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Cognitive Behavioral Therapy among Ghana's Rural Poor Is Effective Regardless of Baseline Mental Distress[†]

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We study the impact of group-based cognitive behavioral therapy (CBT) for individuals selected from the general population of poor households in rural Ghana (N = 7,227). Results from one to three months after the program show strong impacts on mental and perceived physical health, cognitive and socioemotional skills, and economic self-perceptions. These effects hold regardless of baseline mental distress. We argue that this is because CBT can improve well-being for a general population of poor individuals through two pathways: reducing vulnerability to deteriorating mental health and directly increasing cognitive capacity and socioemotional skills. (*JEL* D12, I12, I15, I31, I32, O12, O18)

Spurred in part by the inclusion of mental health as a key sustainable development goal, a growing "global mental health" movement argues for improved access to therapy (e.g., Patel and Prince 2010; Patel et al. 2018). How broad might the impact of this movement be? We argue that increasing access to mental health therapy in low-income countries should be seen as a core means of improving well-being and increasing socioemotional skills and cognition in the *general* population, with relevance beyond treating those with a diagnosable mental health condition.

We base this argument on the results of a large-scale randomized controlled trial (N = 7,227, with 5,937 in control and 1,290 in treatment)[1] evaluating the impact of untargeted, group-based, cognitive behavioral therapy (CBT) in rural Ghana. Using

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[1] These reflect the endline analysis sample size. We believe that our study is among the largest randomized evaluations of CBT ever conducted.

short-run endline data from one to three months after the intervention, we first show that therapy led to meaningful average increases in mental health, perceived physical health, socioemotional and cognitive skills, and perceived economic status. For example, those in the treatment group report having good mental health 0.53 more days per month; increase self-efficacy by 0.29 standard deviations; improve their score on a digit span test (a measure of cognition) by 0.08 standard deviations; and perceive themselves to have 0.20 standard deviations higher economic status. Our cognitive skill measures are of particular interest because they are less prone to experimenter demand effects. We then show, perhaps surprisingly, that impacts on mental health, perceived physical health, and socioemotional and cognitive skills are not limited to those identified as having mental distress at baseline; treatment effects are positive and large for both those with *and* without baseline distress.

These results indicate that the program is relevant for a general population of low-income individuals, not just those with diagnosed mental health issues. We identify two key mechanisms for this result. First, we argue that low-income individuals are especially *vulnerable* to deteriorating mental health, and therapy preemptively alleviates this vulnerability. Second, we argue that CBT has a direct effect on cognitive and socioemotional skills even for those who do not or will not suffer from mental health difficulties.

Our argument that CBT alleviates vulnerability depends on a key contextual observation: there is a high degree of churn between distress states in our sample. Analyzing just the control group, we find that 43 percent of those who report no mental distress at baseline report mental distress at endline 5 to 8 months later; meanwhile, 33 percent of those who report moderate to severe mental distress at baseline report no mental distress at endline. These figures should be understood in the context of high levels of distress: at baseline 55 percent have some form of psychological distress and 16 percent have severe psychological distress. While mental distress is undoubtedly measured with error, we have three reasons to believe that our results remain relevant. First, the Kessler Psychological Distress Scale is a well-tested and widely used metric for psychological distress (Furukawa et al. 2003; Kessler et al. 2010). Second, we find strong decreases in distress, suggesting that the measure does accurately capture some aspect of mental health. Third, even if the observed churn is a by-product of measurement error, our results show that in this population it would be a mistake to target mental health treatments only to those identified as distressed at baseline.

Our argument that CBT has a direct effect even for those who do not experience mental health challenges draws on the concept of "bandwidth" defined by Mullainathan and Shafir (2013) and Schilbach, Schofield, and Mullainathan (2016), which these authors characterize as an individual's cognitive capacity and their ability to plan, allocate attention, initiate and inhibit actions, and control impulses (measured in our data by our cognitive and socioemotional skills indices, respectively). These authors argue that being poor leads people to misallocate their mental resources toward short-term financial problems, thus reducing bandwidth available for other tasks. We first review the theory behind CBT and our particular curriculum and argue that the theoretical mechanism through which CBT is thought to operate suggests that it should engender a better allocation of bandwidth across tasks, drawing a link between therapy and the behavioral economics of scarcity. Second, we show that the CBT program had large impacts on key measures of cognitive and socioemotional skills, which should increase when available bandwidth is increased. Specifically, we show a 0.27 standard deviation increase in a socioemotional skills index including self-control and a 0.08 standard deviation increase in a cognitive skills index including measures such as digit span and Raven's Progressive Matrices.

Our work builds on several important literatures. Development economists have long recognized vulnerability as a key part of poverty: being poor not only means having a low income but also facing frequent negative shocks that threaten to induce a state of destitution (e.g., Morduch 1994; Ligon and Schechter 2003; Collins et al. 2009). A related literature spanning both psychology and economics argues that poverty leads to mental health difficulties (e.g., Lund et al. 2011; Ridley et al. 2020; Frasquilho et al. 2015; Kuhn, Lalive, and Zweimüller 2009). Chemin, de Laat, and Haushofer (2013) explicitly show the negative mental health impact of a *transitory* exogenous economic shock. Taken together, the twin claims of vulnerability to economic shocks and a causal effect of shocks on mental health motivate our hypothesis that the poor are vulnerable to mental health difficulties.

Second, several papers argue that poverty changes psychology and decision-making beyond mental health. Banerjee and Mullainathan (2010) argue that poverty leads people to give into temptation, Mullainathan and Shafir (2013); Shah et al. (2018); and Schilbach, Schofield, and Mullainathan (2016) argue that the poor spend significant mental resources on short-run financial problems, reducing bandwidth available for other tasks, and Bessone et al. (2021) argue that the poor's living environment directly reduces mental resources. We contribute to this literature by arguing that CBT can be conceptualized as a broad program to improve decision-making quality, helping individuals better allocate their mental resources. We also link this literature to a large literature showing important economic returns to socioemotional, "noncognitive" skills (Heckman, Stixrud, and Urzua 2006; Alan, Boneva, and Ertac 2019; McKelway 2021).

Third, we contribute to a growing literature that studies the economic impacts of therapy. Several papers study the impact of therapy on economics outcomes but typically for a highly selected group of individuals. For example, Blattman, Jamison, and Sheridan (2017) study the impact of therapy for ex-combatants in Liberia on earnings, Heller et al. (2017) evaluate the impact of a CBT-type program for youth in high-crime schools on graduation rates, Baranov et al. (2020) study the impact of therapy for recent mothers suffering from prenatal depression on financial empowerment and investment in children, and Patel et al. (2017) measure the impact of therapy on the days an individual is unable to work. Lund et al. (2018) provide an important meta-analysis of this linkage.

Finally, while CBT programs predominantly target specific populations typically diagnosed with mental health issues, exceptions do exist. For example, Howell et al. (2019) study therapy for medical students, Bolton et al. (2007) evaluate a program for refugee camp residents, and Kew et al. (2016) study therapy for a population recently diagnosed with asthma. In each case, the targeted individuals are predicted to have a higher chance of mental health distress because of some specific life situation. Our study builds on this targeting approach by studying the impact of CBT in a general population of the poor in rural Ghana; we posit this as an important step for testing the broader relevance of CBT.

Our study is most similar to the contemporaneous work of Haushofer, Mudida, and Shapiro (2020). Similar to us, these authors study a psychotherapy program delivered to a general population in a low-income country, Kenya.[2] Their results differ markedly from ours. They find no statistically significant impact of CBT on mental health or economic outcomes measured 13 months after the program. They propose that their program is unsuccessful precisely because it does not target a population with a specific difficulty. While many of the design elements of our two studies are similar (both targeted low-income households using a poverty proxy rather than poor mental health, both involve a CBT-inspired program, and both were delivered by lay counselors), the two substantively differ in both the intensity of treatment and measurement time frame. Our program consisted of 12 weekly 90-minute sessions, whereas the Kenya study was 5 weekly 90-minute sessions. Our results are very short run, measured on average two months after the end of the therapy; their surveys took place, on average, 13 months later.[3] If time frame explains the difference, then this poses an important challenge: how can programs maintain impacts? It could be that therapy "booster sessions" may be cost effective. Nevertheless there is reason to hope that fading impacts are not inevitable. Baranov et al. (2020) find that CBT impacts persist for at least seven years. We will continue to measure impact, and thus future work will illuminate whether or not our observed impacts fade.

I. Intervention

A. *Cognitive Behavioral Therapy*

CBT is a widely used and widely studied approach to the treatment of multiple mental health conditions. CBT is designed on the premise that individuals have automatic responses to stimuli and that these responses are sometimes subject to "cognitive distortions." These distortions in turn lead to the misinterpretation of stimuli, affecting the way people view themselves, others, and the future (Beck et al. 1979). CBT encourages individuals to recognize their automatic responses and question their thought distortions.

The conceptual framework for CBT gives a clear sense of why the poor might be at both greater risk of mental health difficulties and vulnerable to deteriorating mental health. Those who find themselves in a steady state of poverty are constantly presented with negative stimuli, raising significant scope for distortion. For example, an individual born into a poor family may misinterpret their low income as evidence of low levels of talent, leading to mental distress. The poor also face many idiosyncratic shocks in their lives, and there is significant scope for distortions to lead to misinterpretation. An individual who experiences a bad

[2] Online Appendix Table 1 compares the two studies.

[3] Haushofer, Mudida, and Shapiro (2020) argue that their null results are unlikely to be due to fading impacts because they fail to detect effects for a subsample surveyed seven months after the end of their program. Our even shorter-run outcomes are perhaps the key to our positive findings. An alternative explanation may be that our population is poorer and faces more vulnerability to distress. The countries are similar in per capita income (US$4,993 in purchasing power parity terms in Ghana and US$4,204 in Kenya), and in both implementations poor households were targeted. However, the study in Kenya took place in Nakuru Country, the second-wealthiest county (of 47), whereas our study took place in regions of Ghana with rates of poverty above the national average.

harvest due to insufficient rainfall might, for example, conclude that "my efforts never pay off."[4] A similar perspective is present in "diathesis-stress" models in the psychology literature, which argue that psychopathology arises from the interaction between a biological predisposition (diathesis) and stress in the environment, in our case poverty.[5] These observations are at the core of our interpretation that CBT may be appropriate for many of the world's poor: large numbers of the world's poor are likely to suffer from poor mental health, and even those who are not currently suffering are vulnerable to deteriorating mental health.

The CBT framework also provides an alternative way to conceptualize the mechanisms that Mullainathan and Shafir (2013) conjecture drive the negative effects of scarcity and trap the poor in poverty. Key to their claim that scarcity leads to negative outcomes is the notion that responses to scarcity—for example, "tunneling," or rumination on short-term needs—are a misallocation of mental resources.[6] One way to understand this scarcity-induced misallocation is as an automatic, distorted response to financial stress. This observation opens the door to think of CBT's focus on automatic thoughts, and explicitly evaluating their accuracy, as a way to learn to avoid the negative outcomes of scarcity and in particular the resulting decrease in mental bandwidth available for important tasks. Indeed, several of the key lessons of the CBT curriculum we use, and CBT in general, address bandwidth-draining behaviors. For example, our CBT program manual devotes time to discussing the dangers of "mental filtering" or dwelling on specific issues; "catastrophising" or overemphasizing small problems; and "should statements," which require an individual to reach the correct outcome for all problems, suggesting corner solutions to effort allocation. Thus, CBT might plausibly be useful in guiding the automatic response of individuals exposed to stressors on a regular basis, regardless of their current mental health status.

The potential of CBT as a general method to improve well-being is further aided by the fact that group-based CBT is usually delivered using a strictly controlled manual, allowing CBT to be moved out of a clinical setting. Recent research has demonstrated the ability of lay counselors to deliver CBT to individuals in several low-income countries when targeted at groups with existing mental disorders, such as depressive and anxiety disorders (Patel et al. 2010; Dias et al. 2019), perinatal depression (Rahman et al. 2008), and posttraumatic stress disorder (Smith et al. 2007).

B. *Counselor Characteristics and Training*

We study a CBT curriculum designed by one of us (Ofori-Atta) and intended to be implemented by recent college graduates with a degree in psychology or a related field and requiring no further qualifications or training. The program was designed to ultimately be integrated with Ghana's National Service Scheme (NSS). The NSS mandates that recent college graduates work for one year in public service, and in conjunction with Psych Corps Ghana (a program run through the University of Ghana

[4] De Quidt and Haushofer (2016) argue for a negative impact of poverty and shocks on mental health even in the absence of the thought distortions that are a mainstay of the therapy literature.

[5] See, for example, Colodro-Conde et al. (2018); Ingram and Luxton (2005); and Arnau-Soler et al. (2019).

[6] Similarly, a large research in psychology emphasizes that mental illness captures attention in an unproductive way; see, for example, Gotlib and Joormann (2010).

Medical School), recent college graduates with backgrounds in psychology are posted to district hospitals throughout the country (Ofori-Atta, Ketor, and Bradley 2014).

The research nonprofit organization Innovations for Poverty Action (IPA) recruited 37 staff to deliver the program. Half served as lead counselors and the other half as assistant counselors. All staff had at least a bachelor's degree (one had an advanced degree); their most common majors were psychology (65 percent), another health-related field (13 percent), and development studies or social work (13 percent). The median counselor member received their tertiary degree two years (mean 2.76 years) prior to being hired.

All counselors received two weeks of classroom training and performed one week of piloting. Additionally, at the end of each week, all counselors in a given district met with a lead counselor, who debriefed them on the previous week's activities and helped them prepare for the coming week.

C. *Curriculum and Program Delivery*

The CBT program consisted of 12 weekly 90-minute sessions, delivered to a group of ten, and took place in the community where people lived. The 12 sessions covered four modules: healthy thinking, including identifying and challenging thought distortions; solving problems at home and at work; managing relationships, including communication, self-esteem, and being good to yourself and others; and goal setting and goal-directed behavior. Sessions included a combination of the counselors and assistant counselors introducing the material, having individuals discuss hypothetical scenarios as a group and in pairs, and thinking about how they could apply the lessons they learned to their own lives. As with most CBT interventions, counselors assigned homework tasks after each session and reviewed these in the next session. The full CBT program manual is available on the authors' websites.[7]

II. Research Design

A. *Sample Selection*, *Randomization*, *and Participation Rates*

The population we study is composed of households in the 40 poorest compounds[8] from 258 eligible[9] rural communities in 14 districts across two ecological zones (the "Northern belt" and "Middle belt") in Ghana.[10] Figure 1 shows the construction

[7] Also at https://bit.ly/BBKOAU-CBT

[8] A compound is one or more households living in separate dwellings within a single structure.

[9] To identify the eligible communities, District Assembly staff identified 366 rural communities in which their records suggested at least 50 compounds were present. We applied two further inclusion criteria: the community was accessible by road and did not have an existing "graduation" program involving a productive asset transfer. The second criteria was motivated by the objective of a planned subsequent study comparing the impact of several interventions, including a cash transfer and a graduation program implemented by Heifer International, similar to those reported in Banerjee et al. (2015). This CBT study in its entirety (i.e., the intervention and the endline data collection) were completed *prior* to the announcement (and start) of the planned follow-on study; thus, we evaluate and report on the results of the CBT without needing to consider the later, randomly assigned interventions.

[10] IPA administered a census in each of the 258 communities and verified that each community contained at least 45 compounds. In each community, we selected the 40 compounds with the lowest average household proxy means test score and, for each compound, randomly chose one household to include in our eligible population. We worked with one household per compound because of concerns about within-compound spillovers.

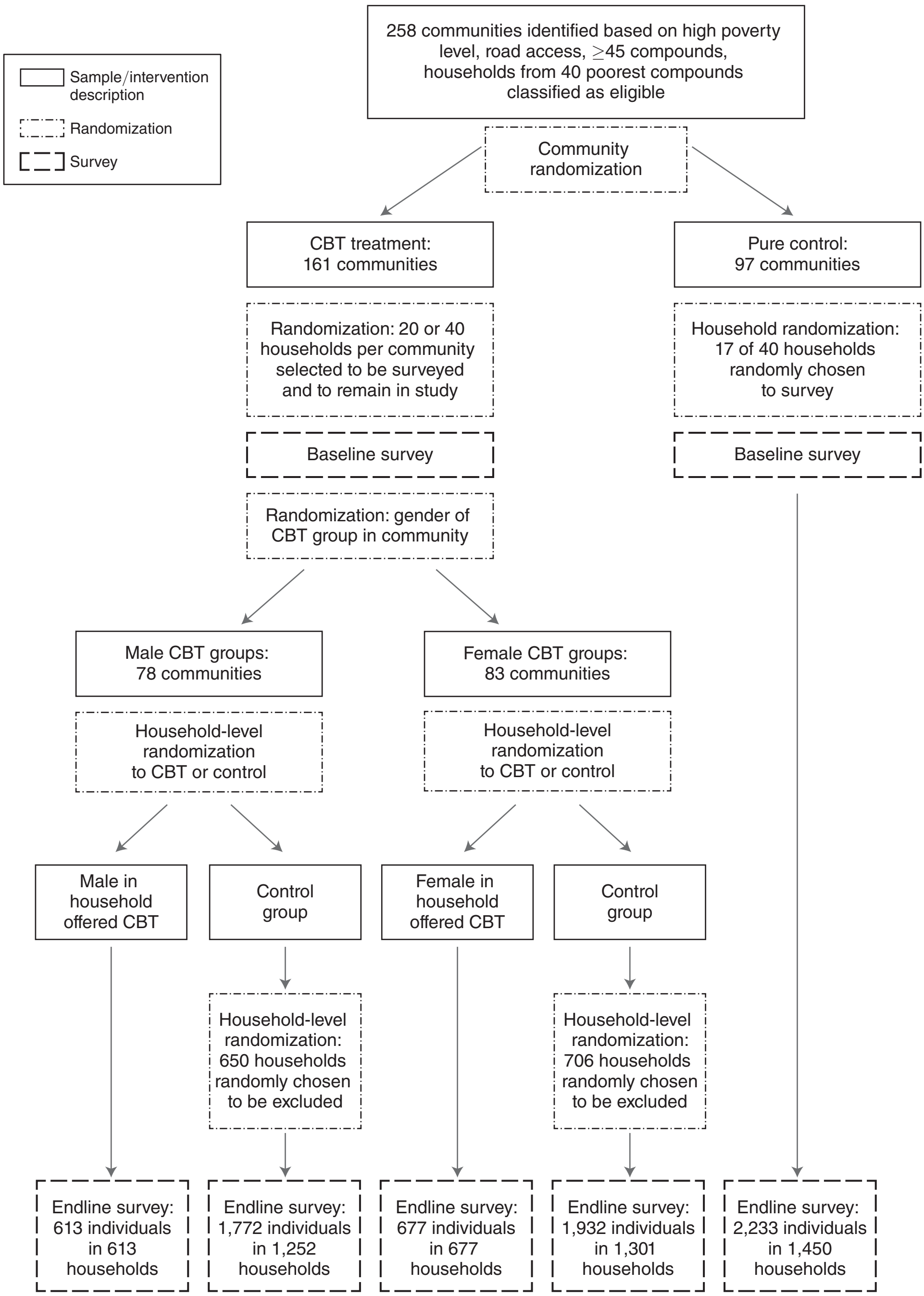

Figure 1. Experimental Design

of the sample and assignment to treatment. The 258 eligible communities were randomly assigned to treatment (161 communities) or control (97 communities) groups. In each of the control communities, 17 of the 40 households were randomly selected into our sample. In the treatment communities, either all 40—or a randomly selected 20 of the 40—households were selected into our sample depending upon the community's randomized status for planned (but not announced) future interventions (see footnote 9).

After a baseline survey (details below), we randomly assigned the treatment communities to either "Female CBT" (83 communities) or "Male CBT" (78 communities) groups. In each of the Male or Female CBT communities, ten households were randomly chosen to receive CBT; the remainder were kept as control households. In those households assigned to "Male CBT," the male household head received the offer of CBT; in "Female CBT" households, it was the female household head or spouse of the household head. For budgetary reasons, we excluded a randomly selected subset of our control group sample from the endline sample frame.

Online Appendix A provides further details on the sample selection, community criteria, and randomization procedures.

Take-up of the program was high: 90 percent of individuals offered CBT attended at least one session. The average attendance was 74 percent among the full sample and 83 percent for those who ever attended a session.

B. *Sample Characteristics and Attrition*

Our baseline survey contained a household survey measuring consumption, assets and wealth, income, and other household characteristics. Several household survey variables are reported in Table 1. Our sample population is poor and agrarian. Roughly half of males and 40 percent of women have attended any primary school. Half of the households practice open defecation, and in the past year adults in almost one-third of the households went for at least a day without food because of lack of resources. Almost all households have a farm with about five acres of land. Nearly half of the households raise goats, sheep, or pigs, and almost 60 percent have poultry. Forty percent of the households have at least one nonfarm enterprise, but only 5–6 percent have a member with formal employment.

We conducted the endline survey one to three months after the intervention; 13 percent of our sample attrited between baseline and endline. Online Appendix Table 3 reports the overall differential attrition rate and also tests for differential attrition by baseline demographic, economic, and mental health characteristics. We see no evidence of differential attrition by treatment status.

C. *Outcome Data*

Both baseline and endline surveys contained two "adult" surveys, which were administered to the household head and their spouse.[11,12] We include the responses of both adults in control households; in households where an individual received CBT, we only include treated individuals.[13]

We report outcomes across five broad categories: perceived mental health; perceived physical health; socioemotional skills; cognitive skills; and economic

[11] For polygynous households, we randomly selected one wife for both the survey and CBT treatment offer.
[12] We did not administer a household survey at endline.
[13] That is, we exclude from our analysis spouses of individuals who received CBT rather than code them as "treated" or "control."

Table 1—Baseline Summary Statistics

	Treatment mean	Control mean	p-value: test treatment coefficient = 0
	(1)	(2)	(3)
Panel A. Male respondent characteristics			
Age	43.07	42.99	[0.663]
Married	0.90	0.93	[0.580]
Polygynous	0.17	0.18	[0.908]
Ever attended school	0.50	0.46	[0.386]
Experiencing mild, moderate, or severe distress	0.49	0.53	[0.123]
30 days in last month in which poor physical or mental health prevented work or regular activities	26.83	26.86	[0.882]
Observations	709	3,822	
Panel B. Female respondent characteristics			
Age	38.38	39.31	[0.075]
Married	0.75	0.75	[0.096]
Ever attended school	0.40	0.40	[0.797]
Experiencing mild, moderate, or severe distress	0.58	0.57	[0.599]
30 days in last month in which poor physical or mental health prevented work or regular activities	26.69	26.93	[0.456]
Observations	782	5,249	
Panel C. Household-level characteristics			
Household size	7.69	7.55	[0.371]
Number of children under age five	1.53	1.47	[0.525]
Household walls made of mud	0.73	0.75	[0.116]
Household connected to electric grid	0.45	0.43	[0.512]
Household practices open defectation	0.54	0.53	[0.751]
Any adults skipped meals last year	0.50	0.53	[0.631]
Any adults went whole day without food last year	0.29	0.29	[0.551]
Household has agricultural plot	0.89	0.88	[0.154]
Acres owned	5.15	5.06	[0.331]
Household has nonfarm enterprise	0.42	0.41	[0.488]
Any household member worked for agricultural wage	0.08	0.08	[0.775]
Any household member works in formal employment	0.06	0.05	[0.017]
Household has cattle	0.09	0.09	[0.285]
Household has goats/sheep/pigs	0.48	0.47	[0.066]
Household has poultry	0.61	0.59	[0.340]
Household has any financial savings	0.40	0.39	[0.964]
Observations	1,570	5,760	

Notes: Columns 1 and 2 report means of the Treatment and Control individuals/households. Panels A and B report comparisons for men and women, respectively, randomized to either receive CBT or to receive nothing. Panel C reports comparisons at the household level. Column 3 presents p-values from the test that the treatment coefficient is equal to zero in a regression comparing the groups. This regression is estimated with equation (1) in the main text (without controls for the baseline characteristics, given that baseline characteristics are what we are comparing). P-values are calculated via randomization inference, in which we rerun our full randomization procedure to assign placebo treatments and compare our true estimates to the placebo distribution of estimates; the full procedure is described in online Appendix B. We surveyed heads of household and spouses for the adult survey; observations in panel C are not equal to the sum of panels A and B for three reasons. First, some (but not all) households had both a male and female adult. Second, in this comparison, we are not including spouses of individuals receiving CBT (given that they are neither cleanly "treated" or "control"). Third, while a household needed to be surveyed to be included in the study, the relevant adult being present was not a prerequisite. We are thus missing some adults in panels A and B who were nonetheless randomized to receive the program.

self-perceptions. For each category we report indices created as per Kling, Liebman, and Katz (2007), as well as treatment effects for each subcomponent. Our mental health index is created from three measures: the Kessler Psychological Distress Scale (Kessler et al. 2002) ("Kessler score"), a self-rating of mental health taken from the Behavioral Risk Factor Surveillance System (BRFSS),[14] and a self-report of days in the month without poor mental health. We use the Kessler score from our baseline survey as the main measure of baseline mental health. Our physical health index is created from the BRFSS self-rating of physical health, a self-report of the number of days without poor physical health, and a self-report of work days missed last month due to poor health.[15] This last question could be added to either the mental or physical health index, and our decision to allocate it to physical health is somewhat arbitrary. It is important to note that mental health improvements may lead to perceived changes in physical health and hence improvements in self-reported physical health.

Our index of noncognitive or socioemotional skills has three subindices: generalized self-efficacy, a measure of optimistic self-belief (Schwarzer and Jerusalem 1995); grit, a measure of passion for and perseverance with long-term goals (Duckworth and Quinn 2009); and self-reported self-control (Tangney, Baumeister, and Boone 2004). Four measures comprise our index of cognitive skills: performance on Raven's Progressive Matrices (Raven 1941); a forward digit span test; a backward digit span test; and a Stroop-like test of executive function (Stroop 1935; adjusted here for a population with limited literacy).

Finally, our economic self-perceptions index is composed of two measures: self-reported economic status today and expected status in five years (both reported using the Cantril Ladder) (Kilpatrick and Cantril 1960).

As in any evaluation that uses self-reported data, we are concerned about experimenter demand effects. We believe that the cognitive skill measures are of particular interest because they are not strictly self-reported. Scoring higher in a Raven's, digit span, or Stroop test requires an actual improvement in performance. The only route through which demand effects might influence the results is if those in the treatment are inspired to put more effort into the tasks in response to perceived experimenter demands. Previous work also suggests that our mental health measures have some resilience to demand effects. A literature in psychology studies demand effects for depression-related measures and has found minimal evidence of such effects for the Patient Health Questionnaire-9 and Center for Epidemiological Studies Depression Scale (Beard et al. 2016; McMillan, Gilbody, and Richards 2010). These measures are similar to the Kessler score that we use. This literature compares survey responses among individuals receiving therapy to structured clinical interviews (considered the gold standard in diagnosing depression). The studies find high agreement between interview and survey-based measures, both in terms of levels and improvements. While encouraging, this literature has not considered the question of whether the correlation between the two measures differs by treatment status.

[14] The question is "In general, would you say your mental health is excellent, very good, good, fair, or poor?"
[15] The BRFSS question is "In general, how would you rate your health?"

III. Results

A. *Prevalence and Transition Rates of Psychological Distress*

We first show that the poor are vulnerable to psychological distress. Table 2 reports the incidence of psychological distress (measured by the Kessler score) and transition probabilities into and out of states of psychological distress over the span of five to eight months in our study sample (panel A) and over four years in a similar population from the Ghana Socioeconomic Panel Survey (panel B). Despite not sampling based on existing mental health, the rate of psychological distress is high, with 55 percent reporting symptoms associated with some degree of psychological distress (compared to 58 percent in the general population in the same geographic regions, panel B). To compare, in the United States, the 2007 BRFSS documents only 13 percent with any level of psychological distress (Dhingra et al. 2011).

Our assertion that CBT is applicable as a mental health intervention for individuals not currently experiencing mental illness depends in part on the observation that low-income individuals diagnosed as "well" at a given point in time are nonetheless at elevated risk for subsequent transitions into psychological distress. The high degree of churn into and out of psychological distress shown in Table 2 supports this view. Among individuals observed to have no psychological distress at baseline, 43 percent have some form of distress at endline; 10 percent have severe psychological distress. In fact, of the 16.2 percent of individuals whose symptoms suggest severe psychological distress at endline, a roughly equal number come from individuals whose baseline responses indicate no distress and those with responses indicating severe psychological distress (0.45 well at baseline $\times$ 0.10 $=$ 0.045; 0.16 with severe psychological distress at baseline $\times$ 0.26 $=$ 0.043). Our results suggest that a mental health program restricted to individuals with existing psychological distress may miss a large number of at-risk, or vulnerable, individuals.

B. *Average Treatment Effects and Effects by Baseline Distress*

Our impact estimates are based on comparing all those randomly assigned to receive CBT to all those randomly assigned to control. That is, our control group consists of both households in CBT treatment communities that were randomly allocated to the control condition and also households in control communities.[16]

Our main results, reported in Tables 3 and 4, show impacts of CBT on mental and perceived physical health, cognitive and socioemotional skills (indicative of an increase in available bandwidth), and economic self-perceptions. We estimate average treatment effects in column 2 using the specification

$$y_{ivt} \;=\; \alpha + \beta_1 \cdot CBT_{ivt} + \beta_2 \cdot y_{iv0} + X_{ivt}\Pi + \epsilon_{ivt}, \tag{1}$$

where y_{ivt} is an outcome variable for individual i in village v at time period t, CBT_{ivt} is an indicator variable for being offered the CBT program, y_{iv0} is the outcome of

[16] We find qualitatively similar results when applying a more restrictive control group definition, reported in online Appendix Tables 8–12, as evidence of spillovers are small and generally not statistically significant.

Table 2—Incidence and Transition Rates of Mental Distress

		Endline mental distress				
Level of baseline mental distress, control group	Share at baseline (1)	No mental distress (N = 2,562) (2)	Mild mental distress (N = 1,211) (3)	Moderate mental distress (N = 892) (4)	Severe mental distress (N = 904) (5)	Total (6)
Panel A. Transition matrix for control group						
(a) No baseline mental distress (N = 2,486)	0.45	0.57	0.19	0.14	0.10	100%
(b) Mild baseline mental distress (N = 1,309)	0.24	0.42	0.24	0.17	0.17	100%
(c) Moderate baseline mental distress (N = 863)	0.15	0.36	0.24	0.18	0.22	100%
(d) Severe baseline mental distress (N = 911)	0.16	0.31	0.23	0.20	0.26	100%
(e) Share at endline		0.46	0.22	0.16	0.16	
	Share above diagonal (worsened mental health)				0.31	
	Share at diagonal (no change in mental health)				0.38	
	Share below diagonal (improved mental health)				0.31	
		2013 mental distress				
Level of 2009 mental distress	Share 2009 (1)	No mental distress (N = 1,799) (2)	Mild mental distress (N = 585) (3)	Moderate mental distress (N = 268) (4)	Severe mental distress (N = 90) (5)	Total (6)
Panel B. Transition matrix, Ghana socioeconomic panel survey, northern, upper east, Brong Ahafo, Ashanti regions, nonregional capitals						
(a) No 2009 mental distress (N = 1,159)	0.42	0.70	0.19	0.08	0.03	100%
(b) Mild 2009 mental distress (N = 822)	0.30	0.65	0.22	0.10	0.03	100%
(c) Moderate 2009 mental distress (N = 461)	0.17	0.59	0.25	0.12	0.04	100%
(d) Severe 2009 mental distress (N = 300)	0.11	0.60	0.23	0.13	0.04	100%
(e) Share in 2013		0.66	0.21	0.10	0.03	
	Share above diagonal (worsened mental health)				0.17	
	Share at diagonal (no change in mental health)				0.38	
	Share below diagonal (improved mental health)				0.44	

interest at baseline,[17] and X_{ivt} are the variables used in the rerandomization procedure (listed in online Appendix Table 2).

Columns 3 to 5 present heterogeneous treatment effects and tests for equality by baseline psychological distress.[18] These estimates come from a regression of the form

$$(2) \qquad y_{ivt} = \alpha + \beta_1 \cdot CBT_{ivt} \cdot distressed_{iv0} + \beta_2 \cdot CBT_{ivt} \cdot notdistressed_{iv0} + \beta_3 \cdot distressed_{iv0} + \beta_4 \cdot y_{iv0} + X_{ivt}\Pi + \epsilon_{ivt}.$$

Given the multistage nature of our randomization, we use randomization inference to test our null hypotheses of no treatment effect and no heterogeneity of

[17] When baseline measures are missing, they are coded as 0 with an indicator variable for "missing baseline value."

[18] We test for heterogeneity using a binary indicator of any psychological distress to maximize our statistical power to detect such an effect. We similarly do not see evidence of heterogeneity when using our continuous measure of baseline Kessler score, reported in online Appendix Tables 4 and 5.

Table 3—CBT Treatment Effects—Health Outcomes

	Average treatment effects		Heterogeneity by baseline mental distress		
	Control mean	CBT average treatment effect, full sample	CBT average treatment effect, minor, moderate, or severe baseline distress (Kessler 20+)	CBT average treatment effect, no baseline distress (Kessler <20)	*P*-value from test: homogenous treatment effect by baseline distress, 3 = 4
	(1)	(2)	(3)	(4)	(5)
Panel A. Mental health outcomes					
Mental health index	0.00	0.15	0.12	0.18	
RI p-value		[0.000]	[0.008]	[0.009]	[0.385]
Kessler score	21.53	−1.36	−1.08	−1.61	
RI p-value		[0.000]	[0.002]	[0.006]	[0.422]
No distress (Kessler <20)	0.46	0.06	0.05	0.05	
RI p-value		[0.004]	[0.034]	[0.146]	[0.974]
No moderate or severe distress (Kessler <25)	0.68	0.06	0.05	0.07	
RI p-value		[0.001]	[0.010]	[0.041]	[0.603]
No severe distress (Kessler <30)	0.84	0.04	0.02	0.07	
RI p-value		[0.010]	[0.273]	[0.019]	[0.106]
Mental health self-rating (1/4)	2.90	0.07	0.07	0.05	
RI p-value		[0.052]	[0.070]	[0.442]	[0.702]
30 minus days in month with poor mental health	24.85	0.53	0.23	1.20	
RI p-value		[0.097]	[0.522]	[0.052]	[0.169]
Panel B. Perceived physical health and effects on labor					
Perceived physical health and labor index	0.00	0.13	0.11	0.13	
RI p-value		[0.000]	[0.004]	[0.065]	[0.873]
Physical health self-rating (1/4)	3.05	0.12	0.10	0.14	
RI p-value		[0.000]	[0.004]	[0.010]	[0.500]
30 minus days in month with poor physical health	24.73	0.89	0.70	1.11	
RI p-value		[0.001]	[0.036]	[0.056]	[0.566]
30 minus days in month in which poor mental or physical health limited labor or normal activities	26.09	0.344	0.469	−0.003	
RI p-value		[0.160]	[0.101]	[0.995]	[0.407]

Notes: Each cell in column 2 is from a single specification estimating the Intention to Treat treatment effect, coefficient Beta 1 in equation (1) in the main text. Each regression contains between 7,179 and 7,227 observations. Each row for columns 3 and 4 are from a single specification with between 6,743 and 6,787 observations, which include dummy variables for baseline psychological distress and interactions of being randomized into the CBT program interacted with whether the individual had psychological distress at baseline, coefficients Beta 1 and Beta 2, respectively, in equation (2) in the main text. Column 5 reports the *p*-value from the test that the coefficients in columns 3 and 4 are equal. All *p*-values (in each of columns 2, 3, 4, and 5) are calculated via randomization inference, in which we rerun our full randomization procedure to assign placebo treatments and compare our true estimates to the placebo distribution of estimates; the full procedure is described in online Appendix B.

treatment effects by baseline distress and gender.[19] Specifically, for each of the randomizations we initially performed to determine an individual's final treatment status (community-level randomization, gender of CBT recipients in a community, individual's assignment), we replicate our initial procedure and, using the same rerandomization selection process, assign placebo treatments. Following this placebo assignment, we test for average treatment effects and heterogeneity of the (placebo) treatment by baseline distress (and gender, in our online Appendix). We perform this procedure 2,000 times (following the practice laid out by Young 2019)

[19] Results are extremely similar when we instead cluster at the village level, reported in panel A of online Appendix Tables 8–12.

Table 4—CBT Treatment Effects—Bandwidth and Economic Perceptions

	Average treatment effects		Heterogeneity by baseline mental distress		
	Control mean (1)	CBT average treatment effect, full sample (2)	CBT average treatment effect, minor, moderate, or severe baseline distress (Kessler 20+) (3)	CBT average treatment effect, no baseline distress (Kessler <20) (4)	*P*-value from test: homogenous treatment effect by baseline distress, 3 = 4 (5)
Panel A. Socioemotional skills					
Socioemotional skill index	0.00	0.27	0.25	0.29	
RI p-value		[0.000]	[0.000]	[0.000]	[0.623]
Generalized self-efficacy score	0.00	0.29	0.29	0.30	
RI p-value		[0.000]	[0.000]	[0.000]	[0.893]
Grit score	0.00	0.19	0.18	0.20	
RI p-value		[0.000]	[0.001]	[0.004]	[0.836]
Self-control score	0.01	0.12	0.10	0.15	
RI p-value		[0.005]	[0.058]	[0.028]	[0.482]
Panel B. Cognition					
Cognition index	0.00	0.08	0.08	0.08	
RI p-value		[0.012]	[0.043]	[0.170]	[0.969]
Raven's Progressive Matrices, indexed	0.00	0.03	0.02	0.08	
RI p-value		[0.555]	[0.701]	[0.259]	[0.484]
Digit span: forward, indexed	−0.01	0.08	0.08	0.05	
RI p-value		[0.023]	[0.058]	[0.470]	[0.632]
Digit span: backward, indexed	0.00	0.07	0.05	0.08	
RI p-value		[0.033]	[0.194]	[0.162]	[0.702]
Executive function test, indexed	0.01	0.05	0.06	0.03	
RI p-value		[0.170]	[0.193]	[0.654]	[0.715]
Panel C. Economic self-perception					
Perceptions of economic status index	0.01	0.20	0.20	0.09	
RI p-value		[0.000]	[0.000]	[0.228]	[0.190]
Self-reported economic status	3.06	0.44	0.45	0.22	
RI p-value		[0.000]	[0.000]	[0.187]	[0.184]
Projected economic status in five years	5.73	0.36	0.38	0.16	
RI p-value		[0.000]	[0.003]	[0.386]	[0.303]

Notes: Each cell in column 2 is from a single specification estimating the Intention to Treat treatment effect, coefficient Beta 1 in equation (1) in the main text. Each regression contains between 7,218 and 7,227 observations. Each row for columns 3 and 4 are from a single specification with between 6,778 and 6,787 observations, which include dummy variables for baseline psychological distress and interactions of being randomized into the CBT program interacted with whether the individual had psychological distress at baseline, coefficients Beta 1 and Beta 2, respectively, in equation (2) in the main text. Column 5 reports the *p*-value from the test that the coefficients in columns 3 and 4 are equal. All *p*-values (in each of columns 2, 3, 4, and 5) are calculated via randomization inference, in which we rerun our full randomization procedure to assign placebo treatments and compare our true estimates to the placebo distribution of estimates; the full procedure is described in online Appendix B.

and compare the distribution of coefficients (and differences in coefficients for measures of heterogeneity) from these placebo assignments to our coefficients from our true treatment status. Our "RI *p*-values" report the results of this procedure.[20] Our analysis strategy was not preregistered.

[20] This procedure is described in greater depth in online Appendix B.

Table 3 reports effects of the CBT intervention on mental and perceived physical health outcomes. We find that CBT leads to large improvements in both domains. We estimate a statistically significant 0.15 standard deviation improvement in our mental health summary index. Breaking this down, individuals receiving CBT have lower Kessler scores, are 10 percent (6 pp, p-value $=$ 0.004) less likely to have any psychological distress, 20 percent (6 pp, p-value $=$ 0.001) less likely to have moderate psychological distress, and 23 percent (4 pp, p-value $=$ 0.010) less likely to have severe psychological distress. Individuals also report a 10 percent reduction in the number of days with poor mental health (0.53 days, p-value $=$ 0.097) and an improvement in the BRFSS self-report on mental health.

We also estimate a 0.13 standard deviation (p-value $=$ 0.000) improvement in the index of perceived physical health and its effects on labor. This can be broken down into a 17 percent reduction in the number of days with poor physical health (0.89 days, p-value $=$ 0.001), a 4 percent improvement in the BRFSS physical health self-rating, and one-third of an additional day of labor and normal activity per month. This latter effect is reasonably large but not statistically significant.[21] Again, we note that while many of these measures (notably the BRFSS measures) have been found to correlate with real-world health outcomes,[22] our physical health outcomes are not objective health measures.

We find that the program was effective in improving mental and self-reported physical health for both distressed and nondistressed individuals. For each of the outcomes reported in Table 3, comparing treatment effects on those identified as distressed versus nondistressed at baseline, we are not able to reject equality of treatment effects at the 10 percent level (column 5); in two cases the estimates approach statistical significance (p-values $=$ 0.11, 0.17), but even in these two cases the treatment effect is *larger* among individuals scored as "well" at baseline. Perhaps more importantly, we consistently reject the null that there are no impacts of CBT on mental and physical health outcomes for both subgroups (8 of 11 outcomes both for those distressed and not distressed at baseline). This is consistent with the idea that some not distressed went on to become distressed (or would have) and hence CBT was valuable for them and also that some distressed individuals would have recovered regardless of the intervention.

Table 4 tests our hypothesis that CBT can improve the allocation of bandwidth and hence the socioemotional skills of low-income individuals. Panel A shows that the treatment led to a 0.27 standard deviation improvement in our index of socioemotional skills. The CBT program led to improvements in all three submeasures: generalized self-efficacy, grit, and self-control. In panel B we see a modest but statistically significant 0.08 standard deviation increase in the cognition index. This smaller effect is consistent with the perceived wisdom that cognitive skills are harder to move in a sample of adults. We observe statistically significant positive treatment effects on two submeasures of cognitive performance: the forward and backward digit span tests. We are unable to reject the null of no impact on Raven's Progressive

[21] This likely reflects the fact that measure has relatively high "leverage" in our randomization inference, with many 0s and some 30s).

[22] See, for example, Case and Deaton (2020) or Idler and Benyamini (1997), the latter of which documents that health self-report questions predict mortality in 27 countries even after controlling for objective health measures.

Matrices or a Stroop test. Once again we find that CBT led to improvements on these measures for individuals both with and without baseline distress. We also see little evidence of heterogeneity by gender.

Panel C shows effects on economic self-perceptions. We find a statistically significant 0.20 standard deviation improvement in perceived economic status. Breaking this down, two mechanisms through which depression has been hypothesized to affect economic productivity are through increasing the psychic cost of effort and through distorted (negative) thoughts about the future. We find evidence of improvements in the second domain but are unable to reject the null of no improvements in labor supply as a result of the program. In particular, individuals report expecting to be 0.36 (p-value $=$ 0.000) points higher on a ten-point economic Cantril Ladder in five years' time. On average, individuals report 0.34 fewer days in which poor mental or physical health kept them from engaging in their regular activities (Table 3, panel B), including work and self-care, but this result is not statistically significant in our randomization inference procedure (p-value $=$ 0.160). There is some evidence here that impacts are concentrated among the subsample with psychological distress at baseline. For example, on our measure of days in which poor health kept individuals from engaging in their regular activities, we observe a treatment effect of 0.47 days (p-value $=$ 0.101) for those with distress and -0.003 (p-value $=$ 0.995) for those without, although this difference is not statistically significant at conventional levels. For none of these outcomes are we able to reject the null of equal treatment effects, but our summary index's treatment effects are concentrated among individuals with psychological distress at baseline. Again, there is little to suggest heterogeneity by gender.

Online Appendix Tables 6 and 7 repeat the above analysis, testing for heterogeneity by gender.[23] We are uniformly unable to reject the hypothesis that treatment effects are the same for men and women.[24] Moreover, for both genders, we are able to reject the null of no treatment effects for both the mental health and perceived physical health indices, suggesting that the effects are not concentrated among either gender.

IV. Conclusion

We find that a CBT program delivered by nonspecialist providers in a low-income population in Ghana reduces psychological distress, improves self-reported mental and physical health, increases cognitive and socioemotional skills, and improves short-term self-perception of economic status. We argued that the results, albeit measured at a short-time horizon of one to three months postintervention, are suggestive of a bipartite expansion of the domain of applicability for CBT: the poor are vulnerable to mental health problems and CBT can successfully inoculate a broad proportion of the population against the possibility of future mental health problems, and the poor can generally benefit from CBT whether they have mental health problems or not because CBT improves bandwidth allocation and hence increases socioemotional and cognitive skills.

Our results also corroborate previous work (e.g., Singla et al. 2017) showing that therapy can be delivered successfully by nonspecialist providers in low-income

[23] We use the same specification as in equation (2) (i.e., with gender in place of distress level).

[24] Rates of baseline distress for men and women are 43 and 47 percent, respectively.

countries. We show that this pattern holds in a large sample when delivered via a group program to a general low-income population rather than targeted at a specific form of mental illness.

We suggest further research to determine whether impacts persist in the long run and if impacts fade what strategies may prevent fading. Furthermore, we suggest that further work aim to understand how such programs may (or may not) produce complementarities if implemented alongside economics-focused programs. Lastly, although this was implemented at scale, we suggest that further operational work could prove important for establishing operational guidance for training and implementing at scale and in other contexts.

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