

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. We find that employment confers significant psychosocial benefits beyond the impacts of cash alone, with effects concentrated among males. The cash arm does not improve 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 utility 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 suffers from two key limitations.¹ First, the challenge of selection, whereby those who are unemployed differ from the employed in ways that are likely correlated with their psychological wellbeing. Second, the inability to disentangle the mechanisms that drive the relationship between employment and wellbeing, whereby the pecuniary channel of easing resource constraints is conflated with the psychological channel of alleviating loneliness, lack of purpose, loss of agency, or the like.

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 seek to address both limitations in the literature by exogenously offering employment opportunities to some individuals, who we then compare to individuals who benefit from the pecuniary dimension of employment alone. 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 We designed a job that sought to mimic natural forms of employment in the camps available through other NGOs, including features identified by the sociological literature as beneficial: the occupation of potentially idle time, active engagement, sociability, and purpose.³ Our control arm receives no work and a nominal fee for weekly survey participation. A comparison of these two 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. The eight week duration of the work and cash provisions is well beyond that of the average daily labor opportunities arising in our setting.

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

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

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

³Despite these features, the work was not uniquely stimulating: in order to track quality of work performance over time, the job involved a simple and repetitive activity performed several times per day over the course of two months.

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 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.⁴

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 five 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 increase in our ‘mental health index,’ 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 9 percentage points (11%) less likely to be depressed and 7 percentage points (21%) less likely to be moderately or severely depressed. 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 better on simple memory and math tests, and are less risk averse.

Second, we find that employment generates benefits that are significantly greater than that of cash alone. We can decisively reject equality of effects between employment and cash for our mental health index, physical illness, cognitive performance, and risk aversion. These differences are substantial: employment improves mental health at a magnitude four times greater than cash alone. Our results cannot be explained by large differences in how time is spent, in how cash is consumed, nor in expectations of future work and income. This suggests that the sizable non-pecuniary benefits to psychosocial wellbeing that we observe are likely due to a dimension of the work, or the experience of working, itself.

Third, we find that the psychosocial impact of cash alone is quite small, at a statistically insignificant 0.05 standard deviation change in mental health. At a value of USD \$120 PPP and equivalent to more than a doubling of consumption, we find this result surprising. 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,

⁴Such 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)

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 an important set of questions around the value of cash transfers in environments with scarce employment opportunities, with potential policy implications for UBI and the future of work.

Fourth, we find that individuals can 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.

Finally, we find that the gender of our participants is highly predictive of the non-pecuniary value of employment: males in the employment arm exhibit large psychosocial improvements while those in the cash arm experience a near-zero effect. These improvements are sizable. Employed males are 30% less likely to be moderately or severely depressed than their non-employed counterparts, a shift that is tangibly reflected through a 22% decline in the number of days they report having suicidal thoughts. In contrast, while women also benefit from employment, they benefit nearly as much from cash alone.⁵ This is reified in our estimated impacts on household bargaining, which are consistent with much of the cash transfer literature (Bastagli et al., 2019): we find that cash, with or without employment, leads to equally meaningful increases in a woman’s intolerance for physical abuse and beliefs around her prerogative to make decisions in her household.

We examine several other pre-specified margins of heterogeneity: exposure to violence in Myanmar, baseline sociability levels, baseline depression levels, and an experimentally induced variation in degree of certainty around future work schedules. We find suggestive evidence that the employment program is significantly more impactful for those who have experienced greater violence, are more sociable, or are more depressed at baseline.

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, Klehe, and Kira, 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.,

⁵As a benchmark, a recent evaluation of a year-long psychoeducation program for Rohingya refugee women documents a 0.15 standard deviation reduction in depression (Islam et al., 2021). This effect size falls in between the impact of the employment program on males (of 0.21 SD) and females (of 0.12 SD).

⁶The 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.

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 build upon 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. 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. Bhanot, Han, and Jang (2018) serves as valuable groundwork, as the intervention examined is of shorter duration and a type of work that is more distant from more realistic forms of employment. We design this study as a field experiment with a plausible and longer-duration form of gainful employment that capitalizes not only on occupying idle time but also on sociability, being engaged throughout the day, and having at least nominal purpose behind the work - elements common to most, even tedious, forms of employment (Terkel, 1974).

While our experimental design is shaped by the lived experiences of Rohingya refugees, the defining set of constraints they face are shared across many populations of interest. Participants in our study are cash-poor and therefore deprived of basic necessities for daily living,⁷ 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 forcibly displaced persons globally (45.7 million), as they are to the incarcerated (10.35 million), as well as many of the world's rural 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).

Second, this experiment offers direct evidence of whether cashfare or workfare programs are a more cost-effective means of improving psychological wellbeing. More broadly, we contribute to a policy literature around the merits of employment programs relative to cash-based interventions such as unemployment insurance and Universal Basic Income (UBI), particularly in developing country contexts (Banerjee, Niehaus, and Suri, 2019). 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 Mariantal, 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*

⁷Despite 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.

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). The number of forcibly displaced has grown rapidly in recent years, reaching a historic high of 80 million in 2020 (UNHCR, 2020). 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 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 The Rohingya

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. In 1824, Burma was colonized by the British, who introduced a system of ethnic classification defining 135 sub-races that not include the Rohingya. In 1982, an independent Burma passed the Citizenship Act which required national identity cards specifying ethnic membership in one of the recognized sub-races — effectively excluding the Rohingya from citizenship (Wade, 2017). The first major campaign of ethnic cleansing against the Rohingya occurred in 1978 when the Burmese military, tasked with performing a census of the border regions to determine citizenship, conducted

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

indiscriminate attacks across Rohingya villages in Rakhine state. This led to an estimated quarter million people fleeing into neighboring Bangladesh. Subsequent ethnic cleansing campaigns in 1992 and 2012 sent additional waves of Rohingya into Bangladesh (Watch, 1996).

2.2 Recent Events and Camp Context

On August 25, 2017, the Rohingya insurgent group Arakan Rohingya Salvation Army (ARSA) launched coordinated attacks on a military base and 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 the first three weeks of the “clearance operations.” 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. They joined another 250,000 to 300,000 “Old Rohingya” who had left Myanmar in earlier years of ethnic cleansing. The largest and most densely populated refugee camp on earth was constructed in a matter of weeks (Hussam, 2019).

Operations within the camp are coordinated and overseen by the Bangladesh Government’s Ministry of Disaster Management and Relief (MoDMR), which is represented across camps by the Refugee Relief and Repatriation Commissioner (RRRC) and within each refugee settlement by Camp-in-Charge (CiC) officials. International institutions (BRAC, UNHCR, IOM among others) actively work with the government to facilitate service delivery (including food, shelter, clean water, and sanitation). 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, the army, the CiC, and the refugee community. According to the UNHCR, 80% of the Rohingya population rely on life-saving assistance. Nevertheless, many Rohingya 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/or seeking informal work opportunities (which are few and far between). The income they earn is used to purchase basic items such as clothing, salt, vegetables or fish, hygiene products, and household ware at the local markets. These markets also sell recreational goods, including cigarettes, makeup, jewelry and electronics.

Though Bangladesh has maintained open borders for the steady inflow of refugees, negotiations between the governments of Bangladesh and Myanmar around repatriation began promptly after the initial influx. Protests and international pressures forced the Bangladeshi government to delay plans for repatriation until November 2018, then, amidst further protests, indefinitely. Not wishing to encourage the long-term stay of the Rohingya, the Government of Bangladesh has enacted measures to discourage integration of refugees with host communities. In particular, refugees are not allowed to work (Bhatia et al., 2018). Many are left idle in the camp, leaving some vulnerable to various forms of human or drug trafficking (Watch, 2019). 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. The typical (though scarce) employment opportunities that camp residents may avail of are as day laborers in agriculture or construction; operation of small street stalls for vegetables or toys; and private tutoring for those who are more educated. Some are employed by local NGOs on activities like running cooking centers, women’s and children’s centers, etc. Several institutions have also organized cash-for-work programs in the camps (World Vision, 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, operating small street stalls, or rickshaw pulling.

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. 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.⁹ In total, we assembled a sample of 745 individuals across the three camp sites.¹⁰

Intervention Details 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.¹¹ 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:

⁹We 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.

¹⁰We 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.

¹¹We 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.

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.

What is the value of 450 taka per week in the context of the Rohingya refugee camps? At 1800 taka per month, 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.¹² 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 alternative 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, equivalent to less than 50% of the monthly cash transfer in the experiment.

We now turn to the nature of the employment we offer. Our work was designed to be easily completed by women or men of any literacy level and working age within the study population. It was further designed to occupy the employee multiple times throughout the course of the day in a manner that required some nominal level of engagement with individuals outside the home and possessed a clear, purposeful objective. Specifically, 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 four times per day. 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. The objective of the work (as described to our participants) was 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.

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

¹²In 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.

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. To ensure compliance with the work schedule, we stationed a tamper-proof box in a pre-chosen household within each block 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 ‘book-end’ that day’s submission. The respondent’s submission was marked late if it was inserted after that paper. Supervisors determined which household in the block would host the collection box (henceforth referred to as the “facilitator” household), 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).¹³ 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.

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

¹³We did not have auditors in the camps watching our workers given both logistical infeasibility and concern that workers may feel insecure.

contextual demands, adapting or eliminating various questions from such modules which were culturally insensitive or incoherent given the experiences of the Rohingya. Surveys were translated and back-translated from English to Bengali to 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: 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, Rotter’s thirteen-question Locus of Control Instrument, respectively) that we adapted by selecting and contextualizing questions that were appropriate given the Rohingya’s recent experiences. 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 past thirty; and cognitive health by employing a digit-span memory test and a series of basic arithmetic problems. We explore economic decision-making along two dimensions: time preferences (Andreoni and Sprenger, 2012; Giné et al., 2018) and risk preferences (Holt and Laury, 2002). Our primary measure of time-use is the self-reported average number of hours that respondents spend idle. We further examine how the interventions shift time allocation by categorizing activities into a hierarchy of substitutability: productive activities which are more difficult to substitute away from (bathing, market, chores, collection of rations, eating, child-rearing), and unproductive activities which can be more easily replaced (sitting at tea stalls, praying, sleeping, visiting friends/relatives, playing games, playing sport, sitting idle). Finally, we also 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. 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, and ε_{ibc} is an error term clustered at the block level. We include fixed effects for camp γ_c and enumerator δ_e .¹⁴ We control for the baseline value of the outcome variable, when available, in an ANCOVA specification following ?. Our coefficients of interest are β_1 , the impact of cash, and β_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: $\beta_1 = \beta_2$.

¹⁴We 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.

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, η_t represents a dummy for the weekly visit number t , and γ_c , δ_e , X_{ibc} , ε_{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 improvements as well. Those offered employment experience a substantial reduction in symptoms of depression, as captured by the PHQ9 module, as well as feelings of anxiety or frustration, as captured by the stress index. They exhibit higher life satisfaction, sociability, beliefs about their own self-worth, a sense of control over events in their lives, and security in both the present and the anticipated long-term future. To provide context for these results, the employment arm results in 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. Employed individuals are 14% more likely to sleep peacefully and rank themselves as feeling 13% more secure and stable than their control counterparts.

The positive effects of employment extend to a host of measures beyond the psychosocial. Table 3 presents results on physical health, cognitive health, and incentivized measures of risk and time preference. We observe a decline in the days reported sick, an improvement in the cognitive index (a combined measure of memory and basic arithmetic ability), and a decline in risk aversion. The documented impact on risk aversion suggests that the employment arm serves as a form of psychological ‘insurance,’ consistent with the impact of employment on stability, allowing participants to exercise greater risk. This is consistent with a key motive underlying universal basic income (UBI) (Banerjee, Niehaus, and Suri, 2019); interestingly, however, we document no parallel decrease in risk aversion in the cash transfer arm.¹⁵

5.4 Non-pecuniary impact of employment

The employment arm generates significantly larger improvements in psychosocial wellbeing than the cash arm: we can reject equality between the two arms at the one percent level. This result is manifested across all subcomponents of the index, with the exception of life satisfaction, which

¹⁵We find no evidence of negative impacts to the withdrawal of the work or cash interventions in our six week followup; see Appendix Table A5.

increases equally under both treatment arms.

Weekly measures of stress and sociability reiterate these findings: as demonstrated in Figure 4, those in the employment arm benefit significantly more than their cash counterparts on both margins. Notably, work recipients experience a reduction in stress from the very first week of the intervention (Panel A), and this difference persists over the course of experiment. Similarly, conversations that are perceived as positive (Panel C), which drive the differential effect we document in sociability, likewise begin immediately and persist.

Impacts of the employment arm are likewise significantly larger than those of the cash arm for our physical, cognitive, and risk preference outcomes. Figure 5 documents these differences at the weekly level; as above, patterns appear at or near the beginning of the experiment and remain for the duration of the interventions.

5.5 Impact of cash

Relative to those in the control group, individuals in the cash arm experience a 0.05 SD improvement in their mental health index. The imprecision of the estimate, however, means that we cannot reject a null effect of cash on psychosocial wellbeing. Point estimates for all subcomponents of the index lie below 0.1 SD and remain noisy, with the exception of life satisfaction, which exhibits a 0.22 SD improvement among cash recipients.¹⁶ We find these effects to be quite small, with the mental health index impact of cash measuring at one-fourth the size of the impact of employment. We consider these magnitudes relative to the broader cash transfer literature in Section 5.10.

As with the previous patterns, the impact of cash on physical, cognitive, and risk preference outcomes again remain noisy and close to zero.

5.6 Alternative mechanisms

Our results are indicative of the presence of significant non-pecuniary benefits to the experience of employment. However, they may also be due to other changes induced by the employment arm relative to the cash arm: namely, differences in how time is spent, how cash is consumed, and how expectations of future work are formed. We consider each in turn.

Time use Individuals in the employment arm report spending an average of 2.5 hours per day engaged in the work assignment. What activities are being replaced by this time spent working? Table 4 examines how cash and work arms use their time. We document no statistically significant difference between the two treatment arms in the number of hours that respondents report spending idle per day, nor any differences in the time they devote to various activities throughout the day (divided for parsimony into ‘productive’ and ‘unproductive’ activities; individual activities, not shown, exhibit no detectable differences either).

¹⁶This latter result is consistent with the conclusions of McGuire, Kaiser, and Bach-Mortensen (2020)’s meta-analysis of cash transfer experiments, in which life satisfaction is consistently found to exhibit significantly larger changes than measures of depression in response to cash transfer programs.

While puzzling, we offer two considerations. First, we suspect that the work task, rather than supplanting any single activity during a worker’s day, instead sheds a few minutes off of many activities: one may wake up thirty minutes earlier, nap fifteen minutes less, spend ten fewer minutes eating, and lounge twenty fewer minutes by the tea stall. Given the already nonregimented and amorphous nature of time in the refugee camps, paired with the second considerable challenge of collecting retrospective time use data, it is perhaps unsurprising that respondents do not register these small adjustments to other activities.¹⁷ As such, we cannot rule out that the effects of the employment arm may arise from how time is spent, but our results suggest that *substantial* differences in time use cannot explain the non-pecuniary impacts we document.

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.¹⁸

Panel A of Table 5 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 either).¹⁹ Panel B demonstrates that both groups lend, save and repay loans at significantly higher rates than their control counterparts, but not differentially from one another. 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.

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,

¹⁷We piloted a variety of strategies for collecting time use data and found challenges with each; we settled on asking respondents to recollect how much time they spent on a series of daily 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, was not an option to us given the lack of mobile phones and cellphone reception in the camps.

¹⁸Notably, 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.

¹⁹We 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.

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, intended 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.²⁰ 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. We also utilize this certificate experiment to consider the role of status in the psychosocial value of employment (Marmot et al., 1991; Redelmeier and Singh, 2001; Anderson and Marmot, 2012): we may expect that association or engagement with an NGO confers a higher status to individuals, and the certificate should again make this relationship maximally public and salient.

Table A6 presents the results. We see no impact of the certificate, neither in levels nor differentially by treatment arm, on our mental health index.²¹ Granted, even with a salient certificate, a null effect cannot definitively rule out that participants expect future income streams from working beyond that communicated by the certificate. We rely on data on the actual likelihood of employment after the intervention as an additional test of this channel.

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. 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 the psychosocial gains from employment we document.

²⁰The 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.

²¹One 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. We examine the types of future employment that participants acquire in our followup survey to further gauge how likely this is.

5.7 Labor supply

We estimate significant non-pecuniary benefits of employment on psychosocial, physical, and cognitive wellbeing, and these effects appear to be driven by the nature of the employment itself rather than substantial changes in time use, differences in consumption, or greater expectations of future income. However, it remains an open question whether respondents are aware of and able to price these benefits into their labor supply decisions. We examine this possibility through a 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 titrated 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.

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,²² 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.²³

5.8 Gender

We now consider whether the non-pecuniary benefits of employment are greater for males or females in our sample. Existing literature offers plausible reasons for either group to be the primary

²²We felt that larger fees might not be regarded as realistic tradeoffs and consequently be met with suspicion or confusion.

²³Experimental 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 very limited amount of funds remaining, which we wished to use to complete one extra week of work, but that we did not have enough 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. Second, when we asked a respondent if (s)he was interested in working, we emphasized that we could easily find someone else to complete the work, so (s)he should only participate if (s)he truly wished to. Third, when we offered the alternative of taking a brief survey for a small fee, we emphasized that the survey, like the work, would likewise be a beneficial service to us, further reducing the possibility of reciprocity or demand effects. While we cannot claim to have entirely ruled out such effects, one would have to price these effects at greater than 200 taka, or the mean in baseline savings, to rule out the presence of any non-pecuniary value of work in respondents' reservation wages.

beneficiary. 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)). 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 7 presents the impacts of the employment and cash arms separately by gender. We find 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 A8 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 7, 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 7, 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.

5.9 Additional Analyses

Motivated by the refugee camp context we operate in, we pre-specified a collection of participant features along which the non-pecuniary benefits of employment may vary: exposure to past violence, baseline depression, and baseline sociability. Given the daily existential uncertainty that forcibly displaced persons face, we additionally consider how greater predictability around the work task may impact psychosocial wellbeing. We briefly consider each in turn.

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.²⁴ 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 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 A11, 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 A11, 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

²⁴A 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.

baseline sociability) may find the work emotionally taxing. Results are presented in Appendix Table A12, 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 A11, 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 A13. 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.²⁵ 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.

Future Uncertainty Palpable in the camps is the deep uncertainty that refugees face about their futures. When asked what most occupies the mind during idle time, 46% of our sample volunteer concerns about the future. While alleviating long-term and existential uncertainty around refugees' future is beyond the capacity of this study, we estimate the role of alleviating short-term uncertainty through the nature of the work we provide.

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." We therefore build a mechanism experiment into our work treatment arm, in which we vary the degree of certainty with which refugees may envision their daily activities for the coming two months. In particular, a randomized subset of those in our work arm receive a calendar marked with every date of work for the duration of employment. The remainder receive a calendar with no schedule and are instead informed once a week about their schedule for the following week. To eliminate differences in expected pecuniary returns, information around total work and total pay are held

²⁵By 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.

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.²⁶ We can therefore conclude little from this experiment; our largely null results may have been a product of the countervailing forces of reduced uncertainty but an increased sense of responsibility, or may simply have been due to a weak intervention. While inconclusive, our conversations with refugees on the burden of an uncertain future reinforce that these questions remain an important space for future work.

5.10 Discussion

We now turn to examining our results within the broader context of cash transfer, anti-poverty, and psychosocial health interventions. Our point estimates suggest that the psychosocial value of cash, at 0.05 SD improvement in our mental health index, is one fourth that of employment, at 0.21 SD. How do these magnitudes compare to existing estimates of similar programs?

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. Given the profoundly constrained income-earning opportunities and broader material poverty experienced by camp residents, we find this near-zero impact of a large cash transfer surprising; the differences by gender perhaps suggest that a particular mental or emotional state may be necessary for cash transfers to yield positive psychosocial impacts on their recipients.

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.

Benchmarking the employment impact A 0.12 SD improvement in mental health also lies well below the 90% confidence interval for the estimated impact of employment, reiterating the existence of non-pecuniary psychosocial impacts of employment in our setting. How do our employment impacts then compare to alternative anti-poverty programs or targeted psychotherapy

²⁶We 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.

programs? Ridley et al. (2020) perform a meta-analysis of the mental health impacts of multifaceted anti-poverty interventions (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.

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. These benefits are concentrated among men, and workers are able to at least partially price them into their labor supply decisions. Finally, while cash alone generates psychosocial impacts comparable to that of the cash transfer literature for women in our sample, the impacts of cash on men are substantially smaller. We offer three 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 findings in this experiment, hopeful that the upwards of one million Rohingya who have shared the experiences of our sample population is meaningful. However, the context we explore of constrained labor market opportunities paired with material poverty and a limited choice set of alternative leisure activities is encountered globally by forcibly displaced migrants, the incarcerated, and many of the world’s rural poor.

Second, 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, implying a high valuation of work. 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 longer term and intergenerational psychosocial benefits to accumulated wages that are not captured in this field experiment.

Lastly, we sought to design the most realistic form of employment that would be amenable to

both men and women of low literacy in the refugee camp context, incorporating dimensions that the sociological literature has highlighted as potential sources of psychosocial value. While not the objective of this paper, continuing work may seek to identify which dimensions of employment are especially impactful: one's identity as an employed person? A feeling of busyness? Physical activity, sociability, or purposefulness? We view this present work as opening up a set of questions around why employment may be psychosocially valuable, why material support alone may be ineffective despite profound poverty, as well as deeper questions around the perception of time in environments where scarcity of meaningful activity may transform a valuable resource into an amorphous and costly experience.

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Tables

Table 1: Balance

	(1)	(2)	(3)	(4)	(5)	(6)
	Control	Cash	Work	(1) vs. (2)	(1) vs. (3)	(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)

	(1) PHQ	(2) Stress	(3) Life Sat.	(4) Social	(5) Self Worth	(6) Control	(7) Stability	(8) MH Index
Work	-0.185*** (0.060)	-0.258*** (0.096)	0.301*** (0.075)	0.167** (0.082)	0.143* (0.080)	0.310*** (0.116)	0.249*** (0.081)	0.214*** (0.039)
Cash	0.001 (0.071)	-0.060 (0.108)	0.237*** (0.087)	0.083 (0.100)	-0.075 (0.087)	0.047 (0.144)	0.055 (0.102)	0.045 (0.049)
Adj. p-val Work	0.006	0.007	0.001	0.015	0.023	0.007	0.006	
Test Work=Cash	0.006	0.022	0.350	0.324	0.002	0.031	0.033	0.000
Adj. p 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* (0.411)	-0.044 (0.049)	0.182*** (0.068)	-0.656** (0.291)	-0.119 (0.323)
Cash	-0.054 (0.479)	0.007 (0.055)	0.057 (0.077)	0.028 (0.342)	-0.074 (0.336)
Adj. p-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
Adj. p 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 time use

	(1) Hours Idle	(2) Time Chores	(3) Time Social	(4) Time Ration
Work	0.030 (0.100)	-0.053 (0.184)	0.076 (0.150)	-0.107 (0.094)
Cash	0.078 (0.119)	-0.171 (0.180)	-0.036 (0.187)	-0.116 (0.107)
Test Work=Cash	0.603	0.424	0.507	0.888
Adj. p Work=Cash	1.000	1.000	1.000	1.000
Control Mean	2.284	3.154	2.870	0.321
Observations	726	726	726	726

Notes: ‘Hours Idle’ is the average number of hours respondent reports being idle per day in the previous week. ‘Prod. Time’ is made up of the following activities: bathing, market, chores, collection of rations, eating, child-rearing; ‘Unprod. Time’ is made up of the following activities: sitting at tea stalls, praying, sleeping, visiting friends/relatives, playing games, playing sport, sitting idle. 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 consumption

Panel A			
	(1)	(2)	(3)
	Luxury	Necessary	Total Cons.
Work	17.762 (31.484)	228.285 (155.611)	285.395 (176.621)
Cash	-18.144 (36.522)	194.457 (164.093)	208.658 (191.747)
Adj. p-val Work	0.277	0.277	0.277
Test Work=Cash	0.244	0.821	0.659
Adj. p Work=Cash	1.000	1.000	1.000
Mean in Control	347.47	1777.38	2132.99
Observations	726	726	726

Panel B			
	(1)	(2)	(3)
	Savings	Borrowing	Lending
Work	196.441*** (52.220)	-685.075*** (187.721)	50.277*** (16.266)
Cash	128.096** (61.683)	-761.448*** (220.438)	34.053* (18.562)
Adj. p-val Work	0.001	0.001	0.001
Test Work=Cash	0.283	0.676	0.424
Adj. p Work=Cash	1.000	1.000	1.000
Mean in Control	98.64	1986.30	8.15
Observations	726	726	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 6: Impact of certainty treatment

	(1) Stability	(2) Dist. Mon.	(3) Risk Aversion	(4) Time Pref.
Received Schedule	-0.065 (0.077)	-0.172** (0.085)	-0.111 (0.100)	0.041 (0.103)
Adj. p-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: Heterogeneity by gender

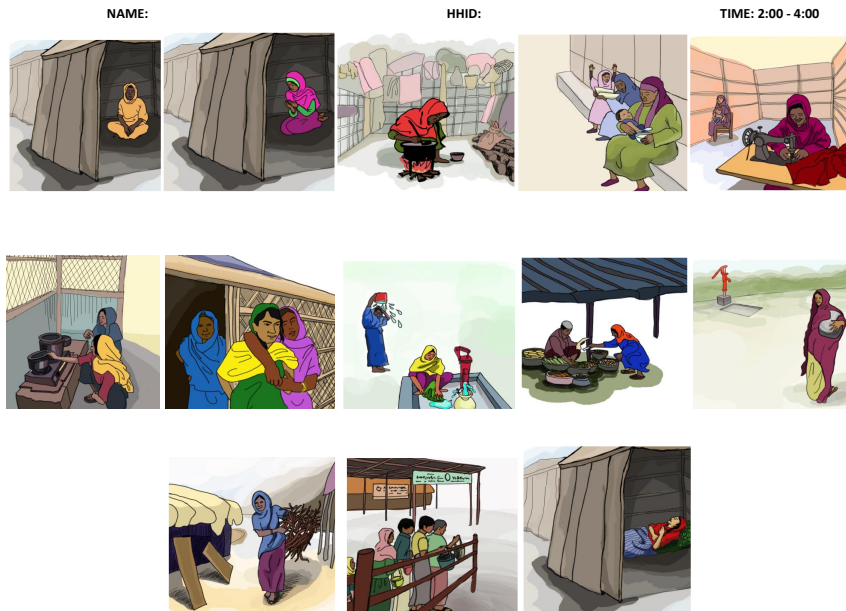
	Mental Health Index		Household Power Index		Work Rights Index	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
Work	0.193*** (0.063)	0.224*** (0.050)	0.312** (0.126)	0.025 (0.094)	0.344*** (0.111)	0.083 (0.098)
Cash	0.125 (0.078)	0.023 (0.059)	0.267* (0.140)	0.078 (0.098)	0.110 (0.118)	0.052 (0.117)
Test: Cash = Work	0.311	0.000	0.742	0.530	0.018	0.745
Adj. p-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

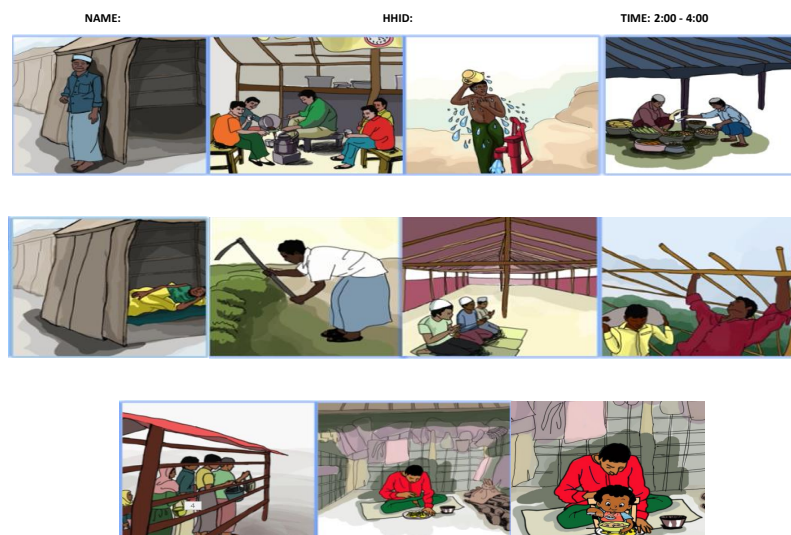
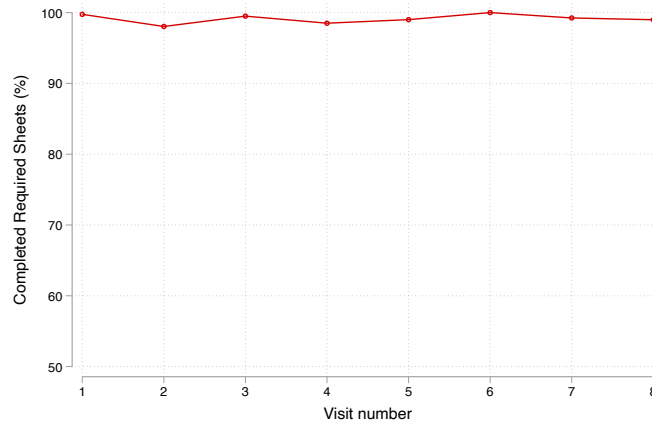
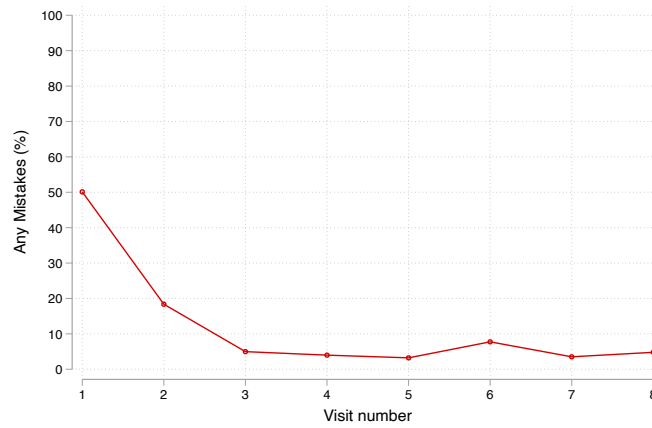


Figure 2: Work Completion Measures

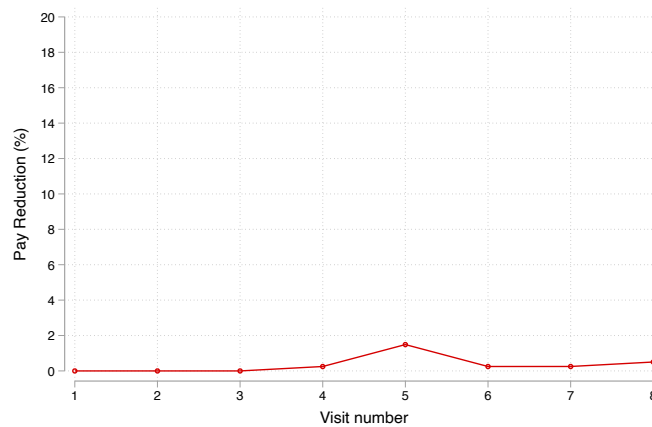
(a) Whether work was completed



(b) Whether any mistakes were made

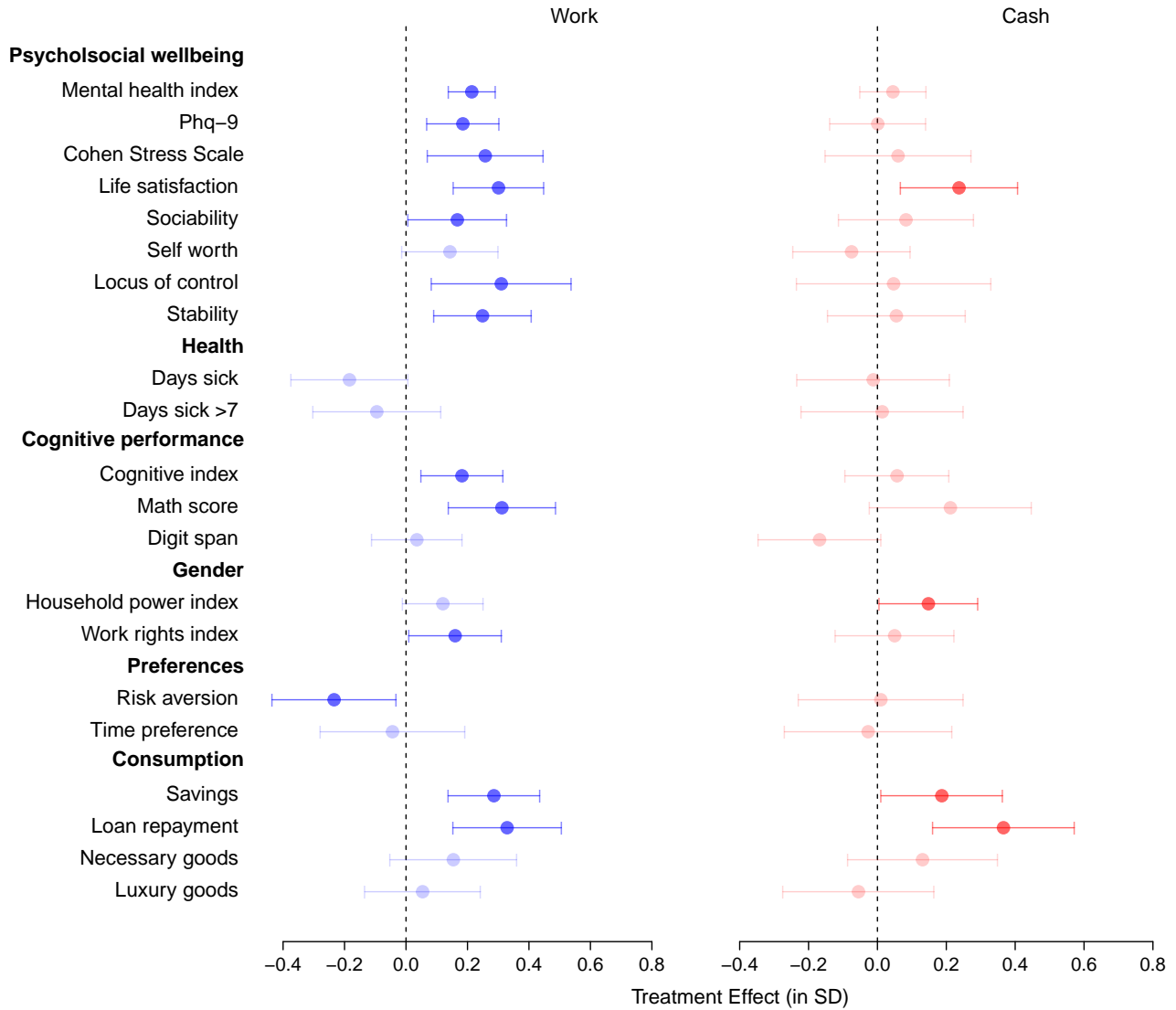


(c) Whether pay was docked



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.

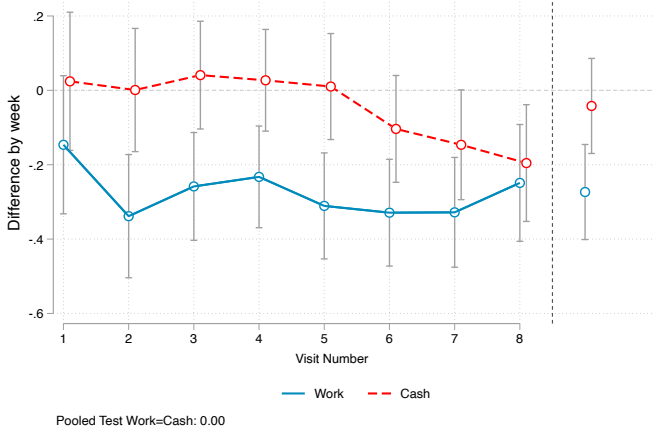
Figure 3: Main Treatment Effects



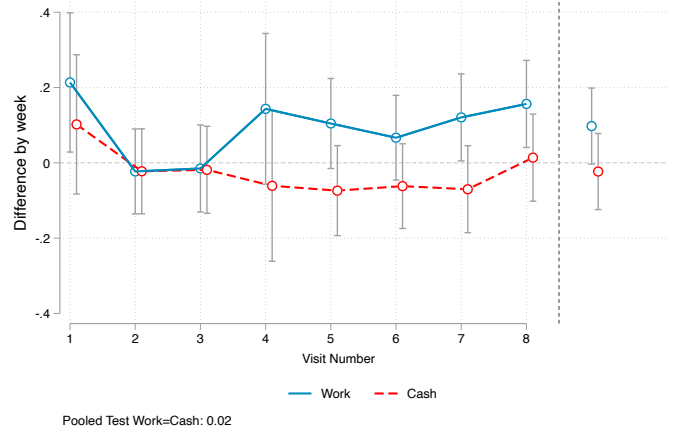
Notes: This figure plots the point estimates and 95% confidence intervals for each outcome in the work and cash treatment groups relative to the control group. All outcomes are standardized. The scales for PHQ-9 and the Cohen Stress Scale have been reversed from previous tables so that positive values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

Figure 4: Weekly trends in psychosocial measures

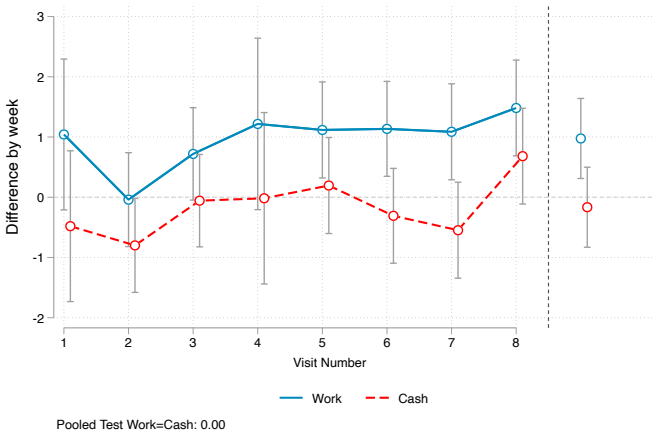
(a) Stress Index



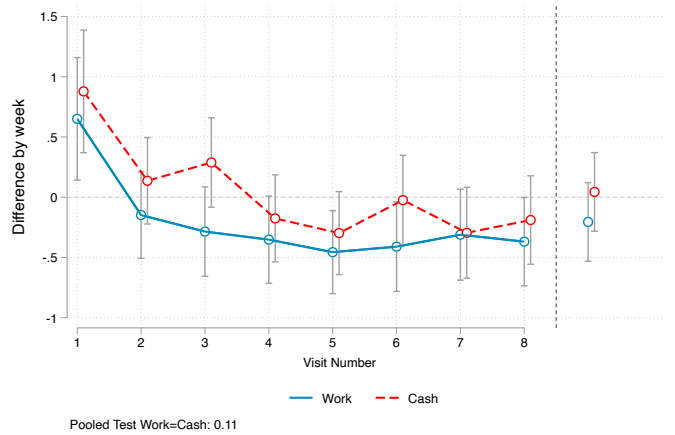
(b) Sociability



(c) Positive Conversations

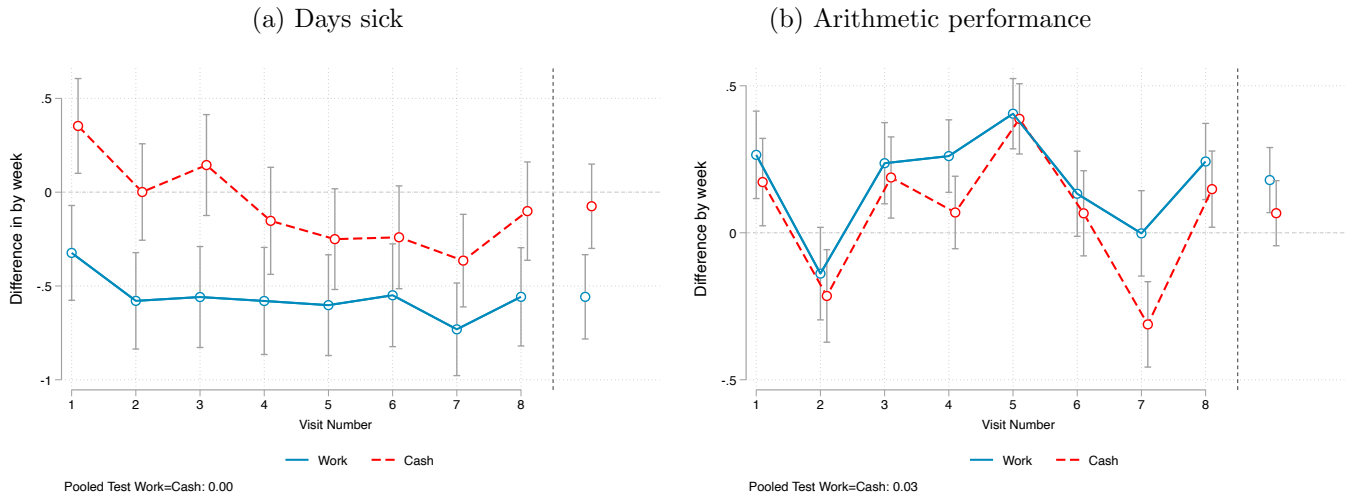


(d) Negative Conversations



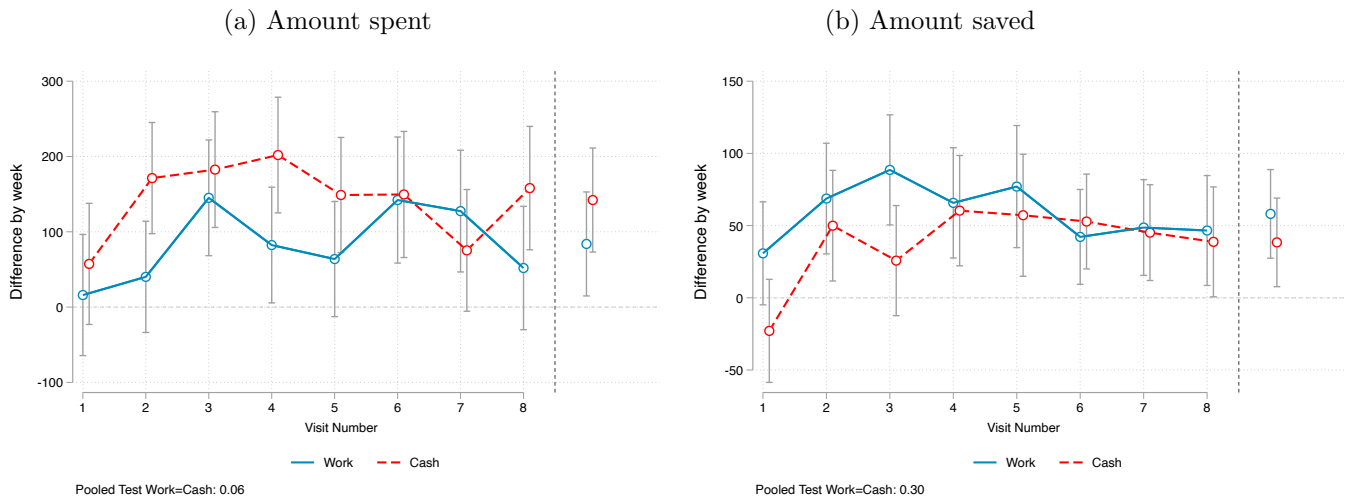
Notes: Each figure plots the impact of the treatment (work or cash) by week relative to the control arm. The estimates to the right of the dotted line represent the pooled effect across all eight weeks.

Figure 5: Weekly trends in physical and cognitive measures



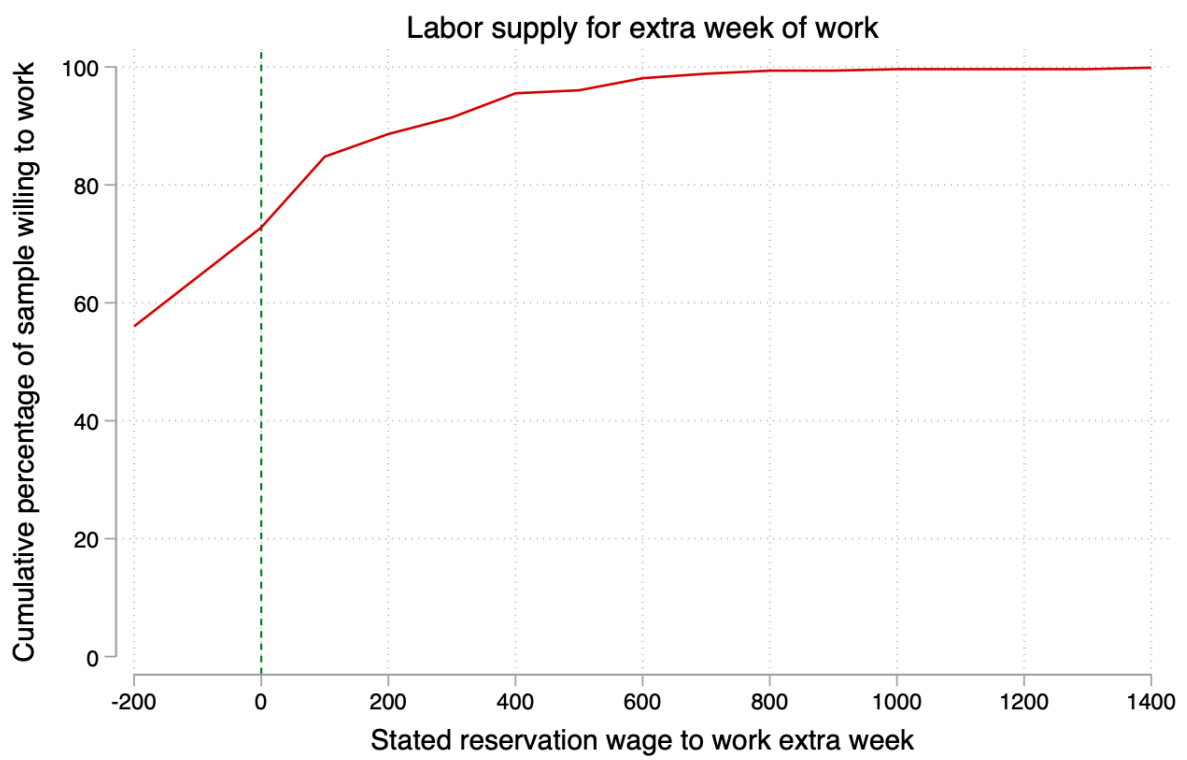
Notes: Each figure plots the impact of the treatment (work or cash) by week relative to the control arm. The estimates to the right of the dotted line represent the pooled effect across all eight weeks.

Figure 6: Weekly trends in spending and saving



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) 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

	Baseline	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*** (0.106)	0.083 (0.102)	-0.115 (0.086)	0.104 (0.108)	-0.285** (0.116)	-0.099 (0.103)
Cash	0.215 (0.132)	0.132 (0.127)	-0.083 (0.106)	0.044 (0.109)	-0.055 (0.130)	0.021 (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
Adj. p-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) Mental Health Index
Cash	0.009 (0.105)
Work	0.245*** (0.081)
Cash * Certificate	0.106 (0.138)
Work * Certificate	-0.006 (0.110)
Certificate Assignment	-0.034 (0.093)
Observations	516

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** (0.012)	0.008 (0.005)	-0.020 (0.048)	-21.328 (23.402)	-0.139 (0.201)	135.167 (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 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 (0.103)	-0.213*** (0.074)	-0.090 (0.165)	-0.324*** (0.122)	0.234* (0.139)	0.338*** (0.093)	0.202* (0.107)	0.133 (0.104)
Cash	-0.064 (0.115)	0.030 (0.084)	-0.010 (0.185)	-0.078 (0.127)	0.323** (0.150)	0.185* (0.103)	-0.012 (0.133)	0.088 (0.129)
Test: Cash = Work	0.604	0.002	0.571	0.014	0.440	0.058	0.085	0.666
Ajd. p-value 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 (0.106)	0.134 (0.107)	0.170 (0.229)	0.369*** (0.131)	0.259 (0.157)	0.289*** (0.094)
Cash	0.106 (0.141)	-0.106 (0.124)	-0.174 (0.315)	0.112 (0.152)	0.201 (0.176)	0.043 (0.121)
Test: Cash = Work	0.676	0.011	0.182	0.050	0.681	0.027
Ajd. p-value 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 A9: Balance on observables: exposure to death in Myanmar violence

	(1)	(2)	(3)	(4)	(5)
	No Violence	Violence	No Vio. vs. Vio.	No Vio. vs. Vio., Town FE	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 A10: Exposure to violence and baseline employment

	(1) Depressed at baseline	(2) Depressed at baseline
Experienced at least one death	0.121** (0.052)	0.147*** (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 A11: Heterogeneity in impact of employment treatment by exposure to violence

PANEL A: Work v. Control			
	(1) Depressed	(2) PHQ	(3) MH Index
Work	0.130 (0.117)	0.013 (0.212)	0.075 (0.121)
Experienced at least one death	0.196* (0.111)	0.133 (0.194)	-0.121 (0.115)
Work * Experienced death	-0.265** (0.124)	-0.230 (0.219)	0.161 (0.127)
Observations	561	561	561

PANEL B: Work v. Cash			
	(1) Depressed	(2) PHQ	(3) MH Index
Work	0.073 (0.107)	-0.028 (0.183)	0.081 (0.102)
Experienced at least one death	0.085 (0.095)	0.070 (0.160)	-0.044 (0.093)
Work * Experienced death	-0.172 (0.113)	-0.179 (0.196)	0.095 (0.109)
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 A12: Heterogeneity in impact of employment treatment by baseline sociability

PANEL A: Work v. Control			
	(1) Depressed	(2) PHQ	(3) MH Index
Work	-0.029 (0.035)	-0.186** (0.079)	0.199*** (0.055)
Sociable	0.076 (0.052)	-0.092 (0.095)	0.061 (0.071)
Work * Sociable at Baseline	-0.165** (0.065)	0.001 (0.117)	0.033 (0.083)
Observations	565	565	565

PANEL B: Work v. Cash			
	(1) Depressed	(2) PHQ	(3) MH Index
Work	-0.049 (0.041)	-0.120 (0.086)	0.153** (0.061)
Sociable	-0.024 (0.050)	-0.022 (0.086)	0.087 (0.059)
Work * Sociable at Baseline	-0.060 (0.064)	-0.135 (0.099)	0.029 (0.072)
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 A13: Heterogeneity in impact of employment treatment by baseline depression

	(1) Depressed	(2) PHQ	(3) MH Index
Work	-0.164* (0.084)	-0.056 (0.124)	0.237*** (0.081)
Baseline Mild Depression	0.209** (0.086)	0.204 (0.135)	-0.035 (0.091)
Baseline Moderate Depression	0.225*** (0.084)	0.252 (0.192)	-0.079 (0.104)
Work * Mild Dep.	0.074 (0.099)	-0.076 (0.142)	-0.038 (0.099)
Work * Mod. Dep.	0.082 (0.095)	-0.297* (0.163)	-0.005 (0.107)
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.2 Figures

Figure A1: Pre-filled calendar

HHID: 1 Respondent Name: _____ Block: _____ Starting date: 6/11

	Saturday	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday
WEEK-1					●	✓	✗
WEEK-2		✓			C	✓	✗
WEEK-3	✓		✓	✓	C		✗
WEEK-4	✓	✓	✓		C		✗
WEEK-5		✓	✓		C	✓	✗
WEEK-6	✓		✓		C		✗
WEEK-7	✓	✓	✓	✓	C		✗
WEEK-8	✓	✓				C	✗
WEEK-9	✓	✓	✓	✓	C		✗

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?