DO FINANCIAL CONCERNS MAKE WORKERS LESS PRODUCTIVE? *

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Workers who are worried about their personal finances may find it hard to focus at work. If so, reducing financial concerns could increase productivity. We test this hypothesis in a sample of low-income Indian piece-rate manufacturing workers. We stagger when wages are paid out: some workers are paid earlier and receive a cash infusion while others remain liquidity constrained. The cash infusion leads workers to reduce their financial concerns by immediately paying off debts and buying household essentials. Subsequently, they become more productive at work: their output increases by 7% (0.11 std. dev.), and they make fewer costly, unintentional mistakes. Workers with more cash on hand thus not only work faster but also more attentively, suggesting improved cognition. These effects are concentrated among more financially constrained workers. We argue that mechanisms such as gift exchange or nutrition cannot account for our results. Instead, our findings suggest that financial strain, at least partly through psychological channels, has the potential to reduce earnings exactly when money is most needed. *JEL codes:* D9, D91, O12, J24, I32.

I. INTRODUCTION

Low-income people frequently report feeling stress, worry, or anxiety about their finances. When money is tight, people cannot

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stop thinking about how they will afford groceries, avoid eviction, care for a sick child, or repay a moneylender (Collins et al. 2009; Morduch and Schneider 2017). These worries—because of their poignancy—can intrude into everyday functions of life. For example, the average American reports spending 6.4 working hours each week distracted by thoughts of finances (Sergeyev, Lian, and Gorodnichenko 2023). Recent work in economics and psychology has attempted to understand such worries, arguing that financial concerns can adversely affect how people think, reason, and choose (Mullainathan and Shafir 2013; Haushofer and Fehr 2014).

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

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

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

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

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

We first gauge whether the cash infusion meaningfully affects financial strain. We examine workers' expenditure patterns. After receiving their interim cash payment, treatment workers immediately pay off loans and increase household expenditures—the two most common sources of lean-season financial stress cited by workers in our sample. In the three days after interim cash receipt, treated workers are 40 percentage points (222%) more likely to repay any loan ($p < .001$), with a 287% increase in loan payment amounts ($p < .001$). The majority of these payments occur on the very same day as the cash disbursement. In addition, on the day they receive their interim payment, treated workers increase spending by 70% on household items, such as food, clothing, soap, and fuel ($p < .001$). Second, we report suggestive evidence from worker self-reports. After the cash infusion, treatment workers report being more focused on their work task and less likely to have thought of financial worries while at work. Together, these patterns suggest that the early-payment treatment generated a meaningful reduction in financial strain.

The reduction in financial strain is accompanied by a sharp increase in workers' actual productivity. The day after receiving a cash infusion, treated workers increase output by 0.109 standard deviations, or 6.9%, relative to the control group ($p = .020$). These gains persist throughout the remaining days of the contract period. They are also concentrated among workers who are poorer at baseline, measured both by having fewer assets and less liquidity. Early payment increases productivity for these poorer workers by 0.204 std. dev. ($p = .003$).[1] Because work hours are fixed and attendance is high (98.3%), these output increases reflect improvements in productivity: how quickly workers produce plates in each hour.

The cash infusion not only increases total plates produced, it also changes *how* workers produce those plates; they appear to plan and focus better. We measure attentiveness at work by examining the physical plates. Producing a leaf plate requires assembling irregularly sized leaves into a clean circle. Doing so efficiently requires planning (how will the leaves fit together) and focus (making sure each stitch is in line with that plan). Failing to do so creates extra work: stitches must be removed or additional leaves added, slowing the worker and reducing overall production and earnings. As a result, finished leaf plates contain traces of how attentive a worker was in making them—the number of leaves or stitches used, and pairs of holes that indicate where mistaken stitches were removed—which we measure, unbeknownst to workers. After treatment-workers receive their interim payment, such "attentional lapses" decline by 0.08 std. dev. ($p = .092$). As with the productivity results, these effects

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

persist until the end of the contract period and they are concentrated among the poorer workers, whose attentional lapses fall by 0.13 std. dev. ($p = .037$). The reduction in attentional lapses is particularly striking given that workers are working faster: more cash-on-hand increases pace while simultaneously reducing the rate of mistakes.

Are workers more attentive because they are less weighed down by financial concerns or because they are simply more motivated? Could any increase in worker motivation or effort mechanically increase attentiveness? To test this, we experimentally vary the piece rate, adjusting the base wage to hold overall earnings constant. Each one-rupee increase in the piece rate raises output by 0.020 std. dev. However, this is not accompanied by any discernible change in attentional lapses: the estimated effect on the attentiveness measures is essentially zero and significantly different from the effect on output ($p = .001$). These findings suggest that attentiveness and effort can operate independently.

Could this pattern of results be explained by mechanisms other than the psychological benefits of relieving financial strain? Our simple framework in Section III shows how these results are inconsistent with a neoclassical model, even accounting for liquidity constraints and an effort margin. In Section VII we argue that they cannot be explained by other factors, including fairness, trust, nutrition, and sleep. For example, in contrast with predictions of fairness or gift exchange models, we find no evidence of treatment effects of announcing pay schedules; effects only arise once treatment workers actually receive the interim payment. Moreover, exploiting random variation in which day treated workers receive their interim payment in each round, we find no evidence that control workers decrease effort upon seeing others paid before them. Similarly, we argue our findings cannot be explained by nutritional changes, which biologically cannot generate increased productivity overnight. In addition, since all food at the worksites is provided by us, we argue that short-run blood sugar spikes from differential food consumption cannot explain our findings—evidenced by the stability of treatment effects over the course of the workday, as well as direct data on breakfast consumption patterns.

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

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

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

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

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

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

II. Context: Financial Concerns

We undertake our study with low-income workers engaged in small-scale manufacturing in Odisha, India. In this area, laborers work in agriculture during peak planting and harvesting periods, which take up about four to six months of the year. In the remaining lean agricultural months, they typically seek short-term contract employment in nonagricultural jobs, such as manufacturing and construction. These jobs are of short duration—with the modal job lasting one day, and lengths typically ranging from one day to a couple of weeks (Breza, Kaur, and Shamdasani 2021). Contract jobs may pay wages daily, at interim intervals, or as a lump sum at the end of the contract period. During lean months, jobs are not easy to find and employment rates are low, with

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

workers finding wage employment only 1.9 days a week on average (Table I, Panel A, column (1)). Combined with intermittent employment, this leads to low and variable income in lean months, the time of our experiment. Consequently, workers report high levels of financial constraints, especially those who are dependent on wage labor for their primary earnings (i.e., who own little or no farmland).

In our sample, 68% of control group workers report outstanding loans at baseline (Table I, Panel B, column (1)). Nearly 54% have outstanding credits with local shops for basic household consumption, consistent with difficulties in meeting basic daily expenditures. In addition, 64% of workers say they would have difficulty coming up with Rs. 1,000 (i.e., four days of wage labor income) in case of an emergency—indicating a low level of cash on hand. These patterns, while stark, are not unique to our setting. The poor report low levels of liquidity and difficulty in financially coping with shocks in a range of contexts, including in the United States and in developing countries (Collins et al. 2009; Lusardi, Schneider, and Tufano 2011; Morduch and Schneider 2017).

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

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

TABLE I

SAMPLE CHARACTERISTICS AND TESTS FOR BASELINE BALANCE

	Control mean (1)	Coeff. on cash (2)	p-value (3)
Panel A: Demographics, labor, and wealth			
Age	39.188	−0.447	.596
	[8.856]	(0.841)	
Years of education	4.721	−0.060	.857
	[3.540]	(0.330)	
Can read newspaper in Odiya	0.630	0.021	.670
	[0.484]	(0.048)	
Married	0.984	−0.012	.468
	[0.127]	(0.016)	
Has any children	0.891	−0.038	.262
	[0.313]	(0.034)	
Primarily daily laborer	0.751	−0.056	.216
	[0.433]	(0.045)	
Days of paid work in past 7 days	1.884	−0.130	.509
	[2.125]	(0.196)	
Days of paid work in past 30 days	8.602	0.098	.889
	[6.307]	(0.701)	
House quality (durable house)	0.238	0.003	.946
	[0.427]	(0.042)	
Owns farmland	0.568	0.012	.788
	[0.497]	(0.046)	
No outstanding food loans	0.459	−0.006	.902
	[0.500]	(0.051)	
Can get Rs. 1K in emergency	0.355	−0.034	.458
	[0.480]	(0.046)	
Wealth index (continuous)	0.406	−0.006	.809
	[0.246]	(0.024)	
Panel B: Financial worries and loans			
Worried about finances	0.883	−0.022	.551
	[0.323]	(0.037)	
Worried about any loan	0.579	−0.031	.513
	[0.495]	(0.048)	
Amount of loans worried about	14,625	−913	.679
	[15,994]	(2,204)	
Has loans	0.683	0.027	.550
	[0.467]	(0.045)	
Has moneylender loans	0.175	−0.021	.572
	[0.381]	(0.037)	

TABLE I

Continued

	Control mean (1)	Coeff. on cash (2)	p-value (3)
Panel C: Baseline attendance and productivity			
Attendance	0.978	0.005	.550
	[0.146]	(0.008)	
Hourly production	3.353	0.073	.643
	[2.159]	(0.158)	
Hourly production (normalized)	1.398	0.030	.643
	[0.900]	(0.066)	
Attentiveness index (continuous)	−0.053	0.000	.999
	[0.783]	(0.052)	
N: workers (control or cash)	183	224	

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

frequently. The results indicate that the struggle to meet daily expenses and pay off loans is especially prominent for workers.

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

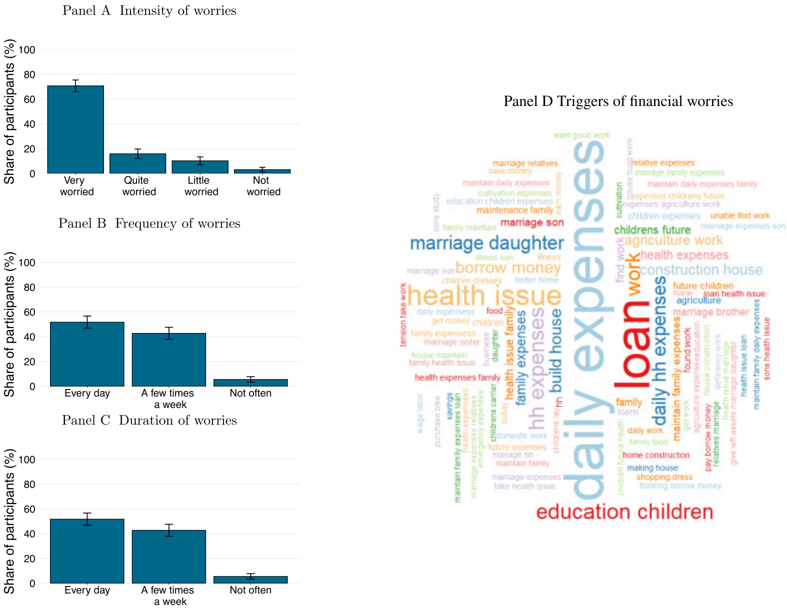

FIGURE I

Financial Concerns

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

Perhaps most relevant to our hypothesis, workers bring worries with them to work. At the end of one workday, we ask workers an open-ended question about what they were thinking about that day while working—with no prompts related to finances, so workers could talk about anything, such as their weekend plans. On a given day, one out of two workers reports ruminating about financial concerns while at work. After this unprompted question, we ask workers specifically whether they thought about their finances while working, and 83% of workers report doing so.

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

III. Simple Framework

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

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

Central to our behavioral model is the notion of two kinds of inputs into production. We call one input e to denote "effortful" inputs, those controllable by the worker. These include physical components, such as the speed of moving one's hands, which might traditionally be called effort, as well as psychological components, such as the decision of how much attention to pay. We call the other input a to denote mental inputs that are beyond a worker's control (i.e., "automatic"). Central to our hypothesis is the idea that some elements of focus are beyond the worker's control. We aim to capture this by assuming that the level of a might be lower or higher depending on the context; for example, a worker who is distracted would have lower a. Both a and e affect output $f(e, a)$.

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

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

The essence of the experimental intervention (described in detail in Section IV) is that some workers are paid earlier than others, and they are told in advance about this upcoming payment three to four days before it occurs. In the model, the first period corresponds to the announcement period (when workers know their payment schedule but payments have not yet been disbursed) and the second period corresponds to the post-payment period (when treated workers have received a payment while control workers have not).

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

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

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

IV. Experimental Design

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

IV.A. Treatment: Variation in Cash on Hand

Our design manipulates financial strain using a naturalistic manipulation: changes in the timing of when wages are paid out. The treatment generates differences in cash on hand while holding other job features constant. This design therefore allows us to construct a test for whether being financially constrained in and of itself affects productivity.

1. *Cash Treatment.* Figure II provides an overview of the timeline for a typical experimental round. Control workers receive all their accrued earnings at the end of the contract period (on workday 12). In contrast, treatment workers receive their earnings in two installments: an interim payment where they

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

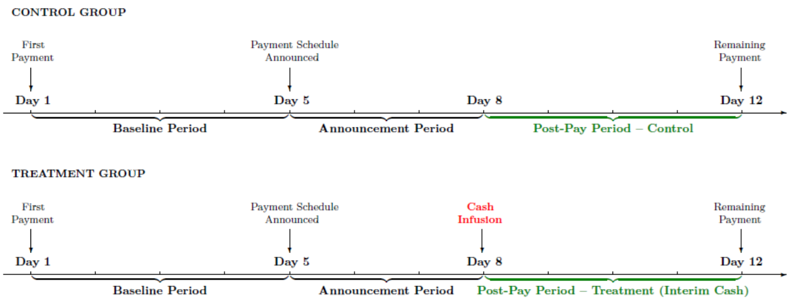

FIGURE II

Experimental Design

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

receive their accrued earnings to date—randomly varied to be on either workday 8 or 9—with the balance of their earnings paid at the end of the contract on day 12 (see Section IV.D for implementation details).

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

2. *Announcement.* The interim payment is not delivered as a surprise. When workers arrive on day 1, they are told that some workers may receive their earnings in two tranches rather than one, and that each worker's exact payment schedule will be announced on day 5. On the morning of day 5, each worker is told individually when he will receive his payment. The subsequent "announcement period" between days 5 and 8 enables

us to test whether workers immediately react to news of their payment schedule, and more broadly whether we see any changes in productivity in anticipation of cash arrival.

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

IV.B. Work Task and Outcomes

1. *Work Task.* Workers produce disposable plates, made from stitching together leaves from sal trees (see Online Appendix Figure A.2). Such plates are a ubiquitous local product used, for example, in virtually all low-tier restaurants in the region. The standards for the plates are set by partnering contractors, and all output is sold to restaurants.

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

Making leaf plates is physically exacting, requiring repeated fine motor movement. It is also cognitively demanding: leaves come in irregular (oval) shapes and sizes, and these varying

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

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

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

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

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

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

IV.C. Additional Treatments

We augment our design with two additional sources of variation.

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

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

IV.D. Implementation and Protocols

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

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

2. *Worksite Setup.* In a typical round, workers worked full-time at the worksite for 12 consecutive days. Hours matched the norms for casual wage work in the villages corresponding to each round. Work typically began at 8 am or 9 am, and ended between 2 pm and 5 pm, with 5 hours of work per day in the modal round.[7]

7. In nine rounds, the workday ended at 2 pm, when laborers in villages go home to have lunch and rest to avoid the afternoon heat. Five- to six-hour

Workers worked individually in their own personal work areas, where they also ate lunch, physically distanced from other workers; this limited the scope for interactions between workers to minimize output comparisons or social conversation at work.

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

3. *Payment Schedule Implementation.* When workers were recruited in their villages, they were informed that they would receive a training payment at the end of day 1 and their remaining earnings on the final day of the contract. When they arrived at the worksite on day 1, they were informed that some workers may be paid in two tranches and that each worker would be informed of his exact payment schedule on day 5. On the morning of day 5 (the announcement day), workers were told as a group that each worker would learn his payment schedule that day, and after this, each worker was individually told his payment schedule by his manager.

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

workdays are common for casual labor jobs in these areas, especially in the lean season due to elevated heat levels. The other rounds had different daily work schedules, for example, from 9 to 5, based on local norms, and some rounds were shorter or longer than 12 days (see Online Appendix C).

payment day, all workers were aware that some payments had occurred at their worksite.

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

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

5. *Randomization.* In each experimental round, workers were randomly assigned to the interim payment (treatment) group or the control group.[8] Within each round, all workers were cross-randomized into Wave A or Wave B, which determined the specific timing of treatments. Among treatment workers, those in Wave A received their interim payment on day 8, and those

8. In most rounds, workers were divided evenly between the two groups. In rounds 1 to 3, the interim-pay group was overweighted in the randomization to make up nearly 70% of the sample.

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

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

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

7. *Surveys and Data Collection.* To maintain a natural work environment and avoid influencing workers' attention through survey activities, we only collected a relatively small set of survey data. All workers completed a short baseline survey including basic demographics such as age, education, measures of income and wealth, and information about outstanding loans and financial

worries. On the last day of each round, we conducted more intensive endline surveys. These collected information about financial worries as well as expenditure patterns and food consumption over the previous three to four days. Finally, we conducted a short survey on day 10 or 11 asking workers about what they thought about while working that day, as summarized in Online Appendix Figure A.3.[9]

V. Data and Empirical Strategy

V.A. Summary Stats, Heterogeneity in Wealth, and Balance

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

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

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

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

V.B. Empirical Strategy

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

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

where y_{irdh} is the outcome of worker i in round-wave r on day d in hour h. Cash_i is a binary indicator for whether an individual is in the interim-pay treatment group. Post-Pay_{ird} is a binary indicator that equals one on the days after the interim payment was disbursed in the worker's wave. $\text{Announcement period}_{ird}$ equals one during the days after the payment schedule was announced through the day the interim payment was disbursed, and equals zero otherwise (see Figure II). Regressions control for round-wave (i.e., strata) fixed effects (δ_r). Finally, X'_{irdh} is a vector of baseline controls, chosen using the post–double-selection LASSO procedure developed by Belloni, Chernozhukov, and Hansen (2014). We show robustness to alternate specifications, including both fewer and more detailed sets of controls, with the results virtually unchanged.

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

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

$$y_{ir} = \beta \text{Cash}_i + X'_{ir}\lambda + \delta_r + \varepsilon_{ir}, \tag{2}$$

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

VI. Results

VI.A. Effects on Financial Strain

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

Table II presents estimates of intent-to-treat regressions at the worker level on expenditures, comparing average expenditures in the three days after the interim cash payment among treatment versus control workers. Panel A shows effects summed over the three days post–interim payment (showing estimates of equation (2)), and Panel B presents estimates separately for each day. After receiving the cash infusion, treatment workers

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

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

TABLE II

EFFECTS ON EXPENDITURES

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

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

immediately pay off loans and increase household expenditures—the two most common sources of financial stress cited by workers in our sample (Figure I, Panel D). Within three days of cash receipt, treated workers increase loan payments by Rs. 271, a 287% increase relative to the control group mean (Table II, column 1, $p < .001$). Treatment workers are 40 percentage points (222%) more likely to pay off any loans or credits (column (2), $p < .001$). The majority of these repayments are made on the same evening as when the cash is disbursed; on the day of the interim payment, workers pay back an additional Rs. 169 in loans and credits (Panel B, column (1))—a 746% increase.

The cash infusion also increases household expenditures, such as food, clothing, soap, and fuel, by Rs. 150 or 40% on average (Panel A, column (3), $p < .001$), and by Rs. 70 or 68% on the day of the interim payment (Panel B, column (3), $p < .001$). Columns (4) to (8) decompose household expenditures into major subcategories. We see significant effects on expenditures on food (25%, column (4)), clothes (242%, column (5)), and household essentials like soap, detergent, petrol, and diesel (172%, column (6)). Given the effects on food, we consider potential effects through nutrition channels in Section VII.B.

We find no detectable effects on other spending categories—agricultural inputs, construction, transfers, and festivals—except for a marginally significant effect on festival expenditures ($p = .092$). We find no treatment effects on purchases of durables (Online Appendix Table A.2, column (1)). Finally, treated workers are also less likely to undertake expenditures on credit during this period, with about a 54% reduction in spending using credit (Online Appendix Table A.2, column (2), $p = .010$), consistent with an improved ability to cope with urgent cash needs post–interim payment. These findings, along with the loan repayments, suggest that on average, workers can borrow if needed but would prefer to hold less debt. In addition, treated workers are 9 percentage points more likely to lend money to other workers at the worksite in the days after the interim payment (Online Appendix Table A.2, columns (3) and (4), $p < .001$); while this suggests the presence of some spillovers, if anything, this should dampen the impact of our treatment by reducing the size of the first stage.

Despite the higher borrowing among control workers, treated workers spend more overall after cash receipt. In the three days after interim payments, treatment workers spend Rs. 371 or 65% more than control workers (Table II, column (9), $p < .001$). In total, treated households spend Rs. 900 in the days after cash receipt, about two-thirds of the average interim payment. The majority of the total spending impact is concentrated in the first day, with an increase of 140% (Panel B, column (9), $p < .001$). These patterns indicate that the cash infusion has the potential to immediately reduce financial strain among treated workers. The ways it potentially does so differ across workers: paying off loans, meeting regular household expenditures, or having more cash on hand to finance shocks.

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

Figure III plots the results of this open-ended exercise. Workers who received the interim payment are 11.5 percentage points (15.5%) more likely to report feeling focused on the work task ($p = .032$). In addition, among the control group, about 60% of workers report thinking about worries related to finances or household expenses while making plates. This is mitigated by treatment: after receiving interim payments, workers are more likely to only report thinking about their work task or other topics outside of financial worries (13.7 percentage points, or 32.7%, $p = .044$).

To supplement this evidence, we borrow from the approach of Shah et al. (2018) to test whether treatment changes the cognitive

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

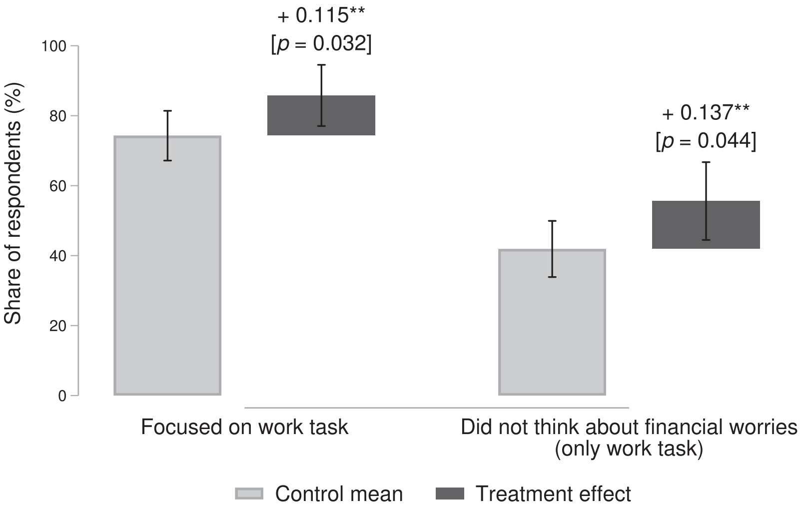

FIGURE III

Thoughts while Working

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

mindset of workers. As described in Section II, we show workers a picture of a low-income individual with negative affect, and ask them to come up with possible reasons the person may be feeling this way (Online Appendix Figure A.1). In response, among the control group, almost all workers (92%) list financial worries as a possible reason for negative affect, and fewer (33%) list any other sort of reason. We examine whether the person's frame of mind allows them to contemplate any other potential reason outside of financial worries for negative affect. Consistent with our hypothesis, workers who receive the interim payment are 10 percentage points (31%) more likely to come up with reasons for negative affect other than financial worries (Online Appendix Table A.3, columns (1) and (2), $p < .05$). Similarly, they are 9 percentage points (66%) more likely to come up with reasons that are more generally distinct from income or being poor, such as that the person might be feeling ill (columns (3) and (4), $p < .05$).[13]

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

VI.B. Productivity Effects

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

In the days following the interim payment, treated workers increase output by 0.109 std. dev., corresponding to a 6.9% increase in output (Table III, column 3, $p = .020$). In contrast, we see no evidence of a treatment effect during the announcement period: the estimated coefficient is 0.014 std. dev. ($p = .685$). Moreover, we can reject that the effects on output during the announcement period and after the interim payment are the same ($p = .008$). This indicates that the treatment effects on productivity do not materialize once workers learn about the interim payment but after they receive the cash in hand.

The effects on productivity are not driven by changes in the extensive margin. As intended by our protocols, average daily attendance is high (98.3%), with no treatment effects of the cash

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

TABLE III

EFFECTS ON WORKER PRODUCTIVITY

	Hourly production					
	(1)	(2)	(3)	(4)	(5)	(6)
Cash × Post-pay	0.097**	0.108**	0.109**	0.111**	0.220***	0.204***
	(0.047)	(0.047)	(0.047)	(0.047)	(0.079)	(0.069)
Cash × Post-pay × Higher wealth					−0.284**	−0.190**
					(0.144)	(0.093)
Cash × Announcement period	−0.002	0.014	0.014	0.012	0.013	0.039
	(0.035)	(0.035)	(0.035)	(0.035)	(0.072)	(0.061)
Cash × Announcement period × Higher wealth					0.013	−0.039
					(0.135)	(0.081)
Linear baseline output	Y	Y	Y	Y	Y	Y
Quadratic baseline output	N	Y	Y	Y	Y	Y
Post-double selection lasso controls	N	N	Y	Y	Y	Y
Day FE and hour FE	N	N	N	Y	N	N
Round-wave FE	Y	Y	Y	Y	Y	Y
p-value Cash × Post-pay = Cash × Announcement period	.006	.008	.008	.007	.000	.000
Wealth index					Continuous	Binary
Coeff.: Cash × Post-pay + Cash × Post-pay × Wealth					−0.064	0.014
Std. err.: Cash × Post-pay + Cash × Post-pay × Wealth					0.093	0.063
p-value: Cash × Post-pay + Cash × Post-pay × Wealth					.489	.819
N: worker-hours	17,441	17,441	17,441	17,441	17,381	17,381

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

infusion on attendance (Online Appendix Table A.5, column (1)). Similarly, there is no scope for treatment response in hours per day as work hours are fixed.[14] Consequently, the impacts in Table III reflect increases in actual productivity: how quickly workers produce plates in each hour. These results are robust to alternate empirical specifications in Online Appendix Tables A.6 and A.7. They are also robust to explicitly controlling the false discovery rate in each family of hypotheses (Online Appendix Table A.8, Panel C).

The productivity effects are concentrated among poorer workers, who increase output by 0.204 std. dev. (13.0%) following the cash infusion (Table III, column (6), $p = .003$). In contrast, we cannot reject that there is no impact on the remaining workers ($p = .819$). We also continue to find no effect during the announcement period, even among the poorer workers.

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

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

15. We do not find differential treatment effects among workers who report having loans that they are worried about, though the results are imprecisely

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

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

Finally, note that these effects capture changes in workers' total output because it is unlikely that the treatment meaningfully affected paid or unpaid work outside of the experiment. In our particular context, after a day of wage work, workers do not tend to engage in secondary work activities, including self-employment and domestic duties (e.g., collecting firewood).

estimated. Because this is one of many different causes for financial worries—and makes up only a fraction of total expenditures—this might not provide a sufficient signal of worries.

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

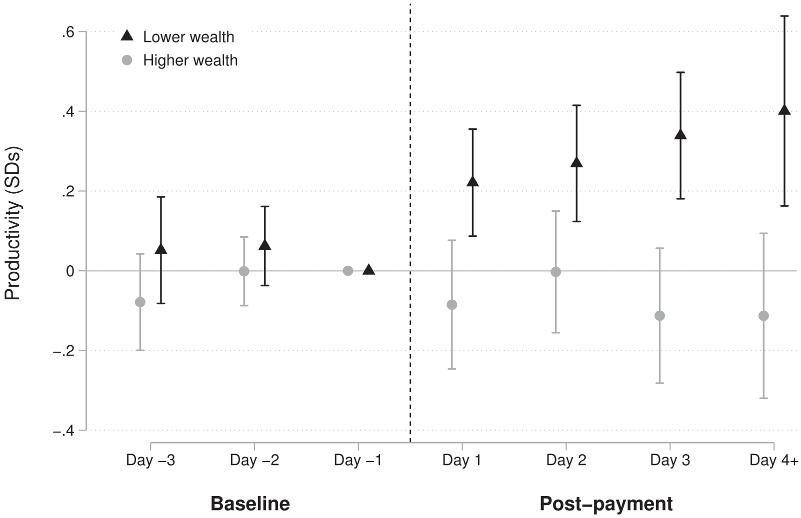

FIGURE IV

Treatment Effects of Interim Cash Payment on Worker Productivity.

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

For instance, using data from a similar population in the same regions of Odisha, Breza, Kaur, and Shamdasani (2021) find that rural casual workers reported doing any secondary activities after a day of wage work on only 1.72% of days.

VI.C. *Attentiveness at Work*

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

Receiving the interim payment increases workers’ attentiveness (Table IV). Across all workers, we find suggestive evidence of an increase in the attentiveness index of 0.077 std. dev. (column (1), $p = .092$) and an increase in the high-attentiveness indicator of 0.095 percentage points (column (2), $p = .001$). These findings are similar if we replace our attentiveness index, which averages across the component measures, with the first principal component of the three measures (Online Appendix Table A.12).

Mirroring the effects of the interim payment on productivity, the effects on attentiveness are concentrated among poorer workers (columns (3)–(5)). Among workers with below-median wealth, receiving a cash influx increases attentiveness by 0.17 std. dev. (column (3), $p = .041$). In contrast, we cannot reject no change in attentiveness among richer workers. These heterogeneity results are similar if we instead examine heterogeneity using the proxy means test measure, house quality, as our wealth indicator (Online Appendix Table A.9, columns (2) and (3)). Finally, again mirroring the effects on productivity, we detect no treatment effects on attentiveness during the announcement period; the improvements in attentiveness only emerge once the money arrives in workers’ hands.

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

We interpret these findings as suggesting that the productivity effects we observe are at least partly mediated through improvements in workers’ cognitive engagement while working.[17]

17. Consistent with the idea that improved attentiveness reflects improved cognition, we find a strong baseline correlation between workers’ attentiveness index and their performance on an incentivized memory task, Corsi, a standard cognitive test in psychology (Online Appendix Table A.13). We undertook this test

TABLE IV

EFFECTS ON ATTENTIVENESS

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

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

Workers increase their pace of work, reducing time per plate, but do so while simultaneously reducing their rate of mistakes. Such attentional effects are consistent with a range of potential psychological mechanisms that could operate by improving attentiveness at work, including cash on hand reducing worries and thus distractions during work, as well as stress, mental health, or happiness.

VI.D. Impacts of Piece-Rate Variation

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

Increasing piece rates raises productivity (Table V, columns (1)–(3)). Each one-rupee increase in the piece rate increases output by 0.020 std. dev. ($p = .042$), while a 1% increase in the piece rate leads to an output increase of 0.058 std. dev. ($p = .038$). This moderate impact is consistent with studies in other contexts, which often find modest piece-rate elasticities in real-effort experiments (DellaVigna et al. 2022). We interpret the output changes from piece-rate changes as an effort response, that is, the extent to which output can be changed by conscious effort within the context of our particular task. In contrast, higher piece rates do not alter the attentiveness measures (columns (4)–(6)). Across specifications, the point estimates are actually negative, but statistically insignificant. This may suggest an increase in mistakes when workers consciously are hurrying to make extra plates. We can reject that the output and attentiveness effects are the same:

in the supplementary piece-rate rounds only to correlate cognitive function with attentiveness. Of course, this is a simple correlation and therefore only suggestive.

TABLE V
PIECE-RATE VARIATION

	Hourly production			Attentiveness index			Attendance		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Piece rate	0.020**			−0.013			−0.000		
	(0.010)			(0.010)			(0.006)		
Log(piece rate)		0.058**			−0.035			0.002	
		(0.028)			(0.029)			(0.017)	
Piece rate = Rs. 3			0.024			−0.004			0.014*
			(0.018)			(0.024)			(0.008)
Piece rate = Rs. 4			0.040**			−0.025			−0.000
			(0.020)			(0.020)			(0.012)
p-value: equality of coefficients									
Piece rate in columns (1) and (4)	.001								
Log(piece rate) in columns (2) and (5)		.001							
Piece rate = Rs. 3 in columns (3) and (6)			.211						
Piece rate = Rs. 4 in columns (3) and (6)			.001						
N: worker-hours	4,374	4,374	4,374	4,373	4,373	4,373	898	898	898

Notes. This table tests for the impact of changing piece rates on worker productivity and attentiveness. The observations come from supplementary rounds (without the interim-pay treatment) with 150 workers. The dependent variables are normalized hourly production (columns (1)–(3)), the attentiveness index (columns (4)–(6)), and daily attendance (columns (7)–(9)). The production and attentiveness measures are normalized using the same control group mean and standard deviations as the measures in the main rounds. The piece-rate wage was randomized to be either Rs. 2, 3, or 4, so the omitted category in columns (3), (6), and (9) is a piece-rate wage of Rs. 2. On the first day of each piece-rate round, workers were paid a flat wage rather than a piece rate. The regressions in columns (1)–(6) use hourly observations after the first day, conditional on attendance. Columns (1)–(3) and columns (7)–(9) control for the same covariate controls used in Table III, column (3). Colums (4)–(6) use the same controls used in Table IV. All regressions control for round fixed effects. Standard errors in parentheses are clustered by worker. * $p < .10$, ** $p < .05$, *** $p < .01$.

a test of equality of coefficients between columns (1) and (4) in Table V has a *p*-value of .001.[18]

VI.E. Priming

As discussed, some workers receive a priming intervention, varying timing to occur before or after the exogenous interim payments (see details in Online Appendix A.2). We find limited evidence for any effects in the one or two hours immediately after workers are primed—the period when priming interventions typically have their strongest effects (Online Appendix Table A.14, columns (1)–(4)). Examining the entire day after priming, we see some suggestive but not statistically significant evidence for productivity effects among poorer workers (Online Appendix Table A.14, columns (5) and (6)). In line with limited priming effects, the treatment effect of receiving the cash infusion is similar across the three priming conditions—no priming, priming before cash infusion, and priming after cash infusion (Online Appendix Table A.15).[19] Overall, the lack of evidence of priming effects is consistent with the broader debate around both the replicability of priming and how to understand its "first stage"—both treatment intensity, which can be non-monotonic in underlying worries, and

18. In contrast, we cannot reject that the impact of interim payments is the same on output versus attentiveness (*p*-values range from .556 to .778). One may still be concerned that because productivity effect sizes are small, we may simply lack the power to detect attentiveness effects. The piece-rate effect on attentiveness is −0.013 std. dev., with a 95% confidence interval of [−0.0327, 0.0073] (column (4)). In the main manipulation, the treatment effect on attentiveness is 71% the size of the treatment effect on productivity (0.109 std. dev. versus 0.077 std. dev.). If productivity and attentiveness move together, then one may expect a piece-rate effect on attentiveness of $0.020 \times 0.71 = 0.0142$. This lies outside the above confidence interval and is 94% larger than the right-hand side of the confidence interval. Although this is not conclusive since attentiveness and productivity may not scale linearly, this back-of-the-envelope calculation suggests that power issues do not necessarily undermine our ability to detect effects. In addition, this does not shed light on whether attentiveness may respond to motivation at higher stakes or to incentives explicitly tied to the attentiveness measures.

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

what specific set of thoughts or pathways are triggered (e.g., Kahneman 2012; Cesario 2014; Banker, Bhanot, and Deshpande 2020; Sherman and Rivers 2021). Rather than using priming to direct attention as a "treatment," directly using attention as an outcome variable (as we do here) may constitute a useful design strategy for sidestepping some of these concerns.

VII. Confounds and Supplementary Tests

VII.A. *Announcement Effects and Perceptions of the Employer*

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

1. *Announcement Effects, Gift Exchange, and Fairness.* If treated workers feel they have been given a gift, they might reciprocate by working harder; conversely, if control workers feel they have been treated unfairly, they may reduce effort. While fairness considerations are undoubtedly important in a range of settings, four pieces of evidence indicate they are unlikely to drive our observed treatment effects. First, the most straightforward fairness stories would not (necessarily) imply that the treatment effects should only arise for poorer workers. Ex post one could argue that poorer workers value the "gift" more, but it is not obvious ex ante that richer workers—who also use the interim payment for immediate expenditures and debt—should not value it at all.

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

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

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

Of course, finding a lack of effects from gift exchange or fairness does not detract from their potential relevance in other

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

21. Specifically, we add the triple interaction "Cash $\times$ Payment day $\times$ Wave B" in Table VI, columns (3) and (4). Under this specification, the double interaction "Cash $\times$ Payment day" captures the payday effect for Wave A (on day 8). The triple interaction captures any incremental payday effect for Wave B (on day 9), that is, the difference between the payday effect for Wave B versus the payday effect for Wave A. Under the fairness confound, this triple interaction should be positive: control workers would be upset about having witnessed Wave A treatment workers be paid on the previous day and drop effort relative to the Wave B treatment workers (who have not yet been paid).

TABLE VI
FAIRNESS CONCERNS

	Hourly production			
	(1)	(2)	(3)	(4)
Cash × 1 day post announcement	−0.015	−0.034		
	(0.036)	(0.039)		
Cash × 2 days post announcement	0.032	0.015		
	(0.036)	(0.038)		
Cash × Announcement period			0.021	0.000
			(0.031)	(0.034)
Cash × Payment day			0.078	0.067
			(0.059)	(0.059)
Cash × Payment day × Wave B			0.007	−0.006
			(0.091)	(0.092)
Cash × Post-pay		0.110^{**}		0.109^{**}
		(0.047)		(0.047)
Post-payment period	N	Y	N	Y
p-value: Cash × Post-pay = Cash × 1 day post announcement		.009		
p-value: Cash × Post-pay = Cash × 2 days post announcement		.029		
p-value: Cash × Post-pay = Cash × Announcement period				.005
N: worker-hours	9,651	17,441	9,651	17,441

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

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

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

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

Second, we again exploit the staggered timing of cash infusion among Wave A versus B treatment workers. If workers increase output because they update their beliefs about the probability of payment, then we might expect Wave B treatment workers—who saw Wave A workers being paid—to also update their beliefs when they arrive at work on day 9. However, contrary to this story, in Table VI, columns (3) and (4), the coefficient on the triple interaction—Cash × Payment Day × Wave B—is not positive; it is close to zero and insignificant. Perhaps more problematic for a trust story, when Wave A workers are paid on day 8 as promised, it is unclear why this should not boost all workers' confidence in being paid as expected.

VII.B. Physiological Channels: Nutrition and Sleep

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

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

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

2. *Sleep*. An additional physiological channel through which the cash infusion could have affected workers is via improved sleep. At endline, we asked workers to rate sleep quantity and quality. Control workers report sleeping about seven hours a night on average. We find no evidence for an increase in the number of hours of sleep (Online Appendix Table A.17, column (1)), or self-reported sleep quality (columns (2) and (3)). The estimated effects are small in magnitude and insignificant. Moreover, Bessone et al. (2021) do not find evidence of changes in worker productivity due to increased night sleep in a low-income sample in urban India, which may be related to the low quality of sleep in low-income contexts (Rao et al. 2021).

VII.C. Mechanisms: Summary and Discussion

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

TABLE VII
TESTS FOR NUTRITION CHANNELS

	Breakfast measures (post-pay period)					Hourly production			
	Had any breakfast (1)	Ate rice (2)	Amount of rice (3)	Ate vegetables (4)	Ate any other item (5)	(6)	(7)	(8)	(9)
Cash	−0.007	−0.002	−4.048	−0.024	0.059				
	(0.013)	(0.025)	(7.223)	(0.042)	(0.044)				
Cash × Post-pay						0.060	0.173**	0.104**	0.083*
						(0.050)	(0.073)	(0.047)	(0.045)
Cash × Post-pay × Hour of day						0.014**	0.008		
						(0.007)	(0.010)		
Cash × Post-pay × Higher wealth							−0.204**		
							(0.103)		
Cash × Post-pay × Hour of day × Higher wealth							0.005		
							(0.013)		
Cash × Post-pay × Last 2 hours of day								0.013	
								(0.020)	
Cash × Post-pay × Last 1 hour of day									0.104***
									(0.026)
Control group mean	0.984	0.938	180.625	0.759	0.266				
N: workers	320	320	320	320	320				
N: worker-hours						17,441	17,381	17,441	17,441

TABLE VII
CONTINUED

	Breakfast measures (post-pay period)					Hourly production			
	Had any breakfast (1)	Ate rice (2)	Amount of rice (3)	Ate vegetables (4)	Ate any other item (5)	(6)	(7)	(8)	(9)
Coeff.: cash effect + interaction								0.117	0.187
Std. err.: cash effect + interaction								0.048	0.054
p-value: cash effect + interaction								.016	.001

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

children cry for an item they want. Any of these could divide attention and reduce the capacity to focus at work.

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

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

VIII. Conclusion

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

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

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

Ghatak 2015; Balboni et al. 2022). Our evidence suggests that direct effects of changes in financial strain could contribute to the positive effects of such interventions. Moreover, these programs may have broader social returns. Except for self-employed people, most workers are not able to internalize the returns of their productivity. Consequently, transfer programs could have supply-side multiplier effects via higher firm productivity, providing an additional rationale for subsidizing such programs.

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

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

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Supplementary Material

An Online Appendix for this article can be found at *The Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/SBHUHX (Kaur et al. 2024).

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