Can Positive Psychology Improve Psychological Well-Being and Economic Decision-Making? Experimental Evidence from Kenya

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I. Introduction

Psychological well-being has become an increasingly important factor in understanding the persistence of poverty. As a first-order welfare outcome, poor mental health is considered one of the most burdensome noncommunicable diseases in the developing world (Kessler et al. 2009; Collins et al. 2011) and is associated with a host of detrimental economic consequences, including impaired human capital development (Currie and Stabile 2006; Heckman, Stixrud, and Urzua 2006; Currie 2009; Krishnan and Krutikova 2013) and reduced productivity (Kessler and Frank 1997; Currie and Madrian 1999; World Health Organization 2013; Frijters, Johnston, and Shields 2014).[1] Furthermore, there is growing evidence that psychological factors associated with poor mental health can lead to poverty traps through suboptimal economic decision-making (Haushofer and Fehr 2014; Genicot and Ray 2017). This evidence is particularly concerning in light of the fact that mental health care is underprovided among low-income populations (World Mental Health Survey Consortium 2004).

Targeted psychotherapy-based interventions have shown promising results, capable of improving mental health even in low-income settings (Bolton et al.

The authors thank James Reisinger, Matthew Cohen, and Lucy Rimmington for excellent research assistance and James Vancel and the staff of the Busara Center for Behavioral Economics for implementation. The authors are grateful to Leslie Martin, Sendhil Mullainathan, Tom Wilkening, and Jenny Williams for their comments. This research was supported by the Faculty of Business and Economics at the University of Melbourne and NIH R01AG039297 (Common Fund). Contact the corresponding author, Victoria Baranov, at victoria.baranov@unimelb.edu.au.

[1] Economists now routinely measure mental health as an outcome of poverty alleviation programs. For example, see the literature on the effects of ultrapoor programs (Banerjee et al. 2015) and studies documenting the effects of cash transfers on mental health (Baird et al. 2013; Kilburn et al. 2015; Haushofer and Shapiro 2016).

Electronically published June 8, 2020
 0013-0079/2020/6804-0007$10.00

2003; Rahman et al. 2008) and affecting sustained behavioral change. For example, interventions based on cognitive behavioral therapy have reduced antisocial behavior among high-risk populations in Liberia and the United States (Heller et al. 2016; Blattman, Jamison, and Sheridan 2017) and have increased empowerment and human capital investment among perinatally depressed mothers in Pakistan (Baranov et al. 2020). However, such interventions are resource intensive and difficult to scale.

A large and growing literature in psychology argues that simple, light-touch psychological interventions, described collectively as positive psychology interventions, are also effective at improving psychological well-being, particularly in subclinical populations (Seligman et al. 2005; Meyers, van Woerkom, and Bakker 2013; Cohen and Sherman 2014). Positive psychology broadens the focus of clinical psychology beyond mental illness to overall well-being and optimal functioning (Duckworth, Steen, and Seligman 2005). A recent meta-analysis based on 39 randomized studies totaling 6,139 participants found that positive psychology interventions improved subjective well-being by 0.34 standard deviations (SD) and reduced depression symptoms by 0.23 SD (Bolier et al. 2013). Since 2013, the National Institutes of Health has provided more than US$6 million in research funding toward incorporating positive psychology interventions in clinical settings from heart disease to diabetes, and 27 clinical trials enrolling a total of 3,179 individuals have been registered online at clinicaltrials.gov. Due to their low cost and ease of administration, positive psychology interventions have been incorporated into workplaces (Meyers, van Woerkom, and Bakker 2013), schools (Shankland and Rosset 2017), and clinical settings (Chakhssi et al. 2018).[2] These interventions are of great interest because they can be easily adapted and deployed in developing country settings.

This study investigates whether a light-touch and low-cost positive psychology intervention can improve psychological well-being and ultimately change behaviors and decisions that can affect the perpetuation of poverty. We deploy three of the most successful protocols from the psychology literature in a developing country setting. The combined intervention provides daily exercises to promote gratitude, self-affirmation (SA), and aspirations priming (AP).[3] This intervention targets belief formation to improve individuals' beliefs about

[2] Several studies in economics have now found that modest interventions that engender a sense of hope can lead to surprising improvements in economic outcomes (Bernard et al. 2014; Lybbert and Wydick 2016; Riley 2017).

[3] The AP component was not derived from the psychology literature but rather from recent literature in economics (Ray 2006; Bernard, Dercon, and Taffesse 2012; Macours and Vakis 2014). Aspirations summarize the preferences, the beliefs, and possibly the constraints acknowledged by an individual about aspects of the future (Bernard and Taffesse 2012).

their life in general, about their own abilities, and about their potential.[4] In contrast to experiments that have induced a scarcity mindset by reminding individuals of their weaknesses (e.g., reminding the poor of their financial demands; see Mani et al. 2013), the gratitude and SA exercises aim to shift individuals' mindsets to focus on their strengths or assets.

We implement the intervention in a field setting in Nairobi, Kenya, using a randomized controlled trial with 220 participants. Participants received daily short message service (SMS) reminders and in-person field visits several days into the experiment to ensure that they understood and completed the exercises. We measure a number of psychological well-being outcomes, including self-reported happiness, subjective well-being, depression symptoms, positive and negative affect, and sleep quality. Because the intervention was aimed at improving an individual's general sense of optimism and self-efficacy, we also collect reported beliefs, intentions, and aspirations. In addition, we assess the effect of the intervention on decision-making using two real-incentive tasks: time preference using a multiple price list (MPL) and cognitive control using the Stroop task.

We find that the intervention worked as intended with regard to improving gratitude: reported feelings of gratitude increased by 0.31 SD. However, we find weak evidence that the program affects our other outcomes; none of the prespecified indexes (psychological well-being, sleep quality, aspirations, beliefs, or intentions) or individual measures show significant effects. This finding contrasts with similar interventions in developed countries, which have produced large effects; for example, Hall, Zhao, and Shafir (2014) report an increase of 0.45 SD in cognitive control after a SA intervention, and recent meta-analyses of positive psychology interventions find effects of 0.34–0.61 SD on subjective well-being (Sin and Lyubomirsky 2009; Bolier et al. 2013). Importantly, our study was highly powered compared with existing ones; for example, we estimate the effect on subjective well-being with greater precision than any study in the meta-analysis by Bolier et al. (2013).

We also find that decision-making was unaffected. We find no effect on real-incentive tasks that measure temporal discounting or cognitive control. In addition, in a post hoc analysis, we checked for long-run effects on a measure of labor supply using administrative data. Our results indicate that, if anything, the

[4] Lybbert and Wydick (2018) develop an economic framework that links aspirations and positive psychology in what they refer to as hope. The article also contains a thorough review of the psychology literature (primarily citing the works on positive psychology upon which our intervention is drawn) and provides a framework for economists to engage in experimental work seeking to understand the role of hope in economic development. Our intervention, largely based on positive psychology, is precisely in line with testing if one can manipulate "hope" to affect well-being and decision-making.

intervention had a slightly negative impact on our measure of labor supply up to 3 years after our endline survey, and we rule out even small positive impacts.

Together, these results raise doubts that light-touch psychological interventions will be an easy and affordable way to improve well-being or decision-making in developing countries. Our findings contribute to an emerging literature on the effects of psychological interventions on economic outcomes, especially among disadvantaged populations and in developing countries. Our results contrast with those of Ghosal et al. (2013), who provided eight sessions of self-esteem training to sex workers in India and found strong increases in psychological well-being as well as in saving and health-seeking behavior. Aspirations interventions consisting of one-off screenings of videos or movies have also yielded positive results: Bernard et al. (2014) find increases in educational investment in Ethiopia, whereas Riley (2017) finds improvements in math performance in Uganda. Yet in our context, we find no effects above and beyond an increase in reported gratitude.

II. Intervention

The goal of our intervention was not to evaluate any one particular protocol but rather to find a light-touch intervention that has a measurable impact on psychological well-being and decision-making in a general population. Based on a review of the literature (see app. B [apps. A and B are available online] for a listing of studies in psychology for Count Your Blessings [CYB] and SA), we combined three of the most successful light-touch interventions into a single intervention adapted to the Kenyan context: CYB, SA, and AP.

Based on positive psychology, CYB interventions, which ask participants to write or think about things they are grateful for, have been shown to increase life satisfaction, positive affect, and subjective happiness and decrease depression, negative affect, and negative health symptoms (Emmons and McCullough 2003; Seligman et al. 2005; Sheldon and Lyubomirsky 2006; Froh, Sefick, and Emmons 2008; Geraghty, Wood, and Hyland 2010a, 2010b; Martìnez-Martì, Avia, and Hernández-Lloreda 2010; Wood, Froh, and Geraghty 2010; Chan 2013; Odou and Vella-Brodrick 2013; Chancellor, Layous, and Lyubomirsky 2015), although some studies have produced contradictory results or have been difficult to replicate (Bolier et al. 2013).[5] The majority of studies with positive and significant results asked participants to count their blessings daily for at least 13 days. This is the approach we adopt here.

[5] The psychological mechanisms responsible for the beneficial effects of gratitude can be explained by the Broaden-and-Build Theory (Fredrickson 2001), which claims that positive emotions broaden the repertoires of thought-action momentarily and build enduring personal resources.

An SA is an act that demonstrates one's adequacy in the face of threats to one's self-integrity (Harris and Epton 2009). In SA interventions, people typically write about core personal values, where personal values are the internalized standards used to evaluate the self. Many SA interventions have been shown to increase the grade-point average of minority students, the acceptance of unwelcome risk feedback or information, and cognitive control and fluid intelligence (Cohen et al. 2009; Harris and Epton 2009; Howell and Shepperd 2012; Sherman et al. 2013; Hall, Zhao, and Shafir 2014). Successful interventions asked participants either to write about traits they value about themselves or a personal event important to them.

The third element of our intervention was an AP exercise. Aspirations do not directly relate to psychological well-being but rather relate indirectly through the concept of self-efficacy (Bandura 1989; Locke and Latham 2002), that is, "the belief in one's capabilities to organize and execute the courses of action required to produce given attainments" (Bandura 1977:2).[6] Recent studies suggest that aspirations can be "raised" using simple methods such as showing "aspirational" videos or cartoons, particularly in developing country settings (Bernard et al. 2014; Macours and Vakis 2014). These studies further show effects on behavior. We included the AP exercise because it was closely linked to hope and positive psychology (see Lybbert and Wydick 2018).

Using the subject pool of the Busara Center for Behavioral Economics, we recruited 220 participants from Kibera, an informal settlement in Nairobi, Kenya, to participate in the study.[7] We selected exclusively primary school graduates (completed Standard 8) to ensure that participants were literate, and a screening literacy test was conducted to ensure the participants could read and write. Participants were told that they were invited to participate in a study evaluating the effects of psychological exercises on well-being. The psychological intervention consisted of three exercises, which were combined into a single intervention to create the treatment condition. The three elements of the treatment are described as follows:

1. **Count Your Blessings (CYB):** This protocol was administered daily for 16 days. All participants were given a packet to be filled out each day. Treatment participants were asked to complete the CYB task daily,

[6] Self-efficacy refers to a person's belief about his or her own ability to achieve certain outcomes (e.g., the probability of success conditional on taking an effortful action). A related concept is locus of control, which refers to the extent to which individuals believe they can control events that affect them (i.e., the component of the probability of success that can be affected by an action).

[7] We recruited 220 participants; however, one individual declined.

instructing them to write down the five things in their life that they were most grateful or thankful for over the past day. The control condition was a pure control and did not ask participants to write anything. Both control and treatment participants completed an identical series of 10 questions (the "daily survey"), comprising basic physical health assessments including sleep quality and exercise, ratings of mood, appraisals of the day, expectations for tomorrow, perception of social support received, and a well-being rating derived from 30 affect terms of commonly occurring affective states (Emmons and McCullough 2003).

2. **Self-affirmation (SA):** This protocol was conducted once only at endline. All participants completed two writing exercises. Participants in the treatment condition were first instructed to spend 3–5 minutes writing in depth about a personal experience during which they felt very successful or proud. Following this, the same participants were then instructed to spend 3–5 minutes writing in depth about a selected value that was important to them, describing why it was so important. Options included athletic ability, music, religious values, and sense of humor. Participants in the control condition were first instructed to write about their daily routine on a typical day and then instructed to write about what they had eaten or drunk in the past 48 hours. The protocol was derived from McQueen and Klein (2006) and Cohen et al. (2009).
3. **Aspirations:** This protocol was conducted once only at endline. All participants read short stories based on real individuals from Kibera and similar informal settlements (the stories were read aloud by a Busara survey administrator, and participants were asked to follow along). In the treatment condition, the stories were about successful individuals, whereas in the control condition, the story was about someone's daily routine. After the readings, participants in both conditions were asked to think about the future: the treatment group was asked to think about the year ahead, whereas the control group was asked to think about tomorrow. The participants then had 5 minutes to write down anything that came to mind about the future. The protocol was adapted from that used in Bernard et al. (2014).

During the baseline survey, participants received a packet with a pen and notepad containing the daily surveys followed by (for the treatment group) a writing section for the CYB exercises. Assessors read the instructions out loud and asked participants to take out the packet and complete the CYB writing task. Assessors checked that all five sections of the forms were filled out and that items listed were legible. Participants were then told that they would receive an

SMS reminder to fill out the form every day.[8] They learned that a follow-up visit would be scheduled 2 days following the baseline. Participants were also told that sheets would be collected at endline, and they were incentivized to complete the study with a payment of KES 500 (US$5). During the field follow-up visit, assessors visited the participants in person to check that the daily sheets and writing tasks were being completed correctly. Visits lasted 20 minutes. After approximately 14 days following the field visits, the assessors called participants to confirm the appointment for endline, at which point the daily sheets would be collected.

Nearly all of the respondents who completed endline had completed and returned at least 14 of 16 daily sheets. In Section V.B, we analyze the written entries to show that respondents followed the CYB protocol and completed the exercises.

At endline, the SA and aspirations treatments were completed using pen and paper prior to the endline survey. The survey team handed out booklets containing instructions for the SA exercises along with writing space, and the printed stories for the aspirations protocol again followed by writing space (the exact protocols are provided in app. B). The survey team read the SA exercises and aspirations stories out loud, asking the participants to follow along and then provided 5 minutes for reflective writing. The SA and aspirations protocols took approximately 25 minutes to complete, and writing sheets were collected before beginning the computer-administered endline questionnaire.

III. Evaluation Design

A. Sampling, Identification Strategy, and Data Collection

To establish a causal relationship between treatment and outcomes, this study uses a randomized design. A total of 220 participants were recruited from the subject pool of the Busara Center for Behavioral Economics in Nairobi, Kenya, with 120 participants randomly assigned to the treatment condition and the other 100 participants to the control condition.[9] The subject pool consists of residents of Kibera, a large slum outside Nairobi. We specifically chose to run our intervention in this setting because we wanted to test if a low-intensity psychological intervention could benefit low-income and non-Western populations. Because slum residents are often detached from extended family and

[8] The SMS reads, "This is a reminder to fill out your daily survey. Please fill it out before you go to bed this evening."

[9] The different numbers between treatment and control groups arose from the fact that the project had to finish before the 2014 Christmas break in Kenya, at which point 100 control and 120 treatment participants could complete the study. The original target enrollment had been 120 participants per group.

under considerable stress from poverty, pollution, and overcrowding, residents of slums are especially at risk for developing psychological disorders (Subbaraman et al. 2014). This is also important because urbanization in developing countries has led to the rapid growth of slums.

Data were collected in the lab at baseline and at endline after the end of the intervention. In addition, some data related to the CYB portion of the intervention were collected daily via surveys that respondents filled out at home and returned at endline. Finally, we use administrative data on participation in other studies at Busara from 2012 to 2017.

B. Power Calculations

The sample size of 120 participants in the treatment group and 100 in the control group allowed us to detect an effect size of 0.38 SD with 80% power. Both our target sample size and our eventual sample size after attrition are larger than the average sample size in the psychology literature for CYB or SA interventions (where the average starting sample size is about 150, see app. B). Based on meta-analyses of the positive psychology interventions (such as CYB), effect sizes for subjective well-being ranged from Cohen's *d* of 0.34 in Bolier et al. (2013) to 0.61 in Sin and Lyubomirsky (2009). Of the 39 studies in Bolier et al. (2013), 26 were self-help studies, and 17 involved similar gratitude or positive writing exercises similar to our intervention. Primary outcomes in the SA literature have been performance in school (Cohen et al. 2009; Sherman et al. 2013) and cognitive control, with effects sizes on cognitive control being 0.45 SD (Hall, Zhao, and Shafir 2014).[10]

In addition, we report minimum detectable effect (MDE) sizes for each outcome, which reflect power using in the observed sample at endline. Because we measure key outcomes before and after the intervention, we are able to offset some of the loss of power due to attrition. For example, using the observed sample at endline, the minimum detectable effect for subjective well-being is 0.3 SD. We discuss the MDEs for other outcomes, particularly with respect to previous studies, in Section V.E.

C. Outcomes

A detailed list of all outcomes and measures used, as well as when they were measured (either in the lab at endline or from the daily sheets), is provided in appendix A. Conceptually, we grouped our measures into three broad domains: (1) psychological well-being, (2) beliefs and aspirations, and (3) intentions and decision-making.

[10] Secondary outcomes for interventions targeted at specific populations included satisfaction in school, self-reported health behaviors, and self-reported physical activity.

We measure a diverse set of outcomes related to psychological well-being, including subjective happiness, life satisfaction, depression symptoms, positive/negative affect, and gratitude. We also collected data on outcomes closely related to psychological well-being that are also present in many of the studies upon which our intervention is based. These secondary measures of psychological well-being include items such as sleep quality, exercise, locus of control, and expectations for tomorrow.

To explore the mechanisms by which the intervention might have an impact on behavior, we measured individuals' beliefs and aspirations. For beliefs, we asked about the ability and trustworthiness of participants' own and other ethnic groups. These variables were designed to capture probabilistic beliefs about things that are not under the control of the individual. For aspirations, we measured the difference between desired and actual income, assets, social status, and education (following Bernard and Taffesse 2012).

To see if the intervention could have an impact on behavior, we asked individuals about their intentions: for example, intentions to reduce consumption of temptation goods such as sweets and sugary drinks, intentions to spend money wisely, and intentions to foster a good relationship with their spouse. We also asked about their willingness to take a human immunodeficiency virus (HIV) test. Willingness to take an HIV test is included as an outcome measure because despite the health benefits of HIV screening, the potentially threatening nature of results and fear of stigma have led to underutilization of testing in Kenya (Ng'ang'a et al. 2014). Previous research has suggested that interventions such as the one we test here can reduce risk-avoidance behavior (Howell and Shepperd 2012; Hall, Zhao, and Shafir 2014).

We also assessed whether the intervention could have an impact on decision-making through two incentivized tasks: one measuring cognitive control (using the Stroop task) and one measuring temporal discounting (using an MPL).[11] We measure these outcomes for several reasons. First, high temporal discounting and low cognitive control have been systematically correlated with poverty (Mullainathan and Shafir 2013; Carvalho, Meier, and Wang 2016), and it is hypothesized that poor psychological well-being may moderate the negative effects of poverty on cognitive control (Shah, Mullainathan, and Shafir 2012; Mullainathan and Shafir 2013) and temporal discounting (Haushofer and Fehr 2014). Second, studies have shown that individuals with poor psychological

[11] The MPL was elicited with choices between today and three different future horizons from 2, 4, and 8 weeks and one choice with a front-end delay (a choice between receiving a payment in 2 weeks vs. 4 weeks). We calculated the implied exponential discount factor (assuming linear utility or risk neutrality) using all four time preference choices. We present results using the average discount factor across these four choices in the main analysis but also show effects for the individual horizons in table A20.

health or even negative mood are more impatient (e.g., Ifcher and Zarghamee 2011 show that experimentally induced changes in mild positive affect have an impact on present bias and temporal discounting) and have lower cognitive control (McDermott and Ebmeier 2009). A large literature on positive affect has consistently shown that experimentally induced positive affect has an impact on a broad range of decision-making areas like problem solving, flexible thinking, and creativity (see Isen 2008 and Lerner et al. 2015 for reviews). Furthermore, theoretical work in psychology has postulated that the emotional state of gratitude reduces impatience, given its role in maintaining social relationships through encouragement of costly reciprocal responses (Dickens and DeSteno 2016). In laboratory experiments, inducing gratitude has been shown to attenuate temporal discounting (Desteno et al. 2014).

Given the hypothesis that poor mental health leads to suboptimal economic decision-making in a way that reinforces the cycle of poverty, ideally we would measure labor supply, work effort, or earnings. Within the time frame of the follow-up, just 2 weeks after baseline, these measures would not likely be affected. To address this issue, we include one additional post hoc outcome: attendance (a partial measure of labor supply) as experimental subjects at Busara, measured for a total of 3 years posttreatment (2015–17). Although this is not a complete measure of labor supply, it has several advantages. First, these administrative data are without measurement error; second, we observe this outcome even if the individual did not complete the endline survey, and thus it is not subject to attrition concerns; and third, it allows us to explore longer-term impacts in the case that the benefits of the intervention took time to translate to behavioral change.

For the empirical analysis, we generate summary indexes (following Anderson 2008) for the following groups of outcomes: psychological well-being, sleep quality, beliefs, aspirations, and intentions. We analyze the real-incentive tasks separately without grouping within an index. Table A1 (tables A1–A20 are available online) shows the summary statistics for the five indexes (indexes were normalized to be mean 0 and 1 SD in the control group) and their components.

IV. Econometric Specifications

A. Basic Specification

The study design and all analysis methods were preregistered before data analysis began (https://www.socialscienceregistry.org/trials/627). Our basic treatment effects specification estimates the following equation:

$$y_{i,t=15} = \beta_0 + \beta_1 T_i + \varepsilon_{i,t=15}, \tag{1}$$

where $y_{i,t=15}$ is the outcome of interest for individual i measured at the endline survey, 15 days after the intervention began, $t = 15$. The dummy variable T_i is equal to 1 if the participant was randomly assigned to the treatment condition

and 0 if assigned to the control. The variable $\varepsilon_{i,t=15}$ is the unobserved error component, which is assumed to be serially uncorrelated. Where possible, we control for baseline levels of the outcome variables, $y_{i,t=0}$, to improve statistical power (McKenzie 2012), thus estimating $y_{i,t=15} = \beta_0 + \beta_1 T_i + \delta y_{i,t=0} + \varepsilon_{i,t=15}$.

B. Accounting for Multiple Inference

To account for multiple inference, we follow three approaches. First, we define summary indexes for psychological well-being and decision-making and ask whether these indexes are affected by treatment. The summary indexes are generated by standardizing all outcomes within a group and then taking a weighted average of all outcomes with weights generated by the inverse of the covariance matrix. As such, it is a generalized least squares (GLS)–weighted index and, thus, places more weight on uncorrelated information. It is analogous to running a seemingly unrelated regression (SUR) on all outcomes (standardized) jointly within a group and restricting the coefficient on treatment to be equal across outcomes. Without any additional assumptions on the measures, this is the most efficient test of whether there is a treatment effect across a group of outcomes. Second, for individual outcome variables within the indexes as well as across indexes, we adjust for multiple inference using a family-wise *p*-value adjustment following Anderson's (2008) variant of Efron and Tibshirani's (1994) nonparametric permutation test. Finally, we also run an SUR across all groups of outcomes to test for the joint significance of all the indexes.

V. Results

A. Balance

Table 1 presents the baseline characteristics for the control group (col. 1) and the coefficient and standard error on treatment in a regression of the characteristic on the treatment dummy (col. 2). Only the score on Raven's Matrices (a measure of cognitive function) was statistically different between the two groups with the treated group having 0.36 fewer correct answers. Overall, the randomization was successful and the sample was well balanced, with a joint test *p*-value of .87 across all available baseline characteristics (table A2).[12]

[12] In table A2, we also include a number of baseline measures that were filled out in the practice daily forms during the baseline survey when participants were asked to complete the CYB writing task (starting with Positive and Negative Affect Schedule [PANAS] Positive Total). Completing the daily questions was not required in the baseline practice, though approximately 100 of these forms were still filled out and returned at endline. This explains why there are fewer observations for these measures at baseline. The index we compute uses all available data (it does not drop observations with missing data, treating missing outcomes similarly to an unbalanced panel regression with missing years) but places more weight on outcomes that have more complete data.

TABLE 1
BASELINE BALANCE

	Surveyed at Baseline			Surveyed at Endline		
	Control Mean (SD) (1)	Treatment (2)	*N* (3)	Control Mean (SD) (4)	Treatment (5)	*N* (6)
Attrition	.28	−.09	219	0	0	168
	(.45)	(.08)		(0)	(0)	
Female subject	.63	0	218	.58	.04	167
	(.49)	(.05)		(.50)	(.06)	
Age	32.17	.46	218	32.59	.99	167
	(9.10)	(1.48)		(9.66)	(1.66)	
Years of education	10.89	−.11	218	11.04	−.37*	167
	(2.03)	(.27)		(1.98)	(.18)	
Unemployed	.24	.03	218	.23	.04	167
	(.43)	(.06)		(.42)	(.06)	
Married	.48	−.10*	218	.46	−.10	167
	(.50)	(.05)		(.50)	(.07)	
Number of children	1.68	.15	218	1.77	.18	167
	(1.61)	(.22)		(1.70)	(.24)	
Cognitive function (Raven's)	0	−.36**	219	.10	−.48***	168
	(1.00)	(.13)		(.98)	(.12)	
WVS happiness	0	−.23	219	−.04	−.19	168
	(1.00)	(.15)		(.93)	(.14)	
WVS life satisfaction	0	−.13	219	−.04	−.14	168
	(1.00)	(.13)		(1.00)	(.16)	
SWLS life satisfaction scale total	0	.25	219	−.09	.35*	168
	(1.00)	(.20)		(.94)	(.18)	
GQ-6 gratitude scale total	0	.04	219	.02	.12	168
	(1.00)	(.08)		(.97)	(.12)	
WVS locus of control	0	.00	219	−.04	−.02	168
	(1.00)	(.15)		(.98)	(.12)	
LOTR life orientation total	0	−.12	219	.03	−.10	168
	(1.00)	(.17)		(1.01)	(.18)	
CES-D total	0	.16	219	0	.08	168
	(1.00)	(.18)		(1.00)	(.17)	
Psych well-being index	0	−.03	219	−.07	.09	168
	(1.00)	(.21)		(.94)	(.21)	
First time as Busara subject	.36	−.11*	219	.34	−.06	168
	(.48)	(.06)		(.48)	(.07)	
Number of Busara sessions (prior to this experiment)	2.64	.44	218	2.75	.33	167
	(2.37)	(.32)		(2.34)	(.37)	

Note. Difference in key demographics and baseline outcome variables between treated and nontreated individuals, estimated with an ordinary least squares (OLS) regression of variable of interest on the treatment dummy. Demographics and outcome variables are listed on the left. Binary outcomes are left unadjusted, whereas all nonbinary outcomes and indexes are always normalized to be mean 0, 1 SD, in the control group. Columns (1)–(3) report the results for all individuals present at baseline. Columns (4)–(6) report the results only for individuals present at endline. Columns (1) and (4) report the mean of the control group at baseline. Columns (2) and (5) report the mean of the treatment group at baseline. Columns (3) and (6) report the sample size at baseline and endline, respectively. For outcome measures reported by respondents on each day for 16 days, "baseline" refers to the result reported on day 1 and "endline" refers to the result reported on day 16. WVS = World Values Survey; SWLS = Satisfaction with Life Scale; GQ-6 = Gratitude Questionnaire Six-Item Form; LOTR = Revised Life Orientation Test; CES-D = Center for Epidemiologic Studies Depression Scale.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

Balance is also achieved in the analytical sample (excluding individuals lost to follow-up) presented in columns 4–6. We note that, as attrition was not statistically differential and because our sample was well balanced at baseline, we would expect that the analytical sample would also remain balanced, which is what we find (the joint test has a p-value of .65). We do note that although the sample is not balanced along the dimension of cognitive function, our results are qualitatively unchanged if we include this variable in the baseline controls (see tables A13–A15).

Attrition. Of the 219 respondents who participated in the baseline survey, 168 participated in the endline survey, an attrition rate of 23%. Attrition was 9 percentage points ($p = .11$) higher among the control group. Due to the timing of our study being conducted near the Christmas holidays, we observed higher attrition rates than anticipated as participants left the city to visit family during the holiday season. Eight participants also returned their packets (after the holiday break) but did not complete endline (including these participants in the nonattritors, attrition was 7 percentage points [$p = .22$] higher among controls). Tests of whether attrition was differential by treatment status, whether attritors were different based on baseline characteristics, or whether characteristics of attritors varied by treatment status all revealed no statistical patterns (at $\alpha = 0.05$).

To assess whether attrition confounds our results, we proceed as follows: First, we define $\text{attrit}_i = 1$ if individual i was surveyed at baseline but not at endline and zero otherwise. We then assess the severity of attrition using two approaches. First, we assess whether attritors are different in terms of a vector of baseline characteristics $y_{i,t=0}$ by estimating $y_{i,t=0} = \beta_0 + \beta_1 \text{attrit}_i + \varepsilon_{i,t=0}$. Next, we measure whether the baseline characteristics of attritors in the treatment group are significantly different from those in the control group. The sample for regression is restricted to attritors: $(y_{i,t=0} | \text{attrit}_i = 1) = \beta_0 + \beta_1 T_i + \varepsilon_{i,t=0}$. A comparison of baseline characteristics, shown in table A3, reveals that attritors were generally similar to nonattritors. Attritors were slightly younger, had fewer children, and had a lower gratitude scale. Table 2 shows that among the attritors treatment assignment was uncorrelated with baseline characteristics with the exception of gratitude, which was marginally significantly lower for attritors assigned to treatment. We fail to reject the null hypothesis that attritors from treatment and control groups are similar across the full set of characteristics (p-value of the joint test is .63). However, the fact that attritors from the treatment arm had a lower gratitude scale by 0.34 SD is concerning as this could bias our findings to appear as if the intervention improved gratitude when in fact it did not. To address potential bias due to attrition, we estimate treatment effects with

TABLE 2
BASELINE DIFFERENCE BETWEEN TREATED AND NONTREATED ATTRITORS

	Control Mean (SD) (1)	Treatment (2)	*N* (3)
Female subject	.75	−.10	51
	(.44)	(.12)	
Age	31.11	−2.45	51
	(7.56)	(2.23)	
Years of education	−.35	.27	51
	(.95)	(.35)	
Unemployed	.29	.02	51
	(.46)	(.09)	
Married	.54	−.06	51
	(.51)	(.10)	
Number of children	1.43	−.17	51
	(1.35)	(.36)	
Cognitive function (Raven's)	−.25	−.02	51
	(1.03)	(.23)	
WVS happiness	.09	−.32	51
	(1.17)	(.28)	
WVS life satisfaction	.09	−.03	51
	(1.02)	(.18)	
SWLS life satisfaction scale total	.23	−.02	51
	(1.13)	(.37)	
GQ-6 gratitude scale total	−.05	−.34*	51
	(1.08)	(.18)	
WVS locus of control	.11	.14	51
	(1.07)	(.36)	
LOTR life orientation total	−.07	−.26	51
	(.99)	(.38)	
CES-D total	.01	.50	51
	(1.01)	(.32)	
Psych well-being index	.19	−.45	51
	(1.14)	(.35)	
First time as Busara subject	.43	−.25*	51
	(.50)	(.13)	
Number of Busara sessions (prior to this experiment)	2.36	.73	51
	(2.44)	(.62)	

Note. Difference in key demographics and baseline outcome variables between treated and nontreated attritors estimated with an ordinary least squares (OLS) regression of variable of interest on the treatment dummy for attriting households only. Demographic and outcome variables are listed on the left. Binary outcomes are left unadjusted, whereas all nonbinary outcomes and indexes are always normalized to be mean 0, 1 SD, in the control group. Column (1) reports the mean of the control group conditional on attrition for a given outcome variable at baseline. Column (2) reports the baseline difference between treatment and control groups within villages conditional on attrition. Column (3) reports sample size. WVS = World Values Survey; SWLS = Satisfaction with Life Scale; GQ-6 = Gratitude Questionnaire Six-Item Form; LOTR = Revised Life Orientation Test; CES-D = Center for Epidemiologic Studies Depression Scale.
* $p < .10$.

inverse probability weights (IPWs) where the weights are calculated as the probability of observing the respondent at endline as a function of all available baseline characteristics (except those measured via the daily surveys, e.g., PANAS), estimated separately for individuals in treatment versus controls groups (Hirano,

Imbiens, and Geert 2003). We also discuss results based on attrition bounds (following Lee 2009).

B. *Manipulation Check*

We first report whether the intervention was successful in manipulating the psychological constructs it was intended to manipulate. The main component of the intervention was the CYB exercise, which has an established manipulation check in the psychology literature measured by the gratitude scale.[13] Although reported gratitude may reflect experimenter demand effects, the goal of the manipulation check is primarily to compare our results to those reported in the previous literature. We find a significant increase in gratitude, by about 0.37 SD ($p < .001$), indicating that the intervention worked as intended and the psychological manipulation was successful. Controlling for baseline gratitude, the estimate drops to 0.31 SD ($p = .01$), which is notable as attrition was slightly differential by baseline gratitude. Adjusting for attrition with IPWs, the estimated effect is nearly identical (table 7). The effect is robust to alternative specifications; for example, controlling for baseline cognitive function (which was not balanced at baseline), the estimate is 0.43 SD ($p < .001$). Furthermore, the magnitude of the effect is very similar to that reported in the psychology literature, the average increase being 0.4 SD across the five studies that reported the effect on the gratitude score (see table 5 of Wood, Froh, and Geraghty 2010 and app. B).

In addition, because we collected the daily sheets from the participants (for daily measures of sleep and PANAS), we were able to observe if individuals actually did the "five good things" daily writing exercise (i.e., the CYB component of the intervention condition). Consistent with our finding of improvements in gratitude, we found that participants did in fact complete most of the daily CYB exercises. Note that this (objective) check that the task was actually being completed was, to our knowledge, not done in any of the studies from the psychology literature. The data revealed that nearly all of the respondents actually wrote five things every day, and fewer than 7% of the treatment group completed less than half of the exercises (defined as writing less than 2.5 items on average across the 16 days). More than 85% wrote at least four items on average. The written responses varied in length and were generally completed correctly in that items were indeed things for which one could be grateful (e.g., spouse, food, religion, sunshine). The average written response was 207 characters with spaces, or about 30 words, with an interquartile range of 89–261.

[13] The SA interventions, however, do not appear to have a similar manipulation check. Meanwhile, the manipulation check for aspirations is the aspirations index, which is not statistically different from zero. In addition, the WVS Locus of Control is also related to the aspirations condition, for which we find positive but insignificant effects (0.11 SD; in table 4).

C. *Main Treatment Effects*

We next turn to the main treatment effects on five index measures presented in table 3: psychological well-being, sleep quality, beliefs, aspirations, and intentions. Column 2 reports the coefficient on the treatment dummy for the five outcome indexes with the family-wise adjusted *p*-values in brackets. The intervention had positive but statistically insignificant effects on overall psychological well-being with improvements of 0.16 SD. However, we found no improvement in sleep quality, a secondary measure of psychological well-being. For outcomes related to aspirations, beliefs, and intentions, the intervention appears to have had a perverse effect, with statistically insignificant reductions of 0.13 SD, 0.07 SD, and 0.16 SD, respectively. Thus, no single domain revealed a significant impact of the intervention even without multiple hypothesis adjustment. Not surprisingly, the joint test that the treatment had a positive effect across all domains fails to reject the null hypothesis.

Next we turn to the effects of the intervention on the individual variables within the indexes to better understand which aspects of well-being and beliefs may have been more responsive to the intervention. Table 4 reports the coefficients on the treatment indicator for the components of the psychological

TABLE 3
TREATMENT EFFECTS: SUMMARY INDEXES

	Control Mean (SD) (1)	Treatment (2)	*N* (3)
Psych well-being index	0	.16	168
	(1.00)	(.13)	
		[.63]	
Sleep quality index	0	−.06	166
	(1.00)	(.15)	
		[.87]	
Aspirations index	0	−.13	168
	(1.00)	(.09)	
		[.57]	
Beliefs index	0	−.07	168
	(1.00)	(.17)	
		[.87]	
Intentions index	0	−.16	168
	(1.00)	(.18)	
		[.76]	
Joint test (*p*-value)		.09*	
Constrained SUR coefficient		−.06	
Constrained SUR (*p*-value)		.43	

Note. Ordinary least squares (OLS) regressions of outcome variables on treatment status. Outcome variables are listed on the left. By construction, summary indexes are mean 0, 1 SD, in the control group. Column (1) reports the variable mean for the control group. Column (2) reports the coefficient from an OLS regression of the outcome variable on treatment status. Column (3) reports the sample size. Standard errors are reported in parentheses and are clustered at the endline session level. Family-wise error rate–adjusted *p*-values are reported in brackets. SUR = seemingly unrelated regression.

* $p < .10$.

well-being and sleep quality indexes. Column 2 provides point estimates from a regression without controlling for baseline levels of the outcome variable. Column 4 provides the point estimates when baseline values of the outcome variable are included as controls. Although a number of measures of psychological well-being and sleep quality are statistically significant at naive p-values, only the Gratitude Questionnaire Six-Item Form remains significant after adjustment. However, apart from gratitude, the intervention had limited effects on individual components of psychological well-being and sleep quality. It appears that the positive coefficient found in the psychological well-being index, which includes gratitude, is mostly driven by the effects on gratitude. Note that controlling for baseline cognitive function, we find that the coefficient on gratitude increases slightly (to 0.43 SD) while we continue to see no evidence of benefits to other domains of psychological well-being (table A14).

We may fail to detect true treatment effects if, by chance, our randomization did not sufficiently balance treatment and control groups. Indeed, our baseline balance table indicates that individuals in the treatment condition had significantly lower cognitive function. However, controlling for baseline characteristics that were imbalanced at baseline (education and cognitive function), our overall results remain similar though the coefficient on the psychological well-being index increases to 0.22 SD, and the negative coefficients on the rest of the outcomes are less negative (see table A13). Further including baseline measures of psychological well-being could also improve precision of these estimates; however, doing so actually reduces the coefficient on the psychological well-being index to 0.08 SD (see table A16, col. 2). We also note that our psychological well-being index includes the gratitude score. As pointed out previously, because gratitude was the direct outcome targeted by the exercises, it also makes sense to look at the psychological well-being index on all psychological well-being measures that are not directly related to the construct being manipulated. Excluding the gratitude score from the index, we see that the effect is smaller (0.10 SD without controls and 0.04 SD with controls).[14]

[14] We also check that the way we construct our summary index of psychological well-being is not driving our null findings. The GLS-weighted index puts greater weight on uncorrelated information and may be inappropriate in situations where the multiple measures are actually capturing one latent trait or construct with error. An alternative approach would be to perform factor analysis to create a factor score using all of the measures of psychological well-being. This approach would effectively place more weight on correlated information captured across the different measures of psychological well-being, essentially removing the measurement error from each measure. However, using the factor score measure of psychological well-being (while excluding the gratitude score), the point estimates are even closer to zero. Without baseline controls, the intervention increased psychological well-being by 0.07 SD, but with baseline controls that estimate is −0.02 SD (cols. 7 and 8 of table A16). Based on our estimates using factor scores, we can rule out effect sizes on psychological well-being of 0.2 SD.

TABLE 4

TREATMENT EFFECTS: PSYCHOLOGICAL WELL-BEING AND SLEEP QUALITY

	Control Mean (SD) (1)	Treatment (SE) (2)	FWER *p*-Value (3)	Treatment (SE) (4)	FWER *p*-Value (5)	*N* (6)
			A. Psychological Well-Being			
GQ-6 gratitude scale total	0	.31***	.14	.37***	0***	168
	(1.00)	(.10)		(.08)		
WVS happiness	0	−.04	1.00	−.07	1.00	168
	(1.00)	(.17)		(.16)		
WVS life satisfaction	0	0	1.00	−.06	1.00	168
	(1.00)	(.14)		(.14)		
SWLS life satisfaction scale total	0	.01	1.00	.26**	.38	168
	(1.00)	(.11)		(.10)		
PANAS negative total	0	−.16**	.59	−.06	.99	166
	(1.00)	(.07)		(.11)		
PANAS positive total	0	.06	1.00	.03	1.00	166
	(1.00)	(.13)		(.16)		
CES-D total	0	−.01	1.00	.03	1.00	168
	(1.00)	(.11)		(.15)		
			B. Sleep Quality			
Hours of sleep	0	.09	1.00	.11	.98	159
	(1.00)	(.16)		(.16)		
Woke up refreshed	0	−.12	1.00	−.12	.98	166
	(1.00)	(.19)		(.15)		
Difficulty sleeping	.24	.07*	.77	.07	.82	166
	(.43)	(.04)		(.04)		

			C. Locus of Control			
WVS locus of control	0	.12	.99	.11	.98	168
	(1.00)	(.14)		(.18)		
LOTR life orientation total	0	−.19	.88	−.23	.86	168
	(1.00)	(.12)		(.15)		
			D. Other Well-Being Outcomes			
Appraisal of day	0	.21	.89	.16	.90	166
	(1.00)	(.15)		(.13)		
Expectations for tomorrow	0	0	1.00	−.07	1.00	166
	(1.00)	(.15)		(.15)		
Helped someone today	.29	.14*	.75	.15*	.69	166
	(.46)	(.07)		(.07)		
Connection with others	0	.21*	.77	.20	.88	166
	(1.00)	(.12)		(.13)		
			E. Physical Activity			
Exercised today	.42	.13**	.47	.15**	.36	166
	(.50)	(.05)		(.06)		
Difficulty with physical activity	.28	.07	.89	.04	.98	166
	(.45)	(.05)		(.06)		
Controls for baseline		Yes		No		

Note. Ordinary least squares (OLS) regressions of outcome variables on treatment status. Outcome variables are listed on the left. Binary outcomes are left unadjusted, whereas all nonbinary outcomes and indexes are always normalized to be mean 0, 1 SD, in the control group. Column (1) reports the variable mean for the control group. Column (2) reports the coefficient from an OLS regression of the outcome variable on treatment status controlling for baseline levels. Column (3) reports the coefficient from an OLS regression of the outcome variable on treatment status without controlling for baseline levels. Column (4) repots the sample size. Standard errors are reported in parentheses and are clustered at the session level. FWER = family-wise error rate; GQ-6 = Gratitude Questionnaire Six-Item Form; WVS = World Values Survey; SWLS = Satisfaction with Life Scale; PANAS = Positive and Negative Affect Schedule; CES-D = Center for Epidemiologic Studies Depression Scale; LOTR = Revised Life Orientation Test.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

1. Comparison to Existing Meta-Analysis

A recent meta-analysis of positive psychology interventions offers an opportunity to compare the effect size and precision achieved here to those obtained in previous work. Bolier et al. (2013) compute standardized effect sizes for impacts on "subjective well-being," which includes measures such as the Satisfaction with Life Scale (SWLS) and positive affect, depending on what is measured in the individual studies. They report a meta-analytic effect of positive psychology interventions on subjective well-being of 0.34 SD. In comparison, we find an effect of our interventions on the SWLS of 0.01 SD. Figure 1 illustrates this result by plotting the treatment effects on subjective well-being, with 95% confidence interval (CI), in our study and in the 28 studies in Bolier et al. (2013) that measure this outcome. Our treatment effect is the third smallest. Importantly, our study has higher precision than all individual studies in Bolier et al. (2013). Thus, we identify a null effect on subjective well-being with higher precision than existing studies. Indeed, the attrition-adjusted MDE in our study for the SWLS measure of subjective well-being is 0.30 SD, and when calculating treatment effects using bounds, the upper limit of the 95% CI for SWLS is 0.32 SD (table A4). Thus, for subjective well-being, the attrition-adjusted CI rejects the meta-analytic effect size reported in the literature.

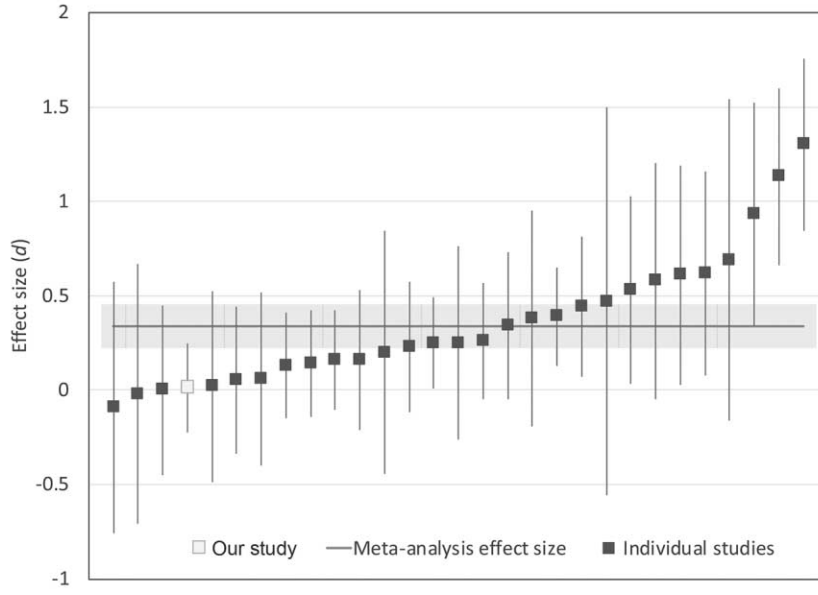

Figure 1. Treatment effects of positive psychology interventions on subjective well-being. Effect sides (in standard deviations) and 95% confidence intervals for positive psychology interventions as reported in Bolier et al. (2013).

2. Beliefs and Aspirations

Table 5 reports the coefficients from estimation of model 1 (without controls for baseline levels of the outcome) for the components of the beliefs and aspirations indexes. There is little evidence of any effect of the intervention on components of beliefs or aspirations, with only one variable—namely, agreement with the statement that those from their own ethnic group are competent—that is statistically significant even with naive *p*-values, but the effect is negative. These results indicate overall small perverse effects of the intervention on beliefs and aspirations, though these are not statistically different from zero.

3. Decision-Making Behavior: Intentions, Cognitive Control, Time Discounting, and Labor Supply

Finally, we turn to test if the intervention affected intentions or the incentivized tasks measuring cognitive control and temporal discounting. In addition, we include estimates for our post hoc measure of labor supply, the attendance at

TABLE 5
TREATMENT EFFECTS: ASPIRATIONS AND BELIEFS

	Control Mean (SD) (1)	Treatment (2)	*N* (3)
	A. Aspirations		
Income aspirations	0	−.10	168
	(1.00)	(.08)	
Assets aspirations	0	−.07	168
	(1.00)	(.13)	
Status aspirations	0	−.16	168
	(1.00)	(.19)	
Education aspirations	0	.02	168
	(1.00)	(.21)	
	B. Beliefs about Ability and Trustworthiness of Others		
Own ethnic group competent	0	−.35**	168
	(1.00)	(.14)	
Own ethnic group trustworthy	0	.08	168
	(1.00)	(.16)	
Other ethnic group competent	0	−.17	168
	(1.00)	(.12)	
Other ethnic group trustworthy	0	.22	168
	(1.00)	(.18)	

Note. Ordinary least squares (OLS) regressions of outcome variables on treatment status. Outcome variables are listed on the left. Binary outcomes are left unadjusted, whereas all nonbinary outcomes and indexes are always normalized to be mean 0, 1 SD, in the control group. Column (1) reports the variable mean for the control group. Column (2) reports the coefficient from an OLS regression of the outcome variable on treatment status. Column (3) reports the sample size. Standard errors are reported in parentheses and are clustered at the endline session level.
** $p < .05$.

Busara sessions, measured up to 3 years after the intervention concluded. Table 6 reports these results. The effects on intentions are negative, though not statistically significant. Similarly, cognitive control, measured by the correct responses in the Stroop task, decreased by 0.37 SD in the treatment group. Reaction time increased, and the number of attempted responses decreased, though these are not statistically different from zero. These results seem to indicate that the intervention reduced cognitive control; however, these patterns are largely driven by baseline imbalance in cognitive function. Controlling for this, the coefficients

TABLE 6
TREATMENT EFFECTS ON BEHAVIOR: INTENTIONS, COGNITIVE CONTROL, TEMPORAL DISCOUNTING, AND LABOR SUPPLY

	Control Mean (SD) (1)	Treatment (2)	*N* (3)
	A. Intentions (Nonincentivized)		
Willingness to take HIV test	0	0	168
	(1.00)	(.16)	
Sweets intent	0	−.12	168
	(1.00)	(.14)	
Money intent	0	−.29*	168
	(1.00)	(.15)	
Spouse intent	0	−.01	122
	(1.00)	(.13)	
	B. Incentivized Behavior		
Stroop correct responses	0	−.37**	168
	(1.00)	(.15)	
Stroop reaction time	0	.16	168
	(1.00)	(.16)	
Stroop attempted responses	0	−.24	168
	(1.00)	(.14)	
Exponential discount factor	.71	.01	168
	(.20)	(.03)	
	C. Labor Supply		
Attendance at Busara 2015–17 (without controls)	4.73	−.84	218
	(4.73)	(.50)	
Attendance at Busara 2015–17 (controlling for preattendance)	4.73	−1.11**	218
	(4.73)	(.46)	
Attendance at Busara 2015 (controlling for preattendance)	1.61	−.36	218
	(1.82)	(.22)	

Note. Ordinary least squares (OLS) regressions of outcome variables on treatment status. Outcome variables are listed on the left. Nonbinary outcomes are normalized to be mean 0, 1 SD, in the control group, with the exception of the exponential discount factor and attendance (measured as number of sessions). Column (1) reports the variable mean for the control group. Column (2) reports the coefficient from an OLS regression of the outcome variable on treatment status. Column (3) reports the sample size. Panel C reports the impact on labor supply as measured by attendance at Busara between 2015–17, controlling for pretreatment attendance (2012–14). Standard errors are reported in parentheses and are clustered at the endline session level. HIV = human immunodeficiency virus.

* $p < .10$.

** $p < .05$.

are much closer to zero (e.g., the coefficient on Stroop correct responses is now −0.16 SD; see table A15). The exponential discount factor was also unaffected by treatment.[15]

Finally, we do not find any evidence that the intervention had an impact on attendance at Busara, with estimates being significant and negative over the 3-year period following the intervention, and insignificant but of a similar magnitude (relative to the control mean) when we only consider the year following the intervention (2015). We can rule out any positive impacts greater than 0.08 SD in 2015 (or 0.02 SD over the full 3-year horizon). We interpret these results as impacts on (partial) labor supply, under the assumption that the intervention did not differentially have an impact on preferences over participating as a subject at Busara.

D. Trends and Heterogeneity

With daily data on psychological well-being from the daily sheets that respondents filled out, we have more data and, thus, power to observe treatment effects and their trends. However, no significant trends or treatment effects on psychological well-being were detected using these additional data (for details, see app. A, sec. A1).

It is also possible that different groups of people were affected differently by the psychological intervention, and we explore this possibility by looking at the heterogeneous treatment effects by the prespecified characteristics at baseline. However, we do not find any consistent patterns of heterogeneity that reveal significant benefits of the intervention on any subgroup, with the caveat that given our sample size we are not well powered to detect even moderately heterogeneous impacts. We also present results of quantile treatment effects (QTEs) for our five main indexes. Overall, the analysis of the QTEs, mirroring

[15] Tables A19 and A20 provide additional analyses using the incentivized decision-making outcomes by including baseline characteristics. Table A20 also shows the results of the four discount factors measured using the four different time horizons. We do this to check that our measures (both the incentivized measures and the psychological variables) are actually capturing meaningful variation. First, we can confirm that these incentivized measures do indeed covary with baseline characteristics; higher cognitive control is predicted by more education, being male, and being younger. However, the psychological well-being measures at baseline do not appear to predict cognitive control. Higher education and cognitive function are positively associated with less temporal discounting and higher cognitive control. Interestingly, contemporaneous measures (endline measures) of gratitude are significantly positively associated with cognitive control and patience (the temporal discount factor). In addition, higher values of the Center for Epidemiologic Studies Depression Scale at endline were associated with more temporal discounting at all four time horizons measured, but not with cognitive control. The PANAS scores (more positive affect and less negative affect) at endline were also statistically associated with higher cognitive control. Meanwhile, life satisfaction (SWLS) at endline was not associated with either temporal discounting or cognitive control.

the patterns in the heterogeneous treatment effects, shows that there are no statistically significant positive effects of the intervention somewhere along the distribution that the average treatment effects were masking (for details of the heterogeneity analysis and QTE estimation, see app. A, sec. A2).

These results from daily reports and our analysis of heterogeneity further support our null findings in the main treatment effects.

E. Discussion

Our results indicate the intervention did not affect any of our prespecified outcomes. Apart from the intended effects on gratitude, we found no effects on psychological well-being or actual decision-making. This section discusses several potential explanations of why we find no effect.

First, we test whether positive findings for psychological well-being or decision-making are being masked by differential attrition. As noted in Section V.C.1, attritors from the treatment arm had lower gratitude by 0.34 SD than attritors from the control arm. Attritors from the treatment arm had 0.45 SD lower psychological well-being scores at baseline, though the difference is not statistically significant. However, several pieces of evidence suggest this is not a likely explanation for our overall null findings. First, these patterns of attrition bias our results toward finding positive impacts on psychological outcomes. Second, table 7 shows that IPW estimates are nearly identical to unadjusted treatment effects (however, nonparametric approaches, such as bounding following Lee 2009 reported in table A4, yield generally fairly wide bounds). Finally, and most importantly, the intervention did not result in any meaningful increase in labor supply (as measured by Busara attendance), and our estimates can generally rule out any positive effects over the 3 years following the intervention. Because the labor supply measure was available for both attritors and nonattritors, we are confident that our null results on economic decision-making are not driven by attrition.

Second, we consider whether our study did not have sufficient power to detect effects. To test for this possibility, we calculated the MDEs (with 80% power at the 5% significance level).[16] The MDEs presented here differ from the power calculated prior to running the experiment, as they account for the smaller endline sample and attrition corrections, as well as potential efficiency gains from having baseline measures for some outcomes. We present MDEs that are adjusted for attrition using IPWs.

[16] This exercise amounts to multiplying the standard error on the treatment coefficient by a constant; details can be found in Haushofer and Shapiro (2016).

TABLE 7
ATTRITION-ADJUSTED TREATMENT EFFECTS

	Control Mean (SD) (1)	Treatment Effects (Unadjusted) (2)	Attrition-Adjusted Treatment Effects Attrition Correction: IPW (3)
GQ-6 gratitude scale total	0	.31***	.31**
	(1.00)	(.10)	(.10)
Psych well-being (excluding gratitude)	0	.08	.09
	(1.00)	(.16)	(.16)
Psych well-being index	0	.14	.15
	(1.00)	(.15)	(.15)
Sleep quality index	0	0	−.13
	(1.00)	(.16)	(.20)
Aspirations index	0	−.13	−.11
	(1.00)	(.09)	(.12)
Beliefs index	0	−.07	−.10
	(1.00)	(.17)	(.20)
Intentions index	0	−.16	−.16
	(1.00)	(.18)	(.19)
SWLS life satisfaction scale total	0	.01	.04
	(1.00)	(.11)	(.10)
PANAS positive total	0	.03	−.05
	(1.00)	(.16)	(.17)
CES-D total	0	−.01	−.03
	(1.00)	(.11)	(.11)
Exponential discount factor	.71	.01	.03
	(.20)	(.04)	(.03)
Stroop correct responses	0	−.37**	−.25
	(1.00)	(.15)	(.16)

Note. Ordinary least squares (OLS) regressions of outcome variables on treatment status. Outcome variables are listed on the left. Nonbinary outcomes are normalized to be mean 0, 1 SD, in the control group, with the exception of the exponential discount factor. Column (1) reports the variable mean for the control group. Column (2) reports the coefficient from an OLS regression of the outcome variable on treatment status, controlling for the baseline measure of the outcome when available, but without adjusting for attrition. Column (3) reports the same coefficient estimated using inverse probability weighting (IPW) to adjust for attrition. Standard errors are reported in parentheses and are clustered at the endline session level. GQ-6 = Gratitude Questionnaire Six-Item Form; SWLS = Satisfaction with Life Scale; PANAS = Positive and Negative Affect Schedule; CES-D = Center for Epidemiologic Studies Depression Scale.
** $p < .05$.
*** $p < .01$.

The attrition-adjusted MDEs for each of the five index measures were 0.42 SD (psychological well-being), 0.56 SD (sleep and beliefs), 0.34 SD (aspirations), and 0.53 SD (intentions).[17] An increase of 0.53 SD in intentions corresponds to an increase in the likelihood of improving intentions along the 7-point Likert scale by an average of 1.6 points across the four categories considered.

[17] IPW-adjusted MDEs are about 20% larger than unadjusted MDEs. Without attrition adjustment, the MDEs for each of the five index measures were 0.36 SD (psychological well-being), 0.41 SD (sleep), 0.47 SD (beliefs), 0.26 SD (aspirations), and 0.51 SD (intentions).

For aspirations, an increase of 0.34 SD corresponds to aspiring to have an income that is US$12,300 higher, or an additional 0.44 year of schooling.[18] The attrition-adjusted MDEs for the incentivized outcomes of cognitive control and temporal discounting were 0.45 SD for the Stroop task (number correct) and 0.08 SD for the exponential discount factor.[19] To benchmark the cognitive control MDE, we note that "scarcity priming" (experimentally induced thought about difficult financial scenarios) reduced cognitive control among the poor sample by 0.9 SD (Mani et al. 2013), whereas the SA exercise with individuals sampled from an inner-city soup kitchen led to a 0.45 SD improvement in cognitive control (Hall, Zhao, and Shafir 2014). As a benchmark for the time preference MDE, Ifcher and Zarghamee (2011) find that a mild positive affect priming (using videos) induced an increase in the exponential discount factor of 0.08 SD, whereas the gratitude induction by Desteno et al. (2014) increased the exponential discount factor by 0.13 SD.

We rely on the previous literature in psychology to determine what effect sizes are reasonable to expect for the psychological outcomes. Based on a meta-analysis of 39 randomized interventions of which 28 measure subjective well-being, Bolier et al. (2013) report a meta-analytic effect size for subjective well-being of 0.34 SD, the primary outcome that has been studied with positive psychology interventions. A common measure of subjective well-being in these studies was the SWLS, which we also measure. We plot our estimated treatment effect on SLWS with the 95% CI alongside all the studies that also reported treatment effects on subjective well-being in figure 1. Our treatment effect is smaller than all but three of these other studies and has the highest precision. The 95% CI of our treatment effect (calculated using bounds) excludes the meta-analytic effect size reported by Bolier et al. (2013).

Another possible reason for our null findings might be that gratitude and other elements of positive psychology do not contribute to psychological well-being in Kenya. However, the positive psychology literature does show effects across diverse settings including China, Japan, Australia, and Spain. There has also been considerable diversity with respect to the subject pool: studies include university students, elementary school students, teachers, elderly populations, and patients with a broad range of medical disorders. For example, two recent studies conducted in Hong Kong (that were not included in the meta-analysis discussed previously) used positive psychology interventions using

[18] For aspirations, there are fewer studies to benchmark our results, but Bernard et al.'s (2014) aspiration intervention (which included aspirational videos and, thus, was more salient than our intervention) increased aspirations by about 0.2 SD.

[19] The MDE for labor supply over the 3-year period was 0.34 SD.

gratitude writing with health care workers (Cheng, Tsui, and Lam 2015) and female prisoners (Mak and Chan 2018). Both found improvements in subjective well-being of 0.9 SD. Another study using Chinese male prisoners from Beijing found that gratitude writing improved subjective well-being by 0.7 SD (Deng et al. 2018).

VI. Conclusion

This study tested the effects of a light-touch psychological intervention on psychological well-being and economic decision-making among residents of slums in Nairobi, Kenya, using a randomized controlled trial. We combined three protocols that have previously been shown in the psychology literature to be effective in improving psychological and behavioral outcomes into a single compound intervention: gratitude writing, SA, and AP. The motivation to combine the protocols into a single one was to create the strongest possible intervention while simultaneously remaining a low-cost and light-touch intervention.

We find little evidence that the intervention improved psychological well-being or economic decision-making outcomes. One possible reason is that we were underpowered to detect small effects; however, we can rule out effect sizes on subjective well-being that have been reported in meta-analyses of these types of interventions. Another possible reason is that these interventions do not translate well to the Kenyan setting. However, we confirmed that participants completed the exercises correctly, and the intervention improved self-reported gratitude with magnitudes comparable to those reported in the literature. Another possibility is that the intervention was "too light touch." However, the goal of our intervention was to specifically test a light-touch and low-cost intervention because there is already considerable evidence on the effectiveness of higher-cost psychological interventions such as those involving targeted videos or cognitive behavioral therapy. Our results suggest that simple, light-touch psychological exercises based on positive psychology might have limited effects on psychological well-being and economic decision-making.

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