

The Psychosocial Value of Employment: Evidence from a Refugee Camp[†]

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Employment may be important to well-being for reasons beyond its role as an income source. This paper presents a causal estimate of the psychosocial value of employment in refugee camps in Bangladesh. We involve 745 individuals in a field experiment with three arms: a control arm, a weekly cash arm, and an employment arm of equal value. Employment raises psychosocial well-being substantially more than cash alone, and 66 percent of the employed are willing to forgo cash payments to continue working temporarily for free. Despite material poverty, those in our context both experience and recognize a nonmonetary, psychosocial value to employment. (JEL C93, D91, I31, J15, J22, O15)

Social scientists have long posited that employment may deliver social and psychological benefits beyond the value of income alone (Morse and Weiss 1955; Jahoda 1981). Identifying the psychosocial benefits of employment has implications for a vast range of policies, from assistance schemes for the unemployed, to government responses to forcibly displaced communities, to a future of automation and the resulting shift away from traditional forms of work. While cross-sectional evidence around this question exists (Case and Deaton 2020; Olesen et al. 2013;

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Kuhn, Lalive, and Zweimüller 2009; Kessler, Turner, and House 1988), this literature encounters two key challenges. First, those who are unemployed differ from the employed in ways that are likely correlated with their psychological well-being, generating a problem of selection. Second, the income earned from gainful employment is likely to confer psychosocial value upon the worker, making it difficult to isolate the nonpecuniary means by which work improves well-being.

This paper presents a causal estimate of the psychosocial benefits of employment among a population of forcibly displaced people, the Rohingya refugees of Myanmar. We run a field experiment in which we randomize 745 camp residents of working age into three arms. In our employment arm, we offer gainful employment in the form of a surveying assignment for an average of three days per week for two months.¹ The surveying task requires workers to walk through their blocks four times per day tallying the various activities their neighbors are engaged with and consumes approximately 2.5 hours per workday, resulting in a form of part-time employment. The job is designed to embody the key features inherent to “work.” Drawing from the economics literature, workers must exert real effort and their task occupies a meaningful portion of their workday. Drawing from the sociology literature, the work involves some degree of sociability and purpose in the completion of a productive task. Employment lasts for eight weeks, a long duration given the scarce daily labor opportunities that arise in our setting.

Relative to this employment arm, our control arm receives no work and a small fee for weekly survey participation. A comparison of the control to the employment arm therefore yields the psychosocial benefits of the employment intervention. In order to estimate the *nonpecuniary* psychosocial value of employment, we include a cash treatment arm, in which no work is offered, but a large fee (equivalent to that received by those in the employment arm) for weekly survey participation is provided.

We work in the Rohingya refugee camps, situated upon the southern tip of Bangladesh. Between August and December 2017, approximately 780,000 Rohingya fled an ethnic cleansing campaign in Rakhine State, Myanmar, crossing into Bangladesh by foot or raft to build and settle into what is presently the largest refugee camp in the world. Formal employment in Bangladesh is illegal for these refugees and strict restrictions on movement limit access to informal work in nearby urban centers. Among our sample of male and female refugees between the ages of 18 and 45 years, 11 percent report having worked in the previous month. They further report spending an average of eight hours of their waking day engaged in “leisure” activities such as taking naps or sitting entirely idle. This (lack of) activity appears to be borne by circumstance rather than by choice: in our qualitative work, refugees regularly request work, and often *haather kaaj*: colloquially, handiwork; literally, a way to keep one’s hands occupied.²

Baseline data shed further light on the potential consequences of such pervasive unemployment: individuals who report having been unemployed the entirety of the

¹We obtained formal permissions from camp administration to engage our study participants in this manner through our nongovernment organization (NGO) partner, Pulse Bangladesh.

²Such expressions of the need to be occupied are not unique to Rohingya refugees. Syrian migrants in the Turkish Killis camp in 2017 echo these sentiments (McClelland 2014).

previous month are 14 percentage points more likely to qualify as depressed according to the Patient Health Questionnaire-9 (PHQ-9), the diagnostic tool we employ to assess depression severity. This correlation is vulnerable to selection into employment and conflated with the lack of income, and thus motivates our experimental methodology to answer the central question of this paper. What are the impacts of employment, beyond that of remuneration alone, on well-being?

We describe our results in four steps. First, we find that the employment arm, for which we have 100 percent take-up, generates significant psychosocial benefits relative to the control. We observe a precisely estimated 0.21 unit increase in our “psychosocial index” ($p = 0.000$), a prespecified aggregation of the standardized measures of depression, stress, life satisfaction, self-worth, sociability, locus of control, and sense of stability. Each of these subcomponents exhibits a significant and meaningful improvement as well: for example, we find that employed individuals are 10 percentage points (12 percent) less likely to be depressed and 5 percentage points (16 percent) less likely to be moderately or severely depressed. Nor are these positive effects of employment limited to the psychosocial index. We find that employed individuals are also significantly less likely to feel physically ill, perform better on memory and math tests, and are less risk averse. As a benchmark, a recent evaluation of a yearlong psychoeducation program for Rohingya refugees in the Bangladesh camps documents a 0.15 standard deviation (SD) reduction in depression (Islam et al. 2021), in comparison to the 0.24 SD reduction in depression from our employment program. As a second benchmark, individuals in our sample who experienced the death of a loved one in the indiscriminate violence in Myanmar exhibit 0.26 SD greater depression severity at baseline than their nondeath counterparts.

Second, we find that employment generates benefits that are significantly greater than that of cash alone, which yields a statistically insignificant 0.06 unit increase in our psychosocial index, consistent with recent meta-analyses of the mental health impacts of cash transfers (Ridley et al. 2020; McGuire, Kaiser, and Bach-Mortensen 2020). We can reject equality of effects between employment and cash for our mental health index ($p = 0.000$), reported physical health ($q = 0.081$), cognitive performance ($q = 0.028$), and risk tolerance ($q = 0.028$).³ These differences are substantial: employment improves mental health at a magnitude four times greater than cash alone.

Third, we find a high willingness to supply labor among study participants even at a wage of zero, suggesting that individuals are able to internalize the psychosocial benefits of employment. Through an incentivized elicitation of reservation wages for an additional week of work, we find that the majority (69 percent) of individuals are willing to work an additional week for zero pay. Among these individuals, the vast majority (77 percent) are willing to forgo an alternative low-effort activity offered by the NGO for which they can earn 200 BDT (approximately US\$2.5, or average savings at baseline) to instead continue working for no pay. We elicit these measures after eight weeks of working, with the intent of

³The measure “ q ” represents sharpened q -values, or p -values corrected for multiple hypothesis testing using Anderson (2008). The mental health index is a single index of psychosocial measures and is therefore not subject to correction for multiple hypothesis testing.

both familiarizing participants with the nature of the work and eliminating novelty effects that might arise in the early days of new employment.

We consider two potential confounds to the experiment: expectations of future work and experimenter demand effects. While we cannot rule out that our work made participants hopeful of eligibility for future work, a randomization of a certificate, intended to provide an explicit boost to the resumes of our employees, has no measurable impact on psychosocial well-being. Nor do we see differences in expected or actual employment among the treated after the experiment. To address experimenter demand, we supplement our psychosocial index with a variety of objective and incentivized measures, from memory and arithmetic questions, to the incentivized risk preference game, to the incentivized labor supply exercise. Each reiterates our findings of significant nonpecuniary benefits to employment.

To what can we attribute these gains? We explore how work makes one feel, how it shapes one's day, and how one spends their earnings. We lack experimental variation along most of these margins, so we cannot rule in or out any mechanism definitively, but we find strong evidence of a self-worth vis-à-vis the family channel: employment significantly increases one's perception of how valuable they are to their family. We find little evidence that the social element of the work or the community-centric purpose embedded in the work generate psychosocial value in our context. Nor do we see that employment alters how one otherwise spends her day, nor how one consumes her earnings, compared to those who receive cash alone.

We view our study as a proof of concept. Our results are likely to be moderated by the nature of the work we intervene with: the employment intervention is clerical rather than heavy manual labor, public rather than isolated, purposeful, engaging a low-stress employer, and part-time. They are also likely to be affected by the context in which we operate: the Rohingya community we engage are socially conservative, vastly un- and underemployed, and forcibly displaced. As such, one should be wary of extrapolating our effects into other contexts and types of employment.

That said, we design a work task that shares features common to work in the developing world: the ILO's (2019) sectoral estimates suggest that the majority of work in developing countries is social and public in nature. Among employed Indians with a grade school education or less, the ISSP (2017) finds that 43 percent occasionally or never engage in hard physical labor at work, 59 percent believe their work is helpful to society, 72 percent rate their relationship with their employers positively, and 35 (10) percent work 40 (8) hours or less weekly. Key features of our context are likewise shared by other subpopulations of interest. Constrained labor market opportunities, material poverty, and limited leisure activities are features common to many of the world's incarcerated (10.35 million), the unemployed in low-income countries (conservatively estimated at 22 million; ILO 2019), as well as many of the world's agricultural poor (300 million, many of whom suffer from seasonal scarcity in labor and consumption: see Devereux, Vaitla, and Swan [2008] for global estimates and Akram, Chowdhury, and Mobarak [2017] for a Bangladesh context).

More directly, we view our findings as relevant to the global phenomenon of forced displacement. The number of forcibly displaced persons has grown rapidly in recent years, reaching a historic high of 80 million in 2020 (UNHCR 2020). At least 70 percent of refugees reside in countries with legal barriers to the right to

work (Schuettler and Caron 2020), notwithstanding the myriad informal labor market barriers refugees may encounter. Mental health is a persistent challenge in these communities (WHO 2021): the 38 percent of our sample screened as moderately or severely depressed is comparable to, for example, Syrian refugees in Greece (Poole et al. 2018). Our study offers a prototype of a scalable form of employment that may meaningfully improve well-being for this vulnerable population.

This study makes three contributions. First, we provide a proof-of-concept causal estimate of the psychosocial impacts of employment conditional on income. There exists a long history of sociological work exploring the costs of long-term unemployment beyond that of income alone (Morse and Weiss 1955; Jahoda, Lazarsfeld, and Zeisel 1971). Conversely, a burgeoning experimental literature documents positive psychosocial impacts of employment, but is not designed to distinguish pecuniary from nonpecuniary channels (Bertrand et al. 2021). Our experiment is motivated by this literature as well as a limited stock of empirical evidence from lab-in-the-field experiments around the costs of idle time (Bhanot, Han, and Jang 2018; Hsee, Yang, and Wang 2010). We differ from this literature by designing a work task more reflective of long term employment in two ways: it requires the investment of long-duration physical and mental effort in order to be compensated, and it substitutes away from leisure, preserving a realistic outside option; individuals proceed with existing activities rather than being contained in a room with no stimulation, which Hsee, Yang, and Wang (2010) have established to be psychologically costly.

Second, this experiment offers insight into whether cashfare or workfare programs are a more cost-effective means of improving psychological well-being. While cash-based programs directly address the loss of income and are relatively straightforward to implement (Hanna and Olken 2018), they do not address the psychosocial costs that may accompany the absence of work. These costs are documented in sociology literature, first articulated in Jahoda, Lazarsfeld, and Zeisel's (1971) seminal work around Marienthal, a town in Austria devastated by deindustrialization in the wake of the global depression of the 1930s. As described by one woman who lost her job, "If I could get back to the factory it would be the happiest day of my life. It's not only for the money; stuck here alone between one's own four walls, one isn't really alive" (Jahoda, Lazarsfeld, and Zeisel 1971, p. 76).⁴ We bring an experimental lens to this question.

Finally, this study contributes to a small but growing literature that engages with refugee populations and the forcibly displaced to causally identify the impacts of various interventions through field experiments; see IPA (2020) for a sample of interventions. Among the existing set of field experiments engaging this population, the majority are psychosocial support interventions and the remainder material interventions (cash transfers, skills training, food provision, etc.). Our research is the first to examine the nonpecuniary mechanisms through which a material intervention (gainful employment) may improve psychosocial well-being. This is a valuable exercise, as aid organizations and policymakers grow increasingly concerned about the protracted nature of most displacement, which, when paired with

⁴More recently, individuals who are incarcerated—as of 2019, 2.3 million within the United States alone—describe similar experiences. "It is the dull sameness of prison life, its idleness and boredom, that grinds me down" (Morris and Rothman 1995, p. 205).

widespread unemployment, may cultivate long term discouragement and a deep lack of hope in a viable future. In addition, while employment and job training programs are common policy levers considered for migrants and those who lack economic stability, this is the first study, to our knowledge, to both probe the underlying mechanisms driving impacts on well-being and offer a benchmark against a standard cash transfer program.

I. Research Context

A. Recent Events

The Rohingya are an ethnic group that, prior to the genocide of 2017, lived predominantly in Rakhine State along the western coast of Myanmar (also known as Burma) (Blakemore 2019). The community traces their origins back to the fifteenth century, when thousands of Muslims settled in the former Arakan Kingdom, which was conquered by the Burmese Empire in 1784 (Albert and Maizland 2020). The Rohingya have since faced multiple waves of discrimination and suppression, with the first major campaign of ethnic cleansing occurring in 1978 when the Burmese military, tasked with performing a census of the border regions to determine citizenship, conducted indiscriminate attacks across Rohingya villages in Rakhine state. This led to an estimated quarter million people fleeing into neighboring Bangladesh. Subsequent ethnic cleansing campaigns in 1992 and 2012 sent additional waves of Rohingya into Bangladesh (Watch 1996).

On August 25, 2017, the Rohingya insurgent group Arakan Rohingya Salvation Army launched an attack on security force outposts in northern Rakhine, killing twelve security personnel. Within hours, Myanmar security forces responded. Satellite imagery documented the destruction of at least 392 villages (40 percent of all settlements in northern Rakhine), with 80 percent burned within three weeks. By October 2018, over 750,000 Rohingya refugees found themselves in a veritable city of makeshift tents along the southern tip of Bangladesh, stretching from Teknaf to Cox's Bazaar. The largest and most densely populated refugee camp on earth was constructed in a matter of weeks (Hussam 2019).

There are currently 34 camps in Bangladesh, each subdivided into blocks ranging in population density from 60 to 130 households. Each block is represented by a local leader (a *majhi*) who is responsible for organizing distribution efforts and serving as a liaison between humanitarian organizations and the refugee community. While refugees receive humanitarian assistance, most are unable to cover their basic needs and look for ways to supplement their income by selling their assets and the rations they receive and seeking informal work opportunities.

B. Camp Life: Descriptive Statistics

Because refugees are not legally allowed to work in Bangladesh (Bhatia et al. 2018), many remain unoccupied in the camp. Some seek occasional employment in the informal sector outside the camps, but this comes with risk as military checkpoints around the camps abound. Among the scarce employment opportunities available are day laboring in agriculture or construction, operating street stalls, pri-

vate tutoring, or assisting in NGOs' cooking centers, child-friendly spaces, or health clinics. The majority of jobs available in the camps are provided by NGOs, several of whom have organized cash-for-work programs in the camps (Mree and Homer 2019). Outside of the camps, a comparable population of Bangladeshis are likewise occupied in agriculture, fisheries, transit, or small street-side enterprises.

The average refugee in our sample is a married man below thirty years of age, with 30 percent of the sample comprised of women. Less than 50 percent received any formal education when in Myanmar 78 percent of our sample qualifies as depressed according to the PHQ-9 screening tool. A typical day in the life of a man in our sample, as understood through a recollection of time spent in the previous day on a variety of activities, consists of three hours socializing (mostly at tea stalls), an hour at the market, an hour and a half in prayer (typically in congregation at the nearby camp mosque), an hour talking on the phone, and two and a half hours spent completely idle. The remainder of the waking day is spent eating, bathing, collecting rations, and engaging in other chores. A typical day in the life of a woman is similar, though socializing happens near the home rather than at tea stalls, she spends twice as much time in prayer (typically at home), significant time with children, and slightly less time completely idle. When asked how one prefers to spend his or her time rather than sit idle, both men and women express time with children, socializing, taking walks, reading religious scripture, or taking a nap as alternatives. These same activities are described as ways of lifting one's spirits or distracting oneself from the difficulties of the past and daily life.

II. Experimental Design

Sampling Strategy.—The research team obtained permission from governmental authorities to operate in three camps in Bangladesh (5, 8W, 17), divided into several hundred blocks of 50 to 150 households.⁵ Camp authorities organized meetings with local *majhis*, or Rohingya block leaders, and the field team to ensure smooth operation of the experiment. Within each camp, we selected nonadjacent blocks to reduce the risk of spillovers, enlisting five households per block. Upon entering a given block, the field team knocked on doors, inquired if the household member (randomly preassigned to be chosen as the male or female head of household) was interested in participating in a study, and confirmed that the respondent met our eligibility criteria.⁶ The sample was chosen so that females comprised one-third of the total.⁷ In total, we assembled a sample of 745 individuals across the three camps.

⁵These permissions included the right to engage the sample in part-time work opportunities, as refugees can be engaged in cash-for-work or volunteer activities for operational needs in the camps as per the guidance on Rohingya volunteers by the Office of Refugee Relief and Repatriation Commissioner-Government of Bangladesh and Inter-Sector Coordination Group (WB 2020).

⁶We had five eligibility criteria: that the individual had not worked in the last 14 days; was within the ages of 18–45 years; was able and willing to work for two months inside the block; was not the *majhi* or a member of the *majhi's* household; and did not receive remittances from abroad. We sought to identify individuals who had not worked in the last 14 days out of equity concerns.

⁷We would have wished for an even split by gender, but found it challenging to recruit women during our pilot. The women in our sample are those for whom their husband granted permission to participate, and should be interpreted as selected along this margin.

TABLE 1—BALANCE IN OBSERVABLES ACROSS TREATMENT ARMS

	Control (1)	Cash (2)	Work (3)	(1) versus (2) (4)	(1) versus (3) (5)	(2) versus (3) (6)
Female	0.32	0.29	0.30	0.24	0.45	0.49
Married	0.82	0.81	0.76	0.34	0.04	0.31
Age	28.39	29.03	28.01	0.74	0.41	0.17
Household size	4.99	5.23	5.14	0.52	0.61	0.78
Formal education	0.48	0.44	0.51	0.70	0.14	0.07
Past agriculture work	0.62	0.66	0.65	0.74	0.64	0.92
Math ability (index)	0.00	0.01	0.04	0.91	0.43	0.25
Digit span score (total)	0.00	0.08	0.13	0.63	0.18	0.35
Well-being	0.00	0.07	0.07	0.22	0.04	0.71
Life satisfaction	0.00	-0.04	0.04	0.62	0.05	0.22
Self-worth (relative)	-0.00	0.11	0.00	0.40	0.32	0.93
Worked in the last month	0.11	0.11	0.10	0.88	0.49	0.39
Worked in Myanmar	0.72	0.72	0.76	0.57	0.81	0.38
Hours idle (average) demean	2.97	3.31	3.01	0.99	0.39	0.46
Idle feelings	1.66	1.73	1.67	0.31	0.06	0.66
Locus of control	0.00	0.10	0.15	0.35	0.07	0.47
Power perceptions	-0.00	-0.07	-0.00	0.19	0.85	0.07
Work perceptions	0.00	-0.09	-0.01	0.61	0.63	0.31
Persistent illness (> 7)	0.30	0.33	0.28	0.89	0.26	0.24
Days healthy	25.51	24.82	25.78	0.39	0.15	0.02
PHQ scale	0.00	0.13	0.00	0.31	0.80	0.18
Depression (base)	0.76	0.82	0.77	0.26	0.91	0.14
Stress (index)	-0.00	-0.13	-0.00	0.24	0.96	0.18
Number of conversations	0.00	0.01	0.02	0.85	0.68	0.46
Number of conversations +	9.25	8.96	9.94	0.34	0.69	0.07
Number of conversations -	3.45	4.04	3.84	0.45	0.40	0.88
Family injuries (Burma)	1.79	1.70	1.68	0.58	0.26	0.72
Observations	165	165	415			
Joint <i>F</i> -test				0.30	0.40	0.27

Notes: Columns 1, 2, and 3 show the average value of the variable in the respective treatment arm. Column 4 shows the *p*-value of the difference in means between the control and cash treatment groups, Column 5 shows the *p*-value between control and work, and column 6 shows the *p*-value between cash and work.

Experimental Design.—We randomly assigned 149 blocks to one of three arms, stratifying by gender (Table 1 reports summary statistics and balance across the three treatment arms). Specifically, five participants were chosen within each block, with one or two of them being women. In each case, we informed participants that the study would last eight weeks and that the field team would be checking in weekly to conduct five-minute surveys and provide compensation. We assigned 33 blocks to the control group, where participants received 50 BTD (US\$0.60) per week as compensation for answering our weekly surveys. An additional 33 blocks were assigned to the cash group, where participants earned 450 BTD (US\$5.30) per week as compensation for survey participation. Finally, 83 blocks were assigned to a work group, where we offered participants gainful employment. We compensated participants in this treatment arm with 150 BTD (US\$1.77) per day of work. Households were assigned an average of three days of work per week, resulting in 450 BTD per week on average over the course of the eight weeks and thereby equivalent to that received by the cash group. All participants were aware of the randomization process: enumerators described the three arms and displayed the random number to the participant as it appeared on their tablet, assigning the participant to his or her treatment group.

Employment Intervention Details.—We now turn to the nature of the employment we offer. Employees were asked to engage in a data collection exercise in which they completed time-use sheets describing the activities of fifteen unnamed, same-sex neighbors of their choosing four times per day. We sought first to incorporate into this task the key features of “labor” as understood in economic theory: the work was designed to require substantive effort, both physically (with repetitive movement in the outdoor heat and crowded camp pathways) and mentally (we document a high volume of mistakes made in early weeks, with work quality improving over time), and occupy a meaningful amount of time, with workers reporting 2.5 hours per working day on the job. We further sought to incorporate key features of employment as described in sociology (Jahoda, Lazarsfeld, and Zeisel 1971; Terkel 1974): the work embedded a nominal social component, with workers needing to step outside of their tents to complete their work. It also had a purpose, as sociological literature describes blue collar or service work to be perceived as valuable by most workers (Lamont 2002; Harris Poll 2018; ISSP 2017).⁸ Given this precedence, we articulated the objective of the work to be that NGOs sought to better understand the refugee experience in order to provide services and would therefore benefit from more accurate data on how refugees spend their time in the camps.

We sought to design a form of employment that was neither “too good” nor “too bad” in the context of the camps. While the task required physical and mental labor, it was not backbreaking, which would have precluded the inclusion of women in our sample. Given the multiple time-specific sheets per day, the work required attention throughout the day, but did not occupy the full workday, as most available work in the camps was likewise “part-time.” The task also involved daily accountability and quality control through the possibility of docked pay, as described in the following section, but employees were never at risk of losing their job nor of being severely reprimanded by their employer. On the margin of sociability, while the work required participants to step outside of their tents, it did not require any conversations with neighbors. Finally, the objective of the employment was framed to echo the purposefulness vis-à-vis one’s community that is inherent to most employment in camp life (building roads, constructing latrines, assisting in children’s centers, and the like). As such, our aim was to construct a form of work that was representative of “employment” broadly construed, and similar to the nature of work an individual in our context might engage in both in terms of effort cost and potential value.

Logistics of Employment Intervention.—In order to ensure that literacy was not an impediment to completing the work, we contracted an artist to design a time-use worksheet visually depicting daily activities in the camps (e.g., napping, eating, going to the market, sitting at a tea stall, sitting idle). We piloted the sheets extensively to ensure that all major activities were included (Figure 1). Upon being randomly assigned to the employment intervention, enumerators explained the work task to households and then showed the participant a five minute video designed by the artist and research team to reiterate the task.

⁸In fact, recent empirical work by Soffia, Wood, and Burchell (2021) finds that 94 percent of workers occupying positions perceived by the sociology literature to be “useless” or “alienating” regard their work as meaningful as well.

We asked that households complete their work on specific days to which they were assigned: work schedules varied week to week, averaging three days weekly. To ensure compliance with the work schedule, we stationed a tamperproof box in a preselected household within each block (the facilitator household) and informed participants that they should submit their tasks into the box at the end of each assigned workday. The facilitator would slip an additional piece of paper into the box at the end of the day to bookend that day's set of submissions, and the respondent's submission was marked late if it was inserted after the bookend. Facilitators were compensated with an additional 50 BTD per week for their services and had no access to the materials inside the box.

Along with dropping off their submissions at the end of each workday, participants were instructed to visit the facilitator's home on their designated "collection day" each week. The facilitator made their home available for a few hours on this day so the enumerator could complete the check-ins with the block's five respondents and pay the participants their respective amounts in a relatively private setting. In blocks assigned to the work treatment, the enumerators first checked the respondents' work (e.g., the number of pages they submitted; whether worksheets were submitted on the correct dates; and the number of mistakes made per sheet). Checking for mistakes involved assessing that the correct number of tick marks were present (corresponding to the number of individuals the participant was asked to survey); whether the patterns across days were identical or distinct (whether sheets had been copied); and whether the handwriting was consistent (whether the work was completed by someone else).⁹ At the end of the interaction, enumerators were instructed to examine the respondents' performance over the previous three weeks. If the work had not been completed correctly three weeks in a row, the enumerator did not pay the participant for that week. Payment occurred at the end of the interaction after the enumerator had administered the standard weekly survey.

Interpreting the Magnitude of Cash Interventions.—As described previously, we offer both the cash and the employment arms 450 BTD per week for two months. At 1,800 BTD per month (US\$60 purchasing power parity [PPP]), this amount is slightly larger than the cost to the World Food Program of the per-refugee monthly ration provision of lentils, oil, and rice.¹⁰ Despite widespread complaints of insufficient provisions, refugees regularly resell portions of these rations—at discounted prices to host community members—to secure the cash required to purchase other basic staple foods such as salt and vegetables. Given that the World Food Program provisions are the only reliable rations that refugees receive, we approximate a cash transfer of 450 BTD per week to at least double potential weekly consumption.

Relative to the wealth refugees possess, 450 BTD per week is likewise sizable: average baseline savings is 195 BTD, with the median refugee reporting 0 BTD in savings. Average baseline borrowing (typically in the form of store credit) is 1,600

⁹We did not have auditors in the camps watching our workers given both logistical infeasibility and concern that workers may feel insecure.

¹⁰In 2019, Rohingya refugee households with one to three members received 30 kilograms (kg) of rice, 9 kg of lentils and 3 liters of cooking oil, with these provisions made monthly. Using the upper bounds on the market price of rice (BDT 60 per kg), lentil (BDT 140 per kg) and soybean oil (BDT 130 per kg), the monthly rations can be estimated at approximately BDT 3,450 for two, or BDT 1,725 per adult.

BTD, with a median of 600 BTD. Refugees have no economically meaningful assets that may be more common among the rural poor, such as land or cattle, given the unanticipated displacement which forced them from their homes. Relative to other employment opportunities, the average reported pay is 300 BTD per day for less than three days. The monthly cash transfer is therefore more than double what a refugee might expect from alternative employment if he or she is fortunate enough to secure a job.

III. Data Collection and Survey Instruments

Timeline and Survey Instruments.—We collected data via a baseline, commencing in November 2019, and endline survey, commencing in February 2020, as well as seven midline surveys conducted prior to payment disbursement each week. These weekly surveys collected a small subset of well-being outcomes. In an effort to ensure that our temporary interventions had no unintended negative mental health consequences on our participants, we also conducted a final short follow-up survey six weeks after the interventions concluded (online Appendix Figure A1). We had 2 percent attrition at endline and followup, neither differential by treatment arm (online Appendix Table A1).

Main Outcome Variables.—Our primary outcome of interest is psychosocial well-being, which we assess through an index of seven mental and social health measures, henceforth referred to as the psychosocial index: depression, stress, life satisfaction, locus of control, sociability, self-worth, and stability. Our measures of depression, stress, life satisfaction, and locus of control are drawn from standard screening tools (PHQ-9, Cohen's Perceived Stress Scale, Diener's Satisfaction with Life Scale, and the Levenson Multidimensional Internal Locus of Control Scales, respectively) adapted for sensitivity to the Rohingya camp context (see online Appendix C for adjustments). The PHQ, our depression screening tool, has been validated against antidepressant medication (Löwe et al. 2006) and employed in the cross section among refugee populations (Poole et al. 2018) as well as in experimental evaluations of psychotherapy programs in South Asia (Patel et al. 2017; Bhat et al. 2021).

For sociability, we inquire about the number of interactions that participants have had throughout the day prior to the survey day. We develop our own questions around self-worth rather than employing the more standard Rosenberg Self-Esteem Scale, which we found inappropriate given the Rohingya's recent experiences. Specifically, we construct an index of self-worth from two questions designed to elicit respondents' beliefs about how they contribute to their family and community. Finally, we adapt the Cantril Self-Anchoring Striving Scale (Cantril 1965) to measure how stable respondents feel in their present lives and in the future.

We additionally examine the impacts of each treatment on physical health, cognitive function, economic decision-making, time use, and consumption. We capture respondents' sense of physical health by asking how many days they have fallen sick in the past 30 days and cognitive function by employing a digit-span memory test and a series of basic arithmetic problems. We explore economic decision-making along two dimensions: incentivized time preferences (Andreoni and Sprenger 2012;

Giné et al. 2018) and incentivized risk preferences (Holt and Laury 2002). We measure time use through the number of hours in the previous day a respondent reports spending idle, as well as the amount of time spent on a variety of other common activities one might do in the camps (including bathing, market, chores, collection of rations, eating, child-rearing, sitting at tea stalls, praying, sleeping, visiting friends/relatives, playing games, and playing sport). Finally, we ask respondents how much they consume, borrow and save over the past week.

We further consider changes in perceptions on gender and power in two ways. First, we generate a household power index, composed of a set of questions on perceptions of gendered decision-making and intimate partner violence. The questions are drawn from Haushofer and Shapiro (2016), which are themselves adapted from the Demographic and Health Surveys. In addition, we produce a work rights index, composed of questions around whether respondents feel that women should be allowed to work inside or outside the home or the camp block.

Each outcome is described in greater detail in online Appendix C. The frequency with which each outcome is collected is also presented in online Appendix C.

Multiple Hypothesis Testing.—We utilize two approaches to address the issue of multiple hypothesis testing. First, we present our primary outcome, psychosocial well-being, as an inverse-covariance weighted index variable following Anderson (2008). We also generate index variables for other outcomes in which this is possible, such as the cognitive index, the household power index, and the work rights index. Our second strategy is to report the sharpened false discovery rate q -values for all outcomes within a particular table, which control for the expected proportion of rejections that are type I errors, likewise described by Anderson (2008).

Preanalysis Plan (PAP).—This study was preregistered in the American Economic Association Registry and successfully underwent the prerresults review process at the *Journal of Development Economics*. The experiment was run prior to the completion of this review process, but all postbaseline data was held with Innovations for Poverty Action administrators and not released to the authors until the prereview process and PAP were complete, which occurred in March of 2021. Deviations from the PAP are minor and described in detail in online Appendix B.

IV. Experimental Results

A. Completion of Work

We first establish that participants in the employment arm engaged in the work they were offered. Figure 2 exhibits the fraction of individuals in the employment arm who completed their work (panel A) and made any mistakes (panel B). No week exhibits below a 98 percent completion rate, indicative of participants' desire to engage in the work. Mistakes were common in the early weeks of employment, but declined to hover around 5 percent from weeks three through eight. This suggests both that the task required some effort, such that many respondents had to learn how to perform well, and that respondents invested this effort and maintained a reasonably high quality of work throughout the experiment. Work quality is further

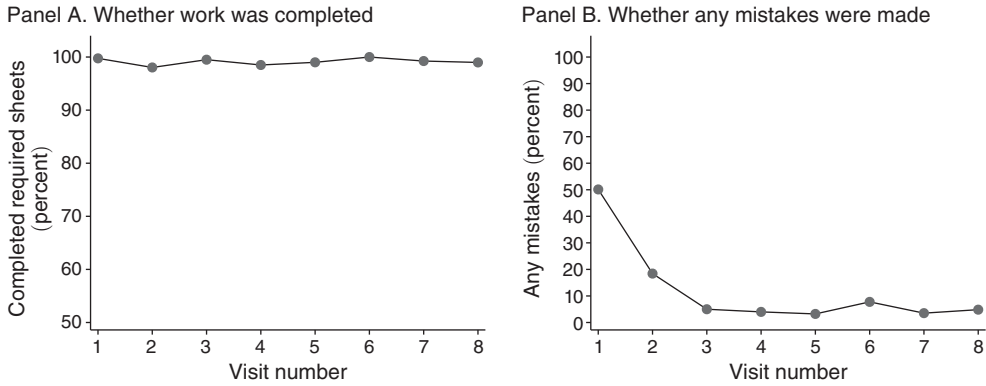


FIGURE 2. WORK COMPLETION MEASURES

Notes: This figure exhibits the fraction of individuals in the employment arm who completed their work (all sheets for all workdays were accounted for, submitted on the correct days, each week) (panel A), or made any mistakes (mistakes included: handwriting was different across sheets, indicating that someone else completed their work for them; the number of tally marks did not equal 15; or the distribution of the tally marks was identical or nearly identical across sheets, suggesting lack of effort) (panel B).

reflected in the frequency of docked pay, which peaks at less than 2 percent (not shown), resulting in individuals in the work treatment arm receiving nearly exactly as much in remuneration as those in the cash treatment arm over the course of the intervention.

B. Empirical Framework

We now estimate the effects of the cash and work treatment using the following regression:

$$(1) \quad Y_{ibc}^1 = \beta_0 + \beta_1 \text{Cash}_{ibc} + \beta_2 \text{Work}_{ibc} + \sigma_g + \gamma_c + \delta_e + Y_{ibc}^0 + X_{ibc} + \epsilon_{ibc},$$

where Y_{ibc}^1 represents the relevant outcome for individual i in block b and camp c , X_{ibc} is a vector of sociodemographic controls selected via double-selection least absolute shrinkage and selection operator (LASSO) to maximize precision following Belloni, Chernozhukov, and Hansen (2014), and ϵ_{ibc} is an error term clustered at the block level. We include fixed effects for gender σ_g , camp γ_c , and enumerator δ_e .¹¹ We control for the baseline value of the outcome variable Y_{ibc}^0 when available in an analysis of covariance (ANCOVA) specification following McKenzie (2012). Our coefficients of interest are β_1 , the impact of cash, and β_2 , the impact of work. We evaluate whether there exist nonpecuniary benefits to work through a test of equality between these two, $\beta_1 = \beta_2$.

¹¹ We follow Di Maio and Fiala (2019) and include enumerator fixed effects to account for the fact that respondents' answers may be influenced by the way enumerators ask more sensitive questions.

TABLE 2—IMPAIRMENTS ON PSYCHOSOCIAL WELLBEING

	Individual components of PS index							
	PS Index (1)	PHQ (2)	Stress (3)	Life sat. (4)	Social (5)	Self-worth (6)	Control (7)	Stability (8)
Work	0.211 (0.041)	0.237 (0.075)	0.237 (0.086)	0.293 (0.073)	0.172 (0.081)	0.137 (0.082)	0.233 (0.082)	0.247 (0.075)
Cash	0.056 (0.050)	0.011 (0.088)	0.056 (0.097)	0.229 (0.084)	0.084 (0.098)	-0.090 (0.090)	0.040 (0.103)	0.066 (0.097)
Sharp. <i>q</i> -val: work		0.004	0.005	0.001	0.012	0.029	0.005	0.004
Test: work = cash	0.000	0.006	0.018	0.326	0.292	0.002	0.026	0.032
Shrp. <i>q</i> -val: work = cash		0.019	0.031	0.103	0.103	0.013	0.033	0.033
Observations	726	726	726	726	726	726	726	726

Notes: This table reports the treatment effect of the employment and cash arms on psychosocial (PS) outcomes. Each column shows the ordinary least squares (OLS) estimates of equation (1) including dummies for each treatment arm and controlling for the baseline measure of the dependent variable (ANCOVA), camp and enumerator fixed effects, and sociodemographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014). Each row presents the coefficients of the specified treatment relative to the control group (which received a small weekly cash payment). All outcomes are standardized. The PS index (column 1) is an inverse-covariance weighted index of the outcomes of columns 2–8. “PHQ” is the standardized score of the PHQ-9 depression screening tool, but with the sign flipped, such that higher values represent reductions in depression severity. Similarly, higher values for “stress” represent reduced stress. All remaining outcomes are likewise presented such that higher scores represent improved well-being. Standard errors are clustered at the block level. The row labeled “shrp. *q*-val: work” reflects *p*-values, referred to as “sharpened *q*-values,” adjusted for multiple hypothesis testing using the false discovery rate procedure of Anderson (2008) for all outcomes in this table. The row labeled “test: work = cash” reflects the unadjusted *p*-value for the test of equality between the work and cash arms, while the row labeled “shrp. *q*-val: work = cash” reflects the adjusted *p*-value for this test of equality.

C. Impact of Employment

Table 2 presents the treatment coefficients of equation (1) for psychosocial outcomes. Relative to those in the control group, individuals in the employment arm experience a 0.21 unit improvement in their psychosocial index, significant at the 1 percent level. Each standardized subcomponent of the index exhibits significant and meaningful improvements as well. Those offered employment experience a substantial reduction in symptoms of depression, as captured by the PHQ, as well as feelings of anxiety or frustration, as captured by the stress index (both signs flipped such that positive coefficients reflect improvements). They exhibit greater life satisfaction (life sat.), are more sociable, possess higher self-worth, feel greater control over events in their lives, and feel more stable in the present and the anticipated long-term future.

As a first benchmark, a randomized evaluation of a yearlong psychoeducation program for Rohingya refugees, which included 44 weekly sessions of counseling and was implemented at approximately the same time in the Bangladesh camps as this study, documents a 0.15 SD reduction in depression severity among treated mothers (Islam et al. 2021), relative to the 0.24 SD reduction we document from the employment intervention. As a second benchmark, we find that individuals who experienced the death of a close friend or family in the indiscriminate violence in Myanmar prior to fleeing (for whom we can demonstrate balance on observables relative to those who did not experience death, see online Appendix Table A3 and online Appendix D) exhibit 0.26 SD greater depression severity than their luckier counterparts. The employment program therefore appears to reduce depression

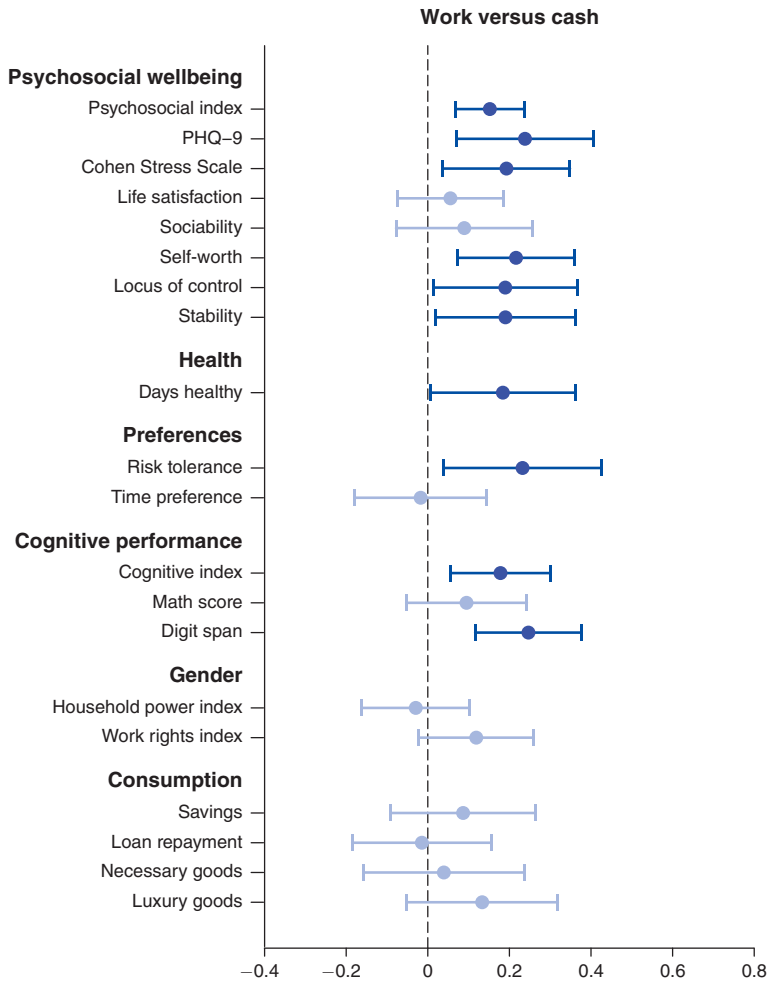


FIGURE 3. MAIN EMPLOYMENT TREATMENT EFFECTS RELATIVE TO CASH

Notes: This figure plots the point estimates and 95 percent confidence intervals for each outcome in the work treatment group relative to the cash group. All outcomes are standardized.

severity by nearly as much as being spared the death of a loved one in Myanmar. To provide context on which dimensions of depression severity were most affected we report effects for each subcomponent of the PHQ module in online Appendix Table A2.

The employment arm not only improves psychosocial well-being relative to the control arm, but also yields significantly larger improvements in psychosocial well-being than the cash arm. Figure 3 offers a visual representation of these nonpecuniary effects (while online Appendix Figure A2 presents the same relative to the control group). The cash treatment yields an imprecisely estimated 0.06 unit increase in the mental health index, and we can reject equality of effects between employment and cash at the 1 percent level. This result is manifested across all

TABLE 3—IMPACTS ON REPORTED PHYSICAL HEALTH, COGNITIVE HEALTH, AND PREFERENCES

	Days healthy (1)	Cognitive index (2)	Risk tol. (3)	Time pref. (4)
Work	0.789 (0.397)	0.231 (0.079)	0.239 (0.104)	−0.032 (0.112)
Cash	0.107 (0.467)	0.071 (0.088)	−0.014 (0.120)	−0.013 (0.117)
Shrp. q -val: work	0.059	0.018	0.047	0.453
Test: work = cash	0.075	0.011	0.009	0.822
Shrp. q -val: work = cash	0.081	0.028	0.028	0.300
Observations	726	726	726	726

Notes: This table reports the treatment effect of the employment and cash arms on physical, cognitive, and risk and time preference outcomes. Each column shows the OLS estimates of equation (1) including dummies for each treatment arm and controlling for the baseline measure of the dependent variable (ANCOVA), camp and enumerator fixed effects, and socio-demographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014). Each row presents the coefficients of the specified treatment relative to the control group (which received a small weekly cash payment). “Days healthy” is a self-reported measure of the number of days the respondent reports feeling healthy (not sick) in the previous 30 days. “Cognitive index” is an inverse-covariance weighted index of forward and backward digit span tests and two arithmetic questions. “Risk tol.” represents an incentivized risk-preference elicitation game in which higher values represent greater risk tolerance, while “time pref.” represents an incentivized time preference elicitation game in which higher values represent greater patience. Standard errors are clustered at the block level. The row labeled “shrp. q -val: work” reflects p -values, referred to as “sharpened q -values,” adjusted for multiple hypothesis testing using Anderson (2008) for all outcomes in this table. The row labeled “test: work = cash” reflects the unadjusted p -value for the test of equality between the work and cash arms, while the row labeled “shrp. q -val: work = cash” reflects the adjusted p -value for this test of equality.

subcomponents of the index, with the exception of life satisfaction, which increases significantly under both treatment arms. In other words, the psychosocial value to employment appears to be driven largely by the nonpecuniary dimensions of the employment experience.

D. Impacts of Employment on Reported Physical Health, Cognitive Function, and Economic Decision-Making

The positive effects of employment extend to other measures beyond psychosocial health. Table 3 presents results on reported physical health, cognitive function, and incentivized measures of risk and time preference. We observe a significant increase in the days reported healthy. This effect may be due to “real” health improvements from increased exercise (which has also been documented to translate to improved mental health in Herbert et al. [2020]) from the employment task or “perceived” health improvements in which improved psychosocial well-being translates into feeling less physically ill. Should the channel be exercise, we may expect health improvements to grow over time. Our weekly data on days healthy suggests this is not the case: we observe the treatment effect on health from the first week of working, and the gap remains steady throughout the following two months (online Appendix Figure A3).

The employment arm also significantly improves cognitive function as measured through an index of memory and basic arithmetic tests, a finding consistent with a large psychology literature documenting the relationship between cognitive processes and depression (Semkovska et al. 2019). As with physical health, improvements to cognitive function are unlikely to be a direct product of the employment task itself, which was specifically designed to require no literacy or mathematical skill. Rather, these results are suggestive of a downstream impact to reducing depression through the experience of employment.

Finally, we find no change in time preferences: treated individuals are no more or less likely to discount the future relative to control counterparts, although results may have differed had we engaged participants in an effort- or consumption-based time preference game rather than a financial one. However, we find a substantial increase in risk tolerance among the employed. A greater preference for risk taking may be indicative of employment serving as a form of psychological “insurance” that allows participants the mental bandwidth to exercise greater risk. This is consistent with the positive impacts of employment on stability as well as with a key motive underlying universal basic income in the developing world (Banerjee, Niehaus, and Suri 2019). Interestingly, however, we document no parallel increase in risk tolerance in the cash transfer arm. Our result on risk preference also echoes a potential consequence of depression and anxiety described in Ridley et al. (2020), although empirical evidence on this relationship remains mixed (Cobb-Clark, Dahmann, and Kettlewell 2019; Bayer et al. 2019).

As with psychosocial health, impacts of the employment arm are significantly larger than those of the cash arm for our physical, cognitive, and risk preference outcomes; impacts of cash alone again remain noisy and close to zero, and we can reject equality between the two arms for each of these outcomes at the 10 percent level, at least. The effects we document therefore appear to be driven by the nonpecuniary value of employment.

Reassuringly, we find no evidence of negative impacts to the withdrawal of the work or cash interventions in our six week followup (online Appendix Table A4). Instead, we find evidence of some positive persistence, with formerly employed individuals reporting significantly greater well-being, control, and physical health, and less stress, than their control counterparts six weeks after the interventions concluded.

E. *Labor Supply*

We estimate significant benefits of employment on psychosocial, physical, and cognitive well-being, and these effects appear to be driven largely by the nonpecuniary dimension of the experience. We now examine whether individuals *perceive* the nonpecuniary benefits of employment through an incentivized labor supply elicitation exercise conducted after the conclusion of the eight-week intervention.

Having experienced the work task and therefore able to realistically value the work, we offer individuals in the employment arm an additional (surprise) week of work at a series of wages following the incentivized Becker-DeGroot-Marschak method. We inform participants that we have a limited amount of funds remaining and are therefore unable to pay everyone their previous wage. This strategy realistically motivates

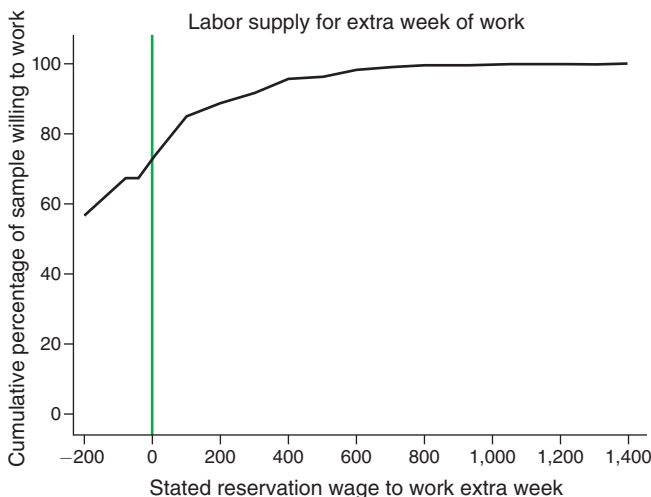


FIGURE 4. LABOR SUPPLY CURVE

Notes: This figure presents the cumulative distribution function of the reservation wage expressed by employment treatment participants for an additional week of work using the incentivized Becker-DeGroot-Marschak mechanism. The horizontal axis is in units of Bangladeshi BTd. The vertical dotted line represents the point at which individuals express a willingness to work one additional week for zero pay. Negative reservation wages are a measure of how much respondents are willing to forgo earning in an alternative (minimal effort) task in order to continue working for one week with no pay.

the reservation wage elicitation exercise and makes clear that there will be no further opportunities for work. We piloted this exercise extensively. To maximize comprehension, we employ a multiple price list strategy, embed repeated confirmations, and conduct a trial run of the exercise for each respondent before the real exercise; this mimics the procedure employed in Burchardi et al. (2021) for which participants in another low-income country field context exhibited high comprehension.

For those individuals who express willingness to work at a wage of zero, we offer an alternative option of answering a brief survey at the end of the week for a small randomized fee; we then use the fraction of respondents who are willing to forgo this paid option and instead work for free as an estimate of the proportion of respondents who have a negative reservation wage of at least the forgone magnitude.

Figure 4 presents a cumulative distribution of the expressed reservation wages among these individuals. Ninety-seven percent of those in the employment arm express interest in working the additional week. Seventy-three percent of those who are interested in working express willingness to work for a wage of zero. Seventy-eight percent of those who are then offered an alternative of 200 BTd (US\$2.5) in compensation for answering a brief survey *continue* to prefer to work for free.

As we did not offer compensation for the alternative beyond 200 BTd, we cannot deduce the precise negative reservation wage for the majority of our sample, but instead view 200 BTd as an upper bound. In other words, we find that 70 percent of all former workers value the nonpecuniary benefits of additional employment at greater than zero (assuming some nonnegligible cost of effort to working), and 55 percent of former workers value these benefits at greater (and given the shallow slope, potentially substantially greater) than 200 BTd, approximately equivalent to

the average held in baseline savings. Notably, 99 percent of all those who expressed interest in working completed the work in the following week at the wage drawn in the elicitation exercise. These results suggest that participants understand well the psychosocial value of this employment opportunity.

F. *Potential Confounds*

Our results are indicative of the presence of significant nonpecuniary benefits to employment. We investigate two potential challenges to this interpretation: the employed develop differences in expectations of future work relative to the cash only individuals, or the employed exhibit differential surveyor demand effects.

Expectations of Future Work.—Despite repeated reminders that the work opportunity we provided would last no more than eight weeks, there remains a possibility that those in the work treatment believed that current employment may make future employment more likely. In other words, employment may carry monetary benefits beyond those of the immediate income received, either through the relationship formed with the NGO or through a boost in the beneficiary's "resume" which makes them more appealing to other potential employers. While resumes are scarce in the camp context and thus an unlikely channel through which the differential benefits of employment might transpire, we sought to estimate such effects by randomizing, at the block level, the provision of paper certificates to half of all participants.

These certificates provided documentation of the beneficiaries' involvement with our project, intending to serve as an explicit boost to their resume (online Appendix Figure A4). If employed individuals derive psychosocial benefits from the expectation of future work, the certificate was designed to make this expectation salient.¹² Table 4 presents the results. Those in the work arm who receive a certificate exhibit no additional improvements in psychosocial well-being.¹³

While helpful, this is not conclusive: perhaps the certificate was not an effective way to raise the perceived probability of future work. We therefore additionally estimate the expected and actual likelihood of employment after the intervention. Online Appendix Table A5 presents the impacts of each treatment arm on postintervention labor market experiences. Six weeks after the intervention, those who were formerly employed are no more likely to find work, receive a higher wage, nor expect work or a higher wage in the future than their cash counterparts.

Experimenter Demand Effects.—Are the effects of employment driven by a desire to please enumerators for the employment opportunity received? We offer three reasons why we view this as unlikely. First, our key margin of comparison is between those in the employment arm and the cash arm, both of whom receive an

¹²The signaling value of the certificate may have been diminished if other employers learned about the nature of the certificate distribution. Our time in the field suggests this is unlikely: we randomized certificate distribution at the block level to limit spillovers, only 5 people in each block of approximately 200 adults were involved in the experiment, and job opportunities were scarce.

¹³The certificate read "I engaged with Pulse Bangladesh to do data collection." It was written this way in order to be generic enough to apply to all the individuals in the experiment, all of whom were providing us data from the weekly surveys.

TABLE 4—EFFECT OF RECEIVING PARTICIPATION CERTIFICATE

	Psychosocial index
Cash	0.098 (0.062)
Work	0.211 (0.055)
Cash × certificate	−0.075 (0.097)
Work × certificate	0.006 (0.085)
Certificate	−0.025 (0.072)
Observations	726

Notes: This table reports the treatment effect of receiving employment and receiving a certificate (and the interaction). In order to control for potential reciprocity effects, we provided these certificates not only to a subset of our employment arm, but additionally to a randomized subset of cash and control arms. The column shows OLS estimates of a regression of the psychosocial index on dummies for work, cash, certificate reception, the interactions, as well as the baseline measure of the psychosocial index, camp and enumerator fixed effects, and socio-demographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014). Standard errors are clustered at the block level.

equivalently substantial sum of cash. As such, both groups are equally “indebted” to the experimenter and therefore equally vulnerable to demand effects. Those in the cash arm may arguably feel more indebted, as they receive cash without completing daily work in return.

Second, we complement our psychosocial index with measures that are not vulnerable to experimenter demand. Demand effects are unlikely to alter one’s cognitive ability as measured through the arithmetic questions and memory tests of our cognitive index. Our risk and time preference games are incentivized with meaningful stakes (respondents gamble with a minimum of US\$1.20 in the risk preference game and trade off US\$3.50 today with higher amounts tomorrow in the time preference game); stake sizes that de Quidt, Haushofer, and Roth (2018) have found effectively eliminate demand effects.

Perhaps employed individuals feel a need to impress the enumerator, as their proximate employer, in a way cash recipients do not. This may lead to reporting better mental and physical health and investing greater effort in the cognitive tasks. However, we find that life satisfaction increases substantially for both groups, inconsistent with a differential desire to impress among the employed. We also observe patterns of treatment effects within our validated PHQ-9 module that are inconsistent with experimenter demand (online Appendix Table A2): effect sizes vary substantially across each question despite similar implications for one’s ability as an employee. Furthermore, an exercise in Section IVG documents that well-being shrinks the longer it has been since an employee worked *within* a given week, a pattern consistent with real changes in mental health related to the experience of employment.

Finally, our incentivized labor supply elicitation exercise offers a revealed preference validation of the psychosocial effects of employment. Ninety-seven percent of former workers express desire to work an additional week: 99 percent of these

individuals complete this additional work, with 70 percent willingly doing so for no pay and 55 percent forgoing at least US\$2.5 to work for no pay. Such behavior underscores that the self-reported psychosocial measures are plausibly reflective of an internalized nonpecuniary value to the experience of employment.

G. Exploring Underlying Mechanisms

The primary objective of this study is a causal estimation of the psychosocial value of employment, broadly construed, beyond that of income alone: we present a proof of concept that employment generates nonpecuniary psychosocial benefit. We now explore: what about employment does so?

We first ask: is the nature of the work itself relevant? In other words, could labeling one group “employed” and paying them over two months for an instance of work have generated the same psychosocial value as what we estimate? To test this, we exploit individual and temporal variation in the days that an employed individual is assigned work.¹⁴ We run the following regression:

$$(2) \quad Y_{ibct} = \beta_0 + \beta \text{DaysSinceWork}_{ibct} + t + \eta_i + \gamma_c + \delta_e + \epsilon_{ibct}$$

where Y_{ibct} represents weekly measures of well-being; $\text{DaysSinceWork}_{ibct}$ represents the number of days between the day of the weekly survey and the most recent day of employment; η_i is individual fixed effects; t is a weekly time trend, and γ_c , δ_e , X_{ibc} , ϵ_{ibct} are as defined above. If the actual experience of working, rather than simply the identity of being employed, affects psychosocial well-being, we expect our coefficient of interest β to be negative: the longer it has been since one last worked, the less one benefits from being employed.

Results are presented in Table 5. One additional day away from work is associated with a reduction in well-being of 0.098 SD ($q = 0.001$) and increase in stress of 0.064 SD ($q = 0.001$). This suggests value in the experience of working, and therefore the nature of the work itself.

Given the role of the experience of working itself, we next consider what features of the work may alter one’s psychosocial state. We first examine how working makes one feel: the self worth imparted by working (through the sense of purpose experienced by the job) vis-à-vis the family and the community, and the sociability facilitated by the job. We then consider features that impact how the work shapes one’s day: the structure imposed by a work schedule and the occupation of time. Third, we consider how working shapes consumption choices, as what is consumed from wages may differ from what is consumed from a cash transfer. Because we lack experimental variation along most of these margins, we cannot definitively rule in or out singular channels. Our evidence suggests that the role of employment in conferring self-worth, upon an individual within the eyes of their family, may be particularly impactful. Finally, we investigate how different workers gain from working, where we consider their gender, baseline exposure to violence, depression and extroversion.

¹⁴Note that every worker works an average of three days per week; we incorporate variation in the number of days per week and across workers *in a given week* in order to embed some unpredictability in work schedules, for the purposes of a subrandomization on structure in work described below.

TABLE 5—PSYCHOSOCIAL IMPACT OF DAYS SINCE LAST WORKED

	Wellbeing (1)	Stress (2)	Sociability (3)
Days since work	−0.098 (0.013)	−0.064 (0.012)	0.042 (0.019)
Shrp. q -val: days since work	0.001	0.001	0.009
Observations	3,148	3,165	3,165

Notes: This table reports the impact of an additional day since a respondent last worked on the three margins of psychosocial wellbeing which were collected in the weekly surveys. Each column shows the OLS estimates with individual, camp, and enumerator fixed effects, a weekly time trend, and sociodemographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014). The sample includes only those in the employment treatment arm. Standard errors are clustered at the block level.

How Working Makes One Feel

Self-Worth vis-à-vis the Family: We document a significant nonpecuniary impact of employment on self-worth. Our self-worth index asks the following question separately for one’s family and one’s community: “Think of the individual you most respect and who brings greatest value to your family (community). If they are ranked 10, where would you rank yourself?” We find that baseline mental health is highly correlated with how one ranks themselves within their family at baseline (online Appendix Table A6). Consistent with this, the nonpecuniary impact of the employment treatment on family-rank is large, at 0.17 SD ($q = 0.01$) greater than the cash arm (column 1 of Table 6). Notably, this is a combination of a 0.11 SD increase in ranking among the work group and a 0.06 SD *decrease* (though imprecise) among the cash group.

The large impact of employment on family status, paired with the potential disempowering effect of cash, hints at these improvements being gendered: because males are the traditional breadwinners in our context, deprivation of that role and receipt of a “handout” may be particularly costly for them.¹⁵ Indeed, we find in Table 7 (columns 5 and 6) that, while women exhibit improvements in their family ranking under both employment and cash arms, males exhibit a substantial increase in their family ranking from employment and a reduction from cash. The nonpecuniary impact of employment on self worth appears to be driven almost entirely by men, who exhibit greater (self-perceived) sense of self-worth within their family when employed, but reduced family status from cash alone.

Self-Worth vis-à-vis the Community (“Status”): The job embedded features that may have conferred a sense of self-worth upon our employees in the eyes of their *communities*. For example, it had a community-centric purpose, was publicly observable, and was a pen-and-paper task rather than manual labor. We examine the community component of the self-worth index to shed some light on the role of these mechanisms, where we interpret an individual’s sense of self-worth within the community as a measure of status within the community. Online Appendix

¹⁵This is despite the fact that we framed the cash transfer as “earned” for the completion of weekly surveys in efforts to limit the potential disempowering effects of cash.

TABLE 6—EMPLOYMENT IMPACTS ON PURPOSEFULNESS

	Value to family (1)	Value to community (2)	Value to community (3)
Work	0.112 (0.072)	0.072 (0.090)	0.006 (0.081)
Cash	-0.063 (0.079)	-0.086 (0.110)	-0.067 (0.100)
Shrp. <i>q</i> -val: work	0.543	0.752	1.000
Test: work = cash	0.006	0.068	0.345
Shrp. <i>q</i> -val: work = cash	0.019	0.073	0.131
Observations	726	726	726

Notes: This table reports the treatment effect of the employment and cash arms on one's ranking of oneself relative to the most valuable person in their family (column 1) or community (columns 2 and 3), where column 3 is conditional on family ranking in order to isolate the effect on community ranking beyond family. Rankings range from one to ten, with ten being the most valuable individual in the family/community and one being the least. Outcomes are standardized. Each column shows the OLS estimates of equation (1) including dummies for each treatment arm and controlling for the baseline measure of the dependent variable (ANCOVA), camp and enumerator fixed effects, and sociodemographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014). Each row presents the coefficients of the specified treatment relative to the control group (which received a small weekly cash payment). Standard errors are clustered at the block level.

Table A6 demonstrates that baseline mental health has no correlation with ranking within one's community beyond that of family, suggesting that mental health is not closely tied to one's perception of their standing within their community. To test this directly, column 2 of Table 6 examines the impact of employment on one's community ranking: the effect is smaller, but remains positive. Because community ranking implicitly includes one's perceived rank within their family as well, column 3 conditions on family rank. Conditioning on this causes the effect of community rank to largely disappear, suggesting that the public, clerical, and community-centric nature of the work are unlikely to be primary driving forces behind greater well-being. We note that this exercise faces several limitations: we cannot establish causality as we condition on an endogenous regressor, standard errors remain too large to reject any impact of employment on standing within the community, and our question on community ranking may preclude other dimensions of status, such as wealth or prestige, that are not captured by concepts such as "respect" or "value." The social status that a refugee in our setting obtains from having a job with an NGO may therefore generate psychosocial value on a margin we cannot estimate.

Sociability: Does the social nature of the work task drive the results? Participants are relatively social at baseline: the average refugee in our sample has conversations with 14 different adults per day and spends nearly four hours engaged in social activities. The nonpecuniary impact of employment on sociability is small, at an imprecise 0.09 SD, or 0.5 additional conversations per day.¹⁶ Furthermore, the marginal increase in conversations among the employed transpire on *nonwork*

¹⁶This is derived from the difference in the work and cash impacts on sociability of Table 2; we then obtain 0.5 more conversations upon converting the standard deviations into number of conversations. Note also that socializing with the "employer," or the enumerators, was held constant across all three treatment arms.

TABLE 7—HETEROGENEITY BY GENDER

	Psychosocial index		PHQ		Value to family	
	Female	Male	Female	Male	Female	Male
Work	0.165 (0.068)	0.233 (0.051)	0.120 (0.124)	0.276 (0.089)	0.091 (0.114)	0.121 (0.089)
Cash	0.086 (0.088)	0.045 (0.057)	0.089 (0.146)	-0.031 (0.101)	0.080 (0.142)	-0.084 (0.109)
Test: cash = work	0.268	0.000	0.799	0.001	0.921	0.021
Shrp. <i>q</i> -val: cash = work	1.000	0.001	1.000	0.008	1.000	0.026
Test: male = female	0.247	0.247	0.047	0.047	0.223	0.223
Shrp. <i>q</i> -val: male = female	0.396	0.396	0.396	0.396	0.396	0.396
Observations	223	503	223	503	223	503

	Stability		Household power index		Work rights index	
	Female	Male	Female	Male	Female	Male
Work	0.241 (0.140)	0.284 (0.087)	0.323 (0.129)	0.019 (0.093)	0.349 (0.104)	0.086 (0.094)
Cash	0.196 (0.163)	0.045 (0.111)	0.279 (0.142)	0.072 (0.097)	0.110 (0.113)	0.040 (0.111)
Test: cash = work	0.724	0.018	0.745	0.532	0.011	0.600
Shrp. <i>q</i> -val: cash = work	1.000	0.026	1.000	0.271	0.146	0.280
Test: male = female	0.174	0.174	0.674	0.674	0.195	0.195
Shrp. <i>q</i> -val: male = female	0.396	0.396	0.491	0.491	0.396	0.396
Observations	223	503	223	503	223	503

Notes: This table reports the treatment effect of the employment and cash arms on psychosocial outcomes separately for male and female participants. Each column shows the OLS estimates of equation (1) including dummies for each treatment arm and controlling for the baseline measure of the dependent variable (ANCOVA), camp and enumerator fixed effects, and sociodemographic controls as determined through a double-selection LASSO procedure (Belloni, Chernozhukov, and Hansen 2014), run separately by respondent gender. Each row presents the coefficients of the specified treatment relative to the control group (which received a small weekly cash payment). All outcomes are standardized. The “household power index” is an index of a series of questions around who in the household makes key decisions, with a higher household power index signaling greater decision-making power for the female head of household. The “work rights index” is an index of a series of questions around whether women should be permitted to work outside of the home. Standard errors are clustered at the block level. The row labeled “shrp. *q*-val: work” reflects *p*-values, referred to as “sharpened *q*-values,” adjusted for multiple hypothesis testing using Anderson (2008). The adjustments are made within gender for all outcomes in this table. The row labeled “test: male = female” reflects the unadjusted *p*-value for the test of equality between the male and female work treatment effects based on a pooled regression (not shown) where the treatment indicators were interacted with the gender of the respondent. The row labeled “shrp. *q*-val: male = female” reflects the adjusted *p*-value for this test of equality across all five outcomes.

days (column 3 of Table 5), suggesting that gains in sociability are unlikely to be due to the nature of the work itself.

How Working Shapes One’s Day

Structure: In an environment where days are unregimented and there exists great uncertainty around the future, does the structure of work lead to mental health improvements? Such a channel would be consistent with psychology literature on behavioral activation, or the act of scheduling structured activities as a means of combating depression (Cuijpers, van Straten, and Warmerdam 2007). To explore this question, we supply a random subset of the employed with a calendar marking every date of work (online Appendix Figure A5). The remainder receive a blank calendar and are instead informed weekly about their schedule. We find no impact

of a schedule on respondent well-being or decision-making (online Appendix Table A7). Despite this exercise, we cannot causally estimate the role of structure alone on well-being, as the structure imposed by regular employment is coextensive with employment itself. Indeed, our measure of stability, which asks respondents how secure they feel at the moment and expect to feel in the future, increases substantially among the employed relative to both control and cash arms.

Time Use: Does employment improve well-being by allowing participants to substitute time away from unsavory or psychosocially costly activities? Online Appendix Table A8 presents how cash and work arms use their time. We document no significant difference between the two arms in the number of hours that respondents report spending across a variety of activities. Notably, while we cannot rule out that time outside due to employment may play a role (e.g., fresh air may boost one's mood), our time-use data indicate that the average refugee already spends at least three hours outside per day, with no measurable difference between employed and cash arms. As we are powered to detect changes of at least 20 minutes for each activity, our results suggest that large substitutions away from unsavory activities are unlikely to be driving the improvements in psychosocial well-being, insofar as the respondent recalls.^{17,18} We also investigate whether those who were more idle prior to being employed benefit more from employment. We find no impact along this margin, suggesting that the elimination of boredom per se is not the driving force behind the psychosocial value of employment (online Appendix Table A10).

How Working Shapes Consumption Choices

Consumption: While those in the employment arm received nearly exactly the same quantity of cash as those in the cash arm, participants in each may have experienced the receipt of this cash differently and therefore spent it differently.¹⁹ We examine directly how income was consumed. Panel A of online Appendix Table A11 demonstrates no statistically significant difference between the employment and cash arms across a variety of consumption categories. Similarly, panel B demonstrates no statistically significant difference in the rates at which the employed and the cash arms save, borrow, or lend.

¹⁷Most respondents do not track their day by time, making collection of reliable time-use data challenging (though recent literature documents the broader unreliability of such data). We piloted a variety of strategies, and settled on asking respondents how much time they spent on a set of activities in the previous day. Methods such as calling respondents at various times of day to document their activities were not feasible in the camps given the lack of mobile phones.

¹⁸Given that individuals in the employment arm report spending an average of 2.5 hours per day engaged in the work assignment, the absence of time-use effects is perhaps surprising. Given the distribution of work time over the course of the day (40 minute intervals), we suspect that rather than supplanting any single activity during a worker's day, the task instead shed a few minutes off of many activities. Both recollection and detection of these small margins of substitution are less likely. As such, while we cannot rule out that the effects of the employment arm may arise from small changes in how time is spent, our results suggest that substantial differences in time use are unlikely to drive the nonpecuniary impacts we document.

¹⁹This potential difference in perception (and in particular, the concern that a beneficiary's dignity may be challenged with the receipt of cashfare) is implicit in the program itself and is therefore part and parcel of the differential psychosocial impacts we are interested in estimating.

How Different Workers Gain from Working

As with the features of work, the features of the employee may likewise moderate the psychosocial benefits of employment. We consider four below.

Gender: Ex ante, men or women may derive greater nonpecuniary value from employment. Whereas males may gain a greater sense of status through employment, employment may differentially raise the household bargaining power of females.²⁰ We find suggestive evidence that the nonpecuniary psychosocial benefits of employment are greater among males (Table 7). The patterns on self-worth described above are further mirrored in the depression scale: males experience a large reduction in their PHQ score when employed, but a slight increase from cash alone, while females experience modest reductions from both the employment and the cash treatments. The positive impact for females under both treatments is echoed in their household power index: the provision of cash, with or without employment, significantly raises women's beliefs about their prerogative to make decisions in the household and their intolerance for intimate partner violence. Notably, the two month employment intervention also manages to significantly shift the beliefs of employed women around whether women should be permitted to work outside the home.

Baseline Violence, Depression, and Extroversion: We explore whether the impacts of employment are mediated by baseline exposure to violence in Myanmar, depression, and extroversion. Columns 1 and 2 of online Appendix Table A12 present suggestive evidence that those who witnessed death in the genocide experience a larger reduction in depression from the employment intervention. Columns 3 and 4 present the parallel results for baseline depression, but we observe no obvious pattern. Columns 5 and 6 examine heterogeneity by extroversion, where we likewise observe no obvious pattern, with reassuringly no evidence that introverted individuals are harmed by the outward-facing work.

Benchmarking Main Effects

Benchmarking the Employment Impact: How do our employment impacts compare to alternative antipoverty programs or targeted psychotherapy programs? Ridley et al. (2020) perform a meta-analysis of the mental health impacts of multifaceted antipoverty interventions (e.g. livestock transfer, business training, employment, etc.) and find an average effect of 0.2 SD per US\$1000 PPP in cash transfers (or 0.024 SD per US\$120); the effects we document are substantially larger. Singla et al. (2017) perform a meta-analysis of the mental health impact of targeted psychotherapy programs in low- and middle-income countries and find an average of a 0.49 SD reduction in depression and posttraumatic stress disorder, against which

²⁰Sociological work suggests that job loss leads to greater male aggression in the home due to a greater sense of powerlessness (Annan and Brier 2010; Kabeer 2015; Bhalotra et al. 2020). McKelway (2020) describes the empowerment effects of employment for women, although it remains an open question whether such gains are derived from the nature of the employment or from its function as an income source.

the impacts of employment that we document are smaller. Perhaps most relevant is a study of a yearlong psychoeducation program directed at Rohingya refugee women residing in the same set of camps as those in this study (Islam et al. 2021). The program provided weekly sessions of in-person psychoeducation and parental counseling and yielded a 0.15 SD reduction in depression severity. In comparison, the employment program we consider generates a 0.24 SD reduction in depression severity.

Benchmarking the Cash Impact: A meta-analysis of the mental health impacts of cash transfer programs by McGuire, Kaiser, and Bach-Mortensen (2020) approximates that transfers which double consumption generate a 0.12 SD improvement in mental health, and transfers of US\$120 PPP are likewise associated with a 0.12 SD improvement in mental health. An alternative meta-analysis by Ridley et al. (2020) estimates impacts of cash that are substantially smaller: US\$1000 PPP cash transfer generates an average mental health impact of 0.12 SD; a linear interpolation implies that our US\$120 PPP transfer would yield a 0.014 SD impact on mental health. Our cash transfer, which is valued at US\$120 PPP and at least a doubling of daily consumption, exhibits a 0.06 unit (though imprecise) change in our psychosocial index of standardized mental health outcomes, falling in between these two meta-analyses.

V. Conclusion

Cumulatively, our analyses shed light on the psychosocial impacts of employment and the various mechanisms mediating the relationship we identify. We design a simple employment intervention, contextually appropriate and amenable to both men and women, which yields improvements in psychosocial well-being substantially greater than that of an equivalent amount of cash. Perhaps most striking in this is how simple the work task is. For all intents and purposes, our participants could have organized a similar activity of their own accord. That they do not do so suggests that the experience of being employed, of engaging in productive and effortful activity in the service of an employer, confers particular value to a task, even at the low intensity of 7.5 hours per week. Indeed, we find that individuals are able to at least partially internalize these benefits in their labor supply decisions. We offer two considerations with regard to these findings.

First, our study finds that the majority of refugees in our setting are willing to work for zero pay, and are in fact willing to forgo a sizable transfer in order to work for free. When choosing between cashfare and workfare programs in similar contexts, policymakers may therefore favor work programs as a means of alleviating both material and psychological poverty. However, our results cannot offer insight into the price of labor in these contexts, as there are long-term psychosocial benefits to accumulated wages that are not captured in this field experiment. The low reservation wages exhibited in this study also suggest a nuanced role for labor regulation: while legal restrictions to labor market opportunities are a likely source of these low reservation wages, legal protections in the form of wage floors may be particularly important to welfare in the restrictive labor environments these laws produce.

Second, we sought to design a study to estimate the psychosocial value of a realistic form of employment, beyond that of income alone, for two reasons. On

the margin of economic theory, neoclassical frameworks of labor supply model the provision of “labor” as a net disutility, while our results suggest that there exist contexts within which this is not the case. On the margin of policy relevance, organizations such as the United Nations Human Rights Commission invest significant capacity into securing refugees the right to work in their host countries (UN 2018); estimates of the psychosocial benefits to employment may serve as valuable evidence for their efforts.

Finally, we underscore the interpretation of our findings as a *proof of concept*. The employment intervention we designed was a part-time activity, was not manual labor, was clerical, purposeful, and involved a relatively kind employer. These are features that are common to many, but far from all, jobs. Our findings instead encourage us to deepen our conception of “labor” and “employment” through future directions of questioning. Among them, which elements of the experience of employment may be most psychosocially valuable? In what contexts might material support alone be a suboptimal means of improving well-being? And how might the scarcity of meaningful activity or direction, so palpable in refugee camps but often encountered beyond, transform the concept of “time” from a valuable resource into an amorphous and costly experience?

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