

# The Psychosocial Impacts of Forced Idleness

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## Abstract

Social scientists have long posited that employment may deliver psychological utility beyond the value of income alone. Existing literature, however, suffers from problems of selection into employment and an inability to disentangle the pecuniary and non-pecuniary mechanisms driving wellbeing. This paper presents a causal estimate of the psychosocial benefits of employment in the Rohingya refugee camps of Bangladesh. We engage 745 individuals in a field experiment with three arms: (1) a control arm, in which no work is offered; (2) a cash arm, in which no work is offered but a weekly fee is provided; 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. Building on existing observations in psychology, we further investigate the causal roles of past trauma and future uncertainty in mediating the impact of employment on psychosocial wellbeing.

Keywords: Employment, Psychosocial, Refugees

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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; Kessler, Turner, and House, 1988). 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 (Ridley et al., 2020; Case and Deaton, 2020), this literature suffers from two key limitations.<sup>1</sup> 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 and including a comparable group that benefits from the pecuniary dimension of employment alone. We run a field experiment in which we randomize 745 individuals of working age into three arms: (1) a control arm, in which no work is offered but a nominal fee for weekly survey participation is provided; (2) a cash arm, in which no work is offered but a large fee (equivalent to three days of daily labor) for weekly survey participation is provided; and (3) a cash-for-work arm, in which gainful employment is offered for an average of three days a week and individuals are paid (in an amount approximately equivalent to the cash arm) during the weekly survey. The work and parallel cash provisions last for eight weeks. This is well beyond the duration of the average daily labor opportunities that arise in our setting, despite the fact that our experiment spans the peak [informal] employment season of the winter harvest. [The job we offer is a surveying task that, beyond occupying potentially idle time, also deliberately incorporates features identified by the sociological literature as beneficial: active engagement, sociability, and purpose.](#)<sup>2</sup>

We work in the Rohingya refugee camps, the largest set of refugee camps in the world situated upon the southern tip of Bangladesh. The setting is one in which nearly all households

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

<sup>2</sup>The task was also designed to mimic natural forms of employment in the camps, although we excluded hard physical activity as this would preclude female participation. What we describe as ‘work,’ or ‘employment,’ is therefore realistic but not representative of all types of work available to our participants.

are constrained in their labor supply choices due to scarce labor market opportunities.<sup>3</sup> This is both substantively and methodologically important, as providing labor market opportunities in an environment where none exist allows us to estimate the psychosocial impacts of paid employment among individuals who otherwise occupy suboptimal allocations of labor and leisure, and who are therefore likely to shift behavior due to the intervention. They experience, as we term in this study, *forced idleness*.

Between August and December 2017, approximately 750,000 Rohingya fled a genocidal campaign in Rakhine State, Myanmar, crossing the border by foot or raft into Bangladesh. Seeking to limit integration and maximize the likelihood of repatriation to Myanmar, the government of Bangladesh has made formal work illegal for the refugees. Strict restrictions on movement limit access to informal work in nearby urban centers. Refugees are desperate for more to do: many ask for “*haather kaaj*,”- colloquially, handiwork; literally, a way to keep one’s hands occupied.<sup>4</sup> Among our sample population of male and female refugees between the ages of 18 and 45 years, individuals report spending an average of three waking hours entirely idle, our strictest measure of unoccupied time excluding diversionary but unproductive activities such as napping, sitting at a tea stall, etc. If the latter are considered, the average respondent in our sample spends eight hours of his or her waking day engaged in unproductive and/or leisure time. Only 10% of our sample report having worked for one or more days in the previous month.

Unemployment is not only pervasive but psychologically costly: “leisure” appears to be a source of great disutility. Both intensive margin idleness and extensive margin unemployment are significantly correlated with our measure of depression (PHQ-9) at baseline: a three hour increase in hours idle is associated with a 0.12 standard deviation increase in the depression scale. Similarly, individuals who report having been unemployed the entirety of the previous month score 0.34 standard deviations higher in the depression scale, translating to a 10% higher likelihood of being moderately or severely depressed (Table A2). Naturally, these are only baseline correlations, but suggest an important role for employment in psychological wellbeing and underscore the necessity of exogenous variation in employment in order to identify a causal relationship.

We enrich our experiment by examining the mediating roles of two features common to many forcibly displaced persons, both of which were powerfully palpable during our own

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<sup>3</sup>The few who do find work are often informed of the opportunity the day prior and, anecdotally, report not being paid the agreed upon wage with little recourse for redressal.

<sup>4</sup>Such expressions of the need for time 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)

time within the Rohingya camps: past violence and future uncertainty. Just as the ubiquity of forced idleness is plain upon entering the camps, likewise are refugees' memories of a traumatic past and fear of an uncertain future. We explore how these latter two features interact with forced idleness, which likely allows for more time to dwell upon each. We are motivated by a literature in psychology that recognizes a key predictor of depression to be rumination, or the repetitive contemplation of typically dark thoughts around past trauma (Michael et al., 2007; Ehring, Frank, and Ehlers, 2008; Roley et al., 2015) and future uncertainty (Liao and Wei, 2011; Boelen, Reijntjes, and Smid, 2016). Our setting permits us to exploit quasi-random variation in the extent to which individuals experienced violence in their recent exodus, as well as impose experimental variation that reduces the level of uncertainty individuals face in their near future, to examine how each may mediate this relationship between idleness and psychosocial wellbeing.

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 costs of forced idleness and consequent benefits of employment. In documentation of the violence that led to the exodus in Myanmar, a United Nations' 2018 Human Rights Council (HRC) Report articulates how "the [military] 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." Described by one eyewitness, "*The first round of shooting was like a rain of bullets. The second round was slow as the soldiers killed the men individually. They aimed a gun at each man and shot.*"

Stories such as these are not difficult to find within the Rohingya camps, and the HRC's documentation of indiscriminate violence 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. 88% of the population within our sample report having experienced at least one death, with a conditional mean of 9.3 deaths. These experiences are significantly correlated with present psychosocial wellbeing: individuals who experienced a death are 0.35 standard deviations higher on the depression scale, translating to a statistically significant 25% higher likelihood of being moderately or severely depressed (Table A3, Columns 1 and 2). Our experiment exploits this quasi-random variation in past violence to investigate whether employment may serve to alleviate the psychosocial costs of past trauma. While the evidence we provide for the indiscriminate nature of the violence is intended to alleviate concerns about selection, we recognize that without additional documentation about the Burmese military's tactics,

some ambiguity remains. Baseline data lend support to this mediating role of employment. Unemployment is associated with a 0.6 standard deviation higher depression score, or a statistically significant 33 percentage point (87%) higher likelihood of being moderately or severely depressed, among those who experience a death relative to those who do not (Table A3, Columns 3 and 4). As above, these correlations are vulnerable to selection into employment, reinforcing the need for a field experiment to estimate the causal impact of past trauma on the psychosocial value of present employment.

The second feature of the forcibly displaced that we embed into our experimental design is the deep uncertainty that the migrant faces about his or her future. When asked what most occupies the mind during idle time, 46% of our sample volunteer concerns about the future. *“I think about where I will be in the future all the time. We are travelers here. We are anxious to go back to our own land.”* *“Sometimes it seems that we might be transferred anywhere. But I know nothing about where we will go. I imagine sometimes that we will be shifted to another place, or thrown away in the river.”*

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.<sup>5</sup> In particular, among those refugees randomized into the cash for work treatment, a randomized subset receive a calendar marked with every date of work for the full eight week duration of employment. The remainder receive no such calendar and are instead informed once a week about their work schedule for the following week. We inform both groups at the outset, however, about the nature of the work, the total labor demand (24 days over eight weeks), and the total wage. By so doing, we aim to build a careful mechanism experiment in which we vary only the degree of certainty with which refugees may envision their daily activities for the coming two months. We stress that in a context where tomorrow’s food supplies are uncertain and the typical (rare) job opportunity is a non-contracted, spot market, day laborer position, daily predictability around employment over two months is likely to be a meaningful source of certainty. Moreover, the ubiquity of idle time in our setting makes it unlikely that the psychosocial value of a known and stable schedule (as workers in Mas and Pallais (2017) were willing to pay for) comes from the ability to better plan one’s other daily productive activities, but rather that any psychosocial boost from a certain schedule emerges from the alleviation of unpredictability in one’s life. This of course is an empirical question, and we collect time-use data to see if uncertainty in scheduling crowds out other welfare-enhancing activities.

Baseline data again offers suggestive evidence that considerations of the future play an

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<sup>5</sup>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)

important role in psychosocial wellbeing: 92% of those who report concerns of the future also report that idle time is “somewhat or very unpleasant.” Notably, unemployment in the last thirty days is associated with a statistically significant 0.6 standard deviation increase in the depression score (46% higher likelihood of moderate or severe depression) among those who are occupied by the future relative to those who are not (Table A4). Yet again, these correlations are informative but susceptible to selection, necessitating exogenous variation in actual certainty about the future in order to determine its causal impact on wellbeing.<sup>6</sup>

We embed a final layer of randomization in an effort to identify the psychosocial mechanism at work and limit conflation with the possible effects of future work expectations among employed individuals. To this end, we present a certificate of participation to a randomized half of our sample (across all treatment groups) which aims to maximize the salience of ‘work experience,’ the channel through which we suspect expectations of future work to flow. This experiment is not definitive, as (1) the non-certificate counterfactual may still be vulnerable to expectations of future work beyond our control, and (2) potential employers who learn about the randomized nature of certificate distribution may discount the value of the certificate. We cluster-randomize the certificate to limit knowledge of the randomization process, and also collect post-intervention data on the likelihood and nature of employment to assess the degree to which either of these confounders may play a part. With these considerations in mind, we use the certificate intervention to gauge the approximate magnitude of the psychosocial impact of this alternative mechanism. We discuss this sub-experiment in detail in the body of the paper, but stress that it is not the central objective of this study.<sup>7</sup>

We conclude our series of interventions with a lab-in-the-field experiment that aims to better understand what dimensions of employment individuals derive the greatest psychosocial benefit from. We offer our formerly Small and Large Cash beneficiaries a one-week employment opportunity to make jewelry; we then vary the features of the work experience with regard to autonomy, sociability and cooperation, and purpose, and we estimate wellbeing, reservation wages, labor supply, and output quality. This experiment offers a (non-exhaustive) lens into the ingredients of psychosocially valuable ‘employment,’ allowing us to better understand alternative employment contexts in which our results may apply.

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<sup>6</sup>This relationship between existential uncertainty and idle time is not unique to this refugee context; it echoes the observations of psychologist Victor Frankel in the concentration camps of Nazi Germany: “A man who could not see the end of his “provisional existence” was not able to aim at an ultimate goal in life. . . . The unemployed worker, for example, is in a similar position. His existence has become provisional and in a certain sense he cannot live for a future or aim at a goal.” (p.70)

<sup>7</sup>In addition to the margins of variation described above, our setting further allows us to explore questions around three important features of the respondent, each of which is likely to mediate the effectiveness of our employment program: baseline levels of sociability, depression, and respondent gender. While not the central objective of this study, these features have important implications for the generalizability of our results; we discuss each in detail in the body of the paper.



While our paper argues [employment may be beneficial due, in part, to](#) a reduction in the amount of time an individual spends idle, we acknowledge that receipt of a job can change other dimensions of a person’s life. In particular, being employed not only reduces idle time, but may also reduce time to engage in activities that make them happy (seeing friends, doing other work) or unhappy (domestic abuse). Furthermore, the cash that individuals *earn* may be received differently and used differently than cash that is simply given.<sup>8</sup> We explore these alternative mechanisms in the final section of the paper. To examine whether employment may crowd out other time-consuming activities, we estimate the impact of each treatment on time-use and domestic violence. To examine the possibility that earned and unearned income are spent differently, we estimate whether our interventions affect the types of purchases that households make.

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. There exists a long history of sociological work exploring the costs of long-term unemployment beyond that of income alone (Morse and Weiss, 1955; Terkel, 1974; Turner, 1995; Colic-Peisker and Walker, 2003; Jahoda, Lazarsfeld, and Zeisel, 1971; Wehrle et al., 2018). Our experiment is motivated by this observational work 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, as well as with the lab experiments of Hsee, Yang, and Wang (2010) and Wilson et al. (2014) both of which find that individuals are willing to pay in order to avoid being idle. 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). Motivated by existing correlational work in

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<sup>8</sup>We try to limit this possibility by framing the cash given to Small and Large Cash recipients as also being earned for participation in weekly surveys.

psychology, we further engage this setting to causally estimate the mediating roles of past trauma and future uncertainty on employment and psychological wellbeing.

Moreover, 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,<sup>9</sup> 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. These features are common to many forcibly displaced persons globally (45.7 million), as they are to the incarcerated (10.35 million, of whom many may likewise be victims of past violence and trauma), as well as many of the world's rural poor (300 million, many of whom suffer from seasonal scarcity of labor and consumption opportunities and the livelihood uncertainties that follow: 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 more cost-effective at improving psychological wellbeing. More broadly, we contribute to a policy literature around the future of work and the merits of employment programs relative to cash-based interventions such as unemployment insurance and Universal Basic Income (UBI). Widespread unemployment has implications not only for the material but also the psychosocial wellbeing of the un- and under-employed. While cash-based programs directly address the loss of income and are relatively straightforward to implement (Hanna and Olken, 2018), they do not address the psychosocial costs that may accompany the absence of work. These costs are well elucidated through case studies in the sociology literature, first articulated in Jahoda, Lazarsfeld, and Zeisel (1971)'s seminal work around Marienthal, a small town in Austria that was devastated by deindustrialization in the wake of the global depression of the 1930s. As described by one woman who lost her job, *"If I could get back to the factory it would be the happiest day of my life. It's not only for the money; stuck here alone between one's own four walls, one isn't really alive."* (Jahoda, Lazarsfeld, and Zeisel, 1971). More recently, individuals who are incarcerated - as of 2019, 2.3 million within the United States alone - describe similar experiences. *"It is the dull sameness of prison life, its idleness and boredom, that grinds me down ... boredom, time-slowness boredom, interrupted by occasional bursts of fear and anger, is the governing reality of life in prison."* (Council, 2014). We bring an empirical lens to this question.

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<sup>9</sup>Despite provision by NGOs of basic staples such as rice, lentils, and oil, and a tiny plot of space upon which to build a shelter, refugees need cash for basic consumption items: clothing, salt, vegetables or fish, hygiene products, household ware, etc.

Finally, this study contributes to a small but growing literature that engages refugee populations and the forcibly displaced in field experiments (see IPA (2020) for a list of interventions). The number of forcibly displaced has grown rapidly in recent years, reaching a historic high of 70 million in 2018 (UNHCR, 2018). Among the existing set of field experiments engaging this population, the vast majority are psychosocial support interventions and the remainder explore 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 both probe the mechanisms and causally estimate the mediating role of features that embody the experience of the forcibly displaced.

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 proposes our main set of hypotheses and analysis plan; Section 6 reviews power calculations, and Section 7 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). Along with the subsequent colonization of Burma by Britain in the early 1800s came the first recorded census of Burma, in which the British administrative state introduced a system of ethnic classification defining 135 sub-races that did not include the Rohingya (Mahmood et al., 2017). British rule was also accompanied by a relaxation of borders within its colonial territory, encouraging migration between Burma and India (of which Bangladesh was at the time a part) in order to address labor shortages. The resulting rapid influx and economic rise of ethnic Muslims from the Bengal region of India heightened existing tensions between minority Muslims and the predominantly Bamar

(Buddhist) population within Burma (Hussam, 2019).

Following independence from Britain in 1948 and a brief period of democratic rule, General Ne Win led a successful military coup in 1962 and placed the country under military rule for the next several decades (Hussam, 2019). The new government inherited the British system of ethnic classification and built its administrative state upon this framework. In 1982, the Citizenship Act required national identity cards specifying ethnic membership in one of the 135 recognized national races — thereby excluding, by construction, the Rohingya from citizenship (Wade, 2017). Multiple waves of suppression of the Rohingya minority ensued over the next 50 years, even as the country transitioned into democratic rule. 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 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 (Hussam, 2019). 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, make-up, jewelry and alcohol.

Though Bangladesh 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 of the camps, but this comes with significant risk as military checkpoints around the camps are abundant. The typical jobs that few refugees have access to in this context are as day laborers (in agriculture or construction); operating a small street stall for vegetables or toys; private tutoring for those who are more educated; working with NGOs on activities like running cooking centers, women’s and children’s centers, etc. 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.

We note that operating in the largest refugee camp in the world, and one which formed in a matter of weeks out of a genocidal campaign of significant proportions, is not easy. Such a setting pairs the inevitably complicated politics of rebuilding society in makeshift camps with the operations of innumerable decentralized NGOs. These complex processes engage a population that bears the trauma of their recent origins and the existential uncertainty of their future, rendering it essential for field operations to consider potential unintended consequences of their activities. Neither diminishes the necessity of engaging with this population in service of welfare-enhancing policy: forcibly displaced migrants are at a historical high and projected to grow substantially in the coming decades, with the International Organization for Migration projecting up to one billion climate migrants alone by 2050 (IOM,

2014).

## 3 Experimental Design

### 3.1 Sample

The research team obtained permission from the RRRC to work in three camps in Bangladesh (5, 8W, 17), which were selected given the healthy relationship cultivated between our research partner, Innovations for Poverty Action (IPA), and the CiC officials in each location.<sup>10</sup> The CiC organized meetings with the local *majhis* to explain how the research team would be interacting with households in their respective blocks. Within each camp, we selected non-adjacent blocks into our sample to reduce the risk of spillovers. Finally, within each block, we selected five households to be part of our sample. Enumerators worked in teams of five, traveling together to two blocks per day to execute their surveying tasks. Upon entering a given block, the field team knocked on doors at random within each block, asked if the household member (pre-assigned as male or female head of household) was interested in participating in a study, and confirmed that the respondent met seven pre-established criteria: they had not worked in the last 14 days; they were within the ages of 18-45 years; they were able and willing to work for two months inside the block; they were not the *majhi* or a member of the *majhi's* household; and they did not receive remittances from abroad. Upon meeting all criteria, the subject was enrolled into the study sample. In total, we assembled a sample of 745 individuals across the three camp sites.<sup>11</sup>

### 3.2 Primary Intervention

We randomly assigned 149 blocks, each with five refugees, to one of three arms. Figure 1 presents the experimental setup. We randomize at the block level to limit any potential spillover. We feel reasonably confident that such spillovers were limited because we remained in close contact with the block *majhi* throughout the experiment, and complaints of inequity would have been quick to be conveyed, as the *majhi* would have been the recipients of the blame. Recent evidence from Egger et al. (2020) also documents that substantially

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<sup>10</sup>We obtained permission to work in a fourth camp, which would have brought our sample to 1000 refugees. However, this camp posed numerous logistical burdens, which made data collection a significant challenge and led to our decision to exclude this camp from the analysis.

<sup>11</sup>We sought to identify individuals who had not worked in the last 14 days out of equity concerns. The vast majority of those of working age encountered in our pilot work were eager to find a job, and we wished to engage those who did not already have access to a work opportunity, but rather found themselves in a state of forced idleness.

larger cash transfers engendered no negative psychosocial outcomes among non-recipients within or across villages.. 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 (the “Small Cash” group), where participants received 50 taka (0.60 USD) per week as compensation for answering our weekly surveys. An additional 33 blocks were assigned to the “Large Cash” group, where participants received 450 taka (5.30 USD) per week as compensation for survey participation. Finally, 83 blocks were assigned to a “Cash-for-Work” group, where we offered participants gainful employment. We compensated participants in this treatment arm with 150 taka (1.77 USD) per day of work. Households were assigned two, three, or four days of work per week, averaging out over the course of the eight weeks to 450 taka per week as in our “Large Cash” group.<sup>12</sup> All participants were informed that their selection (into either Large Cash or Work) was random. We instructed enumerators to display the random number that would appear on their tablet, assigning the participant to his or her treatment group, to the participant as it appeared.<sup>13</sup>

In order to generate exogenous variation in short-term uncertainty, we randomly assigned individuals within work blocks to a “certain” or “uncertain” work schedule. We provided participants in the “certain” group with a pre-filled calendar that highlighted the days they were assigned to work and the days the enumerators would return to collect their work submissions and deliver payments (see Figure 2 for a picture of a sample calendar). Participants in the “uncertain” group received the same work task but no calendar with pre-filled dates. They were instead informed each week about which days they would be hired for the following week. This “uncertain” schedule most closely resembles the reality of working in the informal sector in and around the camps. Our qualitative work suggests that few individuals who do find employment are unlikely to know their schedule more than one day in advance and often complain of being paid less than they were promised.

As this randomization was executed at the household level, we varied the schedule that the two groups received within a block in order to prevent uncertain participants from assuming their schedule was identical to their [potentially] certain neighbors. For example, in a particular week in a particular block, employees in the uncertain group might be assigned

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<sup>12</sup>Payment on a given week was conditional on missing no more than two days (cumulatively over the course of the intervention) of work. It is therefore possible that individuals in the “Cash-for-Work” arm will receive less in income by the end of the intervention than their “Large Cash” counterparts. We suspect that the difference is very little, if anything, given that in our pilot with 300 households, no individual missed a single day of work.

<sup>13</sup>This randomness was in fact crucial to obtaining buy-in from the community and especially the *majhi* (the political and administrative leaders of each block) because the possibility preferential treatment, especially around employment, was a sensitive issue and the *majhi* wanted to make sure such feelings were avoided.

two days of work (Tuesday and Thursday) while those in the certain group could be working four days (Monday through Thursday). These schedule assignments would be reversed for another block in the same week. Across our total working sample, therefore, the schedules across weeks were (on average) identical between certain and uncertain employees.

Our employment task 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 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 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 twenty same-sex neighbors four times per day (centered around prayer times, which were a common source of structure for most refugees). 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 and would 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 create a time-use worksheet visually depicting common daily activities in the camps (sleeping, eating, lounging at a tea stall, sitting idle). We piloted the sheets extensively to ensure that all major activities were included (see Figure 3 for a visual of the time-use sheet and activities). Upon being randomly assigned to the employment intervention, enumerators spent twenty minutes explaining the work task to households and then showed the participant a five minute video designed by the research team articulating the same; this ensured standardized comprehension across participants. Any questions that participants had regarding the task were answered at this time (and in subsequent collection survey days as needed).

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 and who had enough space to accommodate the weekly collection surveys. These facilitators were compensated with an additional Tk. 50 per week for their



services.<sup>14</sup> At the conclusion to each day, the facilitator would drop a piece of cardboard into the box, thereby separating out each day’s work. 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 come to the facilitator’s home on the 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 the case of work blocks, the enumerators first checked the respondents’ work (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 included looking for whether the right number of tick marks (corresponding to the number of individuals the participant was asked to survey) were present and if not, why not; whether the patterns across days were identical or distinct; whether the handwriting was consistent (they did not give it to someone else to fill out). We did not have auditors in the camps watching our workers; this was logistically infeasible since workers could do their work whenever they chose (within certain blocks of time each day).<sup>15</sup> 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, focused work without excessively penalizing for unintentional mistakes. Payment occurred at the end of the interaction, once the enumerator had administered the standard weekly collection survey.

Based upon a pilot we ran four months prior to the scale-up that confirmed refugees’ eagerness to work regardless of the wage offered, we anticipate high rates of completion among our work-task participants. However, it is important that we check for non-compliance among the Cash for Work treatment arm, as failure to complete the work will result in lower wages and consequently different pecuniary benefits between the Large Cash and Cash for Work arms. We will employ our weekly and endline surveys to verify that 1) the total disbursement of cash by the end of the experiment was not significantly different between the Large Cash and Cash for Work groups; 2) that work recipients were indeed employed at a higher rate than cash recipients (i.e large cash recipients did not go out and find work for

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<sup>14</sup>We include an indicator for being one of these facilitators in our robustness checks.

<sup>15</sup>Given this setup, there exists room to shirk, but this is a risk we were willing to take given the inability to feasibly monitor more intensely. The goodwill and excitement to engage that we encountered in our pilot with 300 individuals, and continued to see anecdotally through the full RCT, makes us more confident that shirking was limited.

themselves); and 3) we do not see any differential attrition across arms.

After the first four weeks of the intervention, we provided half of the blocks in our sample (across all treatments) with a certificate confirming that they had engaged with the NGO for data collection activities. This cross-randomization is intended to disentangle the extent to which individuals may derive psychosocial benefits from an anticipation of future work (much like individuals who choose to engage in unpaid internships do so with the hope of a CV boost and future employment) from the psychosocial benefits derived from the work itself. The physical certificate maximizes the salience of ‘experience,’ the channel through which one might anticipate greater employment opportunities in the future. While a null effect of the certificate does not guarantee that this confounding channel does not exist, it suggests that the impact on psychosocial wellbeing of anticipated work opportunities is likely to be small. [We check for differential impacts on actual employment across groups in our six week followup after the intervention has concluded.](#) Importantly, we distribute the certificate to a randomized subset of individuals not only in the Cash for Work group, but also in the Small and Large Cash groups, in order to further partial out any effects of reciprocity that the gifting of a certificate may have on psychosocial wellbeing.

At the conclusion of our endline, we surprised Cash for Work households with the opportunity to engage in one additional week of work.<sup>16</sup> This additional feature of the experiment permits an estimation of individuals’ reservation wages (which could not be elicited for the given work task until they had experience engaging in the work, and is therefore conducted at endline). Moreover, by randomizing the wage offered to the employee, we can estimate their labor supply curve. In an attempt to bound the effect of reciprocity in this exercise (for example, participants may feel obligated to work for us because we had engaged them thus far), we varied how this additional week of work was framed. Specifically, among a randomized subset of participants, we emphasized that “we can find others to do the work if you do not want to” in order to stress that the participant was under no pressure to work and we were not constrained in labor supply.

In order to elicit reservation wages, we first asked households whether they would be willing to volunteer for the additional week of work. For those who said yes, we then asked them whether they would prefer to receive cash (a randomized draw) for answering a few questions rather than volunteering with us. This enables us to estimate the average value placed on the work itself. For those who said no, we engaged in the following process: we first elicited their willingness to accept a given wage, using the titration method, for working

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<sup>16</sup>We justified this surprise by informing participants that we had some limited funds remaining before our project concluded in the following week; this ameliorates further expectations of work after the additional week.

an additional week. We then drew a randomly assigned wage. If this wage was above the respondents' reservation wage, we offered them the job, and also asked them whether they would prefer the same amount in cash for answering a few questions rather than working the additional week. If the draw was below the reservation wage, we concluded the exchange.

While not the focus of the current paper, we also offered our Small Cash and Large Cash groups the opportunity to be employed by us for one week after the endline survey. This exercise was constructed to further understand how the *nature* of work affects labor supply. We designed a simple jewelry-making task which allowed us to conveniently randomize features of the work task (busyness, agency, sociability, and purpose) to examine the channels through which employment may improve psychosocial wellbeing. Further details of this lab-in-the-field experiment are provided in the Appendix. Despite these efforts, we cannot speak directly to whether our results would extend to, for example, a much more physically demanding form of employment, **nor a more tedious task**, that might be accompanied by greater disutility of effort.

## 4 Data Collection and Survey Instruments

### 4.1 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 a deep and iterative process of developing our survey instruments. We started with standardized modules but adjusted heavily to accommodate these contextual demands, adapting or eliminating various questions from such modules which were culturally insensitive or incoherent given the experiences of the Rohingya. Surveys were translated and back-translated from English to Bengali to Rohingya. Source modules and significant adaptations are described below, and further detail can be provided upon request.

Upon launching the full experiment, data were collected via a baseline, midline, and endline survey, as well as nine weekly 'collection' surveys to track a smaller number of outcomes regularly. The baseline was conducted with households prior to revealing treatment status, which was immediately followed by the midline survey to capture the short-run impacts of the treatment. Weekly surveys were conducted before participants collected their payment each week. The endline survey was conducted two days after the end of the work and cash provision period. Finally, we conducted a six-week followup survey to monitor mental health for program participants once the interventions had been concluded.

## 4.2 Main Outcome Variables

We define seven primary outcomes of interest: time-use, mental health, stability, physical health, cognitive function, economic decision making, and willingness to work. These measures, and the survey waves they were collected in, are summarized in Table 4 and 5.

### 4.2.1 Time-Use/Idleness

Our primary measure of time-use is the self-reported average number of hours that respondents spend idle. At baseline, we find that individuals spend an average of three hours per day sitting entirely idle (this excludes unproductive or diversionary activities like sleeping during the day or sitting at a tea stall).

While we do not prespecify the following outcomes, we seek to further understand how the interventions shift time allocation by 1) analyzing what activities respondents report substituting idle time for (enumerated as household chores, looking for a job, childcare, sitting at tea stalls, talking to friends, visiting friends, praying, or taking a nap); and 2) reviewing their detailed time-use allocation reports for the day prior to endline. We provisionally categorize 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).

### 4.2.2 Psychological Wellbeing

**Overall Psychological Wellbeing Index** We construct an overall psychological wellbeing index by aggregating and inverse covariance weighting the standardized outcomes listed below.

**Depression** We measure depression using the nine-question depression scale of the Patient Health Questionnaire (PHQ-9), a standardized screening tool that assesses mental and emotional health disorders.

**Stress** We measure stress using an index comprised of three questions that we adapted to the refugee context from Cohen’s Perceived Stress Scale (the most widely used tool for measuring the perception of stress).

**Life Satisfaction** Life satisfaction is measured with an adapted version of Diener’s Satisfaction With Life Scale. We include four of the five statements along a six-point Likert

scale.

**Sociability** We inquire about the interactions that participants have had throughout the day prior to the survey day. In particular, we record how many different people the respondent had a conversation with and how many of these interactions left them feeling positive.

**Self-Worth** 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. The first question invites respondents to consider the person in their community who contributes the most to their respective family and asks the respondent where they would rank themselves relative to that individual. The second question asks respondents to rank themselves relative to the person they believe contributes the most to their community. The final question asks respondents to rate how much they are able to help their family in ways they wish to. All three questions are measured on a scale from 1-10.

**Agency** We capture respondents’ agency in two ways. First, we build a locus-of-control index, drawn loosely from Rotter’s thirteen-question Locus of Control Instrument. The index is comprised of four questions about the degree to which people believe that they, as opposed to external forces, have control over the outcomes in their lives. Second, we use a simple revealed preference game in which participants are offered an incentivized opportunity to either make a resource allocation decision for their community themselves or defer to another individual (an NGO worker, an “expert”, or another refugee) to make the decision on their behalf.

**Stability** We adapt the Cantril Self-Anchoring Striving Scale (Cantril, 1965) to measure how secure respondents feel in their present lives and in the future. The questions ask respondents to consider a ladder, with the most secure life being a 10, and the least secure life being a 0. Respondents articulate which step on the ladder they feel they are on at present and where they anticipate standing in five years.

### 4.2.3 Gender Dynamics

We construct a measure of household-level 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.

#### **4.2.4 Cognitive Ability**

We measure cognitive ability in two ways. First, we employ a digit-span memory test using both forward and backward sequences of numbers. Second, we ask a series of basic arithmetic problems including multiplication and division questions. Our measure of cognitive ability is the standardized index of these two measures.

#### **4.2.5 Physical Health**

We inquire about the respondent’s physical health, namely whether and for how many days they have fallen sick in the thirty prior days. With 75% of the sample reporting that they were sick in the last 30 days at baseline, we focus on a measure of whether households were severely ill in the past week. Most simple infections will resolve in a week, and chronic conditions should also stabilize within a week with proper care. Our primary measure of physical health is therefore an indicator for whether respondents report being sick for seven or more days.

#### **4.2.6 Financial Wellbeing**

We ask respondents how much they have borrowed and how much they have saved in the prior 90 days. We employ this outcome measure only for comparisons of the small cash group to the large cash (and/or employment) group, as this is the margin along which there exists variation in pecuniary benefits.

#### **4.2.7 Economic Decisionmaking**

We explore economic decisionmaking along two dimensions: time and risk preferences. Time discount factors are estimated by adapting the Andreoni and Sprenger (2012) convex time budget (CTB) method following Giné et al. (2018). Risk preferences (risk aversion) are measured with multiple price list decision tasks (adapted from Holt and Laury (2002) methodologies), adjusted for the Rohingya context for comprehension.

### 4.2.8 Willingness to Work

We capture recipients’ willingness to engage in a work task at endline. We apply the incentivized Becker-DeGroot-Marschak (BDM) method among work group respondents (who now have experience with the work task), and ask them if they are willing to complete an additional week of work at various titrated wages. Pairing these responses with the number of days of additional work the respondent actually completes, we can plot the labor supply curve.

## 5 Hypothesis and Analysis

Across all specifications, we use double-selection LASSO to select controls for precision and we control for baseline measures of our outcomes when they are available through an ANCOVA specification. Unless otherwise specified, each specification will be run for the full set of outcomes described in Section 4.

### 5.1 Main Effect

#### 5.1.1 What is the impact of gainful employment relative to cash alone?

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

Where  $Y_{ibc}$  represents the host of outcomes outlined in Section 4 for individual  $i$  in block  $b$  and camp number  $c$ ,  $X_{ibc}$  is a vector of baseline covariates, and  $\varepsilon_{ibc}$  is an error term clustered at the block level. We include fixed effects for camp  $\gamma_c$  and enumerator  $\delta_e$ .<sup>17</sup>

We seek to understand the impact of employment (*Work* relative to *LargeCash*) on the wellbeing of participants in our study. In order to interpret the magnitude of these effects, we benchmark them against the impact of a cash intervention (*LargeCash* relative to *SmallCash*). If cash generates positive psychosocial benefits, we expect to find that  $\beta_1 \geq 0$ . If employment delivers psychosocial benefits equal to or beyond the benefits of income alone, we expect to find that  $\beta_2 \geq \beta_1$ . The ratio between these two coefficients ( $\beta_2/\beta_1$ ) yields the degree to which the psychosocial benefits of employment outweigh those of cash alone.

We additionally collect weekly data on a subset of outcomes in order to observe temporal dynamics (see Table 5). The time it takes for various effects to materialize can inform the underlying mechanisms at work and policy design around meaningful durations of employment.

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<sup>17</sup>We follow (Di Maio and Fiala, 2019) and include enumerator fixed effects to account for the fact that respondents’ answers may be influenced by the way enumerators ask more sensitive questions.

As such, we run the following specification:

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

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

There exist two caveats to the interpretation that  $\beta_2 \geq \beta_1$  is due entirely to the non-pecuniary psychosocial benefits of employment. First, employment may crowd out other time-consuming activities that may raise or lower ones happiness (eg. seeing friends, alternative work, or exposure to domestic violence). To examine whether such crowd-out happens, we estimate the impact of each treatment on time-use and domestic violence. Second, the income earned from employment may be spent differently, and in turn result in different psychosocial outcomes than cash, which is perceived to be unearned. To examine this possibility, we estimate the impact of each treatment on what participants report spending their money on at endline (which is divided into the following categories: meat and fish, fruits and vegetables, paan and cigarettes, tea and coffee, education, repaying loans, extending loans, healthcare, household supplies, clothing, electronics, and other).

We also recognize that we may find a null effect, which could be driven the lack of psychosocial benefits associated with work or the high cost of effort that nets out the potential benefits. We can use the BDM mechanism we ran at the end of our study period to help tease these two channels apart. If we find that workers prefer to work than not work through the BDM, this suggests that any null effect on psychosocial outcomes is unlikely to be attributable to a high disutility of work (which is presumably something that workers are conscious of and can price).

## 5.2 Mechanisms

We explore a series of pathways through which our employment and cash interventions may affect our primary outcomes. Two of these margins are experimentally induced variations to the employment intervention we administered. The remainder are pathways derived from natural variation in the characteristics of our participants.

### 5.2.1 Features of employment intervention

**Future uncertainty** Does reduced uncertainty about the daily prospects of the next two months improve refugees' psychosocial wellbeing? Empirical work around the relationship



between employment and uncertainty, conditional on income and labor demand, is scarce. There is a small literature that investigates the negative relationship between uncertainty and positive affect (Anderson et al. 2019, Carleton 2016), as well as individuals’ positive willingness to pay to alleviate uncertainty (Lovallo and Kahneman, 2000). However, studies that engage the role of employment are limited. Mas and Pallais (2017) find workers are willing to pay 20% of their wages to prevent an employer from setting an irregular schedule. The setting of their study, however, is substantively different, being online workers in a developed country context where employment opportunities are not scarce. We tackle this question in a setting where unemployment and idle time are ubiquitous, and the uncertainty around both short-term and long-term wellbeing appears to be, at baseline, psychologically crippling.

To answer this question, we restrict our sample to only those individuals who were offered employment. We then compare those who received a calendar outlining their complete work schedule (*Calendar*) to those who did not.<sup>18</sup> We run the following specification:

$$Y_{ibc} = \beta_0 + \beta_1 \text{Calendar} + \lambda_s + \zeta_b + \delta_e + X_{ibc} + \varepsilon_{ibc}$$

We limit our pre-specified outcomes ( $Y_{ibc}$ ) to those collected at midline (all of which were obtained immediately after the baseline survey once the respondent was randomly allocated a work schedule or not), as we anticipate that the effect of providing a schedule is highly time-sensitive and challenging to capture through longer term and more generalized measures of wellbeing.  $\zeta_b$  represents block-level fixed effects (as calendar provision was randomized at the individual, not block, level), and  $\lambda_s$  represents fixed effects for the work schedule each employee (knowingly or unknowingly) received.  $\delta_e$ ,  $X_{ibc}$ , and  $\varepsilon_{ibc}$  are as defined above. If the provision of a calendar for the two months of employment indeed has an impact on psychosocial and financial decision-making measures, we expect to find that  $\beta_1 \neq 0$ . If increased certainty is mediating this effect, we anticipate  $\beta_1 \geq 0$  for the stability outcome. However, the role of increased certainty on risk and time preferences remain directionally ambiguous.

We hypothesize that the psychosocial value of a certain schedule comes from the alleviation of the unpredictability in one’s life. Nevertheless, we acknowledge that a certain schedule also provides individuals with the opportunity to plan their lives around the work they receive, permitting them to make time for other activities they value. We test this possibility by estimating whether the individuals in the uncertain group spend less time in

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<sup>18</sup>Note that both groups were explicitly informed about the total number of days they would work, the nature of the work, and the total wage received. This allows us to isolate the pure impact of uncertainty around daily activities on psychosocial wellbeing.

welfare-enhancing activities. *A priori*, we expect such crowding-out to be unlikely, as most of our sample at baseline spends multiple hours completely idle.

**Future work opportunities** We consider one key alternative mechanism that may challenge the interpretation of positive psychosocial effects of the work intervention as being due to the non-pecuniary nature of work. Namely, current employment may make future employment more likely and therefore carry monetary benefits beyond those of the immediate income received (either through the relationship formed with the NGO or through a boost in the beneficiary’s ‘resume’ which make them more appealing to other potential employers). We view this as highly unlikely (both in actuality and in expectation) within our context given the limited employment opportunities as well as our repeated reminders (at baseline, two weeks before endline, and endline) that the work opportunity we were providing would only last eight weeks, and we would not be conducting any additional activities in the camp thereafter.

Recognizing that this potential future pecuniary benefit of employment is impossible to rule out, however, we randomized the provision of paper certificates which provided documentation of the beneficiaries’ involvement with our project: an explicit boost to their resume. These certificates were signed by our enumerators and included the following text: “*Certificate: This acknowledges that I engaged with Pulse Bangladesh to do data collection.*” (Figure 6). In order to control for any reciprocity effects, we also provided identical certificates to a randomized subset of the cash-only arms.<sup>19</sup> If employed individuals derive psychosocial benefits from an expectation of future work, the certificate should make this expectation maximally salient. This resulting comparison should provide some sense of how concerned one may be about a conflation of purely psychosocial mechanisms with [future] pecuniary mechanisms.

The effect of the embedded certificate randomization is estimated via the following regression:

$$Y_{ibc} = \beta_0 + \beta_1 LargeCash_{ibc} + \beta_2 Work_{ibc} + \beta_3 LargeCash * Certificate_{ibc} + \beta_4 Work * Certificate_{ibc} + \beta_5 Certificate_{ibc} + \zeta_b + \delta_e + X_{ibc} + \varepsilon_{ibc}$$

Where  $Y_{ibc}$  represents our mental health index and the stability outcome for individual  $i$  in block  $b$  and camp number  $c$ , and where  $\zeta_b$ ,  $\delta_e$ ,  $X_{ibc}$ ,  $\varepsilon_{ibc}$  are as defined above. We are interested in whether  $\beta_4 \geq \beta_3 \geq 0$ : in other words, whether there is any differential impact

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<sup>19</sup>The wording of the certificate was made such that it could be applied to both arms; cash-only arms participated in weekly surveys along with all other experiment participants, so technically also engaged in data collection for our project.

of providing the certificate on psychosocial outcomes, and whether this differential impact is over and above that of reciprocity alone (which is identified from  $\beta_3$ ).

One may be concerned that, if other employers learn about the nature of the certificate distribution (i.e. provision to (1) a random subset of workers and (2) some participants who did not engage in active work), the signaling value of the certificate may be diminished, reducing the informativeness of this test. Our time in the field suggests that knowledge of the randomization process is unlikely: we randomized certificate distribution at the block level to limit spillovers, and NGO job opportunities are scarce. We collect data on the types of future employment participants are engaged in six weeks after the intervention to gain some sense of how likely this is.

Even with a salient certificate, a null effect cannot definitively rule out that participants expect future income streams from working on this task beyond that communicated by the certificate. However, it provides some evidence that the impact on psycho-social well-being may be small. Our data on the likelihood of employment after the intervention again offers a partial test of this channel.

### 5.2.2 Features of participants

**Past trauma** Do the impacts of employment vary by the degree to which an individual experiences violence in his or her recent past? We view this pathway as particularly relevant based on our qualitative work with the Rohingya refugees, many of whom expressed that their time sitting idle was consumed by thoughts of the trauma they endured during their exodus from Myanmar. We answer this question by running the following specification on the sample who receives either the large cash intervention or the employment intervention:

$$Y_{ibc} = \beta_0 + \beta_1 Violence_{ibc} + \beta_2 Work_{ibc} + \beta_3 Work * Violence_{ibc} + \theta_l + \gamma_c + \delta_e + X_{ibc} + \epsilon_{ibc}$$

We focus exclusively on measures of mental health and stability for individual  $i$  in block  $b$  and camp number  $c$  ( $Y_{ibc}$ );  $\theta_l$  represents fixed effects for the respondent’s township of origin in Myanmar; and  $\gamma_c$ ,  $\delta_e$ ,  $X_{ibc}$ ,  $\epsilon_{ibc}$  are as defined above. If past violence indeed mediates the psychosocial impact of employment, we expect to find that  $\beta_3 \neq 0$ . Our observations from baseline data suggest that  $\beta_3 \geq 0$ : those who experienced greater violence in the past are likely to experience greater psychosocial benefits from an employment intervention.

We define exposure to violence, *Violence*, as an indicator for whether the household experienced at least one death in his or her family or community during the 2017 exodus from Myanmar. Two pieces of evidence suggest that exposure to violence was indeed quasi-

random: our universe of sociodemographic measures appear balanced between those who did or did not experience at least one death, and reports by the Human Rights Council repeatedly articulate the indiscriminate nature of the Myanmar military’s “clearance operations.” We consider each in turn.

Table 3 presents a balance table between those individuals who reported at least one death in their family or community and those who reported none. We present the results including (1) township-level fixed effects and (2) fifty square mile grid-level fixed effects. We select township fixed-effects because all refugees originate from one of three townships (Maungdaw, Buthidaung, or Rathedaung – see map in Figure 4 of Appendix) and it is possible that the military may have chosen to attack one administrative boundary for political expediency or internal alliances before another (no available evidence suggests this, but we consider the possibility). Second, in the absence of available data on military movement but presuming that the military could not cover all regions at once, we group origin villages into grid cells of fifty square miles (although balance is robust to a variety of cell sizes).<sup>20</sup> Across all available time-invariant sociodemographic measures we collect, features appear balanced between those who did and did not witness at least one death under both the township-level fixed effect specification and the grid-cell level fixed effect specification (Columns 3 and 4, respectively). It is worth noting that sociodemographic measures are balanced even absent controls for location of origin (Column 5). Out of precaution, however, we choose to impose township fixed effects in all violence-related regressions (which we prefer over the grid-cell fixed effects as it reduces room for arbitrary experimenter determination and relies purely on existing geographical/administrative boundaries).

One may be still concerned about strategic movement of the military if it is correlated with unobservables that also mediate the relationship between employment and psychosocial outcomes (for example, perhaps wealthier regions were attacked first, leading to those residents witnessing more violence in Myanmar and potentially benefiting more from employment, given their past social status, in Bangladesh). While there are no news sources that track the geographic movement of the military during the clearance operations, and available satellite images on burning villages are too infrequent to deduce military movement, a large body of detailed qualitative evidence suggests that violence was indiscriminate at local levels. The Human Rights Council of the United Nations commissioned the Independent International Fact-Finding Mission on Myanmar in September of 2018. The 441 page report interviews several hundred victims and eyewitnesses and describes in detail the nature of

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<sup>20</sup>In the grid cell specifications, 66 observations are dropped because the villages the respondents reported coming from could not be matched to existing geocoded village names. All townships could be matched to existing geocoded townships.

violence perpetrated in August through December of 2017 in Rakhine State. Among other similar excerpts, the report describes how *“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.... 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.”* In Figure 5, we present a revealing selection of additional excerpts from the report that further describe the indiscriminate nature of this violence.

**Sociability** Given the social nature of the task (both in terms of the daily work task itself and the need to engage with enumerators at another individual’s home weekly), extroverts may benefit considerably from employment while introverts find the experience psychologically costly. While we do not have a direct measure of extrovertedness, we proxy for it using our baseline measure of sociability: namely, the number of positive social interactions the individual experienced throughout the day prior to the day of survey. We run the following regression:

$$Y_{ibc} = \beta_0 + \beta_1 LargeCash_{ibc} + \beta_2 LargeCash * Sociable_{ibc} + \beta_3 Work_{ibc} + \beta_4 Work * Sociable_{ibc} + X_{ibc} + \gamma_b + \delta_e + \epsilon_{ib}$$

Where  $Y_{ibc}$  represents mental health and stability outcomes for individual  $i$  in block  $b$  and camp number  $c$ ,  $Sociable_{ibc}$  is a dummy for whether the participant was in the top 50th percentile in number of positive conversations at baseline, and  $\zeta_b$ ,  $\delta_e$ ,  $X_{ibc}$ ,  $\epsilon_{ibc}$  are as defined above.

We note that our sociability measure is not a perfect proxy for extrovertedness, since the latter is an inherent feature of an individual while the former is a combination of inherent trait, underlying wellbeing, and local external environment. We may find that participants who rank low in baseline sociability in fact benefit the most from the employment intervention, as it compels them to have positive social engagements that they otherwise were not experiencing. Regardless of the direction of this effect, the implications it has for how to design a psychosocially impactful job and whom to target remain valuable.

**Levels of depression** Our investigation of the heterogeneous effects of employment by baseline depression levels is motivated by 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.

This idea is echoed in recent work by Haushofer and de Quidt (2019), who describe how many depressed patients “are frequently unable to derive pleasure from otherwise enjoyable activities, suggesting a change in preferences.” However, we argue that it is *ex ante* ambiguous whether the most depressed individuals in our sample will react the most or the least to the employment intervention: while those who are severely depressed may not have the psychological foundations necessary to benefit from the potential psychosocial gains that employment can offer, those who are already in a psychologically healthy space may be least likely to need the non-pecuniary benefits of employment. As such, we explore how baseline levels of depression mediate the effect of employment on psychosocial wellbeing, using the medical definitions of mild, moderate, moderately severe, and severe depression as derived from the PHQ-9 scale. We pool the latter two groups for purposes of power (as 9% of our sample qualifies as severely depressed at baseline) and run the following regression:

$$Y_{ibc} = \beta_0 + \beta_1 LargeCash_{ibc} + \sum_{d=1}^4 \beta_d LargeCash_{ibc} * DepressionLevel_d + \beta_3 Work_{ibc} + \sum_{d=1}^4 \gamma_d Work_{ibc} * DepressionLevel_d + \zeta_b + \delta_e + X_{ibc} + Deaths_{ibc} + \varepsilon_{ib}$$

Where  $Y_{ibc}$  represents mental health and stability outcomes for individual  $i$  in block  $b$  and camp number  $c$ ,  $DepressionLevel_d$  is a dummy for each level of depression (mild, moderate, and moderately severe and severe pooled; the omitted category being not depressed),  $\zeta_b$ ,  $\delta_e$ ,  $X_{ibc}$ ,  $\varepsilon_{ibc}$  are as defined above, and  $Deaths_{ibc}$  is a control for the total number of deaths that occurred among the respondent’s friends and family members.

We make no claims about the nature or source of measured depression, though we recognize there is certainly a wide variation in the degree of violence witnessed and perpetrated upon the self among this population. The above is a purely exploratory analysis, but an important one when considering the policy implications of employment interventions within a context such as ours.

**Gender** Our examination of differential effects by gender is motivated by literature in sociology around the loss of work and gender identity (Payne (1998); Schrijvers (1997)), some of which suggests 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)). 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)).

We focus on four outcomes: power perceptions (and the subcomponents of perceptions around intimate partner violence and broader gender norms),<sup>21</sup> time use, mental health outcomes, and stability. In addition to the prespecified primary measure of time-use (on self-reported hours idle), we are also interested in exploring (though we do not prespecify) men and women’s time-use allocation between productive and unproductive activities. We suspect that women may be forced to substitute away from productive activities when employed, while men may find themselves substituting away from idle time. We also examine mental health outcomes, where the total effect on female psychosocial wellbeing is ambiguous. On the one hand, women may experience smaller benefits to mental health if they must forgo other productive activities in order to perform their work tasks. Conversely, women who are able to leave the home may be less vulnerable to domestic abuse within the household and potentially gain a greater sense of agency, translating to a boost in mental health and sense of stability. Drawing from the sociology literature on gender identity, we anticipate the impacts of employment on the mental health and sense of stability of males to be unambiguously positive. We run the following regression:

$$Y_{ibc} = \beta_0 + \beta_1 LargeCash_{ibc} + \beta_2 LargeCash * Male_{ibc} + \beta_3 Work_{ibc} + \beta_4 Work * Male_{ibc} + \zeta_b + \delta_e + X_{ibc} + \varepsilon_{ib}$$

Where  $Y_{ibc}$  represents power perceptions, time-use, mental health, and stability for individual  $i$  in block  $b$  and camp number  $c$ ; and where  $\zeta_b$ ,  $\delta_e$ ,  $X_{ibc}$ ,  $\varepsilon_{ibc}$  are as defined above. This regression allows us to examine the effects of a cash transfer ( $\beta_2$ ) and gainful employment ( $\beta_4$ ) on our outcomes of interest. A comparison of the two coefficients demonstrates whether the non-pecuniary effects of employment are differentially greater for males relative to females.

**Consumption** The refugee camp setting may be unique insofar as the nature of and opportunities for leisure. On the one hand, refugees may not have access to purchasing goods that can enhance their leisure time, making a Large Cash treatment relatively less appealing than

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<sup>21</sup>We prespecify the power perception index as the primary outcome because it is a standardized index drawn from the DHS, but we also suspect that an index as broad as this will be unlikely to shift from our intervention, and therefore are especially interested in how its subcomponents around intimate partner violence may respond to the intervention.

in contexts where recipients can spend money to enjoy extra time. Alternatively, refugees are provided with some (though far from all) basic necessities by NGOs, potentially freeing up cash to spend on leisure goods and making the Large Cash treatment arm more appealing.

We collect data on expenditures at endline over a series of common consumption goods (meat and fish, fruits and vegetables, paan and cigarettes, tea and coffee, education, repaying loans, extending loans, healthcare, household supplies, clothing, electronics, and other). We can divide these expenditures into leisure (ex. paan and cigarettes, tea and coffee, other) and necessary expenses and examine the Large Cash v. Work treatment effect on leisure goods. While far from decisive, if we find a meaningfully higher proportion of Large Cash recipients spending on leisure goods, this suggests that our estimates may be more easily generalizable to spaces where leisure goods are readily available for purchase. If both groups spend equally on necessities, it suggests that the Large Cash arm may be delivering smaller psychosocial benefits in our context than alternative environments where such goods are readily available. That said, our results are likely to apply to contexts in which beneficiaries are as impoverished (as in need of basic necessities) as ours.

### **5.3 Multiple Hypothesis Testing**

Our programs can affect a wide range of psycho-social and economic outcomes. As a result, we will account for multiple hypotheses by computing False Discovery Rate (FDR) q-values. The primary groups of outcomes are listed above, and for each of these outcomes we will construct indices (where possible using inverse covariance weighting) and report both p-values and sharpened q-values.

### **5.4 Differential Attrition and Experimenter demand Effects**

We do not expect to find significant attrition based on our pilot with 300 individuals, in which zero participants attrited. Moreover, weekly reports from the field suggest that respondents were completing the work and showing up to be paid and surveyed. However, should we find differential attrition we will apply Lee bounds as they are commonly used to correct for attrition in RCTs (Kremer, Miguel and Thornton (2009); Baird, McIntosh and Ozler (2011); Hidrobo et al. (2014); Cunha (2014); Drexler, Fischer and Schoar (2014)). Lee bounds rely on a monotonicity assumption (assignment to treatment can only affect attrition in one direction). While we expect this to be the case (Small Cash and Work will attrit more than Large Cash), we can also use alternative methods such as those outlined in Marcours and Millian (2019).

As with many RCTs which rely on self-reported measures of wellbeing, there is also



the concern that experimenter demand effects will bias our treatment effect estimates upwards. Beyond the inclusion of several revealed-preference outcomes, we are hopeful that this concern is minimized within our experimental design relative to the standard experimental design of an intervention and a pure control; in our case, our primary comparison of interest is between Large Cash and Work. Both groups must engage with enumerators every week, and both are told that they are receiving cash for some service (for the former, the service is answering the survey each week; for the latter, the service is the time sheet activity). While possible, we feel it unlikely that Work participants will feel more indebted to the enumerators than the Cash recipients; we suspect that the converse is more likely, since Cash recipients receive quite a bit of money for very little effort. While still not ideal, this would lead to an underestimate of the psychosocial impacts of the work treatment.

## 5.5 Discussion

Cumulatively, our analyses shed light on the psychosocial impacts of employment and the various mechanisms mediating the relationship we identify. The 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; the upwards of one million Rohingya who have shared the experiences of our sample population is sizable alone. However, the psychosocial costs to forced idleness as well as fears of future uncertainty are echoed globally by forcibly displaced migrants (estimated at 70 million and counting (UNHCR, 2018)), the incarcerated (estimated at more than 10 million (Walmsley, 2015)), and the unemployed (estimated at 172 million (ILO, 2019)). Our explorations of heterogeneity by past violence offer some insight into how this may vary by the nature of past experiences. And our investigation into what features of employment may contribute to improved well-being can inform the design of employment programs to maximize their psychosocial impact well beyond the site of refugee camps.

## 6 Statistical Power

We present power calculations for a subset of the primary outcomes described above: four measures comprising our psychosocial wellbeing index (PHQ-9, Life Satisfaction, Stress, and the Stability Index), one measure of cognitive ability (digit span), and one measure of physical health (severe health problems). All power calculations are based on a hypothesis test with a 5% significance level comparing a sample of 165 individuals within 33 clusters

of five people each (the “Large Cash” group) to a sample of 415 individuals within 83 clusters of five people each (the “Work for Cash” group). Outcome means and standard deviations are calculated using our baseline data. Each table presents power calculations for variously-sized treatment effects and across a range of inter-cluster correlations (ICC). We vary the ICC from 0.01 to 0.2 as baseline ICCs were very low (ranging from 0.000 to 0.06).<sup>22</sup> The estimates presented are conservative given that, as specified above, we will be including baseline values as covariates in all regressions, which should significantly improve the precision of our estimates of interest.

Overall, we are well powered to detect treatment effects of approximately 10%-15% (0.2 - 0.25 standard deviations). These numbers are comparable to those found in the literature for similar indices and outcomes: Haushofer and Shapiro (2016) find a 0.26 standard deviation increase in their primary wellbeing index as a result of treatment.

**Mental Health Outcomes:** Respondents’ PHQ-9 at baseline has a mean of 8.3 and a standard deviation of 4.4, however, for interpretation purposes we will examine standardized effects on PHQ. We present power calculations for treatment effects ranging from 0.15 to 0.3 standard deviations. Our calculations suggest we are well powered to detect a treatment effect between 0.25 to 0.3 standard deviations (12.5% to 15%). For the life satisfaction and stress indices, we are powered to detect between a 10% and 12.5% treatment effect off of a mean of 11.09 (s.d. 4.8) and 9.58 (s.d. 3.8), respectively.

Power Calculation for PHQ

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
0.15 Std Dev	0.38	0.34	0.30	0.24
0.20 Std Dev	0.60	0.54	0.48	0.39
0.25 Std Dev	0.79	0.73	0.66	0.56
0.30 Std Dev	0.91	0.87	0.82	0.71

<sup>22</sup>This may not be surprising due to the fact that the blocks are newly formed communities where individuals were quasi-randomly located after fleeing Myanmar.

Power Calculation for Life Satisfaction

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
10%	0.69	0.63	0.57	0.47
12.5%	0.87	0.82	0.76	0.65
15%	0.96	0.93	0.89	0.80
17.5%	0.99	0.98	0.96	0.91
20%	1.00	1.00	0.99	0.96

Power Calculation for Stress Index

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
7.5%	0.53	0.48	0.42	0.34
10%	0.78	0.72	0.65	0.54
12.5%	0.93	0.89	0.83	0.73
15%	0.98	0.97	0.94	0.87
17.5%	1.00	0.99	0.98	0.95

**Stability:** The Stability Index has a mean of 14.3 and standard deviation of 4.5 in baseline data. This index has the highest ICC of our main outcomes (0.06). Despite this relatively high ICC, we are powered to detect a 10% treatment effect.

Power Calculation for Stability Index - Work Treatment

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
5%	0.41	0.37	0.32	0.26
7.5%	0.74	0.68	0.62	0.51
10%	0.94	0.90	0.85	0.76
12.5%	0.99	0.98	0.96	0.91
15%	1.00	1.00	0.99	0.98

The Stability Index is also a core outcome of interest for the certain vs. uncertain work schedule treatment arms. For this experiment, we compare the 208 respondents in the certain arm to the 207 respondents in the uncertain arm. Randomization of the schedule was done at the individual level within blocks, removing concerns about clustering and the ICC. The table below indicates that we are also powered to detect a 10% increase in the stability index for this comparison.

Power Calculation for Stability Index - Certainty Treatment

	Effect Size				
	5%	7.5%	10%	12.5%	15%
Power	0.32	0.60	0.84	0.96	0.99

**Cognitive Ability:** We present power calculations for the sum of the digit span tests (one forwards, one backwards). The average person was able to remember a total of 6.07 digits with a standard deviation of 1.48. We present power calculations for a treatment effect ranging from a change of 0.2 to 0.6 digits. We are well powered to detect a treatment effect of at least 0.4 digits in this outcome.

Power Calculation for Digit Span

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
0.2 Digits	0.35	0.31	0.27	0.22
0.3 Digits	0.65	0.59	0.52	0.43
0.4 Digits	0.88	0.83	0.77	0.66
0.5 Digits	0.97	0.95	0.92	0.84
0.6 Digits	1.00	0.99	0.98	0.95

**Physical Health:** Our measure of physical health is the probability of being severely ill, or having a health problem last more than seven days in the past month. At baseline, 29.3% of respondents reported being severely ill. We present power calculations for treatment effect sizes ranging from 5 to 15 percentage points. Our calculations suggest that we are powered to detect an effect size of 12.5 percentage points.

Power Calculation for Persistent Health Problem

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
5% points	0.22	0.20	0.18	0.15
7.5% points	0.43	0.38	0.33	0.27
10% points	0.66	0.60	0.53	0.44
12.5% points	0.84	0.79	0.72	0.61
15% points	0.94	0.91	0.87	0.77

**Financial Wellbeing:** We have baseline data for two financial outcomes of interest: current savings and current debt from loans. For these outcomes, we are interested in comparing the Small Cash group to the Large Cash group. As before, all power calculations are based on

a hypothesis test with a 5% significance level. However, we are now comparing a sample of 165 individuals (the “Large Cash” group) in 33 blocks to a sample of 165 individuals within 33 blocks (the “Small Cash” group). The average respondent only has 200 taka of savings (s.d. 600) at baseline, and 2,267 taka of debt (s.d. 3151). The ICC for financial outcomes is quite low: 0.000 for savings and 0.012 for loans. However, for robustness we continue to show power calculations with ICC’s ranging from 0.01 to 0.2. The tables below show that we are powered to detect changes in savings levels of approximately 100%. Given the very low levels of current savings, and the relatively sizeable cash transfers we provide in the Large Cash intervention, we find this to be a reasonable effect size. For loans, we are powered to detect a 40% change in debt levels, which is also plausible given our intervention.

Power Calculation for Savings

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
30%	0.16	0.14	0.13	0.11
40%	0.25	0.22	0.19	0.16
50%	0.36	0.32	0.28	0.23
75%	0.66	0.60	0.54	0.44
100%	0.89	0.84	0.78	0.68

Power Calculation for Loans

Effect Size	Inter-Cluster Correlation			
	<b>0.01</b>	<b>0.05</b>	<b>0.1</b>	<b>0.2</b>
30%	0.59	0.53	0.47	0.39
40%	0.83	0.78	0.71	0.60
50%	0.96	0.93	0.88	0.79
75%	1.00	1.00	1.00	0.99
100%	1.00	1.00	1.00	1.00

## 7 Conclusion

This paper aims to provide a meaningful contribution to the literature by establishing the psychosocial benefits of employment. We design a realistic form of gainful employment and offer the opportunity to a randomized subset of refugees. We are able to disentangle the pecuniary from the non-pecuniary mechanisms behind changes in psychosocial wellbeing by comparing this group with one which receives an equivalent amount of cash alone. The

correlations that are evident from our baseline survey between idleness, unemployment, and psychosocial wellbeing, as well as those with past violence and future uncertainty, offer suggestive and encouraging evidence of the negative impacts of idleness and the potential for employment to alleviate such costs.

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# Tables

Table 1: Balance (All)

	(1)	(2)	(3)	(4)	(5)	(6)
	SmallCash	LargeCash	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
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			

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 Small Cash and Large Cash treatment groups. Column (5) shows the p-value of the difference between the Small Cash and Work treatments, while column (6) shows the p-value between Large Cash and Work.

Table 2: Balance (Certainty)

	(1) Uncertain	(2) Certain	(3) Uncertain vs. Certain
Female	0.32	0.28	0.65
Married	0.75	0.77	0.88
Age	27.64	28.38	0.47
Household size	5.06	5.22	0.86
Formal education	0.50	0.52	0.18
Past Ag. Work	0.69	0.62	0.52
Math ability (index)	2.57	2.59	0.75
Digit Span Score (Total)	6.13	6.14	0.72
Wellbeing (index)	0.12	-0.06	0.43
Life Satisfaction	11.45	10.98	0.86
Self-worth (relative)	14.00	13.91	0.47
Worked in the last month	0.10	0.11	0.91
Worked in Myanmar	0.73	0.78	0.83
Hours Idle (avg)	3.00	3.01	0.90
Idle Feelings	1.68	1.65	0.62
Locus of Control	7.20	8.03	0.09
Power Perceptions	10.91	10.81	0.11
Work Perceptions	3.79	3.73	0.72
Persistent Illness (>7)	0.25	0.31	0.15
PHQ Scale	8.14	8.26	0.86
Mod. Depressed	0.37	0.36	0.48
Stress (index)	9.16	9.82	0.49
Number of conversations	16.23	16.73	0.64
Number of conversations + Family Injuries (Burma)	9.83	10.05	0.70
Family Injuries (Burma)	1.69	1.66	0.98
Observations	207	208	

Columns (1) and (2) show the average value of the variable in the respective treatment arm. Column (3) shows the p-value of the difference in means between the Uncertain and Certain treatment groups.

Table 3: Balance (Violence)

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

Columns (1) and (2) show the average value of the variable for respondents who did and did not have a family member killed 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 4: Outcome Variable Descriptions

<b>Psychological Well-being</b>	
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.
<b>Gender Dynamics</b>	
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).
<b>Financial Wellbeing</b>	
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.

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**Economic Decision Making**

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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).

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**Other Outcomes**

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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?"

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Table 5: Outcome Variable Collection Periods

	Baseline	Midline	Weekly	Endline
<b>Psychological Well-being</b>				
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
<b>Gender Dynamics</b>				
Gender Perceptions - Work	X			X
Gender Perceptions - Violence (IPV)	X			X
<b>Financial Wellbeing</b>				
Savings	X		X*	X
Borrowing	X			X
<b>Economic Decision Making</b>				
Risk Preference		X		X
Time Preference		X		X
<b>Other Outcomes</b>				
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.

# Figures

Figure 1: Experimental Design

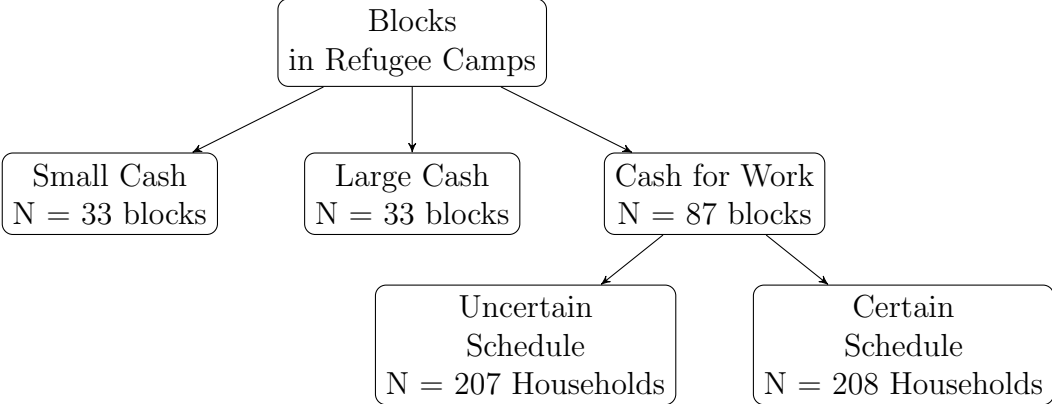


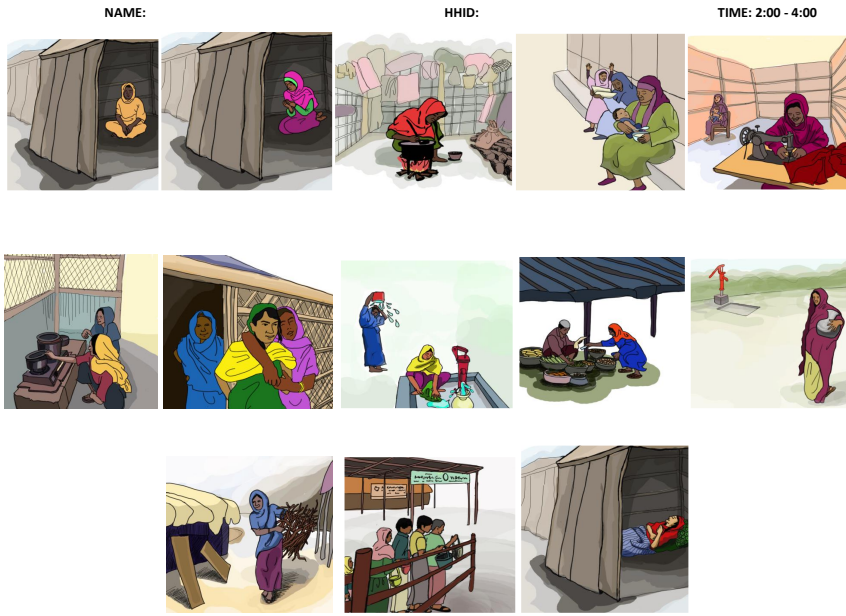
Figure 2: Pre-filled calendar

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 3: Work-Tasks

(a) Female



(b) Male

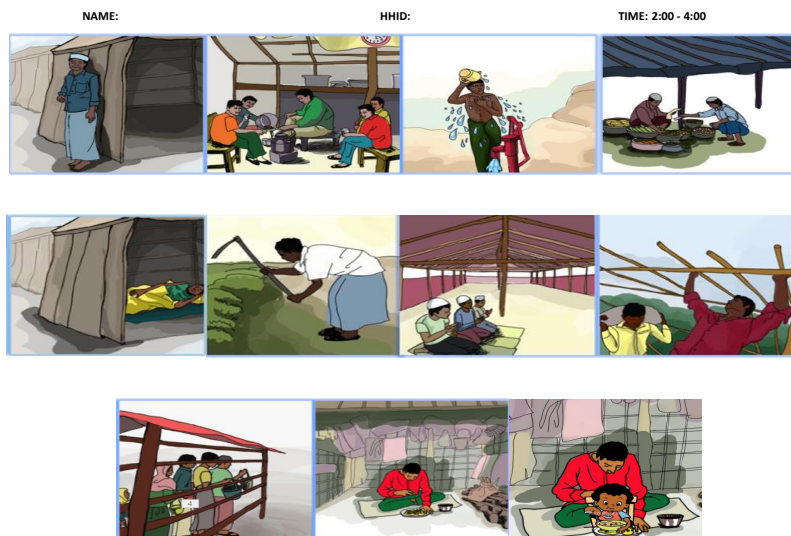


Figure 4: Map of Participant Origins

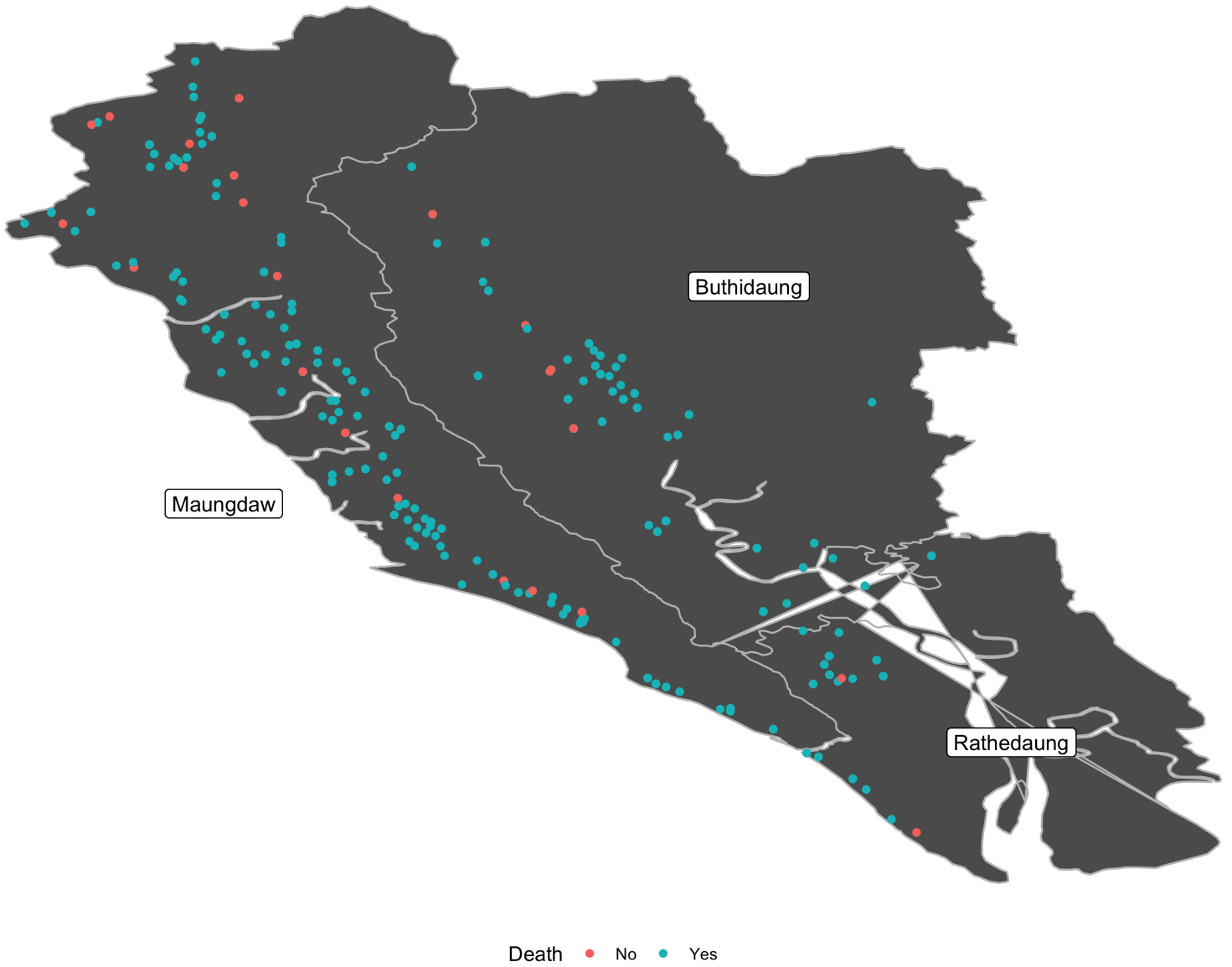


Figure 5: Excerpts from Human Rights Council Report

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.<sup>2037</sup> 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)



Figure 6: Participation Certificate



# A Appendix

## A.1 Tables

Table A1: 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 A2: Mental Health and Idleness

	(1)	(2)	(3)	(4)
	PHQ Scale (Std. Dev.)	PHQ Scale (Std. Dev.)	Mod. Depressed	Mod. Depressed
Hours Idle (avg)	0.039* (0.021)		0.007 (0.010)	
No work in last month		0.343*** (0.118)		0.103* (0.058)
Constant	-0.725*** (0.190)	-0.909*** (0.206)	0.185** (0.093)	0.115 (0.101)
Mean of DV	0.00	0.00	0.38	0.38
Controls	Y	Y	Y	Y
Obs	745	745	745	745

**Notes:** The table shows cross-section relationship between four mental health outcomes and two measures of idleness at baseline. Hours Idle is the self-reported average number of hours spent sitting idle (excluding recreational or other unproductive activities). "No work in last month" is an indicator variable for those who self-reported as having no paid work in the past month. All regressions include controls for respondent age, marital status, household size, education, being a head of household, and gender.

Table A3: Violence and Work

	(1)	(2)	(3)	(4)
	PHQ Scale (Std. Dev.)	Mod. Depressed	PHQ Scale (Std. Dev.)	Mod. Depressed
Any Death	0.353*** (0.109)	0.092* (0.053)	-0.177 (0.292)	-0.198 (0.143)
No work in last month			-0.176 (0.289)	-0.180 (0.142)
No Work X Death			0.603* (0.314)	0.333** (0.154)
Constant	-0.881*** (0.196)	0.134 (0.096)	-0.726** (0.321)	0.293* (0.157)
Mean of DV	0.00	0.38	0.00	0.38
Controls	Y	Y	Y	Y
Obs	745	745	745	745

**Notes:** “No work in last month” is an indicator variable for those who self-reported as having no paid work in the past month. “Any Death” is an indicator variable for the respondent having a family member or friend who was killed in Myanmar. All regressions include controls for respondent age, marital status, household size, education, being a head of household, and gender.

Table A4: Future Orientation and Work

	(1)	(2)	(3)	(4)
	PHQ Scale (Std. Dev.)	Mod. Depressed	PHQ Scale (Std. Dev.)	Mod. Depressed
Think of Future	0.188** (0.073)	0.062* (0.036)	-0.343 (0.216)	-0.090 (0.106)
No work in last month			0.062 (0.160)	0.022 (0.079)
No Work X Future			0.600***	0.172 (0.112)
Constant	-0.702*** (0.181)	0.174** (0.088)	(0.228) -0.770*** (0.229)	(0.112) 0.150 (0.113)
Mean of DV	0.00	0.38	0.00	0.38
Controls	Y	Y	Y	Y
Obs	745	745	745	745

**Notes:** “No work in last month” is an indicator variable for those who self-reported as having no paid work in the past month. “Think of Future” is an indicator variable for respondents who report thinking primarily about the future during time spent idle. All regressions include controls for respondent age, marital status, household size, education, being a head of household, and gender.

## A.2 Secondary Intervention

To better understand which non-pecuniary dimensions of employment are most valued by employees, we offered our small and large cash groups the opportunity to engage in a week’s worth of work after the completion of our endline survey. We designed a bracelet-making task and randomized respondents, by block, into four variations of the work experience: busyness (our control group), agency, sociability, and purpose. In the first group, we informed participants that a supplier from Dhaka had ordered 10,000 beaded bracelets from us. We provided a prototype bracelet that we asked respondents to recreate (with a specific color pattern). We estimated that each bracelet would require five minutes to complete, and respondents were tasked with completing 25 bracelets per day. We asked that participants come to an assigned work space in the block to do their work, but that each person work in silence to avoid distractions and mistakes. We provided each participant with the three bead colors they would need to complete the pattern.

In the second group, we introduced an element of “sociability” to the work by requiring that participants interact with one another to create the bracelets. Each participant was only provided with one bead color and needed to exchange with others to create the required pattern. In the third group, we introduced an element of “agency” by requiring participants to develop their own patterns with all three bead colors, asking them to be as creative as they wished and design beautiful products. In the fourth group, we additionally introduced an element of “purpose” by informing participants that their bracelets would be donated to children in the camps.

Participants completed the work in the facilitator’s home (the site at which they had previously collected cash payments and completed surveys). We provided participants with four bags labeled with their respondent ID and instructed them to place their completed work into one of the bags at the end of each day. Like the time-use sheets, they then dropped this bag in a tamper-proof box in the facilitator’s home, and the facilitator separated each day’s work with a piece of cardboard. This ensured that the employment activity was of meaningful length not only within the day but over the course of the week.

To test whether different features of the work affect participants’ willingness to work, we run the following regression:

$$Y_{ibc} = \beta_0 + \beta_1 \text{Sociability} + \beta_2 \text{Purpose} + \beta_3 \text{Agency} + \zeta_b + \delta_e + X_{ibc} + \varepsilon_{ib}$$

In which  $Y_{ibc}$  represents (1) the wage at which the respondent is willing to work; (2) the number of days worked; and (3) the number of bracelets completed correctly.  $\gamma_c$  represents block-level fixed effects and  $\delta_e$ ,  $X_{ibc}$  and  $\varepsilon_{ibc}$  are as defined above. If provision of a calendar for

the two months of employment indeed has an impact on psychosocial and financial decision-making measures, we expect to find that  $\beta_1 \neq 0$ . If increased certainty is mediating this effect, we anticipate  $\beta_1 \geq 0$  for the stability and financial outlook outcomes. However, the effects on risk and time preferences remain directionally ambiguous.

### 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 (Small cash, 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 (Big 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?

**Disclosure Statement: Reshmaan Hussam**

Reshmaan Hussam declares that she has no relevant or material financial interests that relate to the research described in the paper *The Psychosocial Impacts of Forced Idleness*.

**Disclosure Statement: Gregory Lane**

Gregory Lane declares that she has no relevant or material financial interests that relate to the research described in the paper *The Psychosocial Impacts of Forced Idleness*.

**Disclosure Statement: Erin Kelley**

Erin Kelley declares that she has no relevant or material financial interests that relate to the research described in the paper *The Psychosocial Impacts of Forced Idleness*.

**Disclosure Statement: Fatima Zahra**

Fatima Zahra declares that she has no relevant or material financial interests that relate to the research described in the paper *The Psychosocial Impacts of Forced Idleness*.