159

Notes: This table shows the impact of the early-payment treatment on attentiveness. Attentiveness index is based on three hourly measures of attentional errors: the average number of leaves, stitches, pairs of holes from removed stitches across all plates (including rejections) produced during the production slot. The three measures are normalized and then averaged to created the Attentiveness Index. High Attentiveness indicates that the index value is greater than the median. All regressions include the controls described in equation 2. Standard errors are clustered by worker.

Table 4: Persistence of Early Pay Impacts

	Hourly production			**Attentiveness index**			**High attentiveness**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash × Post 1 day	0.131**	0.250***	0.230***	0.107*	0.268***	0.244***	0.198***
	(0.065)	(0.087)	(0.080)	(0.061)	(0.091)	(0.077)	(0.050)
Cash × Post 2 days	0.155**	0.288***	0.252***	0.085	0.258***	0.218***	0.228***
	(0.061)	(0.094)	(0.084)	(0.064)	(0.095)	(0.081)	(0.054)
Cash × Post 3+ days	0.081	0.293***	0.235***	0.017	0.198	0.161	0.109
	(0.065)	(0.097)	(0.084)	(0.084)	(0.132)	(0.114)	(0.070)
Cash × Post 1 day × Wealth		-0.281	-0.174		-0.449**	-0.285**	-0.186**
		(0.173)	(0.124)		(0.199)	(0.116)	(0.073)
Cash × Post 2 days × Wealth		-0.349*	-0.194		-0.478**	-0.276**	-0.213***
		(0.179)	(0.121)		(0.214)	(0.120)	(0.075)
Cash × Post 3+ days × Wealth		-0.550***	-0.319**		-0.465	-0.282*	-0.204**
		(0.203)	(0.126)		(0.285)	(0.152)	(0.092)
Wealth measure		Wealth index	High wealth		Wealth index	High wealth	High wealth
N: worker-hours	22,849	22,656	22,656	15,265	15,159	15,159	15,159

Notes: This table shows the impact of the early-payment treatment on worker productivity and attentiveness. The dependent variables are normalized hourly production (columns 1-3), attentiveness index (columns 4-6), and a dummy for whether attentiveness index is above the median (column 7). Post 1 day (2 or 3 days) is a dummy capturing whether the participant received payment 1 day ago (2 or 3 days ago). We interact these variables with wealth index (columns 2 and 5) and high wealth indicator (columns 3, 6, and 7). All regressions include the controls described in equation 2. Standard errors are clustered by worker.

Table 5: The Impact of Announcing Early Payment Schedule

	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post-payment		0.132**	0.137**	0.133**	0.133**	0.126**
		(0.056)	(0.057)	(0.061)	(0.061)	(0.059)
Cash × Post-announcement	0.021	0.024	0.016			0.017
	(0.044)	(0.042)	(0.040)			(0.043)
Cash × 1 day after announcement				-0.024		
				(0.035)		
Cash × 2+ days after announcement				0.028		
				(0.048)		
Cash × 1-2 day after announcement					0.003	
					(0.036)	
Cash × 3+ days after announcement					0.026	
					(0.054)	
Cash × Payment day			0.053**	0.049**	0.052**	
			(0.021)	(0.020)	(0.021)	
Later cash × 1 day before payday						0.049
						(0.039)
Post-payment period	N	Y	Y	Y	Y	Y
P-val: Cash × Post-pay − Cash × Post-announce		0.000				
P-val: Cash × Post-pay − Cash × Payday			0.087	0.137	0.179	
P-val: Cash × Post-pay − Later cash × 1 day before						0.206
N: worker-hours	15,207	22,849	22,849	22,849	22,849	22,849

Notes: This table shows the impact of announcing payment schedule differences on worker productivity. The post-announcement period is defined as the period between schedule announcement (i.e. beginning of day 5in the standard schedule) until the Early Pay Treatment (i.e. end of day 8 in the standard schedule). All regressions include the controls described in equation 2. Standard errors are clustered by worker.

Table 6: The Impact of the Early Payment via Nutrition Channels

	Hourly Production		**Breakfast Measures**				
			Had any breakfast	Ate rice	Amount of rice	Ate vegetables	Ate any other item
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cash × Post	0.132**	0.132**					
	(0.063)	(0.061)					
Cash × Post × Last 2 hours of day	-0.032						
	(0.027)						
Cash × Post × Last 1 hour of day		-0.041					
		(0.030)					
Cash			-0.007	-0.002	-3.889	-0.021	0.057
			(0.013)	(0.025)	(7.154)	(0.041)	(0.043)
P-val: cash effect + interaction	0.07	0.11					
N: worker-hours	22,849	22,849					
Control group mean			0.98	0.94	180.63	0.76	0.27
N: workers			320	320	320	320	320

Notes: This table shows the impact of the early-payment treatment on worker productivity during the last two hours of the day (column 1) and during the last hour (column 2). These regressions include the controls described in equation 2. The table also shows the impact of early-payment treatment on breakfast consumption (columns 3 through 7). The breakfast measures are the average probability of eating any specific item or the average amount of rice consumed over the two mornings after the Early-Pay treatment. These regressions include the controls described in equation 1. Standard errors are clustered by worker.

Table 7: The Impact of the Priming Intervention on Worker Productivity

	(1)	(2)	(3)	(4)
Post-priming	0.045**	0.081*	0.119*	0.003
	(0.022)	(0.046)	(0.065)	(0.057)
Post-priming × Pre-payment		-0.056	-0.111	0.053
		(0.057)	(0.076)	(0.076)
Post-priming × High wealth			-0.088	
			(0.087)	
Post-priming × Pre-payment × High wealth			0.122	
			(0.111)	
Post-priming × High worrisome loans				0.178*
				(0.091)
Post-priming × Pre-payment × High worrisome loans				-0.258**
				(0.110)
P-val: sum of top two coefficients		0.369	0.812	0.161
P-val: sum of all four coefficients			0.336	0.396
N: worker-hours	22,849	22,849	22,656	22,656

Notes: This table shows the impact of the priming intervention on worker productivity. “Post-priming" refers to whether the individual has received the priming intervention earlier that day. “Pre-payment" indicates that worker has not yet received a lump-sum cash payment. “High wealth” indicates that worker’s average wealth index value is greater than or equal to the median. “High worrisome loans” indicates the total amount of loans, about which the worker is very worried, is greater than the median. All regressions include the controls described in equation 2, including the variables related to the Early-Pay Treatment. Standard errors are clustered by worker.

A Supplementary Appendix

A.1 Appendix Tables

Table A1: Balance Across Experimental Arms

	Priming Treatment						Early Cash Treatment		
	Control	Early	Late	$1 = 2$	$1 = 3$	$1 = (2 \bigcup 3)$	Control	Treatment	$7 = 8$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Demographics									
Age	38.91	39.60	39.38	0.58	0.85	0.76	39.19	39.37	0.54
	(0.73)	(0.70)	(0.93)				(0.66)	(0.60)	
Years of education	4.41	4.72	4.30	0.32	0.73	0.39	4.69	4.36	0.89
	(0.27)	(0.28)	(0.36)				(0.26)	(0.23)	
Can read newspaper in Odiya	0.61	0.65	0.66	0.42	0.36	0.32	0.63	0.64	0.66
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
Panel B. Labor and Wealth									
Primarily daily laborer	0.73	0.71	0.74	0.83	0.78	0.99	0.75	0.70	0.26
	(0.04)	(0.04)	(0.05)				(0.03)	(0.03)	
Days of labor (last 7 days)	1.67	1.88	1.95	0.33	0.29	0.24	1.88	1.76	0.42
	(0.16)	(0.16)	(0.19)				(0.16)	(0.12)	
Days of labor (last 30 days)	8.59	8.67	8.75	0.96	0.89	0.92	8.60	8.71	0.88
	(0.61)	(0.52)	(0.78)				(0.50)	(0.51)	
Income quartile	2.41	2.46	2.31	0.70	0.48	0.95	2.39	2.42	0.79
	(0.09)	(0.08)	(0.11)				(0.08)	(0.07)	
Wealth index	0.39	0.40	0.39	0.44	0.63	0.44	0.40	0.38	0.90
	(0.02)	(0.02)	(0.03)				(0.02)	(0.02)	
High wealth (above median)	0.53	0.54	0.47	0.79	0.59	0.95	0.56	0.49	0.51
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
Owns farmland	0.56	0.55	0.59	0.98	0.40	0.68	0.56	0.56	0.69
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
Non-mud house	0.20	0.25	0.24	0.25	0.24	0.18	0.24	0.22	0.92
	(0.03)	(0.03)	(0.04)				(0.03)	(0.03)	
No outstanding credits	0.47	0.46	0.45	0.76	0.68	0.68	0.46	0.46	0.98
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
Can get 1k in emergency	0.32	0.35	0.26	0.45	0.63	0.76	0.36	0.29	0.44
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
N: workers	155	160	93				183	225	

Notes: This table shows the tests of differences in worker characteristics before each treatment. Cols. 1-6 show balance for the priming treatment and cols. 7-9 for the Early cash payment treatment. Cols. 1-3 and 7-8 show the average value of each variable for one of the experimental conditions. Col. 4 and 5 shows the p-value of the coefficients of a regression of the variable on the early and late priming groups. Col. 6 shows the p-value for a regression of the variable on being in either priming groups. Col. 9 shows the p-value for a regression of the variable on being in the early payment group. All regressions control for round FEs because treatment assignment is only random conditional on round. All the variables are either baseline characteristics or averages of participants' performance before the payment schedule is announced.

Table A2: Balance Across Experimental Arms (continued)

	Priming Treatment						Early Cash Treatment		
	Control	Early	Late	1 = 2	1 = 3	1 = (2 U 3)	Control	Treatment	7 = 8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel C. Financial worries and loans									
Very worried about finances	0.66	0.68	0.85	0.75	0.00	0.12	0.69	0.72	0.57
	(0.04)	(0.04)	(0.04)				(0.04)	(0.03)	
Very worried about loans	0.40	0.38	0.48	0.61	0.34	0.94	0.41	0.42	0.71
	(0.04)	(0.04)	(0.05)				(0.04)	(0.03)	
Has loans	0.69	0.68	0.80	0.70	0.12	0.65	0.68	0.73	0.57
	(0.04)	(0.04)	(0.04)				(0.03)	(0.03)	
Has moneylender loans	0.16	0.15	0.24	0.81	0.17	0.63	0.17	0.17	0.61
	(0.03)	(0.03)	(0.04)				(0.03)	(0.03)	
Loan amount (Rs.)	10451.88	11752.43	12217.67	0.55	0.70	0.55	10429.35	12132.37	0.68
	(1102.84)	(1371.01)	(1751.99)				(1083.40)	(1128.14)	
Panel D. Productivity and attentiveness									
Attendance	0.98	0.97	0.99	0.25	0.47	0.65	0.97	0.98	0.60
	(0.01)	(0.01)	(0.01)				(0.01)	(0.01)	
Productivity pre-priming	3.67	3.41	3.78	0.05	0.40	0.08	3.38	3.76	0.55
	(0.17)	(0.12)	(0.17)				(0.13)	(0.12)	
Productivity	2.97	2.80	3.14	0.08	0.87	0.20	2.77	3.08	0.38
	(0.14)	(0.09)	(0.13)				(0.11)	(0.09)	
Attentiveness index	-0.41	-0.21	-0.12	0.21	0.25	0.16	-0.25	-0.28	0.26
	(0.13)	(0.11)	(0.15)				(0.10)	(0.10)	
High attentiveness	0.81	0.85	0.92	0.81	0.76	0.76	0.84	0.86	0.33
	(0.09)	(0.09)	(0.10)				(0.09)	(0.07)	
N stitches per plate	81.74	77.71	68.13	0.08	0.58	0.13	85.53	70.18	0.94
	(5.48)	(5.29)	(7.38)				(4.86)	(4.66)	
N leaves per plate	14.50	14.38	14.31	0.77	0.94	0.81	14.56	14.29	0.69
	(0.24)	(0.18)	(0.25)				(0.20)	(0.17)	
N holes per plate	1.74	1.42	1.58	0.31	0.99	0.48	1.47	1.66	0.16
	(0.21)	(0.14)	(0.38)				(0.13)	(0.21)	
Joint Orthogonality Test				0.98	0.35	0.84			0.60
N: workers	155	160	93				183	225	

Notes: This table shows the tests of differences in worker characteristics before each treatment. Cols. 1-6 show balance for the priming treatment and cols. 7-9 for the Early cash payment treatment. Cols. 1-3 and 7-8 show the average value of each variable for one of the experimental conditions. Col. 4 and 5 shows the p-value of the coefficients of a regression of the variable on the early and late priming groups. Col. 6 shows the p-value for a regression of the variable on being in either priming groups. Col. 9 shows the p-value for a regression of the variable on being in the early payment group. All regressions control for round FEs because treatment assignment is only random conditional on round. All the variables are either baseline characteristics or averages of participants' performance before the payment schedule is announced.

Table A3: Impact of Early Payment on Expenditure Patterns; Heterogeneous Treatment Effects with High Wealth

	Loans and Credits		**Expenditures**	
	Any Payment	Amount	Food	All Expenses
	(1)	(2)	(3)	(4)
Cash	0.50***	255.29***	61.40*	383.46***
	(0.06)	(55.63)	(31.25)	(95.16)
Cash × High wealth	-0.19**	29.38	10.60	-27.74
	(0.09)	(104.12)	(45.66)	(134.75)
P-val: cash effect + interaction	0.000	0.001	0.041	0.000
N: workers	399	399	399	399

Notes: Standard errors are clustered by worker.

Table A4: The Impact of Early Payment on Worker Productivity; Robustness Checks

	Normalized hourly production		Share of rejections	Attendance
	(1)	(2)	(3)	(4)
Cash × Post	0.118*	0.128**	0.009	-0.012
	(0.064)	(0.058)	(0.006)	(0.016)
Control group mean	-0.031	0.031	0.013	0.983
Include absent workers	Y	N	N	
Include rejections	N	Y	Y	
N: worker-hours	23,277	22,849	22,722	
N: worker-days				4,039

Notes: This table shows the impact of the Early-Payment Treatment on worker productivity. Share of rejections is the number of plates that did not meet quality standards out of all the plates produced in the hour. Regressions shown in columns 1 through 3 include standard controls. Attendance indicates that worker was present at the worksite on that day. The last regression controls for individual and day in study fixed effects. Standard errors are clustered by worker.

Table A5: Attention: Decomposition

PANEL A: Overall Impacts

	Number of leaves (1)	Number of stitches (2)	Number of double holes (3)	Anderson index (4)
Cash	-0.0785	0.117	-0.166*	0.0884
	(0.087)	(0.108)	(0.086)	(0.062)
N: worker-hours	15,265	11,620	15,265	15,265

PANEL B: Heterogeneous Treatment Effects

Cash	-0.193*	-0.00328	-0.106	0.141**
	(0.100)	(0.121)	(0.090)	(0.069)
Cash × High wealth	0.197*	0.181	-0.0720	-0.113
	(0.109)	(0.149)	(0.114)	(0.073)
N: worker-hours	15,159	11,620	15,159	15,159

Notes: This table shows the impact of the early-payment treatment on different measures of attentiveness. The outcomes in columns 1-3 are the average number of leaves, stitches, pairs of holes from removed stitches across all plates (including rejections) produced during the hour, respectively. The three measures are normalized by the control group's average and standard deviation. In column 4, the outcome is a weighted average of these three measures in which the weights are chosen to reduce the variance of the index Anderson (2008). Standard errors are clustered by worker.

Table A6: The Impact of Piece Rate Variations on Worker Productivity and Attentiveness

	Hourly production			Attentiveness index		Attendance
	(1)	(2)	(3)	(4)	(5)	(6)
Log(piece rate)	0.047	0.052**		-0.020		0.003
	(0.039)	(0.023)		(0.027)		(0.016)
Piece rate = Rs. 3			0.021		0.002	
			(0.015)		(0.023)	
Piece rate = Rs. 4			0.036**		-0.015	
			(0.016)		(0.019)	
Include absent workers	Y	N	N	N	N	
P-val: equality of coefficients						
Log(piece rate) in (2) and (4)				0.005		
Piece rate = Rs. 3 in (3) and (5)					0.345	
Piece rate = Rs. 4 in (3) and (5)					0.004	
N: worker-hours	4,520	4,374	4,374	4,373	4,373	
N: worker-days						904

Notes: This table shows the impact of the higher piece-rate on worker productivity, attentiveness, and attendance. Piece-rate wage varied from Rs. 2 to 4, so the omitted category in columns 3 and 5 is when piece-rate wage was Rs. 2. Regressions in columns 1 through 5 control for individual, day in study, and round times work hour fixed effects, and also include a dummy for whether any production slot on the last day was interrupted. Standard errors are clustered by worker. The last regression (column 6) controls for individual and day in study fixed effects. Stardard errors are clustered by worker.

Table A7: 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 relationships between worker productivity, attentiveness, and cognition. Regressions in Panel A use the data from main rounds 1 to 12. Averages of normalized measures are calculated at the worker level using observations prior to schedule announcement. Regressions in Panel B use the data from supplementary rounds. Averages of normalized measures are calculated at the worker level using observations after the first (training) day. All regressions control for round fixed effects. Standard errors are clustered by worker.

Table A8: Trust

	(1)	(2)	(3)
Cash × Payment day	0.044	0.050	0.053*
	(0.036)	(0.037)	(0.030)
Later cash × 1 day before payday	-0.037	-0.052	0.029
	(0.059)	(0.059)	(0.046)
Cash × Post-payment			0.128**
			(0.060)
Post-payment period	No	No	Yes
N: worker-hours	15,207	15,207	22,849

Notes: Standard errors are clustered by worker.

A.2 Deviations in Work and Payment Schedules

The standard schedule refers to the 12-day, 5-hour work schedules 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. Payment schedule was announced at the beginning of day 5. The Early-Pay Treatment happened at the end of the day 8, and priming sessions were conducted on day 6 or 10. For those with a delayed schedule (Control Group II and Early-Pay Group II), the Early-Pay Treatment and priming sessions happened one day later. For the Early-Pay Treatment, workers received wages earned up to one day before the payday, i.e. payment lag was one day. Any deviations from this standard schedule is described below and are summarized in Panel A of Appendix Table A.2.

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 snack at the end of each day.

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 schedule announcement on day 6.

Round 13-14 were shorted to 6 days. 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 early payments comparable to the other rounds, Early-Pay Groups' initial payment included a bonus of Rs. 200 in addition to all wages earned up to the payment day (i.e. including first day's wage). Late-pay group received this bonus on the last day, along with other payments. Workers also received an attendance bonus of 200 if they missed none of the last five workdays.

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, day 2 of round 3, and day 3 of round 12.

In supplementary rounds, there was no variation in payment schedule, but instead a 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 (about Rs. 272 per day) 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.2.

Table A9: 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: Round 12 follows the standard schedule but is shortened by one day. Its schedule is equivalent to skipping day 5 and having the schedule announcement on day 6. Payment schedule was not separately announced during round 13, but was announced on day 2 in round 14. In rounds 13-14, everyone received a bonus of 200 (which was combined with the Early-Pay Treatment for the Early-Pay Group), and the attendance bonus was 200.