

Improving Women’s Mental Health during a Pandemic[†]

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This paper evaluates a randomized, over-the-phone counseling intervention aimed at mitigating the mental health impact of COVID-19 on a sample of 2,402 women across 357 villages in Bangladesh. We find that the provision of two hours of mental support plus information on COVID-19 improves mental health ten months postintervention, leading to reductions of 20 percent in the prevalence of moderate and severe stress and 33 percent in depression. Our results suggest that this type of low-cost intervention (\$14 per person) can be effective in providing rapid psychological support to vulnerable groups in times of crises. (JEL I12, I18, I31, J16, O12, O15)

Large-scale shocks caused by events such as epidemics, natural disasters (e.g., floods, earthquakes, etc.), conflicts, and organized violence can have far-reaching consequences for people witnessing these events, confronting them with a variety of health, social, and economic threats and anxieties. These impacts are particularly severe in low- and lower-middle-income countries (LMICs), as mitigation measures in such contexts are limited due to lack of resources and inadequate state support. Consequently, people are vulnerable to widespread and acute psychological distress, especially the most vulnerable groups among them that live in poverty (Ridley et al. 2020).

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[†]Go to <https://doi.org/10.1257/app.20210655> to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

A recent example is the COVID-19 pandemic, which, since its onset in 2020, has caused loss of life at a devastating scale and dramatic disruption to economic activity, raising concerns about the mental health deterioration for those falling into poverty. The adverse impact on mental health is exacerbated by isolation and loneliness due to the necessary social distancing measures and stay-at-home orders. For LMICs, the public health and economic impact has been particularly devastating due to the vulnerability of the population and the lack of strong safety nets (Egger et al. 2021; Miguel and Mobarak 2022). The United Nations warned of a looming global mental health crisis (United Nations 2020b), while experts highlighted the need to urgently and systematically address the mental health consequences for vulnerable groups (Holmes et al. 2020; Galea, Merchant, and Lurie 2020). Psychosocial support interventions have been shown to be effective in addressing mental health problems in low-income settings (Patel et al. 2016). However, in-person counseling was not possible during the pandemic, and using videoconferencing technology is not feasible in low-income settings, whereas a telephone-delivered approach would be a more viable and preferable method in these conditions (Brenes, Ingram, and Danhauer 2011).

This paper evaluates a randomized telecounseling intervention aimed at mitigating the mental health impact of COVID-19 on a sample of 2,402 women across 357 villages in Bangladesh. Our intervention consists of four brief mental health counseling sessions that took place remotely over the phone, roughly every two weeks, starting in mid-July 2020. Widespread use of mobile phones in Bangladesh makes this type of intervention possible.¹ The sessions, which lasted for about 25 minutes each, for a total dosage of about 2 hours, were delivered by locally recruited and trained female para-counselors.² The intervention was designed to be multifaceted: it combines informational value and emotional support in a context where no other mental health support is available. The first session aimed to raise awareness about COVID-19, its symptoms, and the recommended preventive measures. The rest of the sessions covered different aspects of the pandemic's impact on women's physical and emotional well-being and ways to cope with stress and anxiety, following the COVID-19 mental health and psychosocial support guidelines prescribed by the International Federation of Red Cross (2020) and the World Health Organization (2020). The relatively short lengths of the whole intervention and of the individual sessions were chosen taking into consideration that participants already face a higher daily burden of household obligations due to the lockdown, and, therefore, the intervention needed to not require a substantial time commitment.

The economic disruption of the pandemic has had a very negative impact on the livelihoods of people in Bangladesh, with sharp losses in jobs and income (Genoni et al. 2020; Beam et al. 2021) and a majority of rural households reporting to be threatened by food insecurity (Ahmed et al. 2021). We focus on women in a rural setting because they are affected disproportionately by the social and economic impacts of the pandemic, being in a more disadvantageous socioeconomic

¹In the region where our study takes place, roughly 95 percent of the households own at least 1 cellphone (94 percent in rural and 96 percent in urban areas) (BDHS 2017).

²Due to workforce shortages, lay health workers (health workers with no professional certificates or training) have been mobilized to deliver mental health services and have shown to do so effectively in many low-resource settings (Barnett, Lau, and Miranda 2018).

position than men, experiencing a heavier burden of household chores and unpaid care responsibilities, and having an increased risk of being victims of domestic violence. Women in this setting have less decision-making power within the household and tend to take on a greater share of the increased household workload associated with lockdowns and school closures, such as caring for children and the elderly (United Nations 2020a; Giurge, Whillans, and Yemiscigil 2021). There have also been reports and evidence of heightened gender-based domestic violence during the pandemic (United Nations 2020a; Peterman, O'Donnell, and Palermo 2020; Ravindran and Shah 2020). Thus, women bear the brunt of the economic and social consequences of the pandemic, making them a highly vulnerable group whose mental health could, as a result, be suffering disproportionately (Afridi, Dhillon, and Roy 2021; Bau et al. 2022).

In a baseline survey that took place in May 2020, we collected a rich set of data on demographics, knowledge about COVID-19 and compliance with precautionary measures, and the mental health and well-being of participating women. Our main outcomes, perceived stress and depression, were collected in a first endline survey in late 2020 (one month after the end of the intervention) and in a second endline ten months later (or 17 months after the baseline) when the country was under another strict lockdown. We also collected measures of subjective well-being: happiness, life satisfaction, and future aspirations. Finally, we collected information on compliance with COVID-19 measures. All of these outcomes were specified at the AEA RCT Registry (ref no. AEARCTR-0005948).

To assess the broader impacts of the intervention, we also collected a set of additional outcomes that we had not prespecified. First, we investigate whether the provision of emotional support and pandemic-coping information enables women to better provide for their family and support their children.³ We, thus, also collected a measure of food security that captures an important dimension of the perceived economic standing of the households in this setting during pandemic conditions. Additionally, we collected information on investment in parenting activities, some COVID-19-related measures (self-confidence about managing the health crisis and vaccination take-up), gender empowerment, attitudes toward gender norms and intimate partner violence (IPV), and economic preferences (risk, social, and time).

We find that the intervention was very effective in raising mental well-being: one month after the intervention ended, stress levels of the treatment group decreased by 0.70 standard deviations (SD) and depression severity by 0.65 SD relative to the control group. These effects persisted ten months after the intervention ended when we find that stress levels and depression severity in the treatment group were a respective 0.55 SD and 0.51 SD lower than in the control group. These impacts translate into a reduction of 19.5 percentage points in the prevalence of moderate or severe stress and 19.1 percentage points in the prevalence of depression compared to the control group in which 95.7 percent of participants were moderately or severely stressed and 58.3 percent were depressed. In other words, moderate and severe

³An emerging literature in economics examines the two-way causal links between mental health and economic outcomes (Currie and Stabile 2006; Adhvaryu, Fenske, and Nyshadham 2019; Baranov et al. 2020; Haushofer, Mudida, and Shapiro 2020; Ridley et al. 2020; Angelucci and Bennett 2021).

stress fell by 20.4 percent and depression by 32.8 percent ten months posttreatment. Our estimated effect sizes are large when compared to those found in studies that use psychotherapy to improve psychological well-being (Cuijpers et al. 2010, 2013) considering that there are notable differences between our intervention and previous interventions: earlier interventions typically were longer, delivered by mental health professionals, and focused only on people suffering from depression, whereas our intervention was short, delivered by para-counselors, targeted women with varying levels of baseline mental distress, and took place in a context of deteriorating mental health due to the pandemic conditions.

We also observe significant advances in a range of other measures of well-being (happiness, life satisfaction, and future aspirations). Beyond the mental health domain, we find that the intervention enhanced preventive health behavior related to COVID-19 and vaccination take-up. These latter impacts can be plausibly attributed to the informational value of the intervention.

When exploring the mechanisms behind the sustained impact of the intervention on mental health, we find that women continued to make use of the mental health practices that were introduced in the counseling sessions. It is likely that this practice contributed to their improved mental health outcomes after the intervention ended, relative to women in the control group.

We also carry out a heterogeneity analysis with respect to our baseline measure of stress to assess whether participants that had worse mental well-being to start with benefited more from the intervention. We find that women with poorer mental health benefit more in the short term, but the difference in the strength of the treatment effects relative to those with better starting mental health fades out at the ten-month endline.

Besides the mental health gains, the intervention also led to significant impacts on the additional outcomes. In particular, we observe improvements in perceived food security: household-level food insecurity fell by 0.28 SD in the one-month postintervention endline and 0.52 SD ten months postintervention. This effect translates into a 22.1 percent reduction in the prevalence of food insecurity. There is also an impact on parental behavior, as treated women report spending more time helping children with their education: an increase of 0.19 SD in the ten-month endline. These results, consistent with previous evidence of a connection between mental health and poverty (Ridley et al. 2020), suggest that for these women, better mental health is an important step toward redressing household food shortages and gaps in children's learning. We also find that the intervention had an impact on other dimensions: gender empowerment and attitudes toward gender norms and IPV. This suggests that, through the intervention, treated women made advances in outlook and developed wider beliefs about their position within the household and the society at large. Finally, we find some impacts of the intervention on economic preferences: treated women report being more willing to take financial risks and are more altruistic.

This study is related to an emerging literature on the mental health impact of COVID-19.⁴ Most of the existing literature documents the negative impact of COVID-19 on mental health in high-income countries, such as the United States

⁴It is also connected to a broader literature on the mental health impact of quarantines imposed during epidemics (Brooks et al. 2020) and the mental health consequences of disasters (Neria, Nandi, and Galea 2008).

(Adams-Prassl et al. 2022; Fetzner et al. 2021; Giuntella et al. 2021), the United Kingdom (Pierce et al. 2020), Germany (Armbruster and Klotzbücher 2020), or upper-middle-income countries (Altindag, Erten and Keskin 2022), while the evidence from LMCI is rather scarce (Afridi et al. 2021; Bau et al. 2022). We contribute to this literature by not only offering evidence on the extent of the mental health impact of COVID-19 in a low-income setting in a developing country but also by evaluating the effectiveness of a novel, low-cost intervention aimed at helping vulnerable people cope with the adverse mental health impact of the pandemic. To the best of our knowledge, this is the first study to provide rapid causal evidence of the immediate (at one month) and short-term (at ten months) effectiveness of a mental health intervention fielded in the midst of the COVID-19 crisis. Our study thus responds to the urgent call made by mental health researchers for evidence on effective approaches to address the mental health consequences of the pandemic conditions on vulnerable groups (Holmes et al. 2020).

Our paper also contributes to a broader literature that examines psychological interventions among vulnerable populations in low-income settings. Brief and low-cost psychological interventions have been shown to have moderate to strong effects in ameliorating common mental health problems, such as depression, anxiety, and posttraumatic stress (Singla et al. 2017). For instance, mental health counseling interventions of short lengths—as short as four to six weeks in Zimbabwe (Chibanda et al. 2015)—and of a small number of sessions—as many as five sessions over seven months with women participants in Pakistan (Rahman et al. 2019)—have been found to be effective in improving the mental health of participants. In other cases, however, no improvement in mental well-being has been found in relation to a positive psychology intervention (Baranov, Haushofer, and Jang 2020) and a five-week psychotherapy program (Haushofer, Mudida and, Shapiro 2020) in Kenya.

Experts emphasized the need for telemental health services before the onset of the pandemic as a complementary approach to the traditional in-person treatments of mental health illnesses (Brenes, Ingram, and Danhauer 2011; Patel et al. 2016). A meta-analysis of 66 RCTs that delivered mental support through smartphone applications found that app-supported interventions are effective in improving depressive and anxiety symptoms as well as stress levels of participants (Linardon et al. 2019). Under pandemic conditions, such services emerged as a particularly suitable medium to address mental health problems, as they enable reaching patients remotely without the risk of infecting them or the mental health service providers (Zhou et al. 2020; Kola 2020). In this study, we provide evidence that telephone-delivered psychosocial support can be a light-touch and low-cost solution (the cost of our intervention amounts to \$14 per person) in times of crisis, which is an important consideration in the context of low-income countries that lack adequate resources and infrastructure to deliver mental health care face to face.

The rest of the paper is organized as follows: the next section provides some background on COVID-19 in Bangladesh. Section II lays out the research design of the study, while Section III introduces the data, hypotheses, and empirical method. Section IV presents our main results, Section V contains a discussion of potential mechanisms, and Section VI reviews the cost effectiveness of the intervention. Finally, Section VII offers some concluding remarks.

I. Background

The first confirmed case of COVID-19 in Bangladesh was reported on March 8, 2020 and the first death on March 18, 2020. To contain the quick spread of the virus, the government of Bangladesh announced a countrywide lockdown from March 26, which was extended several times until May 30, 2020. Then on April 5, 2021, a second countrywide lockdown was implemented until August 11, 2021. As of October 2021, Bangladesh had recorded over 1.5 million confirmed cases of COVID-19 and 27,000 deaths.⁵

COVID-19 has caused economic growth to stall in Bangladesh due to a combination of a decline in both domestic economic activity and exports (IMF 2021). Like in many other countries, the government of Bangladesh responded to the unfolding economic crisis by announcing a stimulus package that mainly includes subsidized loans to companies to support employment. It also entails a number of other measures aimed at relieving the poor and marginalized groups, including the informal workers that constitute a large share of employed workers in the country.

The pandemic has also disproportionately affected the well-being of women and exacerbated the existing gender inequality problem (United Nations 2020a). In Bangladesh, 92 percent of the total employment of women is in the informal sector (e.g., domestic workers, owners, workers in microenterprises, daily laborers, street vendors, cleaners, etc.), and those in the formal sector work primarily in the Ready-Made Garment sector—both sectors were hit the hardest by the pandemic (United Nations 2020a). United Nations (2020a) also reports that 49 percent of women reported feeling unsafe during lockdowns, 43 percent of rural women are unaware of basic health facilities at local clinics, and most women rely on information from their husbands or other family members, which puts them in more disadvantageous position than men during health emergencies. Moreover, schools remained closed for 18 consecutive months. As a consequence, unpaid and domestic care by women increased by 33 percent immediately after the initial lockdown was implemented.

II. Research Design

A. *The Telecounseling Intervention*

We delivered a psychosocial support intervention to a sample of adult women living in rural areas of southwestern Bangladesh in collaboration with a local research-focused NGO, Global Development and Research Initiative (GDRI). Specifically, we designed and offered telecounseling sessions to these women (hereinafter counselees) roughly every two weeks, with each session running for about 25 minutes (a detailed timeline of the intervention is described below). The total dosage of the intervention was about two hours. The telecounseling sessions were delivered by a team of 18 trained female para-counselors who were recent graduates in either psychology, public health, or

⁵For reference, note that as of October 2021 Bangladesh had 166 confirmed COVID-19-related cases per million people, whereas India had 322 and Pakistan 124.

social sciences from public universities in Bangladesh without any significant prior real-world counseling experience. They were locally recruited, and, thus, had a good understanding of women's lives in general in the particular context.

The selection of para-counselors was carried out by two experts in public health (Tabassum Rahman, a public health expert, and one of the coauthors of this study), one expert in psychology, and a GDRI executive. Following recruitment, para-counselors were trained (via video conferencing) by Tabassum Rahman and a psychologist.⁶

To deliver the counseling sessions, the para-counselors contacted counsees a week before every session to make an appointment. Counseling appointments were made at a time convenient for the participants to avoid adding to their daily burden. They then spoke to counsees during the designated day and time over the phone (more details on the logistics of the mobile phone survey are provided in online Appendix B.7). In total, we ran four sessions with each participant, covering different aspects of COVID-19's impact on their physical and emotional well-being and ways to tackle it. Our intervention did not identify participants as mental health patients; instead, it was educational, focusing on recognizing the difficulties the participants might have been experiencing and the emotions those experiences led to, and empowering the participants with nonpharmacological ways of managing such emotions.

For the counseling sessions, we developed four modules that catered to the psychosocial needs of our participants during the lockdown period, each one aiming to improve specific aspects of their overall well-being. In developing and tailoring the modules to fit the pandemic context, we also closely followed the COVID-19 mental health and psychosocial support guidelines assembled by the International Federation of Red Cross (2020), the World Health Organization (2020), and Brooks et al. (2020) to identify relevant major elements. These guidelines emphasize the information and activities that could mitigate distress and worry during the pandemic, the importance of showing care and empathy to the vulnerable, and the "dos-and-dont's" for para-counselors while offering support to these people.⁷ The aim of incorporating the above elements is to help normalize various negative emotions and promote feelings of safety, calmness, and hope among the distressed.

In particular, the modules integrate the following four domains of processes that contribute to better mental well-being (Singla et al. 2017):

- (i) Behavioral: problem-solving, behavioral activation, relaxation, and exposure;
- (ii) Interpersonal: identifying/eliciting support and communication skills;
- (iii) Emotional: linking affect to events and emotional regulation and processing;

⁶Training included four steps: general training of all para-counselors in group, one-on-one mock telecounseling with a trainer, pilot with a trainer, and feedback on the pilot telecounseling. Training lasted for nine days. Table A1 in online Appendix A highlights the main telecounseling preparations and the associated preparation time.

⁷A version of the guidelines was also used to provide psychosocial support to people in West Africa during the Ebola outbreak. See World Health Organization (2014).

(iv) Cognitive: identifying thoughts, insight building, distraction, and mindfulness;

More concretely, the four modules cover the following main areas: (i) *Awareness*—raising awareness of COVID-19 and its symptoms and the preventive measures to address the fear of infection (involves *behavioral*, *interpersonal*, and *cognitive* processes); (ii) *Coping with stress*—taking care of emotional well-being to cope with stress (involves all four processes); (iii) *Self and childcare*—taking care of physical health of self and child to address health-related anxiety (involves *behavioral* and *interpersonal* processes); and (iv) *Communication*—helping each other and staying connected to cope with isolation (involves all four processes). Figure A1 in online Appendix A summarizes the association between our counseling modules and the four psychological domains.

To summarize, the telecounseling sessions provided both informational value and emotional support, leveraging established methods that help boost mental well-being. The four modules (translated from *Bangla*) are described in detail in online Appendix C (in chronological order), and the exact session modules are available in online Appendix D.

B. Sampling and Randomization

We carried out a randomized controlled trial to evaluate the effectiveness of this intervention. To select our study sample from a list of households previously surveyed by GDRI, we first narrowed it down to households that meet the following criteria: (i) the household has a mobile phone number according to GDRI records, (ii) the phone number is valid, and (iii) the household has at least one adult (18 or above) female household member.⁸ From this list, we randomly selected 2,647 households and eventually enrolled 2,402 eligible women, one from each household, to the telecounseling program. These households are distributed across 357 villages (in 50 union councils—the smallest rural administrative unit in Bangladesh) in the Khulna and Satkhira districts in Bangladesh, roughly 7 households per village.⁹

We compare household characteristics of our sample to that of a rural Bangladeshi sample that has access to mobile phones using the 2016 Bangladesh Household Income and Expenditure Survey (HIES) (Bangladesh Bureau of Statistics 2016). We report this in Table A3 in online Appendix A. Households in our sample appear to be more educated and less likely to work in agriculture relative to the HIES sample but are fairly similar in other characteristics, such as income, household size, and age.

Following enrollment, we randomly assigned women to either the telecounseling treatment arm or to the control arm, in which no counseling is provided to women. Thus, our randomization is at the individual level and ensures that we have both

⁸Our partner NGO, a nonprofit research organization, has a directory of households who, in the past ten years, participated in surveys and RCTs conducted by the NGO in this region. We randomly selected our households from this directory, which is not by design representative of rural households in the region.

⁹Out of 2,647 households, 114 households could not be reached over the phone (they either never answered the phone or their phones were found to be turned off). The remaining 2,533 were invited, and roughly 95 percent of women accepted our invitations and were enrolled in the program (we call them 'takers'). In Table A2 in online Appendix A, we compare the characteristics of takers and nontakers (women that did not accept our invitation) and find them to be very similar. All women in our sample are married.

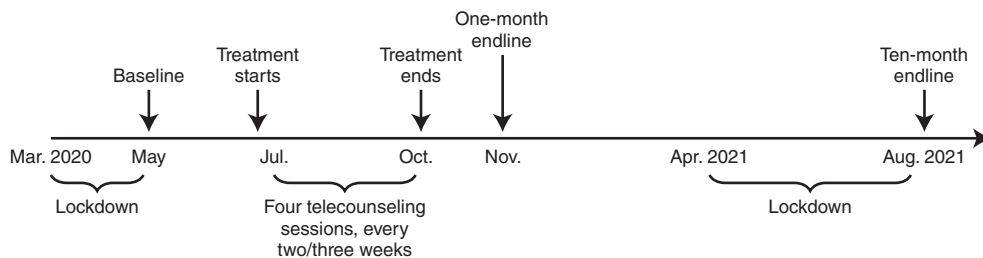


FIGURE 1. INTERVENTION TIMELINE

treatment and control households within each village and also have an equal proportion of households in each treatment arm. However, on some occasions, there were villages that either had one enrolled participant or an odd number of enrolled participants, which resulted in some villages having either only treatment or only control participants and some villages with uneven distribution of treatment and control participants.¹⁰ Eventually, 1,299 women were assigned to the treatment and 1,103 to the control arm. Given the large number of households in each village (more than 500 households on average), the possibility of contamination is very low. Figure A2 in online Appendix A shows a map of the study area with the geographic distribution of the villages in our study.

C. Timeline

The intervention started in mid-July 2020 and ended in mid-October 2020.¹¹ The baseline data for this study was collected between the end of May and mid-June 2020, while the one-month endline data was collected in November 2020 and the ten-month endline in August 2021. Figure 1 highlights the major milestones of this project. We discuss data collection in more detail in Section III.

III. Data, Hypotheses, and Empirical Methods

Between the end of May and the middle of June 2020, GDRI (the local NGO we collaborated with) surveyed the enrolled women over the phone to understand their physical and emotional state during the pandemic. Through this survey, trained enumerators (a different set of individuals from the para-counselors who carried out the intervention) gathered baseline information on some household demographics, socio-economic characteristics, food insecurity, participants' knowledge and perception of COVID-19, how often they comply with COVID-19 health guidelines, their worries

¹⁰For this reason, we cannot use village fixed effects as indicated in the registered preanalysis plan. Instead, we use union council fixed effects (the smallest rural administrative unit) in our regression analysis.

¹¹Due to the *Eid-al-Adha* religious holiday and the continuous heavy rains as part of the monsoon season in the study regions, the scheduling of the counseling sessions was considerably affected. For instance, household chores increase drastically during the monsoon season, so conducting counseling sessions was difficult during heavy rains. Also, mobile phone network disruption and power cuts are very common in rural areas during the monsoon season. Therefore, instead of early September (as mentioned in the preregistration), our intervention ended in mid-October 2020.

and fears, health and well-being, and their stress levels (Vlassopoulos et al. forthcoming).¹² Each telephone interview lasted roughly 30 minutes.

During the first endline that took place in November 2020, we measured the following outcomes that were preregistered at the AEA RCT registry (*prespecified outcomes* hereinafter): mental health outcomes, such as perceived stress and depression; subjective well-being outcomes, such as happiness, life satisfaction, and future aspirations; and, people's compliance with COVID-19 precautionary measures. In addition, we also measured the following outcomes that were not preregistered (*additional outcomes* hereinafter): household-level food insecurity; time-intensive parental investments on children; self-confidence about tackling COVID-19-related emergencies; and various gender attitudes, such as an index of women's empowerment, attitudes toward gender norms, and attitudes toward IPV.

During the second endline in August 2021, we again measured the (i) stress and depression of our participants (*prespecified outcomes*) and (ii) household-level food insecurity, time-intensive parental investments on children, economic preferences such as risk, social, and time preferences, and the COVID-19 vaccination status of self or any family member (*additional outcomes*).

We break down this section into five parts. First, we present our primary and secondary hypotheses (Section IIIA); second, we define our outcome variables in detail and how they are constructed for the empirical analysis (Section IIIB); third, we present some summary statistics and the balance between our treatment and control groups at baseline (Section IIIC); fourth, we set forth our empirical methods (Section IIID); and, fifth, we briefly discuss attrition at endline and how we address it (Section IIIE).

A. Hypotheses

Prespecified Outcomes.—We expect that the intervention—that provides informational content and emotional support—will lead to an improvement in women's mental health (measured using perceived stress and depression), which is the main aim of the counseling intervention. In the context of the pandemic, information provision can be an important stress reliever, as evidenced by recent studies based on India showing that offering accurate information about COVID-19 over the phone can reduce stress and depression (Islam et al. 2021; Sadish, Adhvaryu, and Nyshadham 2021). We also expect to see improvements in other domains of well-being such as happiness, life satisfaction, and future aspirations. Furthermore, our intervention provides valuable information about COVID-19; thus, we also expect our intervention to increase participants' compliance with COVID-19 precautionary measures immediately after the intervention.¹³

¹² Some of the major symptoms of depression (according to the American Psychiatric Association 2013), such as feeling worthless, hopeless, anxious, and lonely, were collected during the baseline.

¹³ We also preregistered *Physical health* of the respondents, children, and other household members (measured using questions on the prevalence of common COVID-19 symptoms) as a health outcome but dropped it at endline because all respondents and household members did not report any symptoms at baseline.

Additional Outcomes.—Because mental health and poverty can reinforce each other (Lund et al. 2011; Haushofer and Fehr 2014; Ridley et al. 2020) and the provision of information related to COVID-19 can help participants mitigate the pandemic shock, we are also interested in assessing whether the intervention impacts the food security of the household. In addition, we might expect improvements in mental health due to the intervention to have spillover effects on other outcomes that have been linked to mental health, such as parental behavior, gender empowerment, attitudes toward gender norms and IPV (e.g., Baranov et al. 2020), and economic preferences (Cobb-Clark, Dahmann, and Kettlewell 2020; Ridley et al. 2020). Finally, because our intervention provides information about COVID-19, we expect that it might raise self-confidence in tackling COVID-19-related issues and vaccination take-up.

B. Data

Out of the 16 outcome variables, 10 outcomes are indices constructed by aggregating responses to several individual questions from the survey; 5 outcome variables—*happiness*, *life satisfaction*, and the three measures of *economic preferences*—were constructed using response scales to single questions from the survey; and *vaccination* was measured using a binary response. All outcome variables have been control group standardized following Kling, Liebman, and Katz (2007) so that each variable has mean 0 and SD 1 for the control group. Specific survey questions used and the procedure for index constructions are discussed in more detail in online Appendix B.1.

Prespecified Outcomes.—We define our mental health outcomes, perceived stress and depression, as follows:

Stress: This measures the degree to which respondents find their lives to be unpredictable, uncontrollable, and overwhelming based on experiences from the preceding week. In addition, it also measures a few current levels of experienced stress, such as feelings of being nervous, upset, and angry. To measure respondents' perceived stress levels, we used the Perceived Stress Scale (PSS) (Cohen, Kamarck, and Mermelstein 1983; Cohen, Kessler, and Gordon 1997), which is one of the most widely used psychological instruments for measuring people's perception of stress. This tool is also clinically validated and widely used by various reputable medical services and is considered "easy to use" and "superior" to other available tools (Lee 2012). PSS consists of ten items, where each item is answered on a five-point scale (score between 0 and 4), and adding up scores from each item gives the total PSS score (between 0 and 40). Thus, a higher PSS score corresponds to higher perceived stress among respondents. Similarly, we use the standard score cutoff suggested by Cohen, Kamarck, and Mermelstein (1983) and Cohen, Kessler, and Gordon (1997) to create a binary outcome: equals 1 if PSS score is greater than 13 and 0 otherwise. We use the term "stressed" to describe participants that are above this cutoff. PSS questions and response scales are explained in detail in online Appendix B.2.

Depression Severity: This measures the degree to which respondents experienced major depressive symptoms, such as feelings of sadness, hopelessness, loneliness, loss of interest and concentration, sleep deprivation, etc., in the preceding week. To identify current depressive symptoms among respondents, we used the ten-item version of the Center for Epidemiologic Studies Depression Scale (CES-D-10) (Andresen et al. 1994). CES-D-10 is a screening tool for identifying major or clinical depression among adults and adolescents (but not ideal for diagnosis) and is widely used. In contrast with the PSS response scales, each item in CES-D-10 is answered on a four-point scale (score between 0 and 3). Thus, adding up scores from each item gives the total depression score (between 0 and 30), where a higher value corresponds to higher depressive symptoms. Similarly, we use the standard score cutoff to create a binary outcome: equals 1 if the depression score is greater than 10 and 0 otherwise. We use the term “depressed” to describe participants that are above this cutoff. CES-D-10 questions and response scales are explained in detail in online Appendix B.2.

In online Appendix B.3 we define our secondary prespecified outcomes: happiness, life satisfaction, future aspirations, and compliance with COVID-19 rules. Here, only compliance with COVID-19 rules can be considered objective because respondents were asked about something factual (e.g., how often they go outside, wash hands, etc.) rather than a preference or subjective view.

Additional Outcomes.—Next, we define the main additional outcomes in the following way:

Food Insecurity: We measure respondents’ perception about food insecurity in their households by using the Food Insecurity Experience Scale (FIES) (Ballard, Kepple and Cafiero 2013). FIES consists of eight items that capture one’s perception about food security, ranging from the perception of being food secure to mild, moderate, or severely food insecure. Thus, it captures situations ranging from having anxiety related to lack of food to severe situations like spending a whole day without any food. The FIES score ranges from 0 to 8. Thus, a higher FIES score is related to higher anxiety about household-level food insecurity. FIES questions and response scales are explained in detail in online Appendix B.4. Since this outcome measures perception rather than actual food consumption among respondents, we consider this a rather subjective measure of food insecurity.

Time-Intensive Parental Investments: This outcome captures how frequently respondents spent time with their children to help out with their studies and play. These two questions have been modified from Strayhorn and Weidman (1988), and each is answered on a 5-point scale (score between 0 and 4). Thus, higher cumulative scores correspond to more parental investments. Although all women in our sample are married, questions on time-intensive parental investments were only applicable to 1,790 women with young (under five) or primary school-attending children. Parental investment questions and response scales are explained in detail in online Appendix B.4. Since this outcome measures something factual, we consider this an objective measure of parental time input.

Finally, we define the following families of secondary additional outcomes in online Appendix B.4: confidence in tackling COVID-19 emergencies, *gender attitude* outcomes, *economic preferences*, and COVID-19 vaccination take-up. Note that of these secondary outcomes, only vaccination take-up is an objective outcome referring to something factual, whereas the remaining are subjective or refer to preferences.

Other Variables.—At baseline, we also collected data on respondents' own and their household characteristics, such as being household head, how worried and scared they are about the pandemic, how their household chores increased during the pandemic, and household head's occupation. We also use previous survey data on respondents' age, years of schooling, number of children under five, household income, household size, and husband's age and education. We define these baseline variables in detail in online Appendix B.5. At the ten-month endline, we also measured women's tendency to give socially desirable responses to survey questions following Bandiera et al. (2020). We define this variable in online Appendix B.6.

C. Balance Check and Summary Statistics

We next provide summary statistics for the variables collected in the baseline survey and check whether randomization has produced balanced treatment and control groups in terms of the baseline characteristics and outcomes.

Table 1 provides summary statistics of respondents' individual and household characteristics. The table presents the mean value for the whole sample and by treatment status and also reports the results of balance tests, which we obtain by estimating OLS regressions with the variable of interest as the dependent variable and the treatment indicator as an independent variable with union council fixed effects and standard errors clustered at the village level. Overall, these balance tests indicate that the sample is balanced. An F -test of joint significance yields a p -value of 0.225.

Table 1 shows that the average participant in our study is 35 years old and has 8 years of education. The majority of respondents have experienced income loss, with almost 60 percent experiencing complete income loss, indicating that indeed the women in our sample are experiencing the adverse economic impact of COVID-19. One possible explanation for why households are experiencing complete income loss is that household heads' primary occupation is in the informal sector (for 60 percent of households), which was severely affected by the countrywide lockdown in March 2020.

In terms of measures of mental health at the baseline, we see in Figure A3 in online Appendix A that a striking 83 percent of respondents are stressed—defined as having a PSS score that is more than 13 out of 40 (Cohen, Kamarck, and Mermelstein 1983; Cohen, Kessler, and Gordon 1997). In addition, most of the women are moderately stressed (roughly 80 percent, where $14 \leq PSS_{\text{moderate}} \leq 26$), with very few cases of severe stress ($27 \leq PSS_{\text{severe}} \leq 40$). This evidence suggests that COVID-19 is having a substantially negative impact on the well-being of this sample of women in rural Bangladesh.

TABLE 1—BASELINE CHARACTERISTICS AND BALANCE

Variables	Pooled (SD)	Control (SD)	Treatment (SD)	<i>t</i> -test <i>p</i> -values
<i>Panel A. Individual characteristics</i>				
Age of respondent	35.51 (9.44)	35.73 (9.37)	35.32 (9.49)	0.203
Education of respondent	8.39 (2.67)	8.32 (2.59)	8.44 (2.73)	0.207
Household chores increased*	0.26 (0.44)	0.26 (0.44)	0.26 (0.44)	0.412
Someone helps with household chores*	0.49 (0.50)	0.50 (0.50)	0.48 (0.50)	0.522
Trusts neighbors*	0.22 (0.41)	0.23 (0.42)	0.21 (0.41)	0.189
COVID-19 perceptions	0.63 (0.16)	0.63 (0.16)	0.63 (0.17)	0.807
Worried index ($0 \leq \text{Index scale} \leq 4$)	2.25 (1.12)	2.25 (1.14)	2.24 (1.11)	0.854
Afraid index ($0 \leq \text{Index scale} \leq 13$)	10.89 (2.40)	10.99 (2.41)	10.80 (2.38)	0.096
Feelings index ($0 \leq \text{Index scale} \leq 4$)	1.58 (1.09)	1.57 (1.09)	1.59 (1.09)	0.553
<i>Panel B. Household characteristics</i>				
Age of spouse	38.13 (7.95)	38.23 (7.99)	38.03 (7.92)	0.467
Education of spouse	8.14 (3.35)	8.10 (3.40)	8.18 (3.30)	0.490
Number of household members	4.39 (1.37)	4.35 (1.27)	4.43 (1.44)	0.116
Monthly household income	9,218 (6,974)	9,189 (6,544)	9,243 (7,321)	0.720
Experienced income loss*	0.94 (0.25)	0.93 (0.26)	0.94 (0.23)	0.159
Experienced complete income loss*	0.59 (0.49)	0.58 (0.49)	0.60 (0.49)	0.397
Number of children under five	0.56 (0.74)	0.56 (0.73)	0.56 (0.74)	0.966
HH works in informal sector*	0.60 (0.49)	0.61 (0.49)	0.60 (0.49)	0.831
Joint <i>F</i> -test <i>p</i> -value	–	–	–	0.225
Sample Size	2,402	1,103	1,299	–

Notes: Age and education are in years. Note also that 65 percent of our sample are aged 30 years or over (balanced across treatment arms). “Household chores increased” = 1 if respondents’ household chores increased after COVID-19 lockdown and 0 otherwise; “Someone helps with household chores” = 1 if a household member helps respondent with daily household chores after the COVID-19 lockdown and 0 otherwise; “Trusts neighbors” = 1 if respondent trusts neighbors and relatives and 0 otherwise; “COVID-19 perceptions” is an average (between 0 and 1) based on true/false responses to 16 COVID-19-related statements, where 1 means having accurate perceptions (see online Appendix B for the questions and their construct); “Worried index” aggregates 4 dummy responses on what respondents are worried about during the pandemic (well-being of family and relatives, providing food to the family, and income); “Afraid index” aggregates 4 responses (three dummies and one answered on a scale between 0 and 10) on what respondents are afraid of during the pandemic (socializing, home visitors, going outside, and contracting the virus); “Feelings index” aggregates 4 dummy responses on respondents’ emotions during the pandemic (feeling anxious, lonely, hopeless, and worthless); “Experienced income loss” = 1 if a household experienced partial or complete income loss after the COVID-19 lockdown and 0 otherwise; “Experienced complete income loss” = 1 if a household experienced complete income loss after the COVID-19 lockdown and 0 otherwise. “HH works in informal sector” = 1 if the household head works in either agriculture or as a day-laborer and 0 otherwise. *t*-test *p*-values are derived from linear regression, with the variable of interest as the dependent variable and the treatment indicator as an independent variable with union council fixed effects and standard errors clustered at the village level. Joint *F*-test does the joint test of orthogonality. Variables with * are indicators.

D. Empirical Methods

Regression Specification.—To test our hypotheses, we estimate regression specifications of the following form:

$$(1) \quad Y_{1ij} = \alpha + \beta T_{ij} + \gamma Y_{0ij} + \mathbf{X}'\zeta + \nu_j + \epsilon_{ij},$$

where Y_{1ij} is the outcome of individual i from union council j measured at the endline. T_{ij} is an indicator for women who received the telecounseling treatment. Y_{0ij} is the baseline analogue of the outcome, which we include when available.¹⁴ \mathbf{X} is a vector of controls that includes the respondent's age, education, occupation, household income loss, number of household members, number of children under the age of five, and husband's main occupation as well as whether the respondent is the head of the household and whether women experienced an increase in household chores following the lockdown. ν_j is union council fixed effects, the smallest rural administrative and local government units in Bangladesh, where each union council is made up of roughly nine villages (so our comparisons are between treatment and control group women in the same union council).¹⁵ We also cluster standard errors at the village level in the main analysis. We estimate and report intent-to-treat (ITT) effects in the paper; however, given the very high session attendance rate of 98 percent (see Table A4 in online Appendix A), ITT effects should be statistically equivalent to treatment-on-treated (TOT) effects. We report OLS estimates throughout this paper.

Corrections for Multiple Hypotheses Testing.—Since we have 16 outcomes and two endlines (total of 24 tests, with some outcomes only measured once), we correct p -values for the number of tests performed using the Westfall-Young (WY) adjustments (Westfall and Young, 1993). WY accounts for correlations across outcomes using bootstrap resampling. Therefore, to check the robustness of our results, we initially use 1,000 replications to compute the Family Wise Error Rate (FWER) adjusted p -values and then repeat the process with 5,000 replications. We consider the group of prespecified and additional outcomes to constitute two separate families of tests. We report these p -values, with 1,000 replications, in all regression tables. Our conclusions remain unchanged if we compute the FWER p -values by considering all 24 tests as one family of hypotheses.

Randomization Inference.—To account for uncertainty in our estimates that arises naturally from the random assignment of participants to the treatments, we also compute p -values using randomization-based inference (RI) following Young (2019). These are constructed by randomly shuffling the treatment dummy and reestimating our β using this placebo assignment 1,000 times and then 5,000 times

¹⁴We only measure perceived stress, food insecurity, and COVID-19 compliance outcomes at the baseline, whereas the remaining outcomes are only measured at the endline. Thus, while estimating the impact on the remaining outcomes, we do not control for their baseline level Y_{0ij} .

¹⁵We have 50 union councils in our sample, with roughly 7 villages or 48 households per union council.

for robustness. In all regression tables, the RI p -values and conventional p -values are nearly identical, which is why we do not report the RI p -values in the tables.

E. Attrition

We had 2,402 women at baseline, 1,103 in the control group, and 1,299 in the treatment group. During the one-month endline, we could follow up on 2,220 women (1,007 control and 1,213 treated), while the remaining 182 women could not be reached (attrition rate of 7.6 percent). At the ten-month endline, we again attempted to follow-up on all 2,402 women from the baseline but could eventually reach and survey 2,254 women (1,028 control and 1,226 treated, with attrition rate being 6.2 percent). The remaining women could not be reached on the phone or they refused to partake in the endline. Nevertheless, over 88 percent of the 2,402 women surveyed in the baseline never attrited, while only 2.2 percent of women could not be reached at either endline (see Table A5, online Appendix A).¹⁶

Although the overall attrition rate at both endlines was relatively low, we observe attrition at the one-month endline survey to be marginally correlated with treatment. That is, at one month, attrition in the control arm was 2.1 percentage points higher (or 10 women more) than attrition in the treatment arm (marginally significant using a Pearson's Chi-squared test or CS-test: $p = 0.06$). However, attrition in the control and treatment groups was statistically indistinguishable at the ten-month endline (CS-test: $p = 0.23$). In Tables A6 and A7 in online Appendix A, we check whether any baseline characteristics predict attrition at the one-month and ten-month endlines and find no strong evidence. We also do not find attrition to be differential by baseline characteristics of women at either endline. Because attrition at the one-month endline was marginally differential by treatment groups, we check the robustness of our one-month endline results by reestimating our main treatment effects (reported and discussed in Section IV) in two ways: (i) using inverse probability weighting (IPW) and (ii) using an attrition bounds analysis following the nonparametric approach of Lee (2009). We report these results in Table A8 in online Appendix A, which shows that our main results (later discussed in Section IV) remain robust to such corrections. More details on attrition analysis are provided in online Appendix B.8.

IV. Results

We have divided the presentation of our results into two parts. First, we present the results related to the impact of the intervention on the set of primary and secondary outcomes we had prespecified. These include the two main mental health outcomes—perceived stress and depression—and a group of secondary outcomes, which encompass happiness, life satisfaction, future aspirations, and compliance

¹⁶There are 34 additional observations in the ten-month endline relative to the one-month endline. This is because we were successful in following up on 130 women at the ten-month endline that could not be reached at the one-month endline, whereas 96 women that were reached at the one-month endline could not be followed up again at the ten-month endline.

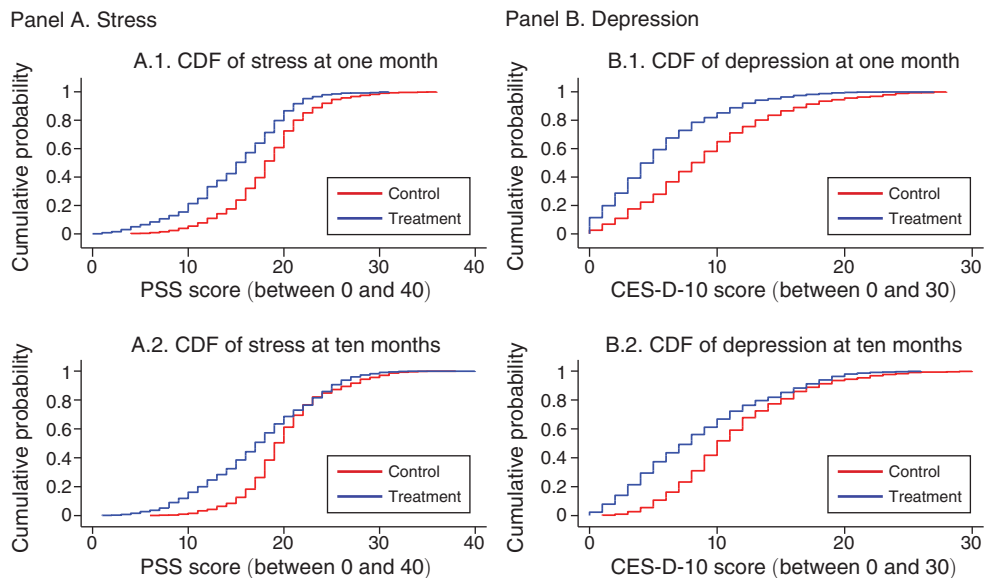


FIGURE 2. CDFs SHOWING TREATMENT EFFECTS AT BOTH ENDLINES

Notes: PSS score is a measure of stress based on ten questions, each answered on a 5-point Likert scale (0–4), and takes the value between 0 and 40. CES-D-10 score is a measure of depression based on ten questions, each answered on a 4-point Likert scale (0–3), and takes the value between 0 and 30.

with COVID-19 health guidelines. Second, we report the results on the effect of the intervention on a range of additional outcomes. These include food insecurity and time-intensive parental investment, confidence about tackling COVID-19 emergencies, COVID-19 vaccine take-up, gender empowerment, attitudes toward gender norms, attitudes toward IPV, and economic preferences.

We standardize all the outcomes used in the present analysis following Kling, Liebman, and Katz (2007) (see online Appendix B.1 for a detailed discussion on the construction of the indices).

A. Prespecified Outcomes

Mental Health Outcomes.—We start by presenting raw comparisons of the distributions of our measures of mental health at one-month and ten-month endlines for the treatment and control groups in Figure 2. In both the case of perceived stress (panel A) and depression severity (panel B) and in both endlines, we see that the cumulative distribution functions of the treatment group lie to the left of the respective distributions of the control group (Kolmogorov-Smirnov test: $p < 0.01$), implying that the telecounseling program significantly improved the mental health of treated women.

This is also confirmed by the regression results presented in Table 2 and summarized in Figure 3. In Table 2 (panel A), we show first treatment effects without any controls in columns 1 and 4 and then with the full set of controls (as defined in

TABLE 2: TREATMENT EFFECTS ON PRESPECIFIED OUTCOMES

Dependent variables	1-month endline			10-month endline		
	Without covar. (1)	With covar. (2)	FWER <i>p</i> -val. (3)	Without covar. (4)	With covar. (5)	FWER <i>p</i> -val. (6)
<i>Panel A. Mental health outcomes</i> [‡]						
Perceived stress	-0.712 (0.061)	-0.696 (0.059)	0.000	-0.576 (0.077)	-0.551 (0.075)	0.000
Stressed* (= 1 if stressed)	-0.229 (0.023)	-0.220 (0.022)	0.000	-0.202 (0.018)	-0.195 (0.018)	0.000
Depression severity	-0.638 (0.052)	-0.652 (0.050)	0.000	-0.525 (0.065)	-0.513 (0.063)	0.000
Depressed* (= 1 if depressed)	-0.200 (0.026)	-0.207 (0.025)	0.000	-0.193 (0.030)	-0.191 (0.029)	0.000
<i>Panel B. Secondary outcomes</i>						
Happiness	0.232 (0.045)	0.219 (0.043)	0.000	-	-	-
Life satisfaction	0.240 (0.047)	0.234 (0.045)	0.000	-	-	-
Future aspirations	0.390 (0.047)	0.374 (0.044)	0.000	-	-	-
COVID-19 compliance	1.187 (0.047)	1.189 (0.047)	0.000	-	-	-
Observations	2,220	2,220	-	2,254	2,254	-

Notes: This table reports results on preregistered outcomes. Treatment effects are estimated using OLS. All outcomes (except for the two dummies in panel A, denoted with *) are standardized indices so that the control group has mean 0 and SD 1. The control group means for “Stressed” and “Depressed” indicators are 0.86 and 0.35 at 1-month endline and 0.96 and 0.58 at 10-month endline, respectively. For outcomes with ‡, negative coefficients mean more favorable outcomes. Columns 1 and 4: treatment effect estimated without any baseline covariates. Columns 2 and 5: treatment effect estimated with all baseline covariates (as in equation (1)): respondent’s age, years of education, occupation, household income loss during the pandemic, household size, number of children under the age of 5, respondent’s household-head status, husband’s main occupation, whether respondent’s household chores increased following the lockdown, and union council fixed effects. Standard errors, clustered at the village level, are in parentheses. Columns 3 and 6 report FWER *p*-values for the full model (as in columns 2 and 5), which are the WY familywise error rate adjusted *p*-values (with 1,000 replications) (Westfall and Young 1993). Robust standard errors clustered at the village level are in parentheses.

model 1) in columns 2 and 5. We focus primarily on the results in columns 2 and 5, as the results with and without the full set of controls are very similar.

We find that the telecounseling intervention was successful in improving mental health outcomes of women captured via both perceived stress and depression severity. At the one-month endline, treated women experienced a 0.70 SD reduction in perceived stress ($p < 0.01$) and a 0.65 SD reduction in depression severity ($p < 0.01$) relative to untreated women (column 2, panel A, Table 2). At the ten-month endline, the respective effects are reductions of 0.55 SD in perceived stress and 0.51 SD in depression severity ($p < 0.01$ for both), suggesting that the intervention had a lasting effect on the mental health of treated women ten months after the end of the intervention while the pandemic was still raging and a second lockdown was underway.

In fact, these estimated effects are large compared to the short-run impact of cognitive behavioral therapy interventions in Pakistan (Baranov et al. 2020) and Kenya (Bryant et al. 2017) and those found by telephone-delivered interventions (Mohr

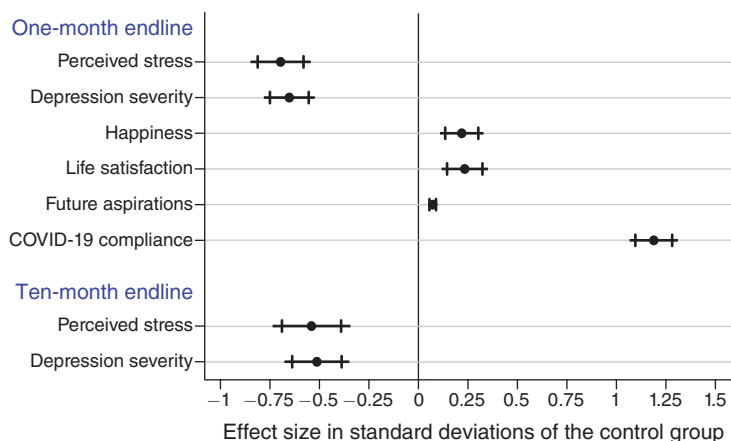


FIGURE 3. TREATMENT EFFECTS ON PRESPECIFIED OUTCOMES

Notes: This figure reports treatment effects in SD units (same as in columns 2 and 6, Table 2), along with 99 percent and 95 percent confidence intervals. All treatment effects are estimated using OLS. Standardized index outcomes (control group has mean 0 and SD 1) were regressed on the treatment dummy, while controlling for all baseline characteristics as in equation (1) (respondent's age, years of education, occupation, household income loss during the pandemic, household size, number of children under the age of 5, respondent's household-head status, husband's main occupation, whether respondent's household chores increased following the lockdown, and union council fixed effects), with standard errors clustered at the village level. Note that negative effects for the mental health outcomes correspond to more favorable outcomes (i.e., reductions in mental health problems), while positive effects for the remaining outcomes correspond to more favorable outcomes.

et al. 2008) and studies that use psychotherapy to improve individual psychological well-being (Cuijpers et al. 2010, 2013), considering that these earlier interventions were typically long, delivered by mental health professionals, and addressed people who suffered from depression. Notably, the impact of such interventions (including our own) is more sizable than the average effect size of economic transfers on mental health, which has been estimated to be 0.10 SD in low- and middle-income countries (McGuire, Kaiser, and Bach-Mortensen 2020). The effects of the current intervention are in the upper range of those reported in a recent meta-analysis of app-supported mental health interventions, in which effect sizes were found to range from 0.28 to 0.58 (Linardon et al. 2019).

These treatment effects on mental health can also be seen under an alternative construction of the dependent variable (stress or depression) as a binary variable based on whether the underlying stress or depression score exceeds a certain threshold that is indicative of moderate to severe stress or depression, respectively. The estimates presented in Table 2 (panel A) confirm that the impact of the treatment is indeed quite large: a reduction of 22 percentage points for the incidence of being stressed and about 21 percentage points for the incidence of being depressed in the first endpoint and a reduction of 19.5 percentage points for the incidence of being stressed and about 19 percentage points for the incidence of being depressed in the second endpoint. These effects imply that the prevalence of severe stress fell by 26 percent and depression by 60 percent relative to the control group, while in the second endpoint, stress fell by 20 percent and depression by 33 percent.

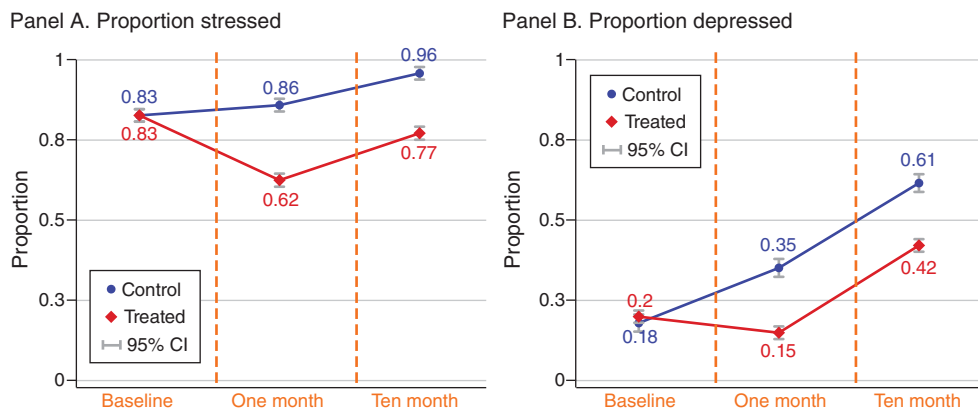


FIGURE 4. MENTAL HEALTH OVER TIME, BY TREATMENT ARMS

Notes: This figure shows the proportion of stressed (graph A) and depressed (graph B) women at each data collection wave. Here, the threshold for someone being mentally stressed is when the PSS score is greater than 13 (note that the PSS score ranges between 0 and 40, where a higher score corresponds to higher stress levels). To compute the baseline depression, we aggregated the four emotions (being anxious, lonely, hopeless, and worthless) measured at baseline and used the median cutoff level to create the baseline “depressed” dummy. Summary of the emotions are given in Table 1.

Figure 4 shows the proportion of stressed (graph A) and depressed (graph B) women at each of the three data collection waves (baseline, one-month, and ten-month endlines) by treatment.¹⁷ The two groups start from having similar fractions of either stressed or depressed women at baseline. Following the intervention we observe a gap emerging between the two groups: in the control group there is a steady increase in the fraction of women that are stressed or depressed, while in the treatment group there is a drop-off in the first endline followed by an increase in the second endline.

Given that the telecounseling sessions focused mainly on ways to help women cope with stress and improve psychological well-being and pandemic-related knowledge, we believe that the above results indicate that the intervention has been quite successful in achieving the desired effects on the targeted women.

Secondary Outcomes.—In Table 2 (panel B), we report results from our secondary prespecified outcomes. First, regarding the effect of the intervention on subjective happiness, life satisfaction, and aspirations for the future (life related, income related, and in general), we find that the intervention significantly improved happiness levels of treated women by 0.22 SD, life satisfaction by 0.23 SD, and future aspirations by 0.37 SD compared to women in the control arm. All of these effects are significant at the 1 percent level.

Furthermore, as one of the modules in the counseling sessions focused particularly on raising health awareness among the counselees by providing valuable information

¹⁷ As a pre-pandemic reference, Hosain et al. (2007) found, using the General Health Questionnaire-60, that the overall prevalence of psychiatric disorders among a sample of working-age individuals in a rural area of Bangladesh was 16.5 percent, with depressive and anxiety disorders being the most common type of mental disorders.

about the spread of the disease and precautionary steps that need to be taken to prevent its spread, we expect the intervention to also affect compliance with COVID-19 health guidance. Indeed, we find that compliance with COVID-19 precautionary measures improved by 1.19 SD for the treated women relative to those in the control group ($p < 0.01$). Similarly, in percentage terms (panel A.2, column 3, Table A10 in online Appendix A), compliance among treated women increased by 50 percentage points (compliance in the control arm is 24 percent).

To unpack these results, we disaggregate the COVID-19 compliance index into several indicator variables constructed using responses to the individual survey questions that comprise the composite index.¹⁸ We find that the intervention affected all underlying questions. These results are reported in Table A11 in online Appendix A.

Social Desirability Bias.—One concern with our study is that the contents of the intervention can induce experimental demand effects on women that received the treatment, leading to an upward bias of the estimated treatment effects reported in Table 2. In Tables A17, A18, and A19 in online Appendix A, we present some analyses following the approach in Bandiera et al. (2020) to explore this, but are not able to conclusively address the issue.¹⁹

B. Additional Outcomes

We next examine whether, as a by-product of improving women's mental health and increasing COVID-19 awareness, the intervention also had an impact on a range of additional outcomes. These results are presented in Table 3 and Figure 5.

First we consider food insecurity. We find that treated women experienced a reduction in household-level food insecurity by 0.28 SD relative to untreated women at the one-month endline ($p < 0.01$) and by 0.52 SD at the ten-month endline ($p < 0.01$). These findings suggest that improved mental health makes women more equipped to manage food for themselves and their families. This is an important consideration given that the pandemic is reported to have caused severe food insecurity across rural households in Bangladesh (Ahmed et al. 2021). Note that the measure we employ is a subjective indicator of food insecurity and is not based on food consumption data.

It is also informative to observe how food insecurity evolved over time across treatment groups. Figure 6 shows this. What we see is that for both groups food

¹⁸The 'COVID-19 compliance' index was constructed using seven individual questions (listed in online Appendix B.3). We convert each compliance-behavior question into a binary outcome (= 1 if the response is either of the maximum 2 points implying higher compliance and 0 otherwise, on a 5-point response scale) and regress each dummy on the treatment indicator with the usual set of controls as specified in regression equation (1).

¹⁹In particular, we assess whether the effects we estimate differ across participants based on their level of agreement with the statement, "I want to be a respectful person in my village," which we elicited in the second endline (see online Appendix B.6 for details of this measure). We assume people who have a higher level of agreement have more social desirability bias. Results reported in online Appendix Tables A17, A18, and A19 indicate that, with a couple of exceptions, there is no differential impact of the intervention by the measure of social desirability of the respondent on our main outcomes in both endlines. Other methods of addressing experimental demand effects, such as using the 13-item Marlowe-Crowne social desirability bias scale (Dhar, Jain, and Jayachandran 2022) or bounding the treatment effects (de Quidt, Haushofer, and Roth 2018), were not feasible in our field context as, due to the pandemic, we conducted surveys over the phone, thus facing tight interview time constraints.

TABLE 3—TREATMENT EFFECTS ON ADDITIONAL OUTCOMES

Dependent variables	1-month endline			10-month endline		
	Without covar. (1)	With covar. (2)	FWER <i>p</i> -val. (3)	Without covar. (4)	With covar. (5)	FWER <i>p</i> -val. (6)
Food insecurity [‡]	-0.310 (0.043)	-0.276 (0.041)	0.000	-0.537 (0.045)	-0.520 (0.047)	0.000
Time-intensive parental investments	0.227 (0.055)	0.220 (0.057)	0.000	0.232 (0.050)	0.192 (0.049)	0.000
Confidence about tackling COVID-19	0.394 (0.051)	0.396 (0.048)	0.000	–	–	–
Gender empowerment	0.128 (0.050)	0.101 (0.049)	0.088	–	–	–
Attitudes toward gender norms	0.173 (0.046)	0.149 (0.046)	0.017	–	–	–
Attitudes toward IPV	0.250 (0.044)	0.231 (0.043)	0.000	–	–	–
Risk seeking	–	–	–	0.441 (0.043)	0.432 (0.044)	0.000
Altruistic	–	–	–	0.456 (0.044)	0.432 (0.045)	0.000
Delay gratification	–	–	–	0.017 (0.060)	0.003 (0.060)	0.825
COVID-19 vaccination (= 1 if vaccinated)	–	–	–	0.059 (0.018)	0.058 (0.018)	0.043
Observations	2,220	2,220	–	2,254	2,254	–

Notes: This table reports results on outcomes that were not preregistered at the AEA RCT Registry. Treatment effects are estimated from OLS. All outcomes are standardized indices (except for “vaccination,” which is a binary variable) so that the control group has mean 0 and SD 1. Control group mean for “vaccination” is 0.216. For outcomes with ‡, negative coefficients mean more favorable outcomes. For all other outcomes, positive coefficients mean more favorable outcomes. Columns 1 and 4: treatment effect estimated without any baseline covariates. Columns 2 and 5: treatment effect estimated with all baseline covariates (same set of controls as Table 2). Standard errors, clustered at the village level, are in parentheses. Columns 3 and 6 report FWER *p*-values for the full model (as in columns 2 and 5), which are the WY familywise error rate adjusted *p*-values (with 1,000 replications) (Westfall and Young 1993). Robust standard errors clustered at the village level are in parentheses.

insecurity followed a “V” pattern, with those in the treatment group experiencing a sharper decline in food insecurity than the control group at the one-month endline. The reduction in food insecurity seen here is possibly due to the fact that, at the time of the first endline, Bangladesh was emerging from lockdown measures, and therefore, households in our sample had likely gained access to more resources relative to the baseline. By the time of the second endline, a second wave of the pandemic was underway and the country had gone into a second lockdown, which probably explains the increase in food insecurity.

We also find an increase in time-intensive parental investment (0.22 SD and 0.19 SD at one-month and ten-month endlines, respectively; $p < 0.01$), suggesting that treated women are better positioned to carry out parental duties. When we check the impact on disaggregated responses that constitute our index of parental investment, we find some interesting patterns. We find that our intervention encouraged treated mothers to help their children with their education and missed school work ($p < 0.01$) but not to spend more time playing with their children ($p = 0.54$).

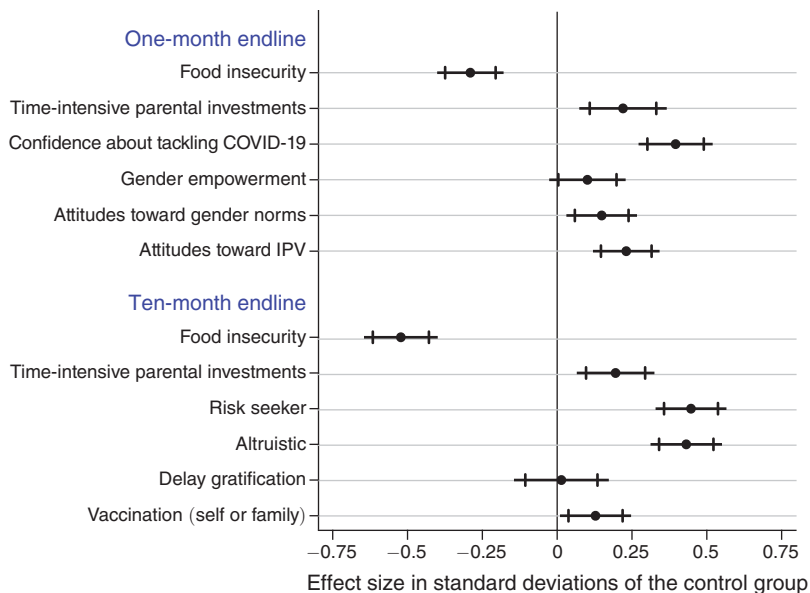
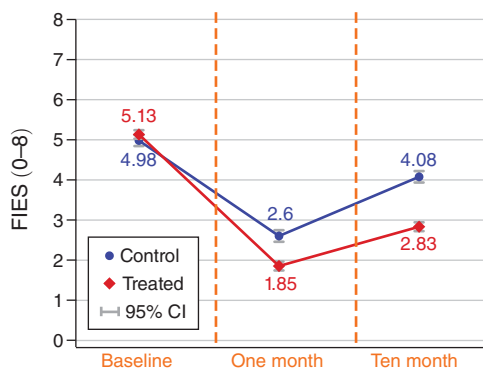


FIGURE 5. TREATMENT EFFECTS ON ADDITIONAL OUTCOMES

Notes: This figure reports treatment effects in SD units (same as in columns 2 and 5, Table 3) along with 99 percent and 95 percent confidence intervals. All treatment effects are estimated using OLS. Standardized index outcomes (control group has mean 0 and SD 1) were regressed on the treatment dummy, while controlling for all baseline characteristics as in equation (1) (respondent’s age, years of education, occupation, household income loss during the pandemic, household size, number of children under the age of 5, respondent’s household-head status, husband’s main occupation, whether respondent’s household chores increased following the lockdown, and union council fixed effects), with standard errors clustered at the village level. Note that negative effects for the household food insecurity outcome correspond to more favorable outcomes (i.e., reductions in food insecurity), while positive effects for the remaining outcomes correspond to more favorable outcomes.

Panel A. Food insecurity experience scale



Panel B. Food insecure

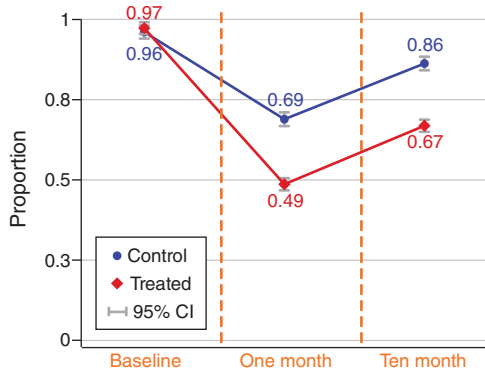


FIGURE 6. HOUSEHOLD FOOD INSECURITY OVER TIME, BY TREATMENT ARMS

Notes: This figure shows household-level food insecurity at each data collection wave. The FIES is a scale between 0 and 8, where a higher number corresponds to high food insecurity. Similarly, the Food Insecure dummy equals 1 if $FIES > 0$ and 0 if $FIES = 0$.

This result is reported in Table A9 in online Appendix A. Considering school closures during the pandemic, allocating more time to help out children with their education is an important remedial input toward children's development.

Thus, these results suggest that while the intervention improved the mental health of the target group relative to the control group, it also had significant spillover effects on their ability to cope with the financial stress that households were experiencing during the pandemic. This finding adds to previous evidence that improvements in mental health can contribute to better economic outcomes for individuals living in impoverished conditions (Ridley et al. 2020), although provision of pandemic-related information could be an alternative channel. We offer some suggestive evidence of potential channels for these findings in Section V.

Furthermore, we find that the intervention had an impact on a range of other dimensions: it advanced gender empowerment (0.10 SD) and improved attitudes toward gender norms (0.15 SD) and intimate partner abuse (0.23 SD), with $p < 0.01$ in all cases. When we also examine the impact of the intervention separately on the individual questions that constitute the indices (Tables A14–A16 in online Appendix A), we find improvements in most subcomponents but not all. First, regarding gender empowerment, we find that women feel more in control over their spouses' income and savings (both $p < 0.01$), making intrahousehold decisions regarding food and financial matters (both $p < 0.01$) and leaving the house on their own ($p < 0.05$) following the intervention. However, control over their own income and savings ($p = 0.24$ and $p = 0.66$, respectively) and opinions/decisions about their children's education and health ($p = 0.23$ and $p = 0.16$, respectively) did not improve. Second, in terms of attitudes and opinions toward gender norms, we find that treated women had improved opinions about female decision-making power in households and society ($p < 0.01$ and $p < 0.05$, respectively) and were more likely to believe that they can make better calculative decisions than men ($p < 0.01$). However, opinions about equal gender rights and being able to disagree with husbands did not change significantly ($p = 0.08$ and $p = 0.60$, respectively). Finally, with regard to justifications about IPV, we find that treated women find it inappropriate if husbands physically abuse or hit wives when wives argue with their husbands and when wives burn food while cooking (both $p < 0.01$). However, their opinions about husbands having the right to hit wives when wives leave the house without husbands' permission and when children are not properly looked after were rather mixed following the intervention.

Next, inspired by recent research suggesting that mental health might impact economic preferences (Cobb-Clark, Dahmann, and Kettlewell 2022; Ridley et al. 2020; Bayer et al. 2019), in the second endline, we collected self-reported measures of a range of economic preferences (risk, social, and time preferences). We find that individuals in the treatment arm become more risk seeking and altruistic than those in the control group, while we do not find significant differences in time preferences postintervention. These findings provide supportive evidence of the existence of a positive link between mental health, risk attitudes, and altruism, though they should be interpreted cautiously in light of the fact that our measures of economic preferences were not incentivized. Note also that an alternative channel for the results on risk preferences is that—irrespective of an effect on mental health—providing

additional information about COVID-19, and thus mitigating a large negative shock, could make people less risk averse.

We also consider the effect of the intervention on self-confidence about tackling COVID-19 emergencies. We find that the confidence to tackle COVID-19 issues increased by 0.40 SD among those targeted via the mental health intervention ($p < 0.01$), which is roughly 12 percentage points (confidence in the control arm is 42 percent), and both improvements are statistically significant at a 1 percent level (see Table A12 in online Appendix A).

Finally, during the ten-month endline—because vaccination against COVID-19 was rolled out—we also asked participants to indicate whether they or any member of their household had been vaccinated. We find that there is an increase of 5.8 percentage points of positive answers to this question in the treatment group, compared to an incidence of 21.6 percent in the control, implying an increase of 26.9 percent in vaccination rates. This impact is quite remarkable given that the counseling sessions did not include any discussion of the benefits of vaccination, though as mentioned above, information about COVID-19 was provided in one of the sessions.

C. Robustness Checks

Our conclusions are robust to a number of additional checks. First, our results are robust to using p -values computed using randomization inference and adjusting for multiple hypotheses testing. Second, our results are robust to having outcomes constructed as binary variables (column 3, Tables A10 and A13 in online Appendix A). Third, results are robust to corrections for attrition bias using IPW and Lee (2009) bounds (Table A8 in online Appendix A).

D. Heterogeneity by Baseline Stress

One might expect that the intervention would benefit more women with poorer starting mental health. To explore this, we estimate an interaction model of the following form:

$$(2) \quad Y_{1ij} = \alpha + \beta_1 T_{ij} + \beta_2 H_{ij} + \beta_3 (T_{ij} \times H_{ij}) + \gamma Y_{0ij} + \mathbf{X}'\zeta + \nu_j + \epsilon_{ij},$$

where H is an indicator for having a baseline PSS score above the median ($median^{PSS} = 18$). We are interested in β_3 , which helps us understand if the treatment effects vary across women with different baseline stress level.

The results presented in columns 1 and 2 in Table 4 (panel A) report the treatment effects at the one-month endline on women with below- and above-median PSS scores at baseline, respectively. We find that the intervention affected both groups of women significantly (both $p < 0.01$). However, the effect is larger in magnitude among women with high perceived stress (or above-median PSS score) relative to women with low perceived stress, as confirmed by the statistically significant interaction coefficient in column 3 of Table 4 ($p < 0.01$). We also find that the treatment effect on depression appears to be larger in magnitude for women with high perceived stress than women with low perceived stress at baseline ($p < 0.05$, column 3). In addition, women

TABLE 4—HETEROGENEITY BY BASELINE STRESS ON PRESPECIFIED OUTCOMES

Dependent variables	PSS dummy			PSS continuous
	Below median (1)	Above median (2)	Coefficient on interaction (3)	Coefficient on interaction (4)
<i>Panel A. 1-month Endline</i>				
<i>A.1. Mental health outcomes[‡]</i>				
Perceived stress	-0.548 (0.071)	-0.837 (0.082)	-0.256 (0.097)	-0.027 (0.011)
Depression severity	-0.538 (0.059)	-0.752 (0.072)	-0.210 (0.086)	-0.016 (0.009)
<i>A.2. Secondary outcomes</i>				
Happiness	0.116 (0.057)	0.305 (0.064)	0.167 (0.082)	0.006 (0.010)
Life satisfaction	0.147 (0.058)	0.313 (0.067)	0.134 (0.082)	0.013 (0.010)
Future aspirations	0.335 (0.058)	0.393 (0.065)	0.060 (0.085)	0.006 (0.010)
COVID-19 Compliance	1.098 (0.065)	1.266 (0.067)	0.173 (0.087)	0.008 (0.010)
<i>Panel B. 10-month Endline</i>				
<i>B.1. Mental health outcomes[‡]</i>				
Perceived stress	-0.602 (0.101)	-0.481 (0.087)	0.167 (0.112)	0.010 (0.013)
Depression severity	-0.494 (0.079)	-0.505 (0.081)	0.030 (0.098)	0.001 (0.012)

Notes: All outcomes are standardized indices such that the control group has mean 0 and SD 1. Columns 1–3 examine heterogeneity by perceived stress dummy measured at baseline, whereas columns 1 and 2 report treatment effects among individuals that reported to have stress below and above the median score (median = 18), and column 3 reports the coefficient on the interaction between the treatment dummy and the median PSS score dummy (= 1 if above the median value), thus showing the difference between column 2 and column 1. Column 4 shows the coefficient on the interaction between the treatment dummy and continuous PSS score (between 0–40) measured at the baseline. All specifications include baseline covariates (as in equation (1)). For outcomes with ‡, negative coefficients mean more favorable outcomes. Robust SE clustered at the village level are in parentheses.

with high perceived stress also experienced significantly larger improvements in terms of their subjective happiness ($p < 0.05$, column 3) and their compliance with COVID-19 rules ($p < 0.05$, column 3). For the remaining outcomes, we do not find significant heterogeneous treatment effects by baseline perceived stress.

Turning to panel B of Table 4, we see that there are no statistically significant differences in the treatment effects for these two subgroups at the ten-month endline. As illustrated in Figure 4, it should be noted that the stress levels of the control group at that point of the pandemic were even higher, at 96 percent, than at the baseline. In other words, the period in which the mental health measures were taken is one of steadily deteriorating mental health for this population. Interestingly, this finding of lack of heterogeneity by baseline mental stress is consistent with the findings reported in (Barker et al. 2022).²⁰

²⁰ As a robustness check, we also control for the interactions between covariates that are correlated with high/low PSS (i.e., ‘number of children’ and ‘increases in household chores’) and the treatment dummy. Following this adjustment, the size of these coefficients decrease slightly, but our conclusions remain unchanged. See Table A20 in online Appendix A.

V. Mechanisms

We find significant impacts of the intervention on mental health outcomes and food security that have lasting effects even ten months after the counseling sessions took place. We next seek to explore various potential pathways that underpin these findings. First, we examine how the counseling sessions were able to have sustained impacts; and second, we investigate potential channels of impact of the intervention on food security.

A. *Why Does the Intervention Have Lasting Impact?*

With regards to mental health outcomes, we examine whether the lasting benefits of the intervention can be attributed to recipients of the counseling continuing to follow, outside of the counseling period, the earlier mental health advice that was offered to them during the counseling sessions. To assess this, we asked respondents at the ten-month endline to report which good mental health practices they had recently regularly followed.²¹ We report these results in columns 1 and 2 of Table 5. We find that indeed women in the treatment group are more likely to report that they followed any of the advice (column 2) and to report a larger number of the recommendations (column 1) than those in the control group. Online Appendix Figure A4 presents a breakdown of the type of advice that respondents cited by treatment. Differences are mainly concentrated in five activities: spending quality time with children, contacting a doctor for COVID-19-related health issues, praying, talking and discussing problems with household members, and breathing exercises.

Note that beyond the impact of the above practices, there are two other key channels through which the telecounseling sessions could have benefited participants, the importance of which is more difficult for us to quantify. First, the sessions contained important information about COVID-19, which has been shown to alleviate stress and depression in similar contexts (Islam et al. 2021; Sadish, Adhvaryu, and Nyshadham 2021). Second, the opportunity to interact and receive emotional support from para-counselors in itself would offer mental health benefits to counselees. We expect that these were important drivers of the mental health impacts that we find.

B. *Channels of Impact of the Intervention on Food Security*

We next consider potential mediators of the positive impact of the intervention on our main economic outcome (food insecurity). That is, we seek to understand what were the underlying coping mechanisms that allowed women in the treatment

²¹The counseling sessions emphasized ten practices that participants were advised to follow during the pandemic: (i) talking and discussing problems with family members within the household; (ii) talking to neighbors, while maintaining 2–3 arms' lengths of distance; (iii) avoiding blaming oneself if something unexpected happens; (iv) walking in the backyard; (v) breathing exercise; (vi) praying; (vii) talking to relatives or family members over the phone; (viii) spending quality time with children; (ix) sharing problems with someone they trust; and (x) contacting doctors if they or any household member have health problems or COVID-19 symptoms. To help initiate contact with nonhousehold members and doctors, mobile phones of counselees were topped up with a small amount at the end of the intervention.

TABLE 5—POTENTIAL MEDIATORS

Variables	Followed advice (1)	Followed advice (=1) (2)	Borrowing ↑ (3)	Contacted pub. offices (4)	Husband work ↑ (5)	New inc. activity (6)
Treatment	2.083 (0.130)	0.368 (0.025)	0.098 (0.023)	0.012 (0.013)	0.007 (0.007)	0.137 (0.028)
Control mean	0.827 [1.173]	0.419 [0.494]	0.231 [0.421]	0.069 [0.254]	0.021 [0.145]	0.261 [0.439]
All controls	Yes	Yes	Yes	Yes	Yes	Yes
Union council FE	Yes	Yes	Yes	Yes	Yes	Yes
FWER <i>p</i> -values	0.00	0.00	0.00	0.54	0.54	0.00
Observations	2,254	2,254	2,254	2,254	2,254	2,254
<i>R</i> ²	0.318	0.221	0.071	0.038	0.064	0.091

Notes: Treatment effects are estimated using OLS. Outcomes in columns 1–6 are as follows: (1) Followed advice if number of mental health counseling advisements followed by respondents during the most recent lockdown ($0 \leq \text{Advice} \leq 10$), where 10 means followed all 10 advice given via telecounseling and 0 means followed none; (2) Followed advice (= 1) equals 1 if followed at least 1 counseling advisement during the most recent lockdown; (3) Borrowings ↑ equals 1 if borrowed money from relatives/neighbors in the last 10 months; (4) Contacted public offices equals 1 if contacted and sought help from public offices in the last 10 months (e.g., from Upazila/subdistrict office) during food shortages; (5) Husband work ↑ equals 1 if husband's income-generating work increased in the last 10 months; (6) New income-generating activities equals 1 if respondent has started any new income-generating activities in the last 10 months. SDs of the control mean are given in brackets. These outcomes were collected at the 10-month endline only. "All controls" include the same set of controls as Table 2. FWER *p*-values are the WY familywise error rate adjusted *p*-values (with 1,000 replications) (Westfall and Young 1993). Robust standard errors clustered at the village level are in parentheses.

group to experience improved food security over those in the control group. We consider four mediators: whether the respondent borrowed money from relatives or neighbors, whether they contacted and received support from a local government office, whether the husband's income from work improved, and whether the respondent undertook any new income-generating activities (cattle or poultry farming, fish farming, sewing, day laborer, working in the city, and other; see online Appendix Figure A5 for a breakdown of the type of activities that respondents reported by treatment). This information was collected in the second endline, and the time frame for all these questions was the previous ten months, that is, since the intervention ended.

These results are presented in columns 3–6 of Table 5. We see that the treatment had significant impact on informal borrowing and income-generating activities, even though no suggestions about informal borrowing or starting new income-generating activities were offered during the counseling sessions. In particular, incidence of borrowing in the treatment group is higher by 10 pp and incidence of undertaking new income-generating activities by 13.7 pp relative to the control group. Given that the incidence of borrowing is 23.1 percent in the control group and that of new income-generating activities is 26.1 percent, these effects suggest that these were two important mediating factors for the impact of the intervention on food security. They also indicate that better mental health is an important factor in enabling women in this context to undertake activities that ensure better food security for their households.

One question that the above findings raise is how treated women are finding the extra time, relative to women in the control group, to devote to new income-generating

activities. While we do not have data to investigate this question, our observations from conversations with a subgroup of women indicate that these do not come at the expense of their leisure or the time they spend helping their children with school, which also increases, as is shown in Table 3. Instead, we suspect that being in a better mental state allows them to be more productive and resourceful in the activities that they undertake (Grossman 1972).

VI. Intervention Cost and Scaling Up

For scalability and replication purposes, we list and categorize the costs of our intervention in Table A21 in online Appendix A. We have spent about \$US18,000 on the intervention, where roughly 10 percent of the cost was fixed and the remaining 90 percent was variable. This corresponds to \$14 per treatment delivery, which is largely comparable to costs of (in-person) psychotherapy interventions in low-income countries but comparatively cheaper than mental health interventions pertaining to cash transfers or pharmacotherapy. The most costly component of our intervention was the salary of para-counselors (60 percent of total), followed by mobile phone top-ups for participants (18 percent of total) and para-counselors (5 percent of total). As traveling costs of para-counselors to various households for treatment delivery can often be time consuming and costly, our telephone-delivered intervention is relatively cheaper and faster, while being highly effective. Moreover, public university students often work as volunteers and are mostly idle during times of crisis (epidemics, natural disasters, war, political unrests, etc.); therefore, they could be hired as volunteer para-counselors to deliver telecounseling to the vulnerable. Doing so would further reduce the intervention cost to less than \$6 per person.

To further inform policymakers and the design of future interventions, we list our primary preintervention tasks and the associated preparation time in Table A1 in online Appendix A. We spent 30 days preparing the telecounseling contents. However, when addressing mental health shocks during other crises and contexts (e.g., natural disasters or among forcibly displaced individuals), adapting the content of the existing session modules to suit these other circumstances/contexts should not be too time consuming. Training the para-counselors (18 graduates of public health, psychology, and social sciences with no prior counseling experience) took nine days. In total, the preparation of the intervention took 5.5 weeks.

While monetizing all the benefits of the intervention for the counselees and their families (including mental health and economic outcomes) is beyond the scope of this paper, it is probably safe to say that these benefits will exceed the \$14 cost per participant. Thus, it would seem that the current telecounseling intervention offers a cost-effective solution to addressing mental health problems in times of crises in contexts with scant resources.

VII. Conclusion

The COVID-19 pandemic has had a profound economic and social impact on households in low-income countries that is being disproportionately borne by women in rural areas. Exposure to economic uncertainty and turmoil has severe consequences

for mental well-being, which are challenging to address due to limited resources and lack of mental health services. We develop and evaluate through a randomized controlled trial a telecounseling mental health intervention aimed to reach women in rural Bangladesh in the midst of a global pandemic.

We find that the intervention leads to large improvements in measures of stress and depression severity—both immediately after the intervention ended and in a follow-up ten months later. Treated women also experience improvements in other measures of well-being (happiness, life satisfaction, and future aspirations) and report higher levels of compliance with prescribed health guidelines related to COVID-19 prevention. In addition, we find impacts on a range of additional outcomes that we had not prespecified, such as food security and parenting investment, as well as on measures of empowerment and attitudes toward gender norms and partner violence, suggesting that the intervention has a broader impact on women's outlook and how they see their role within the household and society more generally.

What explains the effectiveness of this light-touch intervention? We believe that several factors played a key role. First, the intervention was multifaceted, encompassing valuable informational content about the pandemic and suggesting coping strategies, as well as offering emotional support from a trustworthy person. Second, it was administered in the midst of a major pandemic that had severely affected the mental health of participants. Third, the intervention was delivered in a context lacking alternative mental health support and limited access to reliable information about the pandemic.

The telephone-delivered intervention that we implemented has several important advantages that are worth highlighting here. First, it is safe for both participants and the individuals providing the counseling under pandemic conditions that necessitate maintaining a physical distance. Second, it can be delivered privately and discreetly, thus preventing the possible attachment of stigma to recipients of mental health treatment, which has been argued prevents people from seeking treatment (Corrigan 2004). Third, it is low cost to deliver as it does not require extensively trained counselors, unlike other more intensive approaches, such as cognitive behavioral therapy, which is a particularly important consideration in low-resource contexts. Our calculations suggest that the cost of delivering the intervention amounts to \$14 per treated participant, including training and staff costs. This suggests that such light-touch interventions that offer reliable information and focus on building participants' skills in managing their emotions through simple practices that could be embedded in their daily lives are not only effective but also scalable and can be promising in providing rapid psychological support to vulnerable groups in times of crises.

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