# THE PSYCHOSOCIAL VALUE OF EMPLOYMENT

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**Abstract**

In settings where an individual's labor choices are constrained, the inability to work may generate psychosocial harm. This paper presents a causal estimate of the psychosocial value of employment in the Rohingya refugee camps of Bangladesh. We engage 745 individuals in a field experiment with three arms: (1) a control arm, (2) a weekly cash arm, and (3) a gainful employment arm, in which work is offered and individuals are paid weekly the approximate equivalent of that in the cash arm. Compared to both the control and cash arms, the employment treatment generates significant improvements in psychosocial wellbeing. We find no evidence that cash alone improves psychosocial wellbeing, despite the provision of cash at a weekly amount that is more than twice the amount held by recipients in savings at baseline. Consistent with these findings, we find that 66% of those in our work treatment are willing to forego cash payments to instead work for free. Our results have implications for social protection policies for the unemployed in low income countries and refugee populations globally.

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# 1 Introduction

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; Kessler, Turner, and House, 1988), this literature encounters two key challenges.[1] First, those who are unemployed differ from the employed in ways that are likely correlated with their psychological wellbeing, 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 non-pecuniary means by which work improves wellbeing.

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.[2] The surveying task requires workers to walk through their blocks four times per day tallying the various activities their neighbors are engaged with. The job is designed to embody the key features inherent to 'work'. Drawing from the economics literature, workers must exert meaningful effort, both physically and mentally, and their task occupies a substantive amount of time throughout their work day. Drawing from the sociology literature, the work embeds nominal components of sociability and purpose. 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 nominal fee for weekly survey participation. A comparison of the control to the employment arm therefore yields the psychosocial benefits of employment. In order to estimate the *non-pecuniary* psychosocial value of employment, we include a third cash 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, eleven percent report having worked in the previous month; of these, the average duration of employment is three days. They further report spending an average of eight hours of their waking day engaged

[1] Other references in the psychology literature include (Paul and Moser, 2009; McKee-Ryan et al., 2005; Wehrle et al., 2018)

[2] We obtained formal permissions from camp administration to engage our study participants in this manner through our NGO partner, Pulse Bangladesh.

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.[3].

Baseline data shed further light on the potential consequences of such pervasive unemployment: individuals who report having been unemployed the entirety of the previous month are 17 percentage points more likely to qualify as depressed according to the PHQ-9, the diagnostic tool we employ to assess likelihood of depression (Appendix Table A1). This correlation is, of course, 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 is the impact of employment, beyond that of remuneration alone, on wellbeing?

We describe our results in four steps. First, we find that employment generates significant psychosocial benefits relative to individuals in our control arm. We observe a precisely estimated 0.21 standard deviation (SD) increase in our 'mental health index' ($p = 0.000$), a pre-specified composite measure of depression, stress, life satisfaction, self worth, sociability, locus of control, and sense of stability. Each of these subcomponents exhibit a significant and meaningful improvement as well: for example, we find that employed individuals are nine percentage points (11%) less likely to be depressed and seven percentage points (21%) less likely to be moderately or severely depressed. As a benchmark, relative to the 0.19 SD reduction in depression from our employment program, a recent evaluation of an intensive year-long psycho-education program for Rohingya refugees in the Bangladesh camps documents a 0.15 SD reduction in depression (Islam et al., 2021). These positive effects of employment are not limited to the psychosocial. We find that such individuals are also significantly less likely to feel physically ill, perform substantially better on simple memory and math tests, and exhibit significantly less risk aversion in an incentivized risk preference game.

Second, we find that employment generates benefits that are significantly greater than that of cash alone, which, despite a magnitude of more than double weekly consumption, generates a statistically insignificant 0.05 standard deviation improvement in mental health. We can reject equality of effects between employment and cash for our mental health index ($p = 0.000$), physical illness ($q = 0.081$), cognitive performance ($q = 0.081$), and risk aversion ($q = 0.081$).[4] These differences are substantial: employment improves mental health at a magnitude four times greater than cash alone. A set of complementary analyses suggests that our results are unlikely to be explained by expectations of future work and income, differences in how 'earned' relative to 'unearned' cash is consumed, or experimenter demand effects. The sizable non-pecuniary benefits to psychosocial wellbeing that we observe appear to be driven by the experience of employment itself.

[3]Such expressions of the need to be occupied are not unique to Rohingya refugees. Syrian migrants in the Turkish Killis camp in 2017, regarded as one of the best materially-equipped refugee camps in the world, echo these sentiments: *"We wake up, we sleep, we wake up, we sleep, we eat food... There is no purpose in a life like this. One day is like another."* (McClelland, 2014)

[4]'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.

Third, consistent with the non-pecuniary benefits of the employment opportunity, we find high willingness to supply labor among study participants, even at a wage of zero. Individuals therefore appear able to price the psychosocial benefits of employment into their labor supply choices. Through an incentivized elicitation of reservation wages for an additional week of work, we find that the majority (69%) of individuals are willing to work an additional week for zero pay. Among these individuals, the vast majority (77%) are willing to forego at least 200 BDT (approximately $2.5 USD, greater than average savings at baseline) to continue working for free. We elicit these measures after eight weeks of working, with the intent of both familiarizing participants with the nature of the work and eliminating novelty effects that might arise in the early days of new employment.

While we view the objective of this study to be to estimate the psychosocial value of employment as an intervention unto itself, we engage in a series of exercises to explore dimensions of the employment task from which non-pecuniary value may be derived. We first consider key features of the experience of work that may be generating psychosocial value: alternative uses of time, the structure and predictability of the employment task, the social nature of the task, and the status and purpose embedded in the work. We find that none of these features can fully explain the large psychosocial gains to employment we document: instead, features that are coextensive with 'employment,' such as engagement in a productive activity or one's identity as an 'employed' person, are likely to be driving our results. This is reinforced in our next set of analyses, in which we consider what features of an individual might predict greater psychosocial value from employment: here we explore gender as a primary dimension of identity, followed by an individual's degree of extroversion, depression at baseline, and exposure to trauma. We find suggestive evidence that the employment program is significantly more impactful for participants who identify as male, are more sociable at baseline, are more depressed at baseline, or witnessed greater violence during their exodus from Myanmar.

While we explore the non-pecuniary channels through which employment may imbue psychosocial value, we underscore the broader value that offering an employment opportunity, holistically, has on the wellbeing of recipients beyond that of a cash transfer alone. We created a simple employment intervention, contextually appropriate and amenable to both men and women, which yielded improvements in psychosocial wellbeing five times greater than that of an equivalent amount of cash. Perhaps most striking in this is how simple the work task was: for all intents and purposes, our participants could have organized a similar activity of their own accord. That they did not do so suggests that the experience of being employed, of engaging in productive and effortful activity in the service of an employer or broader objective, confers particular value to the task.

We view our findings as first relevant to the global phenomenon of forced displacement. The number of forcibly displaced persons (FDPs) has grown rapidly in recent years, reaching a historic high of 80 million in 2020 (UNHCR (2020)), of which 24 million are refugees. Less than one percent of refugees are resettled per year (UNHCR (2021)), and the average FDP remains displaced for 10 to 26 years (Ferris (2018)): their status is not a transitory one. At least 70% of refugees reside in countries in which there exist legal barriers to the right to work (Schuettler and Caron

(2020)), notwithstanding the myriad informal barriers refugees encounter when seeking to enter the labor markets of their host countries (Zetter and Ruaudel (2016)). In 2018, UN Member States established the Global Compact On Refugees, a policy document underscoring the importance of employment and income generation opportunities for refugees and host communities alike. We hope the findings of this study can inform the efforts of member institutions who seek to implement the pledges of this compact, among other comparable efforts globally (Nations (2018)).

Beyond the forcibly displaced, the set of constraints that define our sample of Rohingya refugees are arguably shared across many populations of interest. Participants in our study are cash-poor and therefore deprived of basic necessities for daily living,[5] lack easy access to both formal and informal employment due to restrictions on mobility, and have little opportunity for leisure activities beyond socializing with friends or the occasional use of a mobile phone. While indeed, the value of employment depends on both the nature of work and the social mores surrounding employment, these three features are common to many of the world's incarcerated (10.35 million), the unemployed in low-income countries (conservatively estimated at 22 million (Organization (2018))), 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, Akram, Chowdhury, and Mobarak (2017) for a Bangladesh context, and Breza, Kaur, and Shamdasani (2020) for an Indian context).

This study makes three primary contributions. First, the study provides a causal estimate of the psychosocial impacts of employment conditional on income, a measure that has implications upon individuals beyond the refugees we study.[6] 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; Terkel, 1974; Turner, 1995; Colic-Peisker and Walker, 2003; Wehrle et al., 2018). Conversely, a burgeoning literature on cash-for-work programs documents positive psychosocial impacts of such interventions, but is not designed to distinguish the pecuniary from the non-pecuniary channels behind the documented effects (Bertrand et al., 2021). Our experiment is motivated by this literature as well as a limited stock of empirical evidence around the psychosocial costs of idle time. We benefit from the work of Bhanot, Han, and Jang (2018), who estimate the value of *occupied time* in a ten day lab-in-the-field experiment in Nairobi, Kenya, in which individuals are randomized into either waiting for one hour for a voucher or sorting lentils for one hour and receiving a voucher of equal value. The authors find that the latter treatment indeed improves psychological wellbeing.[7] Bhanot, Han, and Jang (2018) serves as valuable groundwork, upon which this study builds primarily by designing a work task that is

[5]Despite provision by NGOs of basic staples such as rice, lentils, and oil, and a tiny plot of space upon which to build a shelter, refugees need cash for basic consumption items: clothing, salt, vegetables or fish, hygiene products, household ware, etc.

[6]The bidirectional relationship between mental health and employment is reviewed in Ridley et al. (2020), which also provides meta-analyses of cash transfer and anti-poverty programs on mental health.

[7]These results are consistent with a cross-sectional examination of workfare versus unemployment benefit recipients in Germany (Knabe, Schöb, and Weimann, 2017), in which the former reported greater wellbeing and life satisfaction despite equivalent income.

more reflective of meaningful long term employment along two key margins. First, it is an effortful activity, requiring the investment of both long-duration physical and mental effort in order to be compensated. Second, our intervention substitutes away from leisure, preserving a realistic outside option to participation in the labor market in which individuals carry on with existing activities and maintain flexibility in arranging their time, rather than being restricted to a room with no physical or mental stimulation, which has been established by Hsee, Yang, and Wang (2010) to be psychologically costly. These features shift the intervention from a task that purely occupies idle time to one that mimics the key features of "labor" in economic theory. Beyond these key distinctions, we additionally include a control benchmark of no cash or work, allowing us to price the psychosocial impact of the employment arm, and we elicit incentivized willingness-to-work measures to examine whether individuals can price the psychosocial value of employment into their labor supply decisions.

Second, this experiment offers insight into whether cashfare or workfare programs are a more cost-effective means of improving psychological wellbeing. Widespread unemployment has implications not only for the material but also the psychosocial wellbeing of the un- and under-employed. 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 well elucidated through case studies in the sociology literature, first articulated in Jahoda, Lazarsfeld, and Zeisel (1971)'s seminal work around Marienthal, a small town in Austria that was 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).[8] We bring an empirical 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 vast 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 non-pecuniary mechanisms through which a material intervention (gainful employment) may improve psychosocial wellbeing. This is a valuable exercise, as aid organizations and policymakers grow increasingly concerned about the protracted nature of most displacement, which, when paired with 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

[8]More 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 ... boredom, time-slowing boredom, interrupted by occasional bursts of fear and anger, is the governing reality of life in prison."*(Council, 2014).

impacts on wellbeing and offer a benchmark against a standard cash transfer program.

The remainder of the paper is structured as follows: Section 2 further describes the research context in which we operate; Section 3 outlines the experimental design; Section 4 describes our data collection processes; Section 5 presents the results; and Section 6 concludes.

# 2 Research Context

## 2.1 Recent Events

The Rohingya are an ethnic group that, prior to the genocide of 2017, lived predominately in Rakhine State along the western coast of Myanmar (also known as Burma) (Blakemore, 2019). The community traces their origins back to the 15th 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 lead 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 coordinated attacks on security force outposts across 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 (as they do not possess a legal right to work in Bangladesh).

## 2.2 Camp Life: Descriptive Statistics

Because refugees are not legally allowed to work (Bhatia et al., 2018), many remain unoccupied in the camp. Some men seek occasional employment in the informal sector outside the camps, but this comes with significant risk as military checkpoints around the camps are abundant. Among the scarce employment opportunities available are as day laborers in agriculture or construction,

operation of street stalls, assisting in the running of women's cooking centers, child-friendly spaces, or health clinics, or private tutoring. The majority of jobs available in the camps are provided by NGOs, several of whom have organized cash-for-work programs in the camps (see, for example, Mree and Homer (2019)). Outside of the camps, a comparable population of Bangladeshis (or the old Rohingya who have integrated into the host community) 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% of the sample comprised of women. Less than 50% received any formal education when in Myanmar. 78% of our sample qualifies as depressed according too the PHQ-9 screening tool, with 38% exhibiting signs of moderate to severe depression.[9] 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 chooses to spend his or her time rather than sit idle, both men and women volunteer time with children, socializing, taking walks, reading religious scripture, or taking a nap. This same selection of activities is named when asked what one does to lift one's spirits or distract oneself from the difficulties of the past and daily life.

## 3 Experimental Design

**Sampling Strategy** The research team obtained permission from the RRRC to work in three camps in Bangladesh (5, 8W, 17), which were selected given the relationship cultivated between our research partner, Innovations for Poverty Action (IPA), and the camp authority in each location. The camp authority organized meetings with the local *majhi* to explain how the research team would be interacting with households in their respective blocks. Within each camp, we selected non-adjacent blocks to reduce the risk of spillovers. Within each block, we enlisted five households into our sample.[10] Upon entering a given block, the field team knocked on doors at random, inquired if the household member (pre-assigned as the male or female head of household) was interested in participating in a study, and confirmed that the respondent met our eligibility criteria.[11] In total,

[9] While high, these numbers are comparable to depression rates among other refugee samples (Poole et al., 2018) as well as among Hispanic communities in the United States in 2020 during the height of the COVID-19 pandemic (McKnight-Eily, 2021).

[10] Most blocks host upwards of 100 households.

[11] We had seven eligibility criteria: that the individual had not worked in the last 14 days; were within the ages of 18-45 years; were able and willing to work for two months inside the block; were not the *majhi* or a member of the *majhi's* household; and did not receive remittances from abroad.

we assembled a sample of 745 individuals across the three camp sites.[12]

**Experimental Design** We randomly assigned 149 blocks, each with five refugees, to one of three arms (Table 1 reports summary statistics and balance across the three treatment arms). We randomize at the block level to limit potential spillovers. 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 taka (USD $0.60) per week as compensation for answering our weekly surveys. An additional 33 blocks were assigned to the cash group, where participants received 450 taka (USD $5.30) per week as compensation for survey participation. Finally, 83 blocks were assigned to a work group, where we offered participants gainful employment.[13] We compensated participants in this treatment arm with 150 taka (USD $1.77) per day of work. Households were assigned two, three, or four days of work per week, averaging over the course of the eight weeks to 450 taka per week, as in our cash group. All participants were aware of the randomization process: we instructed enumerators to display the random number that would be revealed on their tablet, assigning the participant to his or her treatment group, to the participant as it appeared.

**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 filled out time-use sheets, reporting on the activities of fifteen same-sex neighbors of their choosing four times per day.[14] We sought first to incorporate the key features of 'labor' as understood in economic theory: the task was designed to require a meaningful amount of 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 substantially over time), and it occupied a meaningful amount of time per day, with workers reporting 2.5 hours per working day on the job. We further sought to incorporate key features of employment as described in sociological literature (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 clear purpose, as ample sociological work demonstrates that blue collar or service work - under which the typical employment opportunities available to our participants may fall - is perceived as meaningful and valuable by workers (for example, Lamont (2002); Poll (2018); Terkel (1974)).[15]

[12]We sought to identify individuals who had not worked in the last 14 days out of equity concerns. The vast majority of those of working age encountered in our pilot work were eager to find a job, and we wished to engage those who did not already have access to a work opportunity.

[13]We load sample onto the work treatment arm in order to power a sub-experiment in which we vary the degree of certainty workers have over their future schedule. We describe this sub-experiment in further detail in Section 5.

[14]The neighbors that each employee selected were not identified to the researchers, ensuring that no participant felt like they were infringing on the privacy of others.

[15]In fact, recent empirical work by Soffia, Wood, and Burchell (2021) finds that the vast majority of workers occupying positions perceived by the sociology literature to be 'useless' or 'alienating' (typically pink and white collar work such as financial services and administration) regard their work as meaningful as well. Despite much conversation around alienation in work (ranging from Marx and Engels (1844) to Graeber (2018)), 'meaning' therefore appears, empirically, to be coextensive with most forms of employment.

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 better 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 back-breaking, 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 a full eight hours of a workday, as most available work in the camps was likewise 'part-time' work. Similarly, 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, but was not as direct a contribution to community wellbeing as is the typical job: building roads, constructing latrines and homes, working in agriculture, assisting in clinics and 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 (eg. 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 (see Figure 1 for a visual of the time-use sheet and activities). 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 articulating the same; this ensured standardized comprehension across participants.

We asked that households complete the work tasks on the specific days they were assigned: work schedules varied week to week, averaging three days weekly. To ensure compliance with the work schedule, we stationed a tamper-proof box in a preselected household within each block (henceforth referred to as 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 submission. The respondent's submission was marked late if it was inserted after the bookend. Supervisors determined which household in the block would host the collection box, selecting a sample household whose dwelling was most centrally located. These facilitators were compensated with an additional 50 taka per week for their services. The facilitator 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 the case of blocks in the work treatment, the enumerators first checked the respondents' work (eg. the number of pages they submitted, with each page representing one of the four times per day the activity should have been completed; 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) and if not, why not; 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).[16] This process was not particularly onerous and was completed rather quickly each week. 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: we implemented this rule in order to encourage high quality work without excessively penalizing for unintentional mistakes. Payment occurred at the end of the interaction, once the enumerator had administered the standard weekly survey.

**Interpreting magnitude of cash interventions** As described previously, we offer both the cash and the employment arms 450 taka per week over two months. What is the value of 450 taka per week in the context of the Rohingya refugee camps? At 1800 taka per month ($60 USD PPP), it is slightly larger than the cost to the World Food Program (WFP) of the per-refugee monthly ration provision of lentils, oil, and rice.[17] 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 WFP provisions are the only reliable rations that refugees receive, we approximate a cash transfer of 450 taka per week to at least double potential weekly consumption.

Relative to the wealth refugees possess, 450 taka per week is likewise sizeable: average baseline savings is 195 taka, with the median refugee reporting zero taka in savings. Average baseline borrowing (typically in the form of store credit) is 1600 taka, with a median of 600 taka. Refugees have no economically meaningful assets that may be more common among the rural poor, such as land or cattle, given the unanticipated and violent displacement which forced them from their homes in Myanmar. Relative to other employment opportunities, eleven percent of our sample report having worked in the previous month; of these, average reported pay is 300 taka 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.

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

[17] In 2019, Rohingya refugee households with one to three members received 30 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/kg), lentil (BDT 140/ kg) and soyabean oil (130/kg), the monthly rations can be estimated at approximately BDT 3450 per two, or 1725 per adult.

# 4 Data Collection and Survey Instruments

**Timeline and survey instruments** Prior to the rollout of the full experiment, the research team spent twelve months engaging in an extensive piloting of our survey instruments as well as a pilot experiment involving 300 households. Sociopolitical, emotional, cultural, and administrative complexities necessitated an iterative process in developing our survey instruments and experimental design. We began with standardized modules but adjusted to accommodate these contextual demands, adapting or eliminating various questions from such modules which were culturally insensitive or incoherent given the experiences of the Rohingya.

Upon launching the full experiment, we collected data via a baseline and endline survey as well as nine weekly surveys which we conducted prior to payment disbursal each week. The weekly surveys were brief, covering a small subset of outcomes. They were designed to explore the point in the employment experience that impacts might materialize. We conducted the endline survey two days after the end of the work and cash provision period. In an effort to ensure that our temporary interventions had no unintended negative mental health consequences on our participants, we also conducted a final followup survey six weeks after the interventions concluded. We had 3% attrition at endline and followup, with neither differential by treatment arm.

**Main outcome variables** Our primary outcome of interest is psychosocial wellbeing, which we assess through an index of seven mental and social health measures, henceforth referred to as the psychosocial (PS) index: depression, stress, life satisfaction, locus of control, sociability, self worth, and sense of 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, Levenson Multidimensional Internal Locus of Control Scales, respectively) that we adapted for sensitivity to the Rohingya camp context (see Appendix Table A2 for adjustments). Our psychosocial measures were drawn from survey tools that have been validated by the psychology and public health literature in several contexts similar to our own. In particular, the PHQ, our depression screening tool, has been validated among refugee populations (Poole et al., 2018), those seeking primary healthcare in South Asia (Patel et al., 2008; Indu et al., 2018), and globally (Kroenke, Spitzer, and Williams, 2001; Kroenke and Spitzer, 2002).

For sociability, we inquire about the number of interactions (positive and negative) 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 three 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 secure 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' physical health by asking how many days they have fallen sick in the past thirty 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 substitution in time 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, playing sport, sitting idle). 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 ask about perceptions around gendered decision-making and intimate partner violence. The questions are drawn from Haushofer and Shapiro (2016), which are themselves adapted from the Demographic Health Surveys. In addition, we measure attitudes towards women's ability to work and freedom of movement by asking respondents whether they feel that women should be allowed to work and whether this holds if the woman must work outside their respective camp block.

Each outcome is described in greater detail in Appendix Table A2. The frequency at which each outcome was collected is presented in Appendix Table A3.

# 5 Experimental Results

## 5.1 Completion of work

We first establish that participants in the employment arm did indeed engage in the work they were offered. Figure 2 exhibits the fraction of individuals in the employment arm who completed their work (Panel A), made any mistakes (Panel B), and received a pay penalty for poor work (Panel C) over the course of the experiment.

Nearly all those offered employment completed their work each week, with no week exhibiting below a 98% completion rate, indicative of participants' desire to engage in the work. Mistakes were common in the early weeks of employment, but rapidly declined to hover around five 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 reflected in the frequency of docked pay, which peaks at less than two percent, 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.

## 5.2 Empirical Framework

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

$$Y_{ibc} = \beta_0 + \beta_1 Cash_{ibc} + \beta_2 Work_{ibc} + \gamma_c + \delta_e + X_{ibc} + \varepsilon_{ibc}$$

where $Y_{ibc}$ represents the relevant outcome for individual $i$ in block $b$ and camp number $c$, $X_{ibc}$ is a vector of sociodemographic controls selected via double-selection LASSO to maximize precision following Belloni, Chernozhukov, and Hansen (2014), and $\epsilon_{ibc}$ is an error term clustered at the block level. We include fixed effects for camp $\gamma_c$ and enumerator $\delta_e$.[18] We control for the baseline value of the outcome variable, when available, in an ANCOVA specification following McKenzie (2012). Our coefficients of interest are $\beta_1$, the impact of cash, and $\beta_2$, the impact of work. We evaluate whether there exist non-pecuniary benefits to work through a corresponding test of equality between these two coefficients: $\beta_1 = \beta_2$.

We examine temporal dynamics (see Table A3 for the subset of outcomes that we collect weekly) via the following specification:

$$Y_{ibct} = \beta_0 + \sum_{t=1}^{8} \beta_t Cash_{ibc} * \eta_t + \sum_{t=1}^{8} \gamma_t Work_{ibc} * \eta_t + \gamma_c + \delta_e + X_{ibc} + \varepsilon_{itbc}$$

where $Y_{ibct}$ represents the measures of stress, sociability, cognitive ability, or physical health, $\eta_t$ represents a dummy for the weekly visit number $t$, and $\gamma_c$, $\delta_e$, $X_{ibc}$, $\epsilon_{itbc}$ are as defined above.

### 5.3 Impact of employment

Figure 3 offers a visual representation of the impact of each treatment on our primary outcomes. Table 2 presents the regression analog for psychosocial outcomes. Relative to those in the control group, individuals in the employment arm experience a 0.21 SD improvement in their mental health index, significant at the one percent level. Each 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. They exhibit higher life satisfaction, are more sociable, report greater beliefs in their own self-worth relative to both their family and their broader community, feel greater control over events in their lives, and feel more secure in both the present and the anticipated long-term future.

To provide context for these results, employed individuals are 14% more likely to sleep peacefully and rank themselves as feeling 13% more secure and stable than their control counterparts. They exhibit a 9.5 percentage point (50%) increase in the likelihood of not being depressed and a 6.5 percentage point (21%) decline in the likelihood of being moderately or severely depressed. As a benchmark, a rigorous evaluation of a nearly year-long psychoeducation program for Rohingya refugees, which included 44 weekly sessions of counseling for mothers and play activities for children and was implemented at approximately the same time in the Bangladesh refugee camps as this study, documents a 0.15 SD reduction in depression among treated parents (Islam et al., 2021), relative to the 0.19 SD reduction we document from the employment intervention.

The employment arm not only improves psychosocial wellbeing relative to the control arm, but

[18] 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.

also yields significantly larger improvements in psychosocial wellbeing than the cash arm. The latter exhibits an imprecisely estimated 0.05 SD improvement in the mental health index, and we can reject equality of effects between employment and cash at the one percent level. This result is manifested across all subcomponents of the index, with the exception of life satisfaction, which increases equally under both treatment arms. In other words, the psychosocial value to employment appears to be driven largely by the non-pecuniary dimensions of the employment experience.

### 5.4 Impacts of employment on 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 physical health, cognitive function, and incentivized measures of risk and time preference. We observe a significant decline in the days reported sick. This effect may be due to 'real' health improvements from potentially increased exercise from the employment task or 'perceived' health improvements in which improved psychosocial wellbeing translates into feeling less physically ill as well. Figure 5, which plots the weekly trend in self-reported physical health, suggests that we are likely capturing a shift in perception (which may very well manifest in objective health improvements, but we cannot speak to this): rather than accumulating over weeks as one might expect from exercise, effects occur from the very first week of employment and persist in magnitude.

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 (Gotlib and Joormann, 2010; 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 to complete. 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 due to the employment arm: individuals are no more or less likely to discount the future relative to control counterparts. However, we find a substantial decline in risk aversion 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 (UBI) in the developing world (Banerjee, Niehaus, and Suri, 2019); interestingly, however, we document no parallel decline in risk aversion 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 at least the ten percent level. The effects we document therefore appear to be driven by the non-pecuniary value of employment.[19]

### 5.5 Labor supply

We estimate significant benefits of employment on psychosocial, physical, and cognitive wellbeing, and these effects appear to be driven largely by the non-pecuniary dimension of the experience. We now examine whether individuals are able to price these benefits into their labor supply decisions 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 (BDM) method. 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 forego this paid option and instead work for free as an estimate of the proportion of volunteers who have a negative reservation wage of at least the foregone magnitude.[20]

Figure 7 presents a cumulative distribution of the expressed reservation wages among these individuals. 97% of those in the employment arm express interest in working the additional week. 73% of those who are interested in working express willingness to work for free. 78% of those who are then offered an alternative of 200 taka (USD $2.5) in compensation for answering a brief survey at the end of the week *continue* to prefer to work for free.

As we did not offer compensation for the alternative beyond 200 taka,[21] we cannot deduce the precise negative reservation wage for the majority of our sample, but instead view $-200$ taka as an upper bound. In other words, we find that 70% of all former workers price the non-pecuniary benefits of additional employment at a positive valuation (assuming some non-negligible cost of effort to working), and 55% of former workers value these benefits at greater (and given the observed slope, potentially substantially greater) than 200 taka. Notably, 99% of all those who expressed interest in working completed the work in the following week at the wage drawn in the elicitation exercise.[22] These results suggest that participants understand well the psychosocial value of the

[19] Reassuringly, we find no evidence of negative impacts to the withdrawal of the work or cash interventions in our six week followup, with suggestive evidence of positive persistence; see Appendix Table A5.

[20] Aside from its use as a measure of negative reservation wages, we offered this alternative option to working, which we described as "equally helpful to us," in order to guard against demand or reciprocity effects when eliciting reservation wages.

[21] We felt that larger fees might not be regarded as realistic tradeoffs and consequently be met with suspicion or confusion.

[22] Experimental demand, reciprocity, and reputation effects were foremost concerns when designing this exercise, and we sought to limit the possibility of these effects in the following ways: First, during the endline survey, we informed our work participants that we had a limited amount of funds remaining and were therefore unable to pay everyone their previous wage. This strategy both realistically motivated the reservation wage elicitation exercise and made clear that our funding would be exhausted by the end of the week and there would be no further opportunities to work with us for pay. Second, when we offered the alternative of taking a brief survey for a small fee, we emphasized

employment opportunity they are offered.

### 5.6 Potential confounds

Our results are indicative of the presence of significant non-pecuniary benefits to employment. Here we propose three alternative interpretations that may challenge the centrality of the experience of employment in the effects we document: differences in expectations of future work, differences in consumption, and experimenter demand effects. We consider each in turn.

**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 bound such effects by randomizing the provision of paper certificates to a subset of our participants.

These certificates provided documentation of the beneficiaries' involvement with our project, intending to serve as an explicit boost to their resume. The documents were signed by our enumerators and included the following text: *"Certificate: This acknowledges that I engaged with Pulse Bangladesh to do data collection."*(Appendix Figure A2). 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 both cash-only arms.[23] If employed individuals derive psychosocial benefits from the expectation of future work, the certificate was designed to make this expectation especially salient. A comparison of the differential impact of the certificate in the employment arm relative to that in the cash arm therefore provides some sense of how concerned one may be about a conflation of purely psychosocial mechanisms with [future] pecuniary mechanisms.

The certificate provision addresses a related potential confound of the experiment: perhaps employed individuals derived psychosocial value from employment because of the association with an NGO that it conferred upon them. We partnered with a local NGO with little name recognition as an *ex ante* means of limiting any role of prestige (note that essentially all employment opportunities in the camps come from NGOs). If this 'status by association' remains a driver of the effects we document, then a certificate which formalizes the employer-employee relationship is likely to generate meaningful psychosocial value.

that the survey, like the work, would be a valuable service to us, further reducing the possibility of reciprocity or demand effects. While we cannot rule out such effects, one would have to price these effects at greater than 200 taka, slightly less than the average held in baseline savings, to rule out the presence of any non-pecuniary value of work in respondents' reservation wages.

[23]The wording of the certificate was made such that it could be applied to both arms; cash-only arms participated in weekly surveys along with all other experiment participants, so technically also engaged in data collection for our project.

Table A6 presents the results. Relative to those in the cash arm, those in the work arm who receive a certificate exhibit no differential improvements in psychosocial wellbeing.[24]

We additionally examine the actual likelihood of employment after the intervention as test of this potential confound. Insofar as we can measure, we find no differences in future income generated by the employment arm relative to the cash arm that might produce psychosocial gains from employment. Appendix Table A7 presents the impacts of each treatment arm on post-intervention labor market experiences. Six weeks after the intervention, those who were formerly employed are three percent less likely to take a day-labor job in agriculture or construction (the most common type of occupation in the camps), and no more likely to take a salaried job such as teaching (the closest position to an NGO worker, which no participants report acquiring). The formerly employed are no more likely to find work, receive a higher wage, nor expect work or a higher wage in the future.

**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 reception differently. In particular, we may be concerned that individuals perceive, and in turn use, cash that is 'earned' differently from cash that is 'given.'

We sought to minimize differences in perception by framing payments in the cash arm as also 'earned:' participants were informed that the cash was their compensation for participation in weekly surveys. Nonetheless, differences may have remained; we now examine whether they manifest in how the cash is consumed.[25]

Panel A of Table 4 demonstrates no statistically significant difference between the employment and cash arms across a variety of consumption categories (divided for parsimony into 'luxury' and 'necessary' goods; individual categories, not shown, exhibit no detectable differences).[26] Similarly, Panel B demonstrates no difference in the rates at which the employed and the cash only arms save or repay loans. Column 3 reports that the employed lend slightly more (approximately 0.3 USD more in the previous week) than their cash counterparts, consistent with employment being a more conspicuous, and therefore more easily taxable, source of income than a cash transfer.

Weekly trends in spending and saving, as depicted in Figure 6, suggest that cash recipients spend marginally more and save less than those in the employment arm, but these patterns appear to converge by the end of the intervention. As such, while we observe small differences in consumption

[24] One may be concerned that, if other employers learn about the nature of the certificate distribution (i.e. provision to (1) a random subset of workers and (2) some participants who did not engage in active work), the signaling value of the certificate may be diminished, reducing the informativeness of this test. Our time in the field suggests that knowledge of the randomization process is unlikely: we randomized certificate distribution at the block level to limit spillovers, and NGO job opportunities are scarce.

[25] Notably, from the policy perspective of evaluating the psychosocial value of workfare versus cashfare programs, this 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.

[26] We did not execute a complete consumption module, as our primary objective was to capture differences in common luxury and necessary goods between the cash and work arms.

behavior between the two groups, they seem an implausible source of the large psychosocial effects we document.

**Experimenter demand effects** Our primary outcomes of interest, being psychosocial in nature, require self-reporting. Are the effects of employment on these outcomes a product of demand effects? Specifically, do those in the employment arm report improved wellbeing in order to please their enumerators for the employment opportunity they have 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 equivalently substantial sum of cash. As such, both groups are equally 'indebted' to the experimenter and therefore, perhaps, 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 consider a series of outcomes that are not vulnerable to experimenter demand. Our cognitive index, comprised of a series of simple arithmetic questions and memory tests, is free of the subjectivity more easily shaped by demand effects. Our risk and time preference games are incentivized with meaningful stakes (respondents gamble with a minimum of 1.20 USD in the risk preference game and trade off 3.50 USD today with higher amounts tomorrow in the time preference game), which (de Quidt, Haushofer, and Roth, 2018) have found effectively eliminate demand effects. Beyond these measures, we observe meaningful treatment effects of employment on questions within our psychosocial indices which are perhaps less vulnerable to demand effects given their gravity: for example, those in the employment arm report a reduction of 0.197 SD in days of suicidal thoughts than their cash counterparts.

Finally, we use our incentivized labor supply elicitation exercise as a revealed preference validation of the psychosocial effects of employment. 97% of former workers express desire to work an additional week: 99% of these individuals complete this additional work, with 70% willingly doing so for no pay and 55% foregoing at least $2.5 USD to work for no pay. Such behavior underscores that the self-reported psychosocial measures are plausibly reflective of an internalized non-pecuniary value to the experience of employment.

### 5.7 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. But what about the experience of employment generates psychosocial value? We engage the richness of our data to explore, as best we can, the various dimensions of employment that may drive the impacts we document. We first consider features of the work experience itself, and then consider what types of individuals gain psychosocial value from employment.

### 5.7.1 Features of work

**Time use** Does employment improve wellbeing by allowing participants to substitute time away from unsavory or psychosocially costly activities? Table 5 presents how cash and work arms use their time. We document no statistically significant difference between the two arms in the number of hours that respondents report spending across a variety of activities, with the exception of going to the market, in which workers report spending 0.13 hours more per day. As we are powered to detect changes of at least twenty minutes for each activity, our results suggest that large substitutions away from unsavory activities are unlikely to be driving the improvements in psychosocial wellbeing, insofar as the respondent recalls.[27][28]

Perhaps those who were, or perceived themselves to be, more idle prior to being employed benefit more from employment? Appendix Table A9 examines heterogeneity in psychosocial impacts by baseline hours idle: we find no differential psychosocial benefits to employment for those who reported being more idle at baseline, suggesting that the elimination of boredom *per se* is not the driving force behind the psychosocial value of employment.

**Sociability** Does the social nature of the work task drive the effects we document? 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. We observe a precisely estimated 0.18 SD ($p = 0.041$) increase in the number of people respondents report conversing with (asked with reference to the previous day) in the employment arm relative to their cash counterparts. This translates, however, to one additional person per day. As described above, the amount of time respondents report spending socializing is also not significantly different between the employment and cash groups. These estimates suggest that, while a possible channel among others, the value of employment is not primarily derived from socializing.[29]

In an analysis described further below, we also find that, among those in the employment arm, reported conversations are significantly higher for those who did *not* work in the previous day relative to those who did (Column 3 of Table 7), underscoring that the improvements we document

[27] Life in the camps is unstructured. Most respondents do not track their day by time, making collection of reliable time use data particularly challenging (though recent literature documents the broader unreliability of such data CITE). We piloted a variety of strategies and encountered challenges to each, settling on asking respondents how much time they spent on a representative set of activities in the previous day. Methods of collecting time use that avoid the problem of recollection, 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 and cellphone reception.

[28] Given that individuals in the employment arm report spending an average of 2.5 hours per day engaged in the work assignment and nearly all workers complete their work, the absence of time use effects is perhaps surprising. The work task was designed to occupy the worker throughout the day, but for short periods: 2.5 hours total on the job implies an average of less than 40 minutes per sheet. Given the distribution of work time over the course of the day, 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: s(he) may have woken up thirty minutes earlier, napped fifteen minutes less, spent ten fewer minutes eating, and lounged fifteen fewer minutes by the tea stall. 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 non-pecuniary impacts we document.

[29] Notably, socializing with the 'employer,' or the enumerators, was held constant across all three treatment arms.

in sociability are unlikely to be due to the nature of the work itself, and may instead be a product of the way employed individuals newly engage with the world.

**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? To explore this question, we build a mechanism experiment into our employment arm, in which we provide a random subset of the employed with a calendar marked with every date of work for the eight week duration. The remainder receive a calendar with no schedule and are instead informed weekly about their work week schedule.[30] To eliminate differences in expected pecuniary returns, information around total work and total pay are held constant between the two arms. Results are presented in Table 6. We find no impact of a certain schedule on our pre-specified outcomes of a respondent's sense of stability, risk, or time preferences, suggesting that the additional predictability imposed by a calendar does little to alter wellbeing.[31]

Despite this exercise, we cannot causally estimate the role of the structure offered by employment alone - relative to no employment - on wellbeing, as the structure imposed by regular long-term employment is coextensive with employment itself. Indeed, we find our measure of stability, which asks respondents how secure they feel at the moment and expect to feel in the future, to meaningfully increase among the employed relative to their cash counterparts, suggesting a meaningful role of these elements of employment in improving psychosocial wellbeing.

**Purpose and status** Did the purposefulness of the employment activity - namely that the survey task may help improve the lives of refugees in the camp - contribute to the benefits we document? Similarly, did workers derive a status boost from publicly performing work which may have been perceived as 'high-status' given the use of a pen and paper?

Our employment intervention was described to be work of value, a design choice we made to mimic the purposefulness of employment typical to camp life - building roads, constructing latrines and homes, working in agriculture, assisting in clinics and children's centers, and the like - as well as literature on the key features of employment. 'Employment,' or 'work,' has been documented in a long history of sociological literature to typically involve a productive activity with a clear objective or goal rather than an undoing of effort (Jahoda, Lazarsfeld, and Zeisel, 1971; Terkel, 1974), with oral histories and surveys in the field identifying blue collar or service work as being perceived as especially purposeful by members of each profession (Terkel, 1974; Lamont, 2002; Poll, 2018). Recent empirical work by Soffia, Wood, and Burchell (2021) further finds that, even among

[30] We are motivated here by psychology literature around the value of setting short-term goals to combat depression and achieve longer term stability (Johnston et al., 2007; Crane et al., 2010; Ahrens, 1987), as well as work that documents individuals' positive willingness to pay to alleviate uncertainty (Lovallo and Kahneman, 2000). Our baseline data also offers suggestive evidence that considerations of the future play an important role in psychosocial wellbeing: 92% of those who report concerns of the future also report that idle time is "somewhat or very unpleasant."

[31] We see a significant negative impact on our pre-specified measure of agency, a revealed preference question in which a respondent expresses willingness to participate in a committee to allocate funding for his or her community. We suspect that this negative impact emerges less from a reduction in agency and more from an avoidance of further obligations: the calendar may have inadvertently overwhelmed respondents' sense of future responsibility.

pink and white collar workers who previous sociological commentary (Graeber, 2018) has popularly opined have little value, only 4.8% of surveyed workers express that they "do not have the feeling of doing useful work." We therefore chose not to embed variation in the experiment around the existence of a purpose to employment.

Similarly, the typical job available to our participants - whether in the camps, in the Bangladesh host community, or prior to their exodus in Myanmar - is publicly observable work. We chose a pen and paper task over manual labor in order to accommodate females in our sample. Given its synonymousness with employment in our context, we likewise chose not to experimentally vary the public nature of the work, from which status might be derived.

Instead, we explore the potential role of both purpose and status by examining heterogeneity by respondent's baseline sense of value to their community. In particular, we ask individuals at baseline to think of the most valuable person in their community, and rank themselves against this individual. We consider this as a measure of one's perceived purposefulness to and status within one's community. If either of these dimensions of the employment task are driving the psychosocial effects we observe, they are likely to be doing so more for those who rank themselves poorly in this measure at baseline, for whom there is room to grow. Appendix Table A9 presents the results of this heterogeneity analysis: those who rank themselves as contributing less to their community at baseline do not appear to benefit differentially more from the employment intervention. While only correlational evidence, these results suggest that the purpose or status derived from the objective of their work - namely, to be valuable to one's community - is not a driving force in the wellbeing effects we find.[32]

**Experience of work** Insofar as we can detect, neither time use, structure in work, the social nature of the work, nor purposefulness to community can meaningfully explain the variation we document in psychosocial wellbeing between the employed and those receiving cash alone. Is the nature of the work itself then irrelevant? In other words, would labeling one group "employed" and paying them over two months for an instance of work have generated the same psychosocial value? To test this, we exploit individual and temporal variation in the days that a member of the employment arm is assigned work. We run the following regression:

$$Y_{ibct} = \beta_0 + \beta DaysSinceWork_{ibct} + t + \eta_i + \gamma_c + \delta_e + \varepsilon_{ibct}$$

where $Y_{ibct}$ represents weekly measures of wellbeing, $DaysSinceWork_{ibct}$ represents the number of days between the day of the weekly survey and the most recent day of employment, $\eta_i$ is individual fixed effects, $t$ is a weekly time trend, and $\gamma_c$, $\delta_e$, $X_{ibc}$, $\epsilon_{ibct}$ are as defined above. If the actual

[32]It is likely that the mere engagement in productive activity may generate psychosocial value in employment. A lab experiment by Ariely, Kamenica, and Prelec (2008) finds that individuals derive greater value from completing a 'meaningful' task (building legos that are preserved) relative to a 'meaningless' task (building legos that are destroyed); at this foundational level, 'purpose' is a likely channel through which psychosocial value is derived from employment. While it remains to be tested whether digging ditches only to refill them, or similar explicitly useless tasks, might still confer psychosocial value in contexts such as ours, they are also not representative of the experience of employment, both in our camp setting and globally.

experience of working, rather than simply the identity of being employed, affects psychosocial wellbeing, we expect our coefficient of interest $\beta$ to be negative: the longer it has been since one last worked, the less one benefits from being employed.

Results are presented in Table 7. One additional day away from work is associated with a statistically significant reduction in wellbeing of 0.05 SD and increase in stress of 0.03 SD. This suggests that margins of the work experience that we are unable to test for given their synonymousness with employment in our context - such as engaging in productive activity, having regularity in one's day, or the identity of being employed - may be contributing to the psychosocial value of employment.

### 5.7.2 Features of the individual

This motivates our second set of analyses, which considers the features of the individual who might benefit most in psychosocial wellbeing from employment. We consider first the role of gender, and then explore how less traumatized, less depressed, and more extroverted respondents react to the employment opportunity.

**Gender** Existing literature offers plausible reasons for either men or women to gain more from employment relative to cash alone. Sociological work around the loss of employment and its relationship with gender identity (Payne (1998); Schrijvers (1997)) suggest that job loss leads to greater male aggression in the home due to a greater sense of powerlessness and lack of agency (Annan and Brier (2010); Heltberg, Hossain, and Anna Turk (2012); Kabeer (2015); Ondeko and Purdin (2004); Wirtz et al. (2014); Patinkin (2014); Bhalotra et al. (2020)). This work is consistent with recent evidence that the COVID-19 lockdowns of 2020, which increased the presence of males in the home due to work-from-home regulations and job loss, was correlated with an increased incidence of domestic and intimate partner violence (Economist (2020); Godbole (2020)). In such a context, males may gain a greater sense of agency and power through employment, and employed women may likewise benefit from less time within the household. We are further motivated by literature in economics around how employment may raise the household bargaining power of females (a more thorough review of which can be found in McKelway (2020)), although it remains an open question in this literature whether such gains are derived from the nature of the employment itself or simply from its function as a source of income. Our analysis offers some insight along this margin.

Table 8 presents the impacts of the employment and cash arms separately by gender. We find suggestive evidence that the bulk of the non-pecuniary benefits of employment is concentrated among males, for whom the psychosocial impact of employment is substantially and significantly different from that of cash. In contrast, females, while benefiting considerably from employment, also appear to benefit from cash alone, and we cannot reject equality between the work and cash coefficients for females. Appendix Table A10 presents results for various features of the mental health index. Males experience a substantial reduction in both depression and stress when employed, but no reduction from cash alone, while females experience modest reductions from both the employment and the cash treatments. Similarly, while only employed males experience an improvement

in self worth, females report increases in self worth (though imprecise) regardless of employment status.

Our finding that females experience psychosocial improvements from the cash treatment arm are echoed in the results for the household power index (Table 8, Column 3). 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. This finding is consistent with Bastagli et al. (2019)'s meta-analysis of cash transfer programs, which finds a marked improvement in female empowerment measures across a variety of cash-transfer field experiments. We find no parallel shift in men's beliefs about female bargaining power in either treatment arm.

Finally, we find the single measure for which employed women shift differentially more than their cash counterparts is the work rights index (Table 8, Column 5): unsurprisingly, only employed women update their beliefs around whether women should be permitted to work outside of the home.

Our finding that males benefit disproportionately from the non-pecuniary dimensions of employment, and experience negligible improvement in psychosocial wellbeing from cash alone, prompts a second examination of the psychosocial benefits of employment in which we focus only on males and pool the two non-employment (cash and control) arms. Breaking out the index for depression, we find that employed males report 22% fewer days with suicidal thoughts, are 31% less likely to be moderately or severely depressed, and are 83% more likely to qualify as not depressed than their unemployed counterparts. Employment appears to confer remarkably meaningful improvements upon the mental health and wellbeing of the men in our setting.

**Baseline violence, sociability, and depression:** We first investigate whether the benefits of employment are mediated by the intensity of violence participants experienced in their recent past. We are motivated here by a literature in psychology that recognizes a key predictor of depression to be the repetitive contemplation of typically dark thoughts around past trauma (Michael et al., 2007; Ehring, Frank, and Ehlers, 2008; Roley et al., 2015). As the vast majority of the 80 million forcibly displaced people globally flee due to conflict and violence, this is a question of particular policy relevance.

The unanticipated and indiscriminate nature of the 2017 Rohingya genocide in Myanmar presents a unique opportunity to examine the impact of past violence on the psychosocial benefits of employment.[33] Exploiting the quasi-random variation in violence, we find that the killing of a loved one is significantly correlated with depression at baseline, and that individuals who experienced death but were recently employed are substantially less likely to be depressed at baseline. However, this pattern is vulnerable to selection into employment, a challenge that our experiment

[33] A United Nations' 2018 Human Rights Council Report emphasizes the indiscriminate nature of this violence (Appendix Figure A3). This is consistent with our baseline data: conditional on township of origin, we find that refugees who report having experienced the death of at least one family member or community member in the military raids are no different on a set of key sociodemographic observables from those who did not experience a death.

allows us to tease out. Utilizing our exogenous work opportunity, we find that those who experienced greater violence at baseline indeed benefit a statistically significant 26.7 percentage points more from the employment intervention than their non-death counterparts in terms of likelihood of depression (Appendix Table A13, Panel A). While the point estimates of the employment treatment effect relative to the cash group suggest that the bulk of this heterogeneous effect is due to the non-pecuniary dimensions of employment, we lack the statistical power to say so definitively (Appendix Table A13, Panel B).

We consider heterogeneity by two additional measures of baseline wellbeing: sociability and depression. The former is motivated by a concern that, while a work task with a sociable component such as the one we offer may be valued by extroverts, introverted individuals (as measured by baseline sociability) may find the work emotionally taxing. Results are presented in Appendix Table A14, Panel A. We find that sociable individuals benefit significantly more from work, in terms of depression rates, than their less-sociable counterparts. As with our analysis on violence, point estimates of the employment treatment effect relative to the cash group suggest that this effect is at least partially due to the non-pecuniary dimensions of employment, but we lack the statistical power to say so definitively (Appendix Table A13, Panel B). Importantly, we find no evidence that the employment intervention is psychosocially harmful to those who are less sociable at baseline.

Our examination of heterogeneity by depression is motivated by a body of psychological literature that explores the potential vicious cycle of depression, in which those who are especially depressed lack the ability to recall positive pasts (Teasdale, 1983) or conceive of possible futures (Roepke and Seligman, 2016), thereby sinking further into depressed states in which they may not be able to benefit psychosocially from employment (Haushofer and de Quidt, 2019). Results are presented in Appendix Tables A16. We find no evidence that this is the case and suggestive evidence that the employment program is, in fact, differentially more impactful for those who were depressed at baseline: moderately depressed individuals experience a marginally significant 0.3 SD greater reduction in their PHQ score than their non-depressed counterparts.[34] However, this pattern is echoed neither in their binary likelihood of being depressed nor in their overall mental health score, prompting a cautious reading of this result.

### 5.7.3 Benchmarking main effects

We now turn to examining our results within the broader context of cash transfer, anti-poverty, and psychosocial health interventions.

**Benchmarking the employment impact** How do our employment impacts then compare to alternative anti-poverty programs or targeted psychotherapy programs? Ridley et al. (2020) perform a meta-analysis of the mental health impacts of multi-faceted anti-poverty interventions

[34]By magnitude, the heterogeneous treatment effect on this intensive margin of depression is consistent with and comparable to the findings of Baranov et al. (2020) and Islam et al. (2021), both of whom find the impacts of psychotherapy programs to be substantially greater for individuals who are depressed at baseline.

(eg. livestock transfer, business training, employment, health subsidies, etc.) and find an average effect of 0.2 SD per $1000 PPP in cash transfers (or 0.024 SD per $120 PPP); the effects we document are nearly ten times this magnitude. 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 PTSD. These programs are targeted towards depressed individuals who receive repeated face-to-face counseling sessions over an average of 2.5 months. Our employment program generates 40% of this effect. Perhaps most relevant is a recent study of a year-long 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 0.14 SD reduction in depression. In comparison, the employment program we consider generates a 0.19 SD reduction in depression across our full sample, with the effect on women at an imprecisely estimated 0.11 SD.

**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 $120 PPP are likewise associated with a 0.12 SD improvement in mental health. In line with these approximations, our cash transfer, which is valued at $120 PPP and at least a doubling of daily consumption, exhibits a 0.12 SD impact (though imprecise) on the mental health index of women. However, 0.12 SD lies outside the 90% confidence interval for the impact we find on men of 0.029 SD. For men, an increase in income alone does not appear to be linked to an improvement in wellbeing in our setting.

This small impact of a large cash transfer on men is perhaps surprising, as our context is one in which participants appear to have a high demand for cash: having lost their home, land, and assets to the Myanmar military when fleeing, the limited rations they receive in lentils, rice, and oil are often resold to secure the cash needed to purchase basic staple foods such as salt and vegetables. We view this finding as opening a set of questions around the value of cash transfers in environments with scarce employment opportunities and limited leisure activities, with potential policy implications for UBI in the developing world and the future of work.

Alternatively, a meta-analysis of the mental health impacts of cash transfer programs Ridley et al. (2020) find that a $1000 PPP cash transfer generates an average mental health impact of 0.12 SD. A linear interpolation implies that our $120 PPP transfer would yield a 0.014 SD impact on mental health, which is well within the confidence interval of our impact of cash estimates.

# 6 Conclusion

Cumulatively, our analyses shed light on the psychosocial impacts of employment and the various mechanisms mediating the relationship we identify. We find that employment engenders significant psychosocial value beyond that brought about by income alone, and workers are able to at least

partially price these benefits into their labor supply decisions. We offer four considerations with regard to these findings.

First, our study engages a migrant population that experienced a level of violence in their exodus that is perhaps uniquely horrific, and as such, one may be concerned about the generalizability of our findings. We do not claim external validity around all results in this experiment, hopeful that the upwards of one million Rohingya who have shared the experiences of our sample population is meaningful. However, our participants are characterized by three features that are likely to be shared by many other subpopulations of interest: they face severely constrained labor market opportunities, are materially impoverished, and have a limited choice-set of alternative leisure activities to engage in when not working. These features are encountered globally not only by many forcibly displaced migrants, but also the incarcerated, the long-term unemployed (particularly in - though not limited to - the developing world), and many of the world's rural poor.

Second, the psychosocial value to employment may extend beyond settings of scarce labor market opportunities. Indeed, if one is employed among a sea of unemployed, their social status and consequently wellbeing are likely to increase (Marmot et al., 1991; Redelmeier and Singh, 2001; Anderson and Marmot, 2012): we document meaningful improvements in self-worth vis-à-vis one's family and one's community among the employed. However, it remains an empirical question whether the psychosocial value of employment is greater when unemployment rates are high, thereby raising the status-utility of employment, or when they are low, raising the status-*dis*utility of unemployment.

Third, our study finds that the majority of refugees in our setting are willing to work for zero pay, and in fact willing to forego a sizable transfer in order to work for free. When choosing between cashfare and workfare programs in similar contexts, policymakers may therefore favor the latter 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 likely to be long-term and intergenerational 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 likely the source of these low reservation wages, legal protections in the form of wage floors may be particularly important to welfare in the environments these legal restrictions produce.

Finally, 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 plausible contexts within which this is not the case. On the most proximate margin of policy relevance, organizations such as the UNHCR invest significant capacity into securing refugees the right to work in their host countries (Nations (2018)); estimates of the psychosocial benefits to employment may serve as valuable evidence for their efforts. Beyond these margins, we view this present study as groundwork for deepening our conception of 'labor' through several 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 an insufficient means of improving wellbeing, despite profound poverty? And how might the scarcity of meaningful activity or lack of future direction transform the concept of 'time' from a valuable resource into an amorphous and costly experience?

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# Tables

Table 1: Balance

| | (1) Control | (2) Cash | (3) Work | (4) (1) vs. (2) | (5) (1) vs. (3) | (6) (2) vs. (3) |
|---|---|---|---|---|---|---|
| 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 Ag. Work | 0.62 | 0.66 | 0.65 | 0.74 | 0.64 | 0.92 |
| Math ability (index) | 2.61 | 2.59 | 2.58 | 0.90 | 0.43 | 0.38 |
| Digit Span Score (Total) | 5.94 | 6.07 | 6.13 | 0.63 | 0.18 | 0.35 |
| Wellbeing (index) | -0.12 | 0.05 | 0.03 | 0.15 | 0.03 | 0.83 |
| Life Satisfaction | 11.04 | 10.85 | 11.21 | 0.62 | 0.05 | 0.22 |
| Self-worth (relative) | 13.95 | 14.62 | 13.96 | 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 (avg) | 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 | 7.44 | 7.40 | 7.61 | 0.92 | 0.27 | 0.36 |
| Power Perceptions | 10.87 | 10.70 | 10.86 | 0.19 | 0.85 | 0.07 |
| Work Perceptions | 3.77 | 3.60 | 3.76 | 0.61 | 0.63 | 0.31 |
| Persistent Illness (>7) | 0.30 | 0.33 | 0.28 | 0.89 | 0.26 | 0.24 |
| Days Sick | 6.07 | 6.38 | 5.58 | 0.98 | 0.04 | 0.05 |
| PHQ Scale | 8.19 | 8.73 | 8.20 | 0.31 | 0.80 | 0.18 |
| Sev. Depressed | 0.05 | 0.11 | 0.09 | 0.06 | 0.05 | 0.59 |
| Stress (index) | 9.48 | 9.94 | 9.49 | 0.24 | 0.96 | 0.18 |
| Number of conversations | 16.13 | 16.35 | 16.48 | 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 | | | |

**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 of the difference between the control and work treatments, while column (6) shows the p-value between cash and work.

Table 2: Impacts on psychosocial wellbeing (SD)

| | | Individual Components of PS Index | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) **PS Index** | (2) PHQ | (3) Stress | (4) Life Sat. | (5) Social | (6) Self Worth | (7) Control | (8) Stability |
| Work | 0.214*** | -0.185*** | -0.258*** | 0.301*** | 0.167** | 0.143* | 0.310*** | 0.249*** |
| | (0.039) | (0.060) | (0.096) | (0.075) | (0.082) | (0.080) | (0.116) | (0.081) |
| Cash | 0.045 | 0.001 | -0.060 | 0.237*** | 0.083 | -0.075 | 0.047 | 0.055 |
| | (0.049) | (0.071) | (0.108) | (0.087) | (0.100) | (0.087) | (0.144) | (0.102) |
| Shrp. q-val Work | | 0.006 | 0.007 | 0.001 | 0.015 | 0.023 | 0.007 | 0.006 |
| Test Work=Cash | 0.000 | 0.006 | 0.022 | 0.350 | 0.324 | 0.002 | 0.031 | 0.033 |
| Shrp. q-val Work=Cash | | 0.018 | 0.035 | 0.112 | 0.112 | 0.015 | 0.035 | 0.035 |
| Observations | 726 | 726 | 726 | 726 | 726 | 726 | 726 | 726 |

**Notes:** All outcomes are standardized. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Impacts on physical health, cognitive health, and preferences

| | (1) Days Sick | (2) Days Sick >7 | (3) Cognitive Index | (4) Risk Av. | (5) Time Pref. |
|---|---|---|---|---|---|
| Work | -0.780* | -0.044 | 0.182*** | -0.656** | -0.119 |
| | (0.411) | (0.049) | (0.068) | (0.291) | (0.323) |
| Cash | -0.054 | 0.007 | 0.057 | 0.028 | -0.074 |
| | (0.479) | (0.055) | (0.077) | (0.342) | (0.336) |
| Shrp. q-val Work | 0.070 | 0.229 | 0.045 | 0.055 | 0.399 |
| Test Work=Cash | 0.064 | 0.204 | 0.030 | 0.016 | 0.850 |
| Shrp. q-val Work=Cash | 0.081 | 0.119 | 0.081 | 0.081 | 0.343 |
| Observations | 726 | 726 | 726 | 726 | 726 |

**Notes:** 'Cognitive Index' is an inverse covariance-weighted index of forward and backwards digit span tests and two arithmetic questions. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impacts on consumption

Panel A

| | (1) Luxury | (2) Necessary | (3) Total Cons. |
|---|---|---|---|
| Work | 39.380 | 43.785 | 91.039 |
| | (31.302) | (152.298) | (176.379) |
| Shrp. q-val Work | 1.000 | 1.000 | 1.000 |
| Mean in Cash | 330.56 | 2036.86 | 2404.53 |
| Observations | 564 | 564 | 564 |

Panel B

| | (1) Savings | (2) Borrowing | (3) Lending |
|---|---|---|---|
| Work | 59.049 | 60.824 | 32.992** |
| | (63.535) | (182.384) | (15.947) |
| Shrp. q-val Work | 0.550 | 0.973 | 0.138 |
| Mean in Cash | 218.94 | 1467.11 | 37.11 |
| Observations | 564 | 564 | 726 |

**Notes:** 'Luxury' is made up of the following consumption categories: meat or fish, paan or cigarettes, tea, and electronics. 'Necessary' is made up of the following consumption categories: fruits or vegetables, health, education, household supplies, and clothing. 'Savings' is the total savings reported at endline; 'Borrowing' is the total amount in loans respondent has at endline. 'Lending' is the total amount lent in the previous two weeks. Quantities reported are total amount spent in given category during the previous two weeks. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Impacts on time use

| | (1) Idle | (2) Chores | (3) Social | (4) Ration | (5) Market | (6) Pray | (7) Wash | (8) Sleep | (9) Child |
|---|---|---|---|---|---|---|---|---|---|
| Work | -0.050 | -0.004 | 0.121 | 0.003 | 0.135** | -0.099 | -0.047 | -0.099 | 0.035 |
| | (0.094) | (0.109) | (0.176) | (0.066) | (0.063) | (0.081) | (0.038) | (0.079) | (0.071) |
| Shrp. q-val Work | 1.000 | 1.000 | 1.000 | 1.000 | 0.480 | 0.836 | 0.836 | 0.836 | 1.000 |
| Cash Mean | 2.652 | 1.053 | 2.655 | 0.155 | 0.528 | 1.655 | 0.742 | 0.696 | 1.174 |
| Observations | 564 | 564 | 564 | 564 | 564 | 564 | 564 | 564 | 564 |

**Notes:** 'Idle' is the average number of hours respondent reports being idle per day. 'Chores' is the amount of time reported spend on household chores such as cooking or fetching water. 'Social' is the amount of time spend socializing with others. 'Ration' is the amount of time spend getting household rations. 'Market' is the amount of time spend at the market. 'Pray' is the amount of time spent praying. 'Wash' is the amount of time spent on personal hygiene. 'Sleep' is the amount of time spent day sleeping. 'Child' is the amount of time spent on childcare. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Impact of certainty treatment

| | (1) Stability | (2) Dist. Mon. | (3) Risk Aversion | (4) Time Pref. |
|---|---|---|---|---|
| Received Schedule | -0.065 | -0.172** | -0.111 | 0.041 |
| | (0.077) | (0.085) | (0.100) | (0.103) |
| Shrp. q-val Schedule | 0.678 | 0.222 | 0.676 | 1.000 |
| Observations | 403 | 403 | 403 | 403 |

**Notes:** Sample includes only those in the employment arm. 'Received Schedule' are those who received the calendar the complete two month work schedule marked. All outcomes are standardized. 'Dist. Mon.' is a revealed preference question on whether respondent is interested in joining a committee to determine how funds will be allocated to the community. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Days since work

| | (1)<br>Wellbeing | (2)<br>Stress | (3)<br>Sociability |
|---|---|---|---|
| Days Since Work | -0.054*** | 0.037*** | 0.051*** |
| | (0.013) | (0.013) | (0.015) |
| Observations | 3159 | 3176 | 3176 |

**Notes:** —. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Heterogeneity by gender

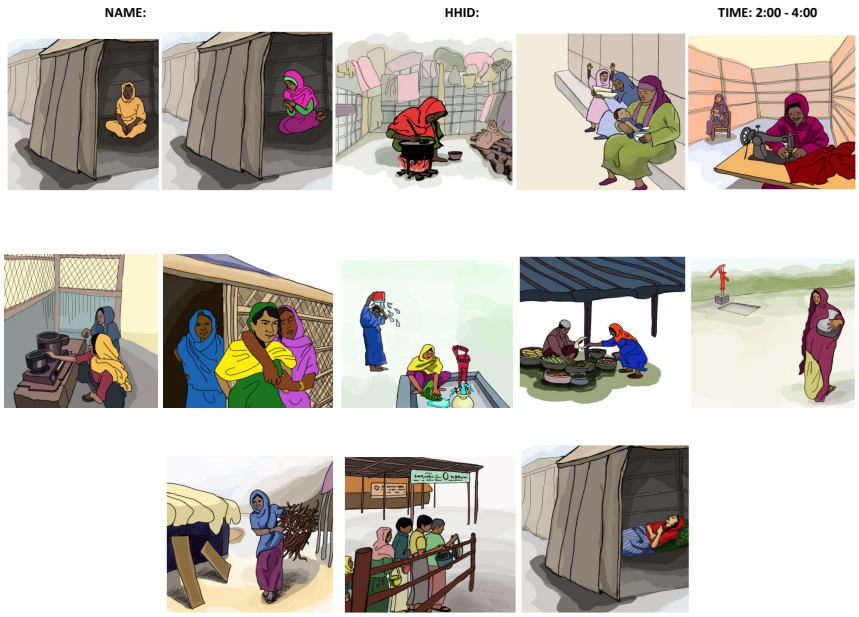
| | Psychosocial Index | | Household Power Index | | Work Rights Index | |
|---|---|---|---|---|---|---|
| | (1) Female | (2) Male | (3) Female | (4) Male | (5) Female | (6) Male |
| Work | 0.193*** | 0.224*** | 0.312** | 0.025 | 0.344*** | 0.083 |
| | (0.063) | (0.050) | (0.126) | (0.094) | (0.111) | (0.098) |
| Cash | 0.125 | 0.023 | 0.267* | 0.078 | 0.110 | 0.052 |
| | (0.078) | (0.059) | (0.140) | (0.098) | (0.118) | (0.117) |
| Test: Cash = Work | 0.311 | 0.000 | 0.742 | 0.530 | 0.018 | 0.745 |
| Shrp. q-val Cash=Work | 1.000 | 0.002 | 1.000 | 0.361 | 0.246 | 0.425 |
| Test: Male = Female | 0.118 | 0.118 | 0.726 | 0.726 | 0.203 | 0.203 |
| Observations | 220 | 502 | 220 | 502 | 220 | 502 |

**Notes:** All outcomes are standardized. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Adjusted p-values are reported using the full set of mental health and empowerment outcomes rather than just the three tests reported above. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

# Figures

Figure 1: Work-Tasks

(a) Female




(b) Male

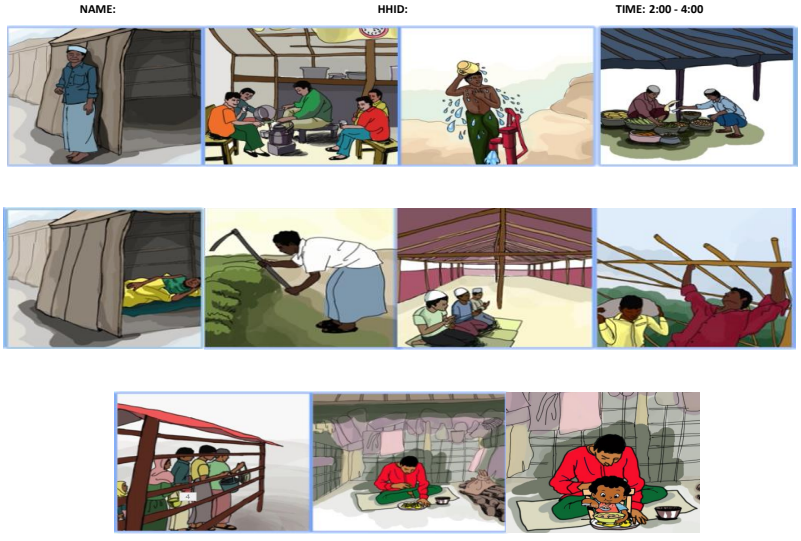
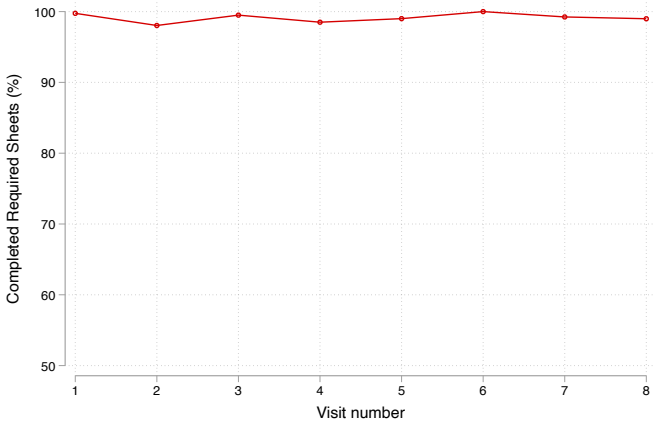
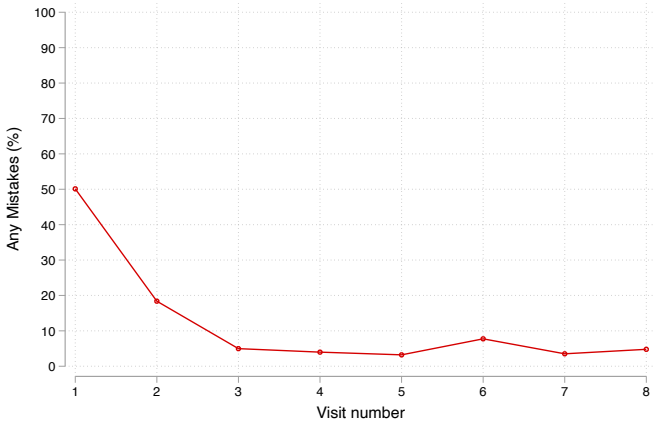


Figure 2: Work Completion Measures

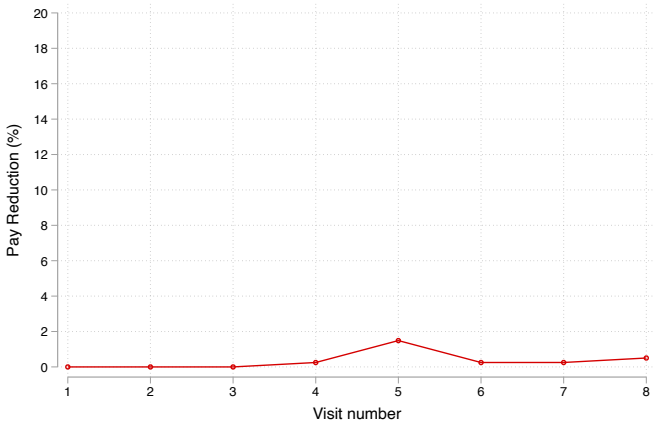
(a) Whether work was completed




(b) Whether any mistakes were made

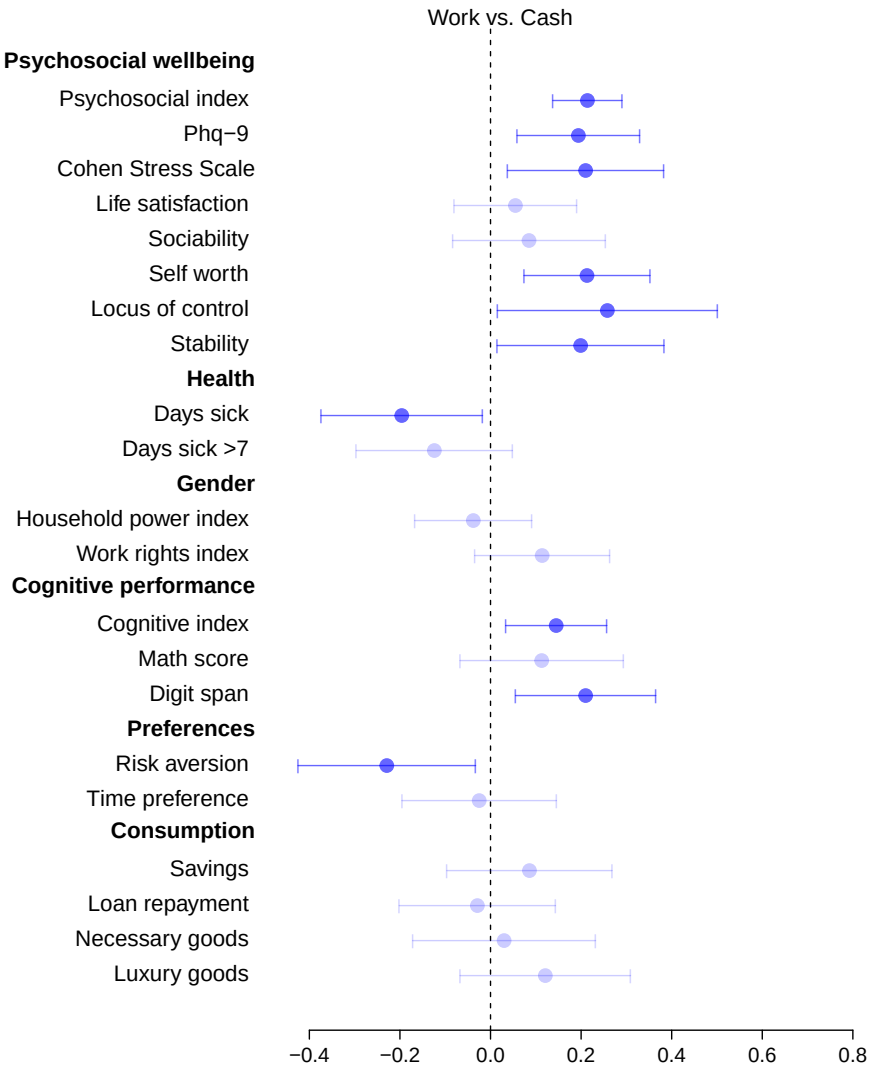



(c) Whether pay was docked




**Notes:** This figure exhibits the fraction of individuals in the employment arm who completed their work (Panel A); made any mistakes (Panel B), and received a pay penalty for poor work (Panel C) over the 8 weeks of the study.

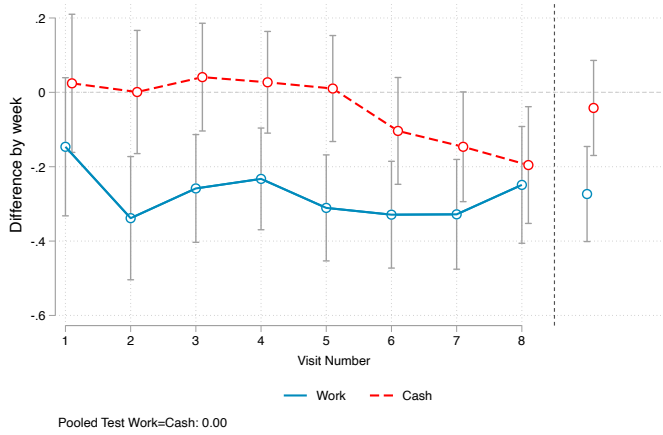
Figure 3: Main Treatment Effects



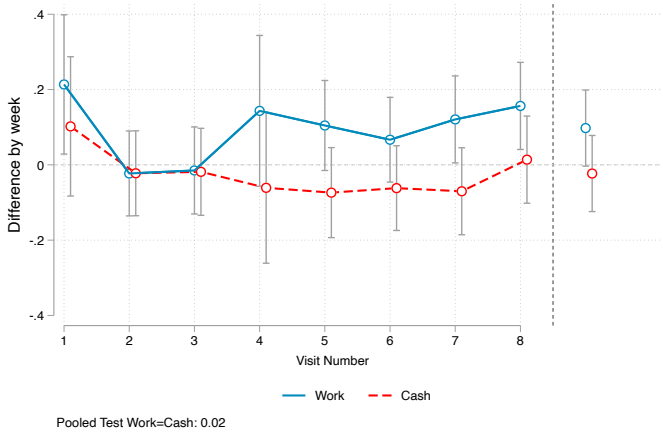

**Notes:** This figure plots the point estimates and 95% confidence intervals for each outcome in the work treatment group relative to the cash group. All outcomes are standardized. The scales for PHQ-9 and the Cohen Stress Scale have been reversed from previous tables so that positive values represent better outcomes.

Figure 4: Weekly trends in psychosocial measures

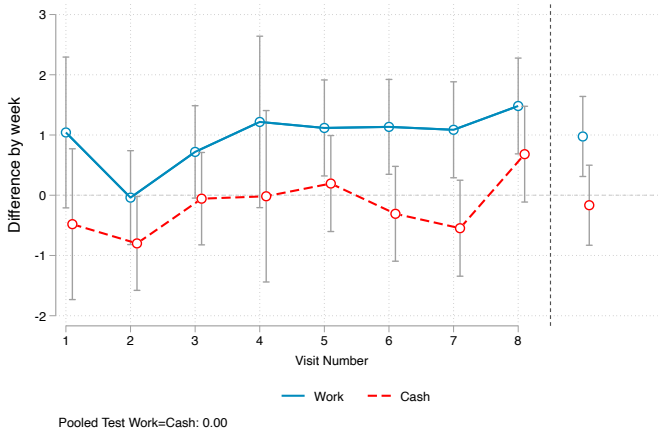
(a) Stress Index



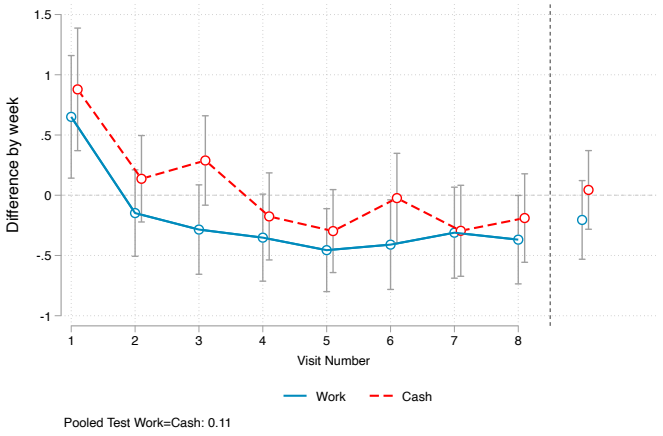

(b) Sociability




(c) Positive Conversations



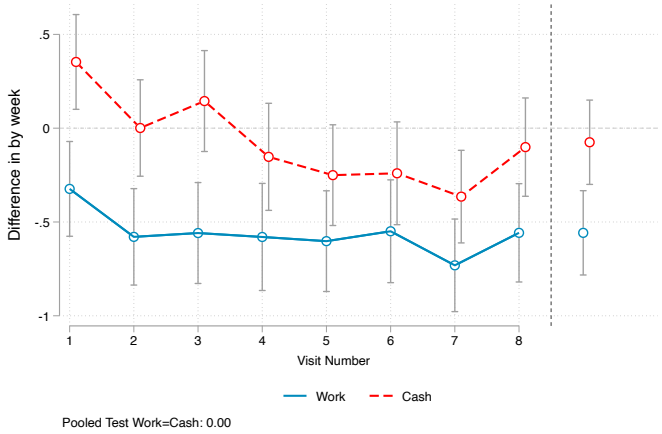

(d) Negative Conversations



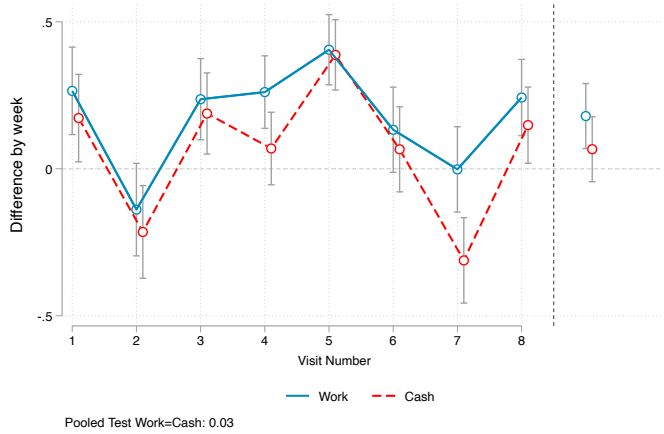

**Notes:** Each figure plots the impact of the treatment (work or cash) by week relative to the control arm. The estimates to the right of the dotted line represent the pooled effect across all eight weeks.

Figure 5: Weekly trends in physical and cognitive measures

(a) Days sick



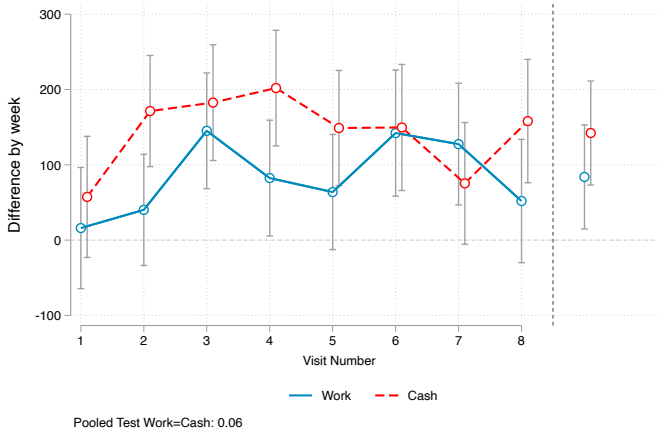

(b) Arithmetic performance



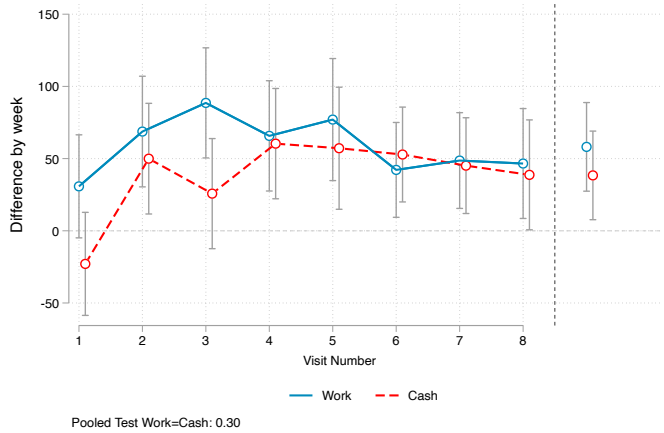

**Notes:** Each figure plots the impact of the treatment (work or cash) by week relative to the control arm. The estimates to the right of the dotted line represent the pooled effect across all eight weeks.

Figure 6: Weekly trends in spending and saving

(a) Amount spent

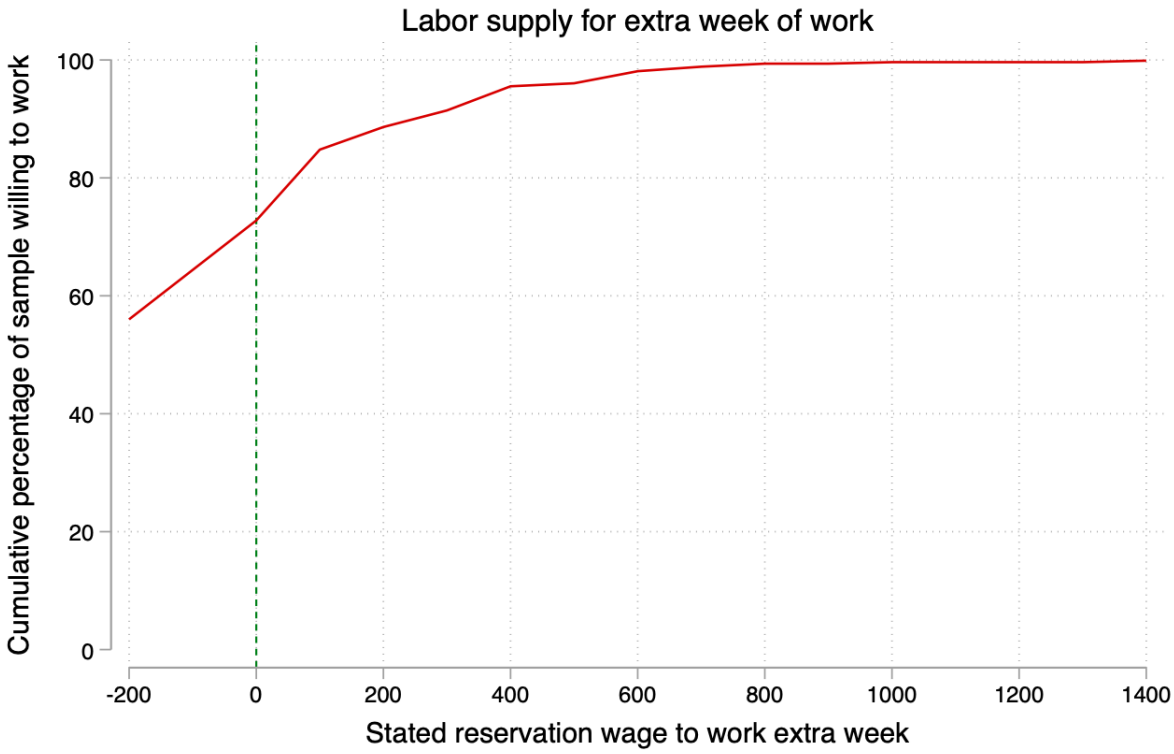



(b) Amount saved




**Notes:** Each figure plots the impact of the treatment (work or cash) by week relative to the control arm. The estimates to the right of the dotted line represent the pooled effect across all eight weeks.

Figure 7: Labor Supply Curve



# A Appendix

## A.1 Tables

Table A1: Baseline Mental Health and Idleness

| | (1)<br>Depressed at baseline |
|---|---|
| No work in last month | 0.160*** |
| | (0.059) |
| Mean of outcome | 0.78 |
| Observations | 726 |

**Notes:** "Depressed at baseline" is a binary variable equalling one if PHQ score is greater than 4 (encompassing those with mild, moderate, and severe depression). Regressions include camp fixed effects and controls selected by lasso. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Outcome Variable Descriptions

| | |
|---|---|
| **Psychological Well-being** | |
| PHQ9 | The standardized total score of 9 questions from the Patient Health Questionnaire-9 (PHQ9) |
| Life Satisfaction Index | A standardized average of survey responses to four questions from Diener's standardized scale, responses made along a seven-point Likert scale. |
| Stress Index | The standardized total score from three elements of adapted from the Cohen Stress scale. "How many of the last 7 days have you [been able to fall asleep peacefully / felt nervous / felt frustrated]?" |
| Sociability (Total) | The total number of conversations in the past day with adults. |
| Sociability (Positive) | The total number of conversations in the past day with adults that the respondent felt were positive. |
| Self Worth Index | The standardized total score from the responses on a scale from 1 to 10 to three questions: "Think of a person you know who you [respect / think helps] the most in your [family / community]. If that person is a 10 where would you put yourself?" |
| Locus of Control | The standardized total score from responses to four locus of control questions. "In the last 7 days, how many days did you feel that to a great extent your life is controlled by accidental/chance happenings..." |
| Allocation Decision Game | Indicator (yes / no) for response to an offer to participate an allocation committee to decide how money is spent. Participants are offered the opportunity to make a resource allocation decision for their community or have another individual (an NGO worker, an "expert", or another refugee) make the decision. |
| Stability Index | The standardized total score from responses to two stability questions using a Cantril ladder. "How secure [do you feel / think you will feel] [at present / five years from now]" |
| Physiological Index | A standardized inverse-covariance weighted average of the above indices. |
| **Gender Dynamics** | |
| Gender Perceptions - Work | The standardized total score of two questions regarding women's work, "How often would you agree that women should be allowed to work for a living [inside /outside] the block?" |
| Gender Perceptions - Violence (IPV) | The standardized total score of five questions regarding norms for intimate partner violence (IPV) from the Demographic and Health Survey (DHS). |
| **Financial Wellbeing** | |
| Savings | Response to the question "How much money do you currently have in savings?" During the collection surveys (midlines) this question instead asked "How much money did you save in the past week?" |
| Borrowing | Total amount of money the household has borrowed. |
| **Economic Decision Making** | |
| Risk Preference | Measured using incentivized responses to the multiple price list decisions adapted from Holt-Laury and Sprenger (2002). |
| TIme Preference | Measured by adapting Andreoni and Sprenger's (2011) convex time budget method following Giné et al. (2018). |

| **Other Outcomes** | |
|---|---|
| Cognitive Ability | A standardized weighted index of the number of correct responses to i) a digit span (forward and backward) memory test and ii) basic arithmetic problems including addition, subtraction, multiplication, and division. Only the arithmetic problems were included in midline. |
| Physical Health | An indicator for prolonged health problems that persisted for more than one week over the past month. Coded from a question asking respondents "In the past 30 days, how many days were you sick?". For the collection surveys (midline), this question was modified to ask "How many of the last 7 days did you feel sick?" |

Table A3: Outcome Variable Collection Periods

| | Basline | Midline | Weekly | Endline |
|---|---|---|---|---|
| **Psychological Well-being** | | | | |
| PHQ9 | X | | | X |
| Life Satisfaction Index | X | | | X |
| Stress Index | X | | X | X |
| Sociability (Total) | X | | X | X |
| Sociability (Positive) | X | | X | X |
| Self Worth Index | X | | | X |
| Locus of Control | X | | | X |
| Allocation Decision Game | | X | | X |
| Stability Index | | X | | X |
| Physiological Wellbeing Index | X | | | X |
| **Gender Dynamics** | | | | |
| Gender Perceptions - Work | X | | | X |
| Gender Perceptions - Violence (IPV) | X | | | X |
| **Financial Wellbeing** | | | | |
| Savings | X | | X* | X |
| Borrowing | X | | | X |
| **Economic Decision Making** | | | | |
| Risk Preference | | X | | X |
| Time Preference | | X | | X |
| **Other Outcomes** | | | | |
| Cognitive Ability | X | | X* | X |
| Physical Health | X | | X* | X |

*Physical Health, Savings, and Cognitive Ability are measured differently at midline than at baseline or endline.

Table A4: Intervention Timeline by Weeks

| | |
|---|---|
| T = 0 | Baseline Survey |
| T = 1 | Work Submission + Midline 1 |
| T = 2 | Work Submission + Midline 2 |
| T = 3 | Work Submission + Midline 3 |
| T = 4 | Work Submission + Midline 4 + Certificate Delivery |
| T = 5 | Work Submission + Midline 5 |
| T = 6 | Work Submission + Midline 6 |
| T = 7 | Work Submission + Midline 7 |
| T = 8 | Work Submission + Endline Survey 1 |
| T = 9 | Additional week of work |
| T = 15 | Endline Survey 2 |

Table A5: Psychosocial impacts in six week followup

| | (1) Wellbeing | (2) Life Satis. | (3) Locus of Cont. | (4) Sociability | (5) Stress | (6) Cognitive |
|---|---|---|---|---|---|---|
| Work | 0.361*** | 0.083 | -0.115 | 0.104 | -0.285** | -0.099 |
| | (0.106) | (0.102) | (0.086) | (0.108) | (0.116) | (0.103) |
| Cash | 0.215 | 0.132 | -0.083 | 0.044 | -0.055 | 0.021 |
| | (0.132) | (0.127) | (0.106) | (0.109) | (0.130) | (0.128) |
| Observations | 699 | 699 | 699 | 699 | 699 | 699 |
| Mean in Control | -0.23 | 0.10 | 0.23 | -0.11 | 0.17 | 0.07 |
| Test: Cash = Work | 0.251 | 0.672 | 0.727 | 0.464 | 0.040 | 0.242 |
| Shrp. q-val: Cash = Work | 0.720 | 1.000 | 1.000 | 1.000 | 0.320 | 0.720 |

**Notes:** Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Effect of receiving participation certificate

| | (1)<br>Psychosocial Index |
|---|---|
| Work | 0.258*** |
| | (0.082) |
| Work * Certificate | -0.021 |
| | (0.111) |
| Certificate | -0.044 |
| Assignment | (0.091) |
| Observations | 409 |

**Notes:** Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Future employment outcomes

| | (1) Day labor | (2) Salaried | (3) Any work | (4) Daily wage | (5) Expects work | (6) Total expected |
|---|---|---|---|---|---|---|
| Work | -0.030** | 0.008 | -0.020 | -21.328 | -0.139 | 135.167 |
| | (0.012) | (0.005) | (0.048) | (23.402) | (0.201) | (305.094) |
| Observations | 542 | 542 | 542 | 138 | 542 | 542 |
| Mean in Cash | 0.99 | 0.00 | 0.29 | 342.83 | 2.81 | 2289.31 |

**Notes:** Outcomes collected during the six-week followup survey. 'Day labor' includes agriculture and construction work. 'Salaried' work includes service and teaching work. 'Total expected' is the total compensation expected in the coming month if one were to find work. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Heterogeneity by baseline idleness

| | (1)<br>PS Index | (2)<br>PHQ | (3)<br>Depressed |
|---|---|---|---|
| Work | 0.170*** | -0.195*** | -0.085*** |
| | (0.048) | (0.070) | (0.031) |
| Work * Baseline | 0.016 | -0.001 | 0.004 |
| Idleness | (0.022) | (0.033) | (0.018) |
| Observations | 564 | 564 | 564 |

**Notes:** Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Heterogeneity by baseline sense of purpose or status

| | (1)<br>PS Index | (2)<br>PHQ | (3)<br>Depressed |
|---|---|---|---|
| Work | 0.157*** | -0.251*** | -0.107** |
| | (0.049) | (0.083) | (0.042) |
| Work * Low baseline purpose | 0.037 | 0.147 | 0.061 |
| | (0.073) | (0.130) | (0.070) |
| Observations | 564 | 564 | 564 |

**Notes:** Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Heterogeneity by gender: subcomponents of mental health

| | PHQ | | Stress | | Life Satisfaction | | Sociability | |
|---|---|---|---|---|---|---|---|---|
| | (1) Female | (2) Male | (3) Female | (4) Male | (5) Female | (6) Male | (7) Female | (8) Male |
| Work | -0.115 | -0.213*** | -0.090 | -0.324*** | 0.234* | 0.338*** | 0.202* | 0.133 |
| | (0.103) | (0.074) | (0.165) | (0.122) | (0.139) | (0.093) | (0.107) | (0.104) |
| Cash | -0.064 | 0.030 | -0.010 | -0.078 | 0.323** | 0.185* | -0.012 | 0.088 |
| | (0.115) | (0.084) | (0.185) | (0.127) | (0.150) | (0.103) | (0.133) | (0.129) |
| Test: Cash = Work | 0.604 | 0.002 | 0.571 | 0.014 | 0.440 | 0.058 | 0.085 | 0.666 |
| Shrp. q-val Cash = Work | 0.227 | 0.010 | 0.295 | 0.012 | 0.156 | 0.002 | 0.142 | 0.153 |
| Test: Male = Female | 0.069 | 0.069 | 0.481 | 0.481 | 0.109 | 0.109 | 0.266 | 0.266 |
| Observations | 220 | 502 | 220 | 502 | 220 | 502 | 220 | 502 |

| | Self Worth | | Control | | Stability | |
|---|---|---|---|---|---|---|
| | (1) Female | (2) Male | (3) Female | (4) Male | (5) Female | (6) Male |
| Work | 0.155 | 0.134 | 0.170 | 0.369*** | 0.259 | 0.289*** |
| | (0.106) | (0.107) | (0.229) | (0.131) | (0.157) | (0.094) |
| Cash | 0.106 | -0.106 | -0.174 | 0.112 | 0.201 | 0.043 |
| | (0.141) | (0.124) | (0.315) | (0.152) | (0.176) | (0.121) |
| Test: Cash = Work | 0.676 | 0.011 | 0.182 | 0.050 | 0.681 | 0.027 |
| Shrp. q-val Cash = Work | 0.199 | 0.153 | 0.295 | 0.010 | 0.156 | 0.008 |
| Test: Male = Female | 0.233 | 0.233 | 0.397 | 0.397 | 0.237 | 0.237 |
| Observations | 220 | 502 | 220 | 502 | 220 | 502 |

**Notes:** All outcomes are standardized. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Adjusted p-values are reported for the test of equality for the work treatment and cash treatment calculated separately by gender. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Balance on observables: exposure to death in Myanmar violence

| | (1) No Violence | (2) Violence | (3) No Vio. vs. Vio. | (4) No Vio. vs. Vio., Town FE | (5) No Vio. vs. Vio, Grid FE |
|---|---|---|---|---|---|
| Married | 0.82 | 0.78 | 0.69 | 0.70 | 0.61 |
| Age | 27.87 | 28.39 | 0.30 | 0.36 | 0.30 |
| Household size | 5.11 | 5.13 | 0.67 | 0.89 | 0.78 |
| Formal education | 0.43 | 0.50 | 0.31 | 0.20 | 0.15 |
| Math ability (index) | 2.64 | 2.58 | 0.20 | 0.17 | 0.14 |
| Past Ag. Work | 0.58 | 0.66 | 0.22 | 0.17 | 0.15 |
| Observations | 91 | 654 | | | |

Columns (1) and (2) show the average value of the variable for respondents who did and did experience the death of a family or community member in Myanmar. All difference in means test control for gender because violence was targeted differently between men and women. Column (3) shows the p-value of the difference in means with no additional controls. Column (4) reports p-values while controlling for township fixed effects, while column (5) includes fixed effects using 55 by 55 kilometer grid cells for respondent location of origin in Myanmar.

Table A12: Exposure to violence and baseline employment

| | (1)<br>Depressed at baseline | (2)<br>Depressed at baseline |
|---|---|---|
| Experienced at least one death | 0.121** | 0.147*** |
| | (0.052) | (0.055) |
| Employed at least one day in last month | | 0.036 |
| | | (0.148) |
| Employed * Experienced death | | -0.227 |
| | | (0.157) |
| Mean of outcome | 0.78 | 0.78 |
| Observations | 726 | 726 |

**Notes:** 'Depressed at baseline" is a binary variable equalling one if PHQ score is greater than 4 (encompassing those with mild, moderate, and severe depression). Regressions include camp fixed effects and controls selected by lasso. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Heterogeneity in impact of employment treatment by exposure to violence

PANEL A: Work v. Control

| | (1) PS Index | (2) Depressed | (3) PHQ |
|---|---|---|---|
| Work | 0.075 | 0.130 | 0.013 |
| | (0.121) | (0.117) | (0.212) |
| Experienced at least one death | -0.121 | 0.196* | 0.133 |
| | (0.115) | (0.111) | (0.194) |
| Work * Experienced death | 0.161 | -0.265** | -0.230 |
| | (0.127) | (0.124) | (0.219) |
| Observations | 561 | 561 | 561 |

PANEL B: Work v. Cash

| | (1) PS Index | (2) Depressed | (3) PHQ |
|---|---|---|---|
| Work | 0.081 | 0.073 | -0.028 |
| | (0.102) | (0.107) | (0.183) |
| Experienced at least one death | -0.044 | 0.085 | 0.070 |
| | (0.093) | (0.095) | (0.160) |
| Work * Experienced death | 0.095 | -0.172 | -0.179 |
| | (0.109) | (0.113) | (0.196) |
| Observations | 560 | 560 | 560 |

**Notes:** "Depressed" is a binary variable equalling one if PHQ score is greater than 4 (encompassing those with mild, moderate, and severe depression). Remaining outcomes are standardized. Sample in panel (a) includes those who received the work opportunity or were allocated to the control arm, with the latter as the omitted category. Sample in panel (b) includes only those who received the work or cash opportunities, with the latter as the omitted category. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Heterogeneity in impact of employment treatment by baseline sociability

PANEL A: Work v. Control

| | (1) PS Index | (2) Depressed | (3) PHQ |
|---|---|---|---|
| Work | 0.199*** | -0.029 | -0.186** |
| | (0.055) | (0.035) | (0.079) |
| Sociable | 0.061 | 0.076 | -0.092 |
| | (0.071) | (0.052) | (0.095) |
| Work * Sociable at Baseline | 0.033 | -0.165** | 0.001 |
| | (0.083) | (0.065) | (0.117) |
| Observations | 565 | 565 | 565 |

PANEL B: Work v. Cash

| | (1) PS Index | (2) Depressed | (3) PHQ |
|---|---|---|---|
| Work | 0.153** | -0.049 | -0.120 |
| | (0.061) | (0.041) | (0.086) |
| Sociable | 0.087 | -0.024 | -0.022 |
| | (0.059) | (0.050) | (0.086) |
| Work * Sociable at Baseline | 0.029 | -0.060 | -0.135 |
| | (0.072) | (0.064) | (0.099) |
| Observations | 564 | 564 | 564 |

**Notes:** "Depressed" is a binary variable equalling one if PHQ score is greater than 4 (encompassing those with mild, moderate, and severe depression). Remaining outcomes are standardized. Sample in panel (a) includes those who received the work opportunity or were allocated to the control arm, with the latter as the omitted category. Sample in panel (b) includes only those who received the work or cash opportunities, with the latter as the omitted category. "Sociable" defined as those who report having above the median number of positive conversations in the day prior to the baseline survey. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: Heterogeneity in impact of employment treatment by baseline depression

| | (1)<br>PS Index | (2)<br>Depressed | (3)<br>PHQ |
|---|---|---|---|
| Work | 0.237*** | -0.164* | -0.056 |
| | (0.081) | (0.084) | (0.124) |
| Baseline Mild Depression | -0.035 | 0.209** | 0.204 |
| | (0.091) | (0.086) | (0.135) |
| Baseline Moderate Depression | -0.079 | 0.225*** | 0.252 |
| | (0.104) | (0.084) | (0.192) |
| Work * Mild Dep. | -0.038 | 0.074 | -0.076 |
| | (0.099) | (0.099) | (0.142) |
| Work * Mod. Dep. | -0.005 | 0.082 | -0.297* |
| | (0.107) | (0.095) | (0.163) |
| Observations | 565 | 565 | 565 |

**Notes:** "Depressed" is a binary variable equalling one if PHQ score is greater than 4 (encompassing those with mild, moderate, and severe depression). Remaining outcomes are standardized. Sample includes those who received the work opportunity or were allocated to the control arm, with the latter as the omitted category. "Mild depression" defined as those who score greater than 4 and less than 10 on the PHQ-9. "Moderate depression" defined as those who score 10 or greater. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Heterogeneity in impact of employment treatment by baseline depression

| | (1)<br>PS Index | (2)<br>Depressed | (3)<br>PHQ |
|---|---|---|---|
| Work | 0.144 | -0.172** | -0.220* |
| | (0.090) | (0.083) | (0.112) |
| Baseline Mild Depression | -0.088 | 0.159* | 0.094 |
| | (0.082) | (0.081) | (0.136) |
| Baseline Moderate Depression | -0.160 | 0.147* | 0.046 |
| | (0.100) | (0.083) | (0.209) |
| Work * Mild Dep. | 0.014 | 0.090 | 0.047 |
| | (0.093) | (0.098) | (0.137) |
| Work * Mod. Dep. | 0.050 | 0.134 | 0.018 |
| | (0.109) | (0.097) | (0.149) |
| Observations | 564 | 564 | 564 |

**Notes:** All outcomes are standardized. Regressions include camp and enumerator fixed effects, controls selected by lasso, and the baseline value of the outcome variable. Standard errors are clustered at the block level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

## A.2 Figures

Figure A1: Pre-filled calendar

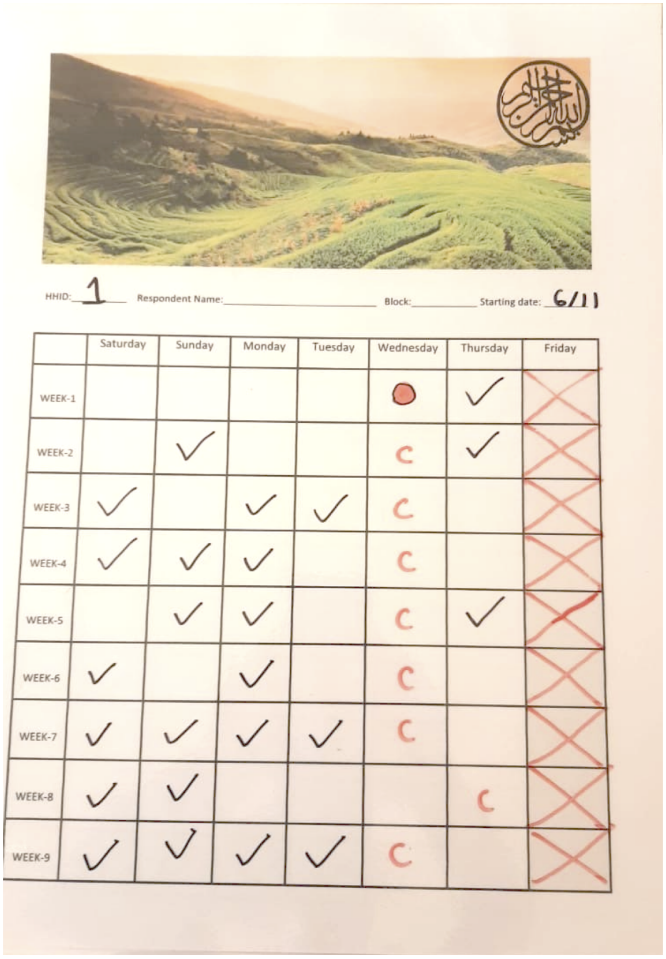


Figure A2: Participation Certificate

Figure A3: Excerpts from Human Rights Council Report

The following is a compilation of excerpts drawn from the United Nations' Human Rights Council Report on Myanmar regarding the "Clearance Operations" in Rakhine State executed by the Myanmar military (referred to below as the *Tatmadaw*) in late August and early September of 2017. These excerpts describe the indiscriminate nature of the violence perpetrated against the Rohingya during these operations. We caution the reader as several of these excerpts are difficult to read. We have left out the most graphic descriptions but direct the reader to the report itself (A/HRC/39/CRP.2) for further evidence of the random nature of violence during the Operations.

- During subsequent operations in villages and towns, the Tatmadaw did also not attempt to distinguish civilians from military objectives. Such indiscriminate attacks resulted in civilian men, women and children being injured or killed, with large numbers of civilians being driven away from their homes and villages. (P.35)

- Information therefore strongly indicates that airstrikes and shelling were used indiscriminately as a more general tactic in the context of "clearance operations," in essence attacking the civilian population as a whole as opposed to being used against specifically identified military targets. (P.35)

- The operations were designed to instill immediate terror, with people woken by intense rapid weapons fire, explosions, or the shouts and screams of villagers. Structures were set ablaze and Tatmadaw soldiers fired their guns indiscriminately into houses and fields, and at villagers. (P.178)

- Many Rohingya were killed or injured by indiscriminate shooting. Rohingya villages were approached without warning, usually from more than one direction, and often in the early morning, by armed Tatmadaw soldiers.... Members of the security forces, primarily Tatmadaw soldiers of the Western Command and the 33rd and 99th LIDs, shot assault rifles towards the Rohingya villages from a distance, not targeting any particular military objective or making any distinction between ARSA fighters and civilians. Men, women and children were all shot at. Many victims referred to the volume of gunfire, with some describing it as "raining bullets." Many were shot and killed or injured while attempting to flee. (P.205)

- One young girl described the operation in Maungdaw Township: "When the soldiers came to my village, we all ran, and they shot at us. We were around 50 people, and maybe half of us were shot. The people shot fell down while they were running. Some died and some escaped. Somehow, I escaped."' (P.205-206)

- One man from Kyein Chaung village tract, known in Rohingya as Boli Bazar, in northern Maungdaw Township explained the circumstances in which his daughter was killed: "I don't know how many people died that day. The military, they were just shooting at whomever. They were shooting at people whenever they saw them, on the streets or in the houses. When they were shooting, there was no time to look back and care for those who were shot. As people were running, they were shooting at them. That is how my daughter died. She was hit fleeing. I couldn't go back and carry her." (P.206)

- Some Rohingya villagers who could not flee, or who sought shelter inside their houses, were also shot and killed or injured, when bullets penetrated thatched roofs and bamboo walls. Villagers were shot in other locations where they had found shelter, including through rapid arms fire into forested hills where they had fled. (P.206-207)

- The Mission has provided detailed accounts above of corroborated mass killings perpetrated in the villages of Min Gyi, Maung Nu, Chut Pyin, Gu Dar Pyin, the villages of Koe Tan Kauk. Dozens, and in some cases hundreds, of men, women and children were killed. Additional organized mass killings are likely to have taken place. Witnesses reported seeing bodies of large numbers of Rohingya, including those with gunshot and machete wounds, as well as decapitated heads, in burned villages en route to Bangladesh. (P.207)

- Rohingya fleeing the "clearance operations" also faced violent attacks at border crossing points, resulting in loss of life and serious injuries. Soldiers opened fire on groups of Rohingya at or close to border crossing points, including large numbers gathered on the shores of the Bay of Bengal or Naf River, while waiting to cross into Bangladesh.2005 A man from Nga Yant Chaung village tract, Buthidaung Township, described arriving at the Naf River in mid-September 2017 and being fired upon by soldiers. Some of the people ran; others, like him, lay on the ground. He said that 25 people were killed, including three of his relatives. (P.208)

- Soldiers also shot at boats carrying Rohingya to Bangladesh, resulting in further casualties. One witness explained how the boat she was in was shot at by soldiers as it crossed the Naf River, killing three men and two women. Another witness described her experience while waiting for a boat: "Soldiers started shooting, so we crawled away and lay down behind the plants in the mud. I saw many people being shot at. Dead bodies of men, women and children were floating in the river." (P.208-209)

- Another feature of the "clearance operations" was the widespread destruction of Rohingya homes and villages, causing further death and injury through burning. Houses were burned both manually using flammable liquid and matches, and by the use of "launchers," weapons firing a munition that explodes upon impact. This latter method in particular meant that victims were often caught by surprise and had little time to escape. (P.209)

- Landmines, planted by the Tatmadaw in and around Rohingya villages as part of the "clearance operations" also caused death and injury. On or around 26 August 2017, a group of Tatmadaw soldiers approached Sin Oe Pyin (Ywar Gyi) hamlet, in Maung Gyi Taung village tract, Buthidaung Township. They systematically planted mines along the main road to the village, with one villager describing them as being placed "15 feet apart." Once the operations began, the landmines killed and injured many who tried to flee.2037 As one villager described, "The mines were put at the entrance of the village, that is the only way out so when people were running they stepped on them and died." Another recalled: "Some people were running and were killed by the mines, as they didn't know that they were planted there. Others were hit by the mines as they were coming back from the field. My 18-year old relative died from an explosion coming back from the paddy field just in front of my house." (P.211)

## A.3 Script to participants

**FOR EVERYONE:** We want to thank you for all the time you have spent with us so far: we have learned so much from you. As a token of our gratitude, we would like to offer you a gift. We do not have a lot of money, but we still want to help by learning about your life and conditions in the camp better so that we can do something in a larger scale in the future. Because we don't have enough for everybody, we are offering a lottery. You might receive: (1) 300 taka today plus a total of 400 taka over the next two months, (2) 300 taka today plus a total of 3600 taka over the next two months, (3) 300 taka today plus a work opportunity from which you can earn 3600 taka over the next two months or (4) Nothing. Most people get nothing (this is the most common happening, most people in your block will receive nothing). Here are a few envelopes, each with a different number on them. I do not know what numbers are in these envelopes. I want you to choose one of these, and tell me the number inside. I will enter it into my tablet and it will tell me which of the gifts you will receive. Does that make sense?

**T-0 (Control, No Work)** Congratulations! You drew a number that entitles you to 300 taka today plus a total of 400 taka over the next two months. *Enumerator: Please give three 100 taka bill to the respondent* This is yours to keep and do what you wish with the money. We will come to your block every week for the next eight weeks to check in and see how you are doing and will ask you some questions again. Next week, you will receive 50 taka if you come to meet us in your block and answer a few questions, and this process will continue for the next 8 weeks, adding up to 400 taka by the end. You will have come to the collection point every week to collect money, you cannot send someone else on your behalf. We have a few remaining questions to ask you – it will take about 30 minutes, and then we will be on our way. Is that okay?

**T-1 (Cash, No Work)** Congratulations! You drew a number that entitles you to 300 taka today plus a total of 3600 taka over the next two months. *Enumerator: Please give three 100 taka bill to the respondent] This is yours to keep and do what you wish with the money.* We will come to your block every week for the next eight weeks to check in and see how you are doing and ask you some questions again. Next week you will receive 450 taka if you come to meet us in your block and answer a few questions, and this process will continue for the next 8 weeks, adding up to 3600 taka by the end. You will have come to the collection point every week to collect money, you cannot send someone else on your behalf. We have a few remaining questions to ask you, it will take about 30 minutes and then we will be on our way. Is that okay?

**T2a: pay for work with a certain schedule** Congratulations! You drew a number that entitles you to 300 taka today plus a work opportunity where you can earn a total of 3600 taka over the next two months. *Enumerator: Please give three 100 taka bill to the respondent.*This is yours to keep and do what you wish with the money. Now let me tell you about the work opportunity. As you know, we are conducting a research project in which we are trying to understand how you

feel about life and how you spend your days in the camps. If we understand this well, we will be able to help you and your community by providing you with the things you need. Does it make sense to you? ENUMERATOR: BEGIN PINK VIDEO HERE. Would you like to accept this work opportunity? Wonderful! Then here are 2 sets of papers for the next 2 days in this current week you will be working. Within each set there are 5 sheets for 5 times during the day on which you will be working. You will get next week's work on the collection day (SPECIFY THE COLLECTION DAY). Here is the calendar that tells you exactly on which days we need you to complete these sheets. At the end of each day, please put the 5-sheet bundle/set in the collection box that will be kept in your block. We will check in with you throughout the week and collect these sheets at the end of the week and make your payment for that week. We have a few remaining questions to ask you, and then we will be on our way. Is that okay?

**T2b: pay for work with uncertain schedule** Congratulations! You drew a number that entitles you to 300 taka today plus a work opportunity where you can earn a total of 3600 taka over the next two months. [Enumerator: Please give three 100 taka bill to the respondent] This is yours to keep and do what you wish with the money. Now let me tell you about the work opportunity. As you know, we are conducting a research project in which we are trying to understand how you feel and how you spend your days in the camps. If we understand this well, we will be able to help you and your community by providing you with the things you need. Does it make sense to you? ENUMERATOR: BEGIN BLUE VIDEO HERE. Would you like to accept this work opportunity? Wonderful! Ok, now let me give you a few final details on your work task. For this coming week, you will have to work on *these two days*. At the end of the day you will have to submit your daily work in the collection box and attend a weekly collection session to collect your weekly payment based on your work. Here are 2 sets of papers for the next 2 days in this current week you will be working. Within each set there are 5 sheets for 5 times during the day on which you will be working. You will get next week's work on the collection day (SPECIFY THE COLLECTION DAY). At the end of each day, please put the 5 sheet set in the collection box that will be kept in your block. We will check in with you throughout the week and collect these sheets at the end of the week and make your payment for that week. Even though we'll pay you this total amount at the end of every week, we don't know which twenty-four days you will work for us in the next 2 months. We will only be able to tell you at the beginning of each week. That means, when you return us your completed work and get your weekly payments, our collectors will tell you the next week's schedule. Your weekly schedule will be uncertain. We have a few remaining questions to ask you, and then we will be on our way. Is that okay?