ONLINE APPENDIX:

DO FINANCIAL CONCERNS MAKE WORKERS LESS PRODUCTIVE?

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1 Appendix Figures and Tables

1.1 Appendix Tables

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

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

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

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

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

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

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

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

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

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

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

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

Table A.4: Correlation between Financial Worries and Happiness

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Notes: This table is continued from the previous page.

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

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

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

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

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

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

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

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

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

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

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

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

Table A.12: Effects on Attentiveness PCA Score

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

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

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

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

PANEL A: Main rounds—Productivity and Attentiveness

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

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

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

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

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

Table A.14: Effects of Priming

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

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

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

Table A.15: Effects of Cash across Priming Conditions

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

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

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

Table A.16: Trust: Effects in Later Rounds

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

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

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

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

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

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

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

1.2 Appendix Figures

Figure A.1: Top-of-Mind Pictures

Panel A

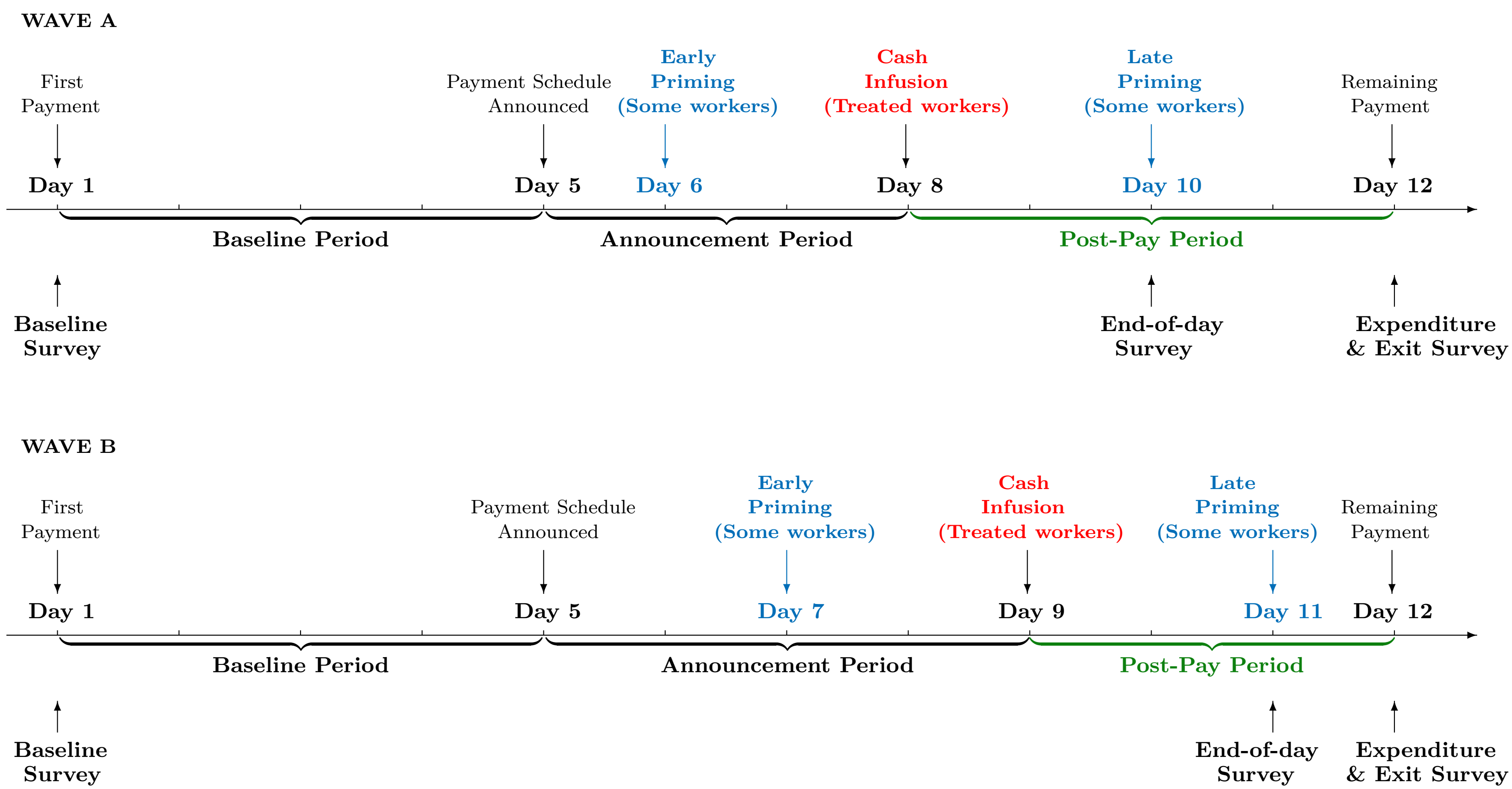
Panel B

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

Figure A.2: Leaf Plate

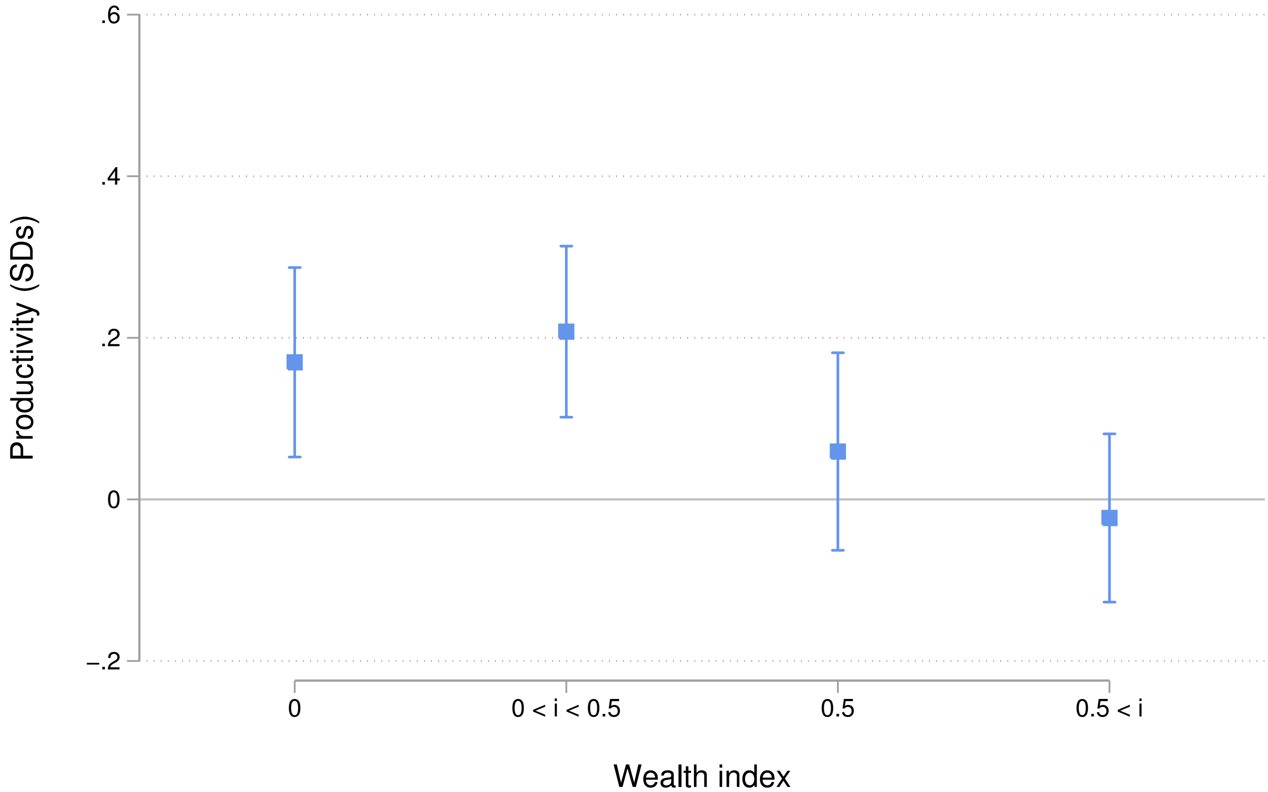
Notes: This figure shows a sal tree leaf plate akin to the ones produced by workers in the experiment. In accordance with quality standards set by partnering contractors, leaf plates were required to (i) meet a minimum size requirement, (ii) have no gaping holes, (iii) have all leafstalks (petioles)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

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



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

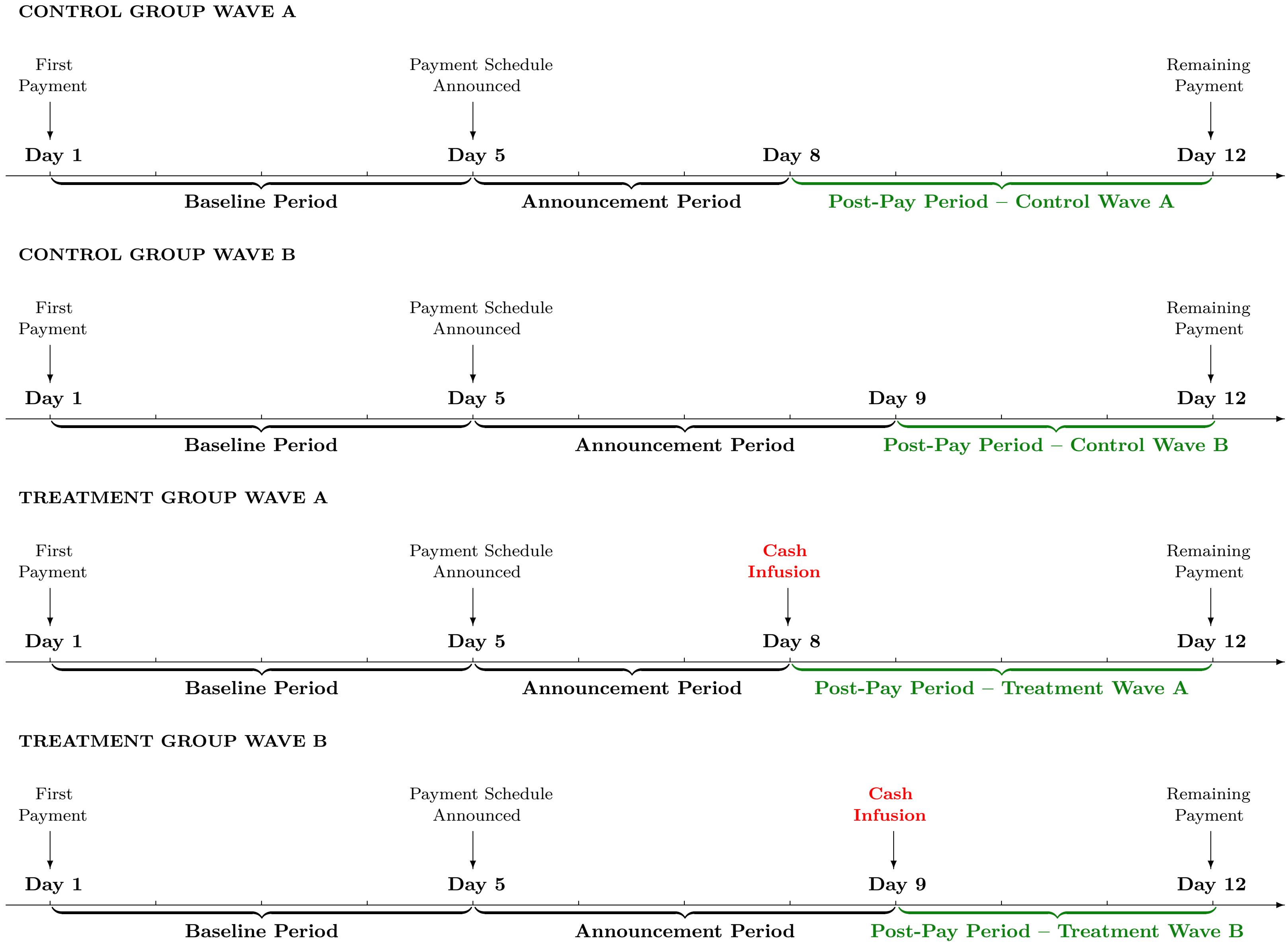
Figure A.4: Treatment Effects on Worker Productivity by Wealth Level



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

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

Figure A.5: Experimental Design – Detailed Timeline



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

1.3 Priming

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

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

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

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

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

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

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

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

2 A Simple Framework

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

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

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

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

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

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

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

2.1 Baseline model: No effect of financial strain on attentiveness

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

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

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

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

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

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

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

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

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

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

- In contrast, the treatment group is paid at the end of both periods 1 and 2, so the per-period budget constraints in periods 2 and 3 are:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

2.2 Augmented model with financial strain attentiveness channel

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

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

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

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

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

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

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

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

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

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

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

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

Taken together with the effects discussed in Prediction 3, this yields the prediction that the treatment effect in period 2 is larger than the treatment effect in period 1. In summary, the sign on the difference in output between treatment and control workers in period 1 is ambiguous due to competing effects of consumption and debt levels on the level of automatic input – but the magnitude of the difference is likely to be small. However, the treatment group should unambiguously produce more output than the control group in period 2 because both channels impacting automatic input work in the same direction. As a result, the treatment effect in the post-pay period should not only be positive but also larger than the effect in the post-announcement period.

2.3 Empirical tests of the full model with both input channels

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

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

Heterogeneity by baseline wealth. What does this model predict about heterogeneous treatment effects with respect to financial strain? For exposition, we focus only on heterogeneity in financial strain as captured by consumption levels. Similar derivations would follow for strain captured by debt levels. Assuming that automatic inputs $a(u'(c))$ are convex in marginal utility, the model predicts that the effect of the early payment on output will be largest for the poorest workers.[9] To see this, consider the treatment effect in period 2:

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

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

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

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

3 Protocols Appendix

This appendix provides additional detail on the study protocols.

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

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

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

Deviations. There were several deviations from the standard schedule:

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

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

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

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

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

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

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

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

Table A.18: Schedule and Wage Summary

PANEL A: Main Rounds Schedule and Wage

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

PANEL B: Supplementary Rounds Wage

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

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

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

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

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

4 Survey Instruments Appendix

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

End-of-day Survey

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

End-of-day Survey

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

<u>Priming Effect</u>

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

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

Expenditure Survey

SECTION A: SURVEY INFORMATION

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

Expenditure Survey

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

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

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

SECTION B: HOUSEHOLD EXPENDITURES

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

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

Expenditure Survey

SECTION C: EXPENDITURE RECALL

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

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

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

Expenditure Survey

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

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

Expenditure Survey

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

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

Expenditure Survey

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

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

Expenditure Survey

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

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

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

Expenditure Survey

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

<table>
<tr><td rowspan="3">C 3.6</td><td>(a) Breakfast</td><td colspan="3">1. [ ] Yes

2. [ ] No → Skip to C4</td></tr>
<tr><td rowspan="2">(b) What did you have for breakfast?</td><td>Item</td><td>Quantity</td><td>Unit</td></tr>
<tr><td>1. |_|_|
2. |_|_|
3. |_|_|
4. |_|_|
5. Others: ________________</td><td>1. ________________
2. ________________
3. ________________
4. ________________
5. ________________</td><td>1. ________________
2. ________________
3. ________________
4. ________________
5. ________________</td></tr>
</table>

Expenditure Survey

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

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

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

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

Expenditure Survey

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

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

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

Expenditure Survey

SECTION D: EXPENDITURE PLANNING

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

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

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

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

Expenditure Survey

	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________
	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________
	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________
	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________
	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________	Categories: ______________ Rs. ______________

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

Expenditure Survey

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

SECTION A: PERSONAL IDENTIFICATION

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

SECTION B: HAPPINESS

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

SECTION C: TOP-OF-MIND

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

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

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

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

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

(Picture A)

(Picture B)

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