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Evaluating the Effectiveness of ACT for Anxiety Disorders in a Self-Help Context: Outcomes From a Randomized Wait-List Controlled Trial

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Rigorous evaluations of cognitive behavioral self-help books for anxiety in pure self-help contexts are lacking. The present study evaluated the effectiveness of an Acceptance and Commitment Therapy (ACT) self-help workbook for anxiety-related concerns, with no therapist contact, in an international sample. Participants (N = 503; 94% mental health diagnosis) were randomized to an immediate workbook (n = 256) or wait-list condition (n = 247). Assessments at pretreatment, 12 weeks, 6 months, and 9 months evaluated anxiety and related symptoms, quality of life, and ACT treatment processes (e.g., psychological flexibility). Participants in the wait-list arm crossed over to the workbook following the 12-week assessment. The workbook condition yielded significant improvements on all assessments from pre- to posttreatment relative to wait-list, and these gains were maintained at follow-ups. The pattern observed in the wait-list condition was virtually identical to the active treatment arm after receiving the workbook, but not before. Attrition was notable, but supplemental analyses suggested dropout did not influence treatment effects for all but one measure. Overall, findings provide preliminary support for the effectiveness of this self-help workbook and suggest ACT-based self-help bibliotherapy might be a promising low-cost intervention for people experiencing significant anxiety-related concerns.

Keywords: acceptance and commitment therapy; anxiety disorders; self-help; bibliotherapy

Over 14% of people worldwide, including 1 in 3 in the U.S., will meet diagnostic criteria for an anxiety disorder at some point in their lives (Kessler et al., 2009; Kessler, Petukhova, Sampson, Zaslavsky, & Wittchen, 2012). Although chronic, costly, and impairing (Kessler, Ruscio, Shear, & Wittchen, 2010), anxiety disorders tend to be highly treatable. For example, therapist-delivered cognitive behavior therapy (CBT) is efficacious for a range of anxiety disorders (Hofmann & Smits, 2008). However, a considerable number of individuals with anxiety disorders do not respond to traditional CBT (Taylor, Abramowitz, & McKay, 2012).

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In an effort to expand the scope and depth of CBT, newer, transdiagnostic approaches have focused on contextual, acceptance-based interventions. For example, Acceptance and Commitment Therapy (ACT; Hayes, Strosahl, & Wilson, 2012) combines mindfulness and acceptance processes with traditional behavior change techniques in the service of supporting value-guided action (Eifert & Forsyth, 2005; Hayes et al., 2012). Over 100 randomized controlled trials (RCTs) support the efficacy of ACT for a diverse range of suffering, including anxiety disorders (A-Tjak et al., 2015; Swain, Hancock, Hainsworth, & Bowman, 2013).

Despite favorable outcomes for ACT and traditional CBT, access to adequate behavioral treatment remains limited. Less than one-third to one-half of people with a diagnosable disorder in the U.S. and across the world receive professional treatment (Kessler et al., 2005; Wang et al., 2007). Several factors appear to limit availability of face-to-face therapy. These include high cost, too few therapists to meet worldwide demand, and limited therapist availability in certain geographic regions (Kazdin & Blasé, 2011; Somers, Goldner, Waraich, & Hsu, 2006). As a result, many people who could benefit from effective treatments continue to struggle with anxiety disorders, and thus identifying effective alternative forms of treatment delivery seems warranted.

One such method might be self-help bibliotherapy. For instance, CBT-informed bibliotherapy appears efficacious for anxiety-related problems (e.g., Farrand & Woodford, 2013; Hirai & Clum, 2006) and might be as effective as therapist-administered treatment (den Boer, Wiersma, & Van den Bosch, 2004). When considering the abundance of self-help workbooks available to the general public, some of which are CBT-informed, it seems bibliotherapy might be an effective, cost-efficient, and readily available alternative for people who struggle with anxiety but lack access to face-to-face treatment (Mains & Scogin, 2003; Newman, Erickson, Przeworski, & Dzus, 2003).

Yet, little evidence directly supports commercially available self-help workbooks for anxiety disorders, especially for an international population. There are several reasons why this might be so. First, although bibliotherapy efficacy studies date back to early behavior therapy, many studies have evaluated self-help materials that are *not* publically available (e.g., treatment protocols not commercially published). Thus, it is unknown if the above findings can be generalized to CBT-based self-help workbooks available to the general public.

Second, few commercially available self-help workbooks are grounded in evidence-based practice or evaluated in RCTs (Malouff & Rooke, 2007; Redding, Herbert, Forman, & Gaudiano, 2008). For example, only a handful of CBT-informed commercially available self-help workbooks for anxiety have received empirical support (e.g., Abramowitz, Moore, Braddock, & Harrington, 2009; Ghosh, Marks, & Carr, 1988; Hecker, Losee, Roberson-Nay, & Maki, 2004). Moreover, the applicability of the above investigations to real-world use of self-help workbooks remains unclear. Because many people likely use self-help materials without therapist guidance, it has been suggested that empirical evaluations should involve limited or no contact with researchers or therapists (McKendree-Smith, Floyd, & Scogin, 2003; Rosen & Lilienfeld, 2015). Yet, most bibliotherapy evaluations entail at least minimal therapist or researcher contact (e.g., weekly phone calls assessing reading progress and comprehension, face-to-face meetings).

Third, most self-help bibliotherapy resources that are evidence-based are disorder specific in nature (e.g., Barlow & Craske, 1994), despite a trend toward transdiagnostic treatment protocols targeting general processes underpinning many forms of psychological difficulties (e.g., Barlow et al., 2011). To the best of our knowledge, no one has examined the effectiveness of a transdiagnostic self-help workbook for anxiety under naturalistic conditions in a clinically distressed sample. Lastly, it is widely recognized that unmet mental health care needs are a significant global problem (Wang et al., 2007). Yet, most evaluations of self-help bibiotherapies have been limited to U.S. samples (e.g., Hecker et al., 2004). Thus, it remains unclear if bibliotherapies are an effective means to reduce, at least in part, unmet global needs related to psychological difficulties.

In sum, many people lack access to effective psychosocial interventions for anxiety disorders, for reasons related to treatment models that focus on face-to-face delivery. Self-help bibliotherapy, as an alternative, cost-efficient treatment delivery system, might reduce suffering associated with anxiety disorders by expanding the reach of evidence-based treatments (Kazdin & Blasé, 2011). Nonetheless, self-help workbooks require rigorous examination to be considered part of effective models of care (e.g., Rosen & Lilienfeld, 2015).

To address the above concerns, the aim of the present study was to evaluate the effectiveness of an ACT transdiagnostic self-help workbook in an international sample struggling with anxiety. The decision to evaluate an ACT self-help workbook was both strategic and pragmatic. ACT has been described as a transdiagnostic approach, emphasizing broad behavioral processes and outcomes and specific, empirically supported treatment components

(Levin, Hildebrandt, Lillis, & Hayes, 2012). Moreover, ACT self-help bibliotherapy, combined with varying degrees of therapist contact, appears efficacious for depression (Fledderus, Bohlmeijer, Pieterse, & Schreurs, 2011), chronic pain (Thorsell et al., 2011), and college student and educator adjustment and anxiety/depressive symptoms (Jeffcoat & Hayes, 2012; Muto, Hayes, & Jeffcoat, 2011). Yet, we are unaware of any evaluations of the effectiveness of an ACT transdiagnostic workbook delivered in a pure self-help context, and specifically with people experiencing significant anxiety problems. Our intention, therefore, was to address this issue and to find out if ACT is effective when packaged, disseminated, and used in a pure self-help context.

In keeping with these broad aims, a randomized wait-list controlled crossover treatment design was used to evaluate an ACT transdiagnostic workbook for people struggling with anxiety-related problems—the *Mindfulness and Acceptance Workbook for Anxiety* (*MAWA*; Forsyth & Eifert, 2007). In so doing, no therapist contact and minimal exclusion criteria were used to approximate real-world, pure self-help conditions under which the general public would use the workbook. Moreover, we wished to examine the impact of the workbook in an international sample, to assess its applicability in a diverse context. We expected that, relative to participants assigned to a wait-list condition, individuals working with the *MAWA* would demonstrate significant improvements in anxiety and related symptoms, quality of life, and ACT-relevant treatment processes (e.g., psychological flexibility). Additionally, we anticipated the wait-list group would show similar improvements after cross-over to the workbook, but not before. Finally, we predicted treatment gains would be maintained at follow-up assessments.

Method

PARTICIPANTS AND RECRUITMENT

Five hundred three participants met eligibility requirements, were randomized to an immediate workbook condition (n = 256) or a wait-list condition (n = 247), and were included in the intention-to-treat (ITT) sample. All reported significant difficulties with anxiety, 475 (94.4%) self-reported receiving a mental health diagnosis, and 281 (55.9%) self-reported an anxiety disorder diagnosis. The sample included residents of the U.S. (n = 336), United Kingdom (n = 54), Canada (n = 43), Australia and New Zealand (n = 41), Ireland (n = 7), and other European, Asian, and North and South American countries (n = 19). Table 1 contains additional sample characteristics, such as specific self-reported diagnoses, demographic information, and use of outside therapy. As shown in Table 1, none of these variables differed by condition.

Few exclusion criteria were used, consistent with our aim to evaluate effectiveness. Eligible participants were at least 18 years old, reported Internet access, denied suicidal ideation, denied previous exposure to the workbook, and reported English literacy at the 8th grade level or above. They also endorsed at least one of four questions asking (a) if anxiety was a problem in their life, (b) if anxiety/fear were interfering with their life and goals, (c) if concerns about anxiety consumed their life, and (d) if anxiety/fear were seen as barriers to a desired life.

Recruitment took place from 2008 to 2009 (final follow-up in 2010), with the aim of recruiting as large a sample as possible in that time window to maximize ecological validity and our ability to generalize broadly with an international sample. Participants were recruited worldwide via the Internet and locally in the Albany, NY area. Interested participants were directed to the study website, which featured information about the study and anxiety disorders. For each assessment completed (excluding baseline), participants were entered into a raffle for a $25 Amazon.com gift certificate (1 in 50 chance of winning).

DESIGN

A wait-list controlled crossover treatment design was used, with participants randomized to the immediate workbook condition or the wait-list condition. Participants who received the workbook right away were given 12 weeks to complete treatment. Following a 12-week waiting period, wait-list participants were crossed to the active intervention arm, were mailed a copy of the workbook, and completed the 12-week treatment. Participants in the immediate workbook condition were assessed at pretreatment, at 12 weeks (posttreatment), at 6 months, and at 9 months. Wait-list participants were also assessed at pretreatment, 12 weeks (following the waiting period), at 6 months (serving as a posttreatment assessment following crossover to the workbook), and at 9 months. Assessments were conducted online using Survey Monkey (www.surveymonkey.com).

TREATMENT

Participants were provided a copy of the *MAWA* free of charge. The workbook contains ACT-based self-help material for all anxiety disorders, as identified in the *Diagnostic and Statistical Manual of Mental Disorders, 4th Edition, Text Revision* (American Psychiatric Association, 2000). Part I

Table 1
Demographic and Clinical Characteristics of the Intention-to-Treat Sample

Characteristic	Total	Workbook (n = 256)	Wait-List (n = 247)	t or χ^2	p
Gender					
Female	78.3% (394/503)	79.3% (203/256)	77.3% (191/247)	0.29	.59
Age in years M (SD)	38.05 (11.01)	38.39 (11.05)	37.69 (10.98)	0.72	.48
Reported race/ethnicity *				0.13	.72
White	86.5% (435/503)	85.9% (220/256)	67.0% (215/247)		
Asian American/Pacific Islander	3.2% (16/503)	3.5% (9/256)	2.8% (7/247)		
Hispanic/Latino	3.2% (16/503)	3.5% (9/256)	2.8% (7/247)		
African American/Black	0.8% (4/503)	0.8% (2/256)	0.8% (2/247)		
American Indian/Alaskan Native	0.6% (3/503)	0.8% (2/256)	0.4% (1/247)		
Other	5.8% (29/503)	5.5% (14/256)	6.1% (15/247)		
Diagnosis				4.09	.85
Agoraphobia	1.6% (8/503)	2.0% (5/256)	1.2% (3/247)		
Generalized Anxiety Disorder	18.7% (94/503)	16.8% (43/256)	20.6% (51/247)		
Obsessive Compulsive Disorder	4.0% (20/503)	3.5% (9/256)	4.5% (11/247)		
Major Depressive Disorder	15.9% (80/503)	17.6% (45/256)	14.2% (35/247)		
Panic Disorder	12.7% (64/503)	14.1% (36/256)	11.3% (28/247)		
Post-Traumatic Stress Disorder	7.8% (39/503)	8.2% (21/256)	7.3% (18/247)		
Social Anxiety Disorder	11.1% (56/503)	11.3% (29/256)	10.9% (27/247)		
Other	22.7% (114/503)	21.1% (54/256)	24.3% (60/247)		
Current therapy	46.7% (234/501)	49.6% (126/254)	43.7% (108/247)	1.74	.19
Current medication	50.0% (250/500)	52.0% (133/256)	48.0% (117/244)	0.09	.79
College degree or higher	61.8% (311/503)	64.5% (165/256)	59.1% (146/247)	1.52	.28
Heard of ACT previously †	32.6% (151/463)	32.6% (76/233)	32.6% (75/230)	0.00	1.00
International	32.8% 164/500	30.6% (78/255)	35.1% (86/245)	1.16	.28
Marital				4.57	.10
Married	41.0% (206/503)	44.5% (114/256)	37.2% (92/247)		
Single	44.9% (226/503)	44.1% (113/256)	45.7% (113/247)		
Other	14.1% (71/503)	11.3% (29/256)	17.0% (42/247)		

Note. ACT = acceptance and commitment therapy; *Compared White to other race/ethnicity combined because of small n; † data missing for 40 participants.

includes psychoeducation about anxiety disorders, the nature of anxiety and fear, and acceptance and commitment therapy (ACT). Part II examines the costs of excessive avoidance and suppression of anxiety and fear, and introduces acceptance and valued living as alternatives. Part III guides the reader through specific ACT interventions that encourage (a) mindful acceptance of anxiety-related thoughts and emotions, (b) defusion from unhelpful thoughts and feelings, (c) development of self-compassion, (d) construction of personal values and goals, and (e) exposure-like ACT exercises designed to develop values-consistent behavior. Specific interventions include, among others, structured mindfulness exercises, classic ACT acceptance metaphors (e.g., "passengers on the bus"; Hayes et al., 2012), defusion exercises (e.g., thoughts on cards and "I'm having the thought that …") and values clarification exercises (e.g., tombstone exercise, values directions worksheet, life compass). These interventions are designed to be relevant across all anxiety and related disorders. The workbook also includes an audio CD containing experiential and mindfulness exercises. There was no therapist or researcher contact as part of treatment.

MEASURES

Outcome Measures

The following self-report measures assessed symptom-specific domains and broad indices of functioning, consistent with ACT's focus on valued living, the *MAWA's* transdiagnostic nature, and the diverse sample.

Anxiety Sensitivity Index (ASI)

The ASI (Peterson & Reiss, 1992) assesses fear of aversive anxiety symptoms. Higher scores represent greater levels of catastrophic fear and negative evaluations of anxiety symptoms, with a clinical cutoff score of 30 (Peterson & Plehn, 1999). The ASI demonstrates good internal consistency (α =

.88; Zinbarg, Barlow, & Brown, 1997) and test–retest reliability (r = .75; Reiss, Peterson, Gursky, & McNally, 1986). Internal consistency was excellent in the present sample, α = .91.

Beck Depression Inventory II (BDI-II)
The BDI-II (Beck, Steer, & Brown, 1996) assesses depressive symptom severity, with excellent internal consistency (αs = .91–.93; Beck, Steer, Ball, & Ranieri, 1996) and test–retest reliability (r = .96; Sprinkle et al., 2002). BDI-II scores are interpreted as follows: 0–13, minimal depression symptoms; 14–19, mild; 20–28, moderate; 29–63, severe. Internal consistency was excellent in the present sample, α = .92.

Beck Anxiety Inventory (BAI)
The BAI (Beck & Steer, 1993) measures severity of anxiety symptoms. BAI scores are interpreted as follows: 0–7, minimal anxiety symptoms; 8–15, mild; 16–25, moderate; 26–63, severe. The BAI shows excellent internal consistency (α = .92) and good test–retest reliability (r = .75; Beck, Epstein, Brown, & Steer, 1988). Internal consistency was excellent in the present sample, α = .93.

Penn State Worry Questionnaire (PSWQ)
The PSWQ (Meyer, Miller, Metzger, & Borkovec, 1990) assesses one's tendency to worry, with higher scores denoting greater worry. Scores at or above 45 suggest pathological levels of worry (Behar, Alcaine, Zuellig, & Borkovec, 2003). The PSWQ demonstrates good to excellent internal consistency (αs = .86–.93) and test–retest reliability (r = .74–.92), and construct validity (Molina & Borkovec, 1994). Internal consistency in the present sample was excellent, α = .90.

Quality of Life Inventory (QOLI)
The QOLI (Frisch, 1994) assesses values and life satisfaction. Broad life domains are rated for importance and satisfaction, with higher scores suggesting greater quality of life. The QOLI has acceptable to good internal consistency (αs = .77–.89), strong test–retest reliability (r = .80–.91), and construct validity (Frisch, Cornell, Villanueva, & Retalzaff, 1992). Internal consistency was good in the present sample, α = .80.

PROCESS MEASURES

The following measures assessed ACT-relevant processes.

Acceptance and Action Questionnaire 16 (AAQ)
The AAQ (Hayes, Strosahl, et al., 2004) assesses psychological flexibility. We used the 16-item version, because this longer form of the AAQ might be more sensitive to clinical change (Arch, Eifert, et al., 2012; Hayes, Strosahl, et al., 2004). Higher scores indicate lower levels of psychological flexibility. The AAQ has acceptable to good internal consistency (αs = .78–.86; Arch, Eifert, et al., 2012), as in the present sample (α = .80).

Believability of Anxious Feelings and Thoughts Questionnaire (BAFT)
The BAFT (Herzberg et al., 2012) assesses the believability of thoughts and feelings (i.e., the extent to which one's relation with anxious private events is fused or defused). Higher scores indicate more fusion with private content. The BAFT shows excellent internal consistency in anxious samples (α = .91), good test–retest reliability (r = .77), and construct validity (Herzberg et al., 2012). Internal consistency was excellent in the present sample, α = .90.

Mindful Attention Awareness Scale (MAAS)
The MAAS (Brown & Ryan, 2003) assesses mindfulness, defined as awareness of and attention to the present moment. Higher scores suggest greater mindfulness. The MAAS demonstrates good internal consistency (α = .87), good test–retest reliability (r = .81), and construct and predictive validity (Brown & Ryan, 2003). Internal consistency in the present sample was good, α = .88.

Self-Compassion Scale (SCS)
The SCS (Neff, 2003) assesses self-compassion, defined as the degree to which individuals exhibit mindful self-kindness and identify with others who suffer. Higher scores indicate greater self-compassion. The SCS demonstrates excellent internal consistency (α = .92) and test–retest reliability (r = .93), and construct validity (Neff, 2003). Internal consistency in the present sample was excellent, α = .94.

TREATMENT FIDELITY

Following the corresponding 12-week treatment period for both groups, participants (a) rated perceived benefit from the workbook ("Overall, how much benefit did you find from reading the workbook and working with the material?" from 0 = *none* to 10 = *a great deal*), (b) estimated the percentage of the workbook they read, (c) estimated how many hours per week they practiced exercises or worked with material from the book, and (d) rated the degree to which they applied workbook material to their lives ("Please rate the extent to which you applied workbook material to your life" from 0 = *not at all* to 10 = *all of the time*).

PROCEDURE

All procedures were approved by the local IRB. Study candidates were directed to the study website,

where they provided informed consent and demographic information, and completed a brief eligibility assessment. Eligible participants were sent an invitation email with a link to the pretreatment assessment. Following completion of this assessment, research personnel, blind to participant identity and baseline scores, randomized participants to condition using a random number table.

All participants were then sent a welcome email, and participants assigned to the immediate workbook condition were mailed the *MAWA*. Although immediate workbook participants were also provided with a suggested 12-week timeline to guide progression through the workbook, they were allowed to progress at their own pace. After 12 weeks, all participants received an email with a link for the 12-week assessment; an assessment that also included treatment fidelity questions for those who had just finished using the workbook.

Following the 12-week assessment, wait-list participants were crossed to the active treatment arm, were mailed the workbook, and were treated identically as those in the initial treatment condition for 12 weeks. Immediately thereafter, all participants were emailed a link to the 6-month assessment. At this time, wait-list participants were asked the treatment fidelity questions. Lastly, an additional 9-month assessment was conducted for both conditions. If participants did not respond to assessments in a timely fashion, email reminders were sent indicating the importance of the questionnaires. The only other contact between researchers and participants were email communications to address technical issues (e.g., accessing the website). This contract included no efforts to coach, advise, or guide participants in using the workbook.

ANALYTIC STRATEGY

Consistent with previous self-help workbook studies (e.g., Muto et al., 2011) and recommendations for effectiveness studies (Armijo-Olivo, Warren, & Magee, 2009), analyses were performed using an ITT approach. All available data were used from all participants randomized to condition. Although an ITT approach is appropriate for trials with missing data (Armijo-Olivo et al., 2009; Pocock & Abdalla, 1998), we anticipated the possibility of attrition, given minimal researcher contact. While ITT analyses often use repeated measures ANOVAs and impute missing data, such an approach is problematic with significant missing data (Unnebrink & Windeler, 2001). Thus, we used a mixed model repeated measures approach with maximum likelihood estimation to estimate treatment effects for outcome and process variables. Mixed models are appropriate for longitudinal and nested data structures, such as the present study, in which repeated measurements are nested within individuals. Moreover, mixed models better account for heterogeneous spacing of observations across participants and violations of the assumption of independence (Singer & Willet, 2003). Likewise, mixed models are highly flexible in handling missing data and are robust to high dropout rates when data are missing at random, in part because they do not impute data, but rather estimate treatment effects using available data (Diggle, Liang, & Zeger, 1994; Gallop & Tasca, 2009). Indeed, mixed models result in less potential for estimation bias in similar data sets when compared to imputation procedures (Gueorguieva & Krystal, 2004; Hamer & Simpson, 2009), and provide accurate estimates of treatment effects when data are missing at random (Hedeker & Gibbons, 1997).

Separate mixed models were tested for each outcome and process variable in SAS PROC MIXED using maximum likelihood estimation. Each model included three predictors as fixed effects: main effects for study condition (immediate workbook vs. wait-list) and time, and a Condition × Time interaction term. The appropriate model covariance structure was determined based on visual inspection of the correlation matrix, information criteria (e.g., AIC, BIC), and Likelihood Ratio (LR) test. Unstructured, Toeplitz, and autoregressive (AR(1)) covariance structures were compared. The above criteria suggested that a Toeplitz covariance structure was best fitting. Degrees of freedom were estimated using Kenward-Roger method.

Multilevel data analysis using maximum likelihood estimation is predicated on the assumption that the data are missing at random. Thus, we tested whether the results were independent of missing data patterns using pattern-mixture models (see Gallop & Tasca, 2009; Hedeker & Gibbons, 1997). Participants were divided into groups based on dropout status (completers vs. dropouts) and the effect of the dropout status variable was built into a multilevel model for each study variable as a random variable, with a focus on the Condition × Time × Dropout interaction. A significant effect of dropout pattern would suggest the corresponding treatment effect varied as a function of completion status.[1]

[1] We tested additional pattern-mixture models that varied the missing data pattern (e.g., missing data at a specific assessment point or multiple assessment points, regardless of timing). In these cases, the observed pattern of results was either consistent with the initial pattern mixture models (non-significant dropout vs. completer pattern effect) or the model did not converge.

To evaluate clinical significance, we examined estimates of reliable and clinically significant change for each outcome variable at both the overall and individual level, based on guidelines from Jacobson and Truax (1991). Participants were required to (a) show improvement greater than measurement error alone (determined using reliability and standard deviation estimates), and (b) demonstrate posttreatment/follow-up scores that either fell below established clinical cutoffs (used when available) or that fell within 2 standard deviations of nonclinical sample means (see Bauer, Lambert, & Nielsen, 2004). Reported percentages are based on participants with both baseline and posttreatment or follow-up scores.

Results

Figure 1 displays a diagram of participant flow. In the immediate workbook condition, 143 participants were lost to the 12-week assessment, with an additional 37 and 3 participants lost to the 6-month and 9-month assessment respectively. In the wait-list condition, 48 participants did not complete the 12-week assessment, an additional 132 failed to respond to the 6-month assessment, and 23 were lost to the 9-month assessment. Completers and dropouts did not differ on demographic variables (*p*s > .06), clinical characteristics (e.g., diagnosis; *p*s > .34), or baseline scores on all symptom and process measures (*p*s > .07). In addition to reminders, an email inquiry regarding reasons for noncompletion was sent to participants who did not complete a follow-up assessment. Of the 93 participants who responded to this inquiry, 36 cited life events unrelated to the study, 30 reported lacking time to continue participation, 23 reported feeling better, 23 said they lost interest, 15 indicated they were not improving or the workbook was not

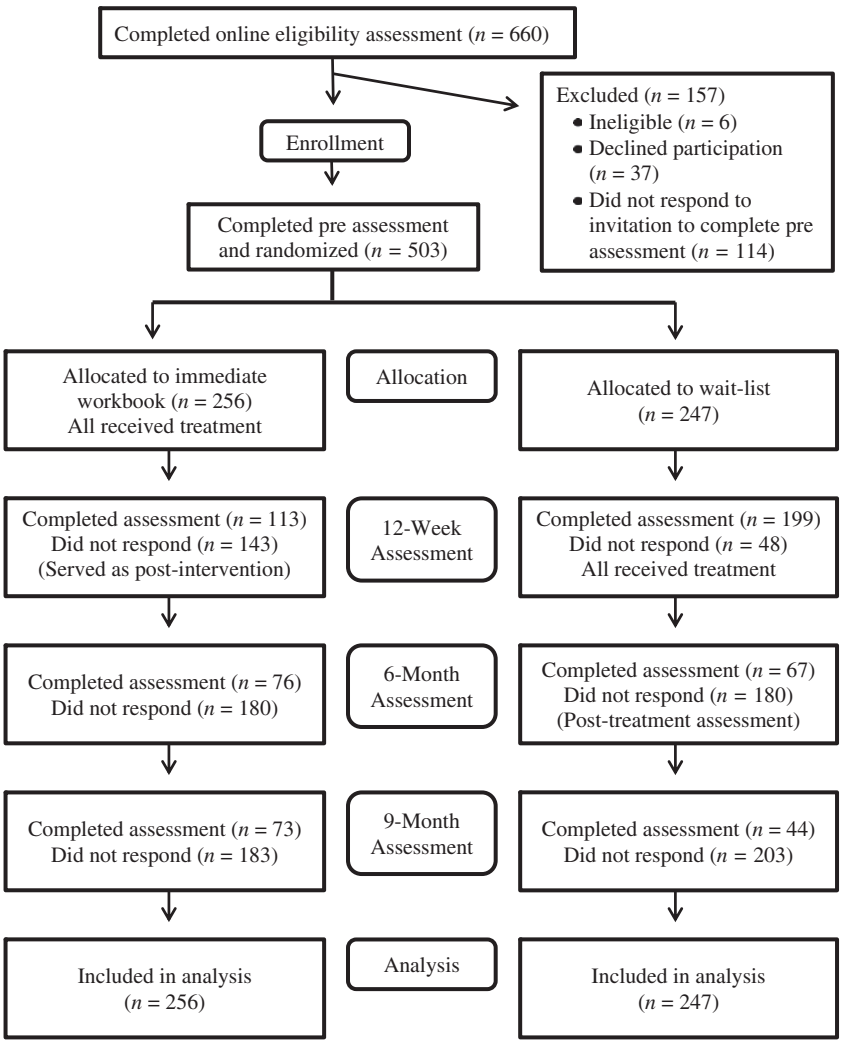

FIGURE 1 Participant flow diagram.

Table 2
Mixed Model Estimated Means and Standard Errors for Outcome and Process Measures*

	Immediate Workbook				Wait-List			
	Pre	12-Week	6-Month	9-Month	Pre	12-Week	6-Month	9-Month
Outcome								
ASI	36.4 (0.9)	24.4 (1.1)	21.8 (1.4)	20.6 (1.5)	35.7 (0.9)	33.2 (1.0)	23.0 (1.4)	19.9 (1.4)
BDI	25.9 (0.8)	14.9 (1.0)	15.1 (1.2)	14.3 (1.3)	26.1 (0.8)	24.2 (0.9)	17.1 (1.2)	13.3 (1.6)
PSWQ	66.6 (0.7)	55.2 (0.9)	54.0 (1.1)	52.0 (1.2)	65.3 (0.7)	64.0 (0.8)	54.7 (1.1)	52.1 (1.5)
QOLI	−0.1 (0.1)	1.0 (0.2)	1.2 (0.2)	1.3 (0.2)	−0.2 (0.1)	−0.3 (0.1)	0.7 (0.2)	0.9 (0.2)
Process								
BAFT	83.3 (1.3)	52.4 (1.7)	51.1 (2.1)	46.9 (2.2)	81.9 (1.3)	79.1 (1.4)	56.7 (2.1)	47.8 (2.8)
MAAS	3.1 (0.1)	3.6 (0.1)	3.7 (0.1)	3.7 (0.1)	3.1 (0.1)	3.2 (0.1)	3.6 (0.1)	3.8 (0.1)
SCS	2.2 (0.0)	3.0 (0.1)	3.0 (0.1)	3.2 (0.1)	2.2 (0.0)	2.2 (0.1)	2.9 (0.1)	3.2 (0.1)

Note. ASI = Anxiety Sensitivity Index; BDI = Beck Depression Inventory; PSWQ = Penn State Worry Questionnaire; QOLI = Quality of Life Inventory; BAFT = Believably of Anxious Feelings and Thoughts Questionnaire; MASS = Mindful Attention Awareness Scale; SCS = Self-Compassion Scale; * See Figure 2 for Beck Anxiety Inventory and Acceptance and Action Questionnaire results.

helpful, 11 reported being too anxious, and 8 cited computer difficulties as reasons for non-completion (many respondents endorsed multiple reasons).

Demographic variables (e.g., ethnicity) and clinical characteristics (e.g., diagnosis) did not discriminate between conditions (all *p*s > .10; see Table 1). Critically, there were no group differences on any outcome or process measure at pretreatment baseline (all *p*s > .11).

For each assessment point, estimated means and standard errors from the mixed model analyses are reported in Table 2. Pretreatment scores on outcome variables fell well within clinical ranges and above clinical cutoffs. For example, the mean BAI score (31.36; *SD* = 14.17) indicated the sample was experiencing severe levels of anxious distress at the beginning of the trial. PSWQ, BDI-II, and ASI scores similarly suggested significant baseline distress.

TREATMENT FIDELITY

Participants in both conditions reported reading 66.5% of the workbook on average, with over one-third reading at least 90% of the book. The mean for the question assessing perceived benefit of the workbook was 6.82 (*SD* = 2.86), indicating participants believed the workbook was helpful. Participants reported practicing exercises or working with material from the workbook approximately 4 hours per week (*M* = 3.91, *SD* = 5.63). The mean for the question assessing how much participants applied workbook material to their lives was 6.20 (*SD* = 2.62), suggesting participants not only read the material, but also applied it to their lives to a moderate degree. There were no differences between conditions on these assessments (all *p*s > .20).

OUTCOME VARIABLES

Pattern-mixture model results demonstrated a nonsignificant pattern of missingness effect (all *p*s > .13) for all outcome variables, indicating that missingness did not meaningfully impact change by condition. These nonsignificant findings support the use of

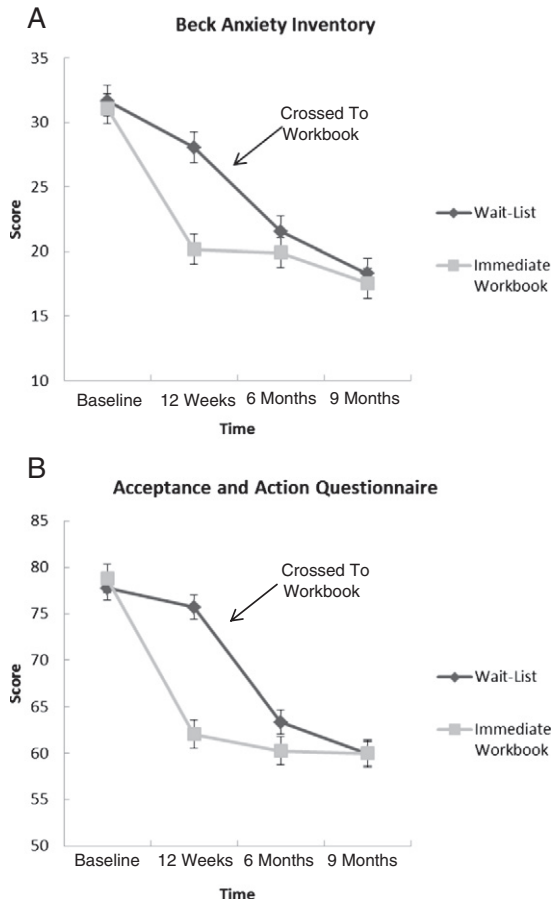

FIGURE 2 BAI (top) and AAQ (bottom) estimated means from the mixed model analyses showing change over time in anxiety symptoms and psychological flexibility for each condition. Vertical bars represent standard errors.

maximum likelihood as an appropriate analytic method for the data.

Outcome variable results from the mixed models can be found in the top portion of Table 3. A significant effect of time was observed for all outcome variables. With exception of the ASI, a significant group effect was also observed. However, these main effects must be considered in relation to significant Condition × Time interactions observed for all outcome variables.

Contrast analyses were performed to examine the nature of the interaction effects. Statistically significant differences in pre-intervention to 12-week assessment change were observed between the immediate workbook and wait-list groups. After 12 weeks, the immediate workbook group demonstrated greater reductions on the ASI, BAI, BDI-II, and PSWQ and greater improvement on the QOLI. This indicates that participants in the immediate workbook condition displayed significant pre-post improvement on all outcome variables relative to their wait-list counterparts.

Once both conditions had been exposed to treatment, no significant between-group differences were observed on the outcome variables at 6- or 9-month assessments (all ps > .11). This indicates that participants in the wait-list condition experienced comparable improvement on outcome variables after, but not before, crossing over to treatment. Within both groups, no significant differences were observed between respective post-intervention assessments and follow-ups (all Tukey–Kramer adjusted ps > .12). This suggests that pre-post improvements on outcome variables were maintained through the follow-up period.

PROCESS VARIABLES

With exception of the SCS, pattern-mixture model results indicated a nonsignificant effect of missingness pattern on change by condition for the process variables (all ps > .14). The significant SCS Condition × Time × Dropout interaction, $F(3, 684) = 3.32$, $p = .019$, indicated that SCS effects were dependent on missingness pattern (completion status). Specifically, treatment effects were larger for completers than for dropouts, such that the difference over time between conditions was larger for completers. However, SCS treatment effects (i.e., Condition × Time interactions) were still significant for both completers, $F(3, 154) = 21.47$, $p < .001$, and dropouts, $F(3, 128) = 17.42$, $p < .001$.

Process variable results from the mixed models can be found in the bottom portion of Table 3. A significant effect of time was observed for all process variables. With exception of the MAAS, a significant group effect was also observed. However, these main effects must be considered in relation to significant Condition × Time interactions observed for all process variables.

Contrast analyses were performed to examine the nature of the interaction effects. Statistically significant differences in pre-intervention to 12-week assessment change were observed between the immediate workbook and wait-list group. After 12 weeks, the immediate workbook group demonstrated greater reductions the AAQ and BAFT, and greater increases on the MAAS and SCS, suggesting that participants in the immediate workbook condition displayed significant pre-post improvement on all process variables relative to wait-list participants.

Table 3
Mixed Model Results for Outcome and Process Measures

	Time		Condition		Condition * Time		Contrast Analyses		
	F	*df*	*F*	*df*	*F*	*df*	*F*	*df*	M_{diff}
Outcome									
ASI	94.19*	3, 400	2.60	1, 645	22.83*	3, 400	58.70*	1, 584	9.42
BAI	83.91*	3, 381	4.38*	1, 636	16.52*	3, 381	41.90*	1, 558	7.32
BDI	83.71*	3, 377	4.92*	1, 637	25.51*	3, 377	67.12*	1, 566	9.17
PSWQ	105.96*	3, 448	3.78	1, 652	33.25*	3, 448	89.98*	1, 587	10.10
QOLI	41.96*	3, 391	10.96*	1, 639	18.33*	3, 391	52.63*	1, 551	−1.14
Process									
AAQ	131.95*	3, 458	8.67*	1, 664	43.91*	3, 458	120.84*	1, 611	14.68
BAFT	180.59*	3, 479	17.15*	1, 665	69.78*	3, 479	187.54*	1, 619	28.14
MAAS	42.24*	3, 446	1.19	1, 655	20.65*	3, 446	51.16*	1, 596	−0.55
SCS	140.49*	3, 484	12.96*	1, 665	50.53*	3, 484	134.29*	1, 628	−0.78

Note. Contrast analyses compare baseline to 12 week change across condition; ASI = Anxiety Sensitivity Index; BAI = Beck Anxiety Inventory; BDI = Beck Depression Inventory; PSWQ = Penn State Worry Questionnaire; QOLI = Quality of Life Inventory; AAQ = Acceptance and Action Questionnaire16; BAFT = Believably of Anxious Feelings and Thoughts Questionnaire; MASS = Mindful Attention Awareness Scale; SCS = Self-Compassion Scale; * = $p < .05$.

Once participants in both conditions had used the workbook, no significant between-group differences were observed on the SCS or MAAS at 6- or 9-month assessments (all *p*s > .28). This indicates participants in the wait-list condition experienced comparable improvement on these measures after, but not before, crossing over to treatment. Within both groups, no significant differences were observed between post-assessments and follow-ups on the MAAS or SCS (Tukey–Kramer adjusted *p*s > .54). This indicates that pre-post improvements on these measures were maintained through follow-ups.

However, significant between-group differences did emerge at 6-month assessment on the AAQ ($F[1, 569] = 4.43$, $p < .05$, $M_{diff} = 4.15$) and BAFT ($F[1, 560] = 5.63$, $p < .05$, $M_{diff} = 7.10$). Participants in the immediate workbook condition evidenced significantly lower scores (i.e., more improvement) on both measures compared to the wait-list group. Although both groups had received the workbook intervention at this point, more time had passed for the immediate workbook participants. At 9-month assessment, there were no longer group differences on the AAQ ($p = .67$) or BAFT ($p = .53$), indicating a similar response to treatment in both arms by the end of the study. Examination of within-group changes on these measures revealed that AAQ scores slightly improved in the wait-list group between 6- and 9-month assessments. Although this change was not statistically significant (Tukey–Kramer adjusted $p = .54$), it was sufficiently large to render the 9-month follow-up comparison with the immediate workbook condition nonsignificant. A similar pattern was observed on the BAFT, yet the improvement between 6- and 9-month follow-up assessments was statistically significant in the wait-list condition, $t(601) = 3.26$, Tukey–Kramer adjust $p < .05$, $M_{diff} = 8.91$. Similar to the AAQ, this improvement was large enough to render the 9-month between condition follow-up comparison nonsignificant.

RELIABLE AND CLINICALLY SIGNIFICANT CHANGE

Table 4 displays the reliable change index and clinical cutoff score used for each outcome variable, posttreatment and follow-up means collapsed across conditions, and change scores collapsed across conditions. On average, the sample demonstrated reliable improvement and completed treatment in the nonclinical range on the ASI immediately posttreatment, and on the ASI and BDI-II at 9-month assessment. Likewise, the sample showed reliable reductions in severity of anxiety symptoms (BAI) which, on average, fell from the severe to the moderate range (Beck & Steer, 1993) following treatment. At the individual level, 28.2% of participants at posttreatment, and 35.1% at 9-month assessment, evidenced reliable improvement and completed treatment in the nonclinical range on the BAI (reliable *and* clinically significant change). Likewise, a substantial number of participants demonstrated reliable, clinically significant improvements at posttreatment and 9-month assessment on the ASI (43.3% and 54.3%), BDI-II (34.1% and 46.5%), PSWQ (18.0% and 26.7%), and QOLI (17% and 14.9%).

Discussion

Despite the proliferation of empirically supported treatments for anxiety disorders, most people across the world do not receive treatment, often because of barriers to face-to-face therapy. With this concern in mind, we conducted a preliminary effectiveness trial of an ACT-based self-help workbook for anxiety difficulties in an international sample. We examined the impact of the workbook on anxiety and related symptoms, quality of life, and ACT treatment processes, aiming to simulate a pure self-help context with no therapist contact. This work is important because the cost of untreated anxiety disorders is great and we simply do not have enough therapists to meet global needs by relying solely on face-to-face treatment (Kazdin & Blasé, 2011). Bibliotherapy is a promising

Table 4
Reliable and Clinically Significant Change

	RCI	Cut-Off Score	Average Pre-Post Change	Average Post-Intervention Score	Average Pre-9-Month Change	Average 9-Month Follow-Up
ASI	11.82	≤30*	12.07	23.06	15.33	19.34
BAI	10.40	≤16*	10.41	19.25	12.54	16.95
BDI	9.81	≤14*	9.94	15.00	11.89	12.79
PSWQ	9.23	≤45*	10.94	54.96	14.44	51.02
QOLI	2.31	≥0.41†	1.01	0.97	1.27	1.19

Note. RCI = Reliable change index; defined as $1.96*SD_{initial}*SQRT(2)*SQRT(1- alpha)$; Reliable change was present if average change exceeded RCI; Clinically significant change was present if average post or follow-up scores exceeded cut-off score; ASI = Anxiety Sensitivity Index; BAI = Beck Anxiety Inventory; BDI = Beck Depression Inventory II; PSWQ = Penn State Worry Questionnaire; QOLI = Quality of Life Inventory; * Cutoff score was determined using suggested clinical cutoffs; † Cutoff score was defined as 2 SD below mean for nonclinical population.

intervention modality that might help meet these needs, but few commercially available self-help workbooks have been evaluated, and fewer have been examined without therapist or researcher contact as part of treatment.

The present study provides preliminary support for the effectiveness of the *MAWA* in a self-help context. Participants in the immediate workbook group demonstrated significant pre-post improvements in anxiety symptoms, anxiety sensitivity, worry, depressive symptoms, and quality of life compared to participants in the wait-list condition. Moreover, the immediate workbook condition showed relative pre-post increases in psychological flexibility, cognitive defusion, mindfulness, and self-compassion. Critically, wait-list participants significantly improved on outcome and process assessments *after*, *but not before*, crossing over to treatment. This suggests improvements were likely not the result of time, enrollment in a clinical trial, or completing assessments. Finally, treatment gains were maintained at follow-up assessments.

Interestingly, small improvements were observed on the AAQ and BAFT between posttreatment and first follow-up assessment, with a significant between-condition difference at the 6-month follow-up assessment. As noted, preliminary analyses indicated no significant between-group differences on demographic, outcome, or process variables. We cautiously attribute this difference to time (i.e., more time to practice skills) because scores had plateaued for the immediate workbook group with more time post-acute intervention, and the between-condition differences disappeared at 9 months once a similar amount of time had elapsed for the wait-list condition (which also evidenced a plateau). Similar continued or delayed improvement from post to follow-up has been found in previous ACT outcome studies (e.g., Hayes, Wilson, et al., 2004). In fact, a recent study evaluating ACT for panic disorder and agoraphobia also observed continued improvement on the BAFT from posttreatment to follow-up (Gloster et al., 2015). This pattern of results may be a particular focus of future research examining the process of change in constructs such as psychological flexibility.

When examining clinically significant change, over one-third of participants evidenced clinically significant improvements in anxiety symptoms, and about half demonstrated such improvements in anxiety sensitivity and depressive symptoms by the end of the study. Yet, less than one-fourth of participants showed clinically significant changes in quality of life, despite experiencing statistically significant improvements and despite the focus of ACT on valued living. This pattern might suggest that more extensive work applying workbook skills is needed to further improve quality of life. Still, it is worth noting that the goal of most self-help interventions is not to produce drastic improvements, but rather to provide moderate or small levels of needed help to large numbers of people, in a way that face-to-face treatment cannot (Kazdin & Blasé, 2011). Nevertheless, rates of clinically significant change observed in the present study are either similar to, or surpass, rates commonly reported in intervention studies using these calculation methods (Bauer et al., 2004; Lambert, 2013).

To provide additional context, we compared our results to those from trials of therapist-delivered ACT for anxiety disorders. For example, improvements in our study are similar to those reported by Arch, Eifert, et al. (2012) in a large RCT comparing ACT and traditional CBT. Baseline ASI, QOLI, and AAQ scores in the present study are comparable to those reported by Arch, Eifert et al. In addition, our observed pre-post improvements on the ASI (M_{diff} = 12.07) and QOLI (M_{diff} = 1.01) are similar to those observed by Arch and colleagues (M_{diff} = 13.16 and M_{diff} = 1.23 respectively). Lastly, changes on the AAQ 16 in the present study (M_{diff} = 15.58) exceed those observed by Arch, Eifert et al. (M_{diff} = 11.81).

Likewise, our results are similar to those reported by Forman, Herbert, Moitra, Yeomans, and Geller (2007) in an RCT comparing ACT and cognitive therapy for anxiety and depression. Pre-post improvements in BAI (M_{diff} = 10.41) and BDI-II (M_{diff} = 9.94) scores in the present study are larger than those reported by Forman et al. (BAI: M_{diff} = 3.10; BDI-II: M_{diff} = 6.39). Although somewhat surprising, this difference might reflect a floor effect, as Forman and colleagues reported lower pretreatment BAI and BDI-II scores compared to the present study. Yet, it is noteworthy that the *MAWA*, used with no therapist contact, might have produced improvements similar to therapist-administered ACT, in a sample reporting similar or greater baseline distress.

The attrition observed in this study also deserves attention. Though some bibliotherapy trials have reported lower attrition rates, these studies often involved significant therapist and/or researcher contact (see Newman et al., 2003), presenting two primary problems. First, although traditional therapy might include guided self-help, evaluating self-help in the context of notable therapist or researcher interaction is not, strictly speaking, self-help, and may not mirror conditions under which individuals use self-help. Second, researcher/therapist contact produces difficulty isolating variables responsible for improvements. For example, some participants cite

talking with researchers via phone as the main reason they improve following self-help bibliotherapy (Smith, Floyd, Scogin, & Jamison, 1997).

With these concerns in mind, and based on previous recommendations that bibliotherapy research use more realistic self-help contexts (see McKendree-Smith et al., 2003), our goal was to assess the impact of the *MAWA* when used by individuals entirely on their own. Efforts were taken to eliminate therapeutic contact. We answered no questions regarding treatment, made little effort to actively retain participants or encourage workbook completion (e.g., no weekly phone calls), and greatly minimized reminder emails related to assessments. Simultaneously, this study was somewhat ambitious in its use of multiple follow-up assessments. Data loss and attrition are potential "side effects" of such naturalistic longitudinal intervention research (see Castonguay et al., 2010) and high attrition rates are not uncommon in self-help research (e.g., Buwalda & Bouman, 2009; Mataix-Cols & Marks, 2006), although attrition is lower in face-to-face CBT (18.4%; Swift & Greenberg, 2012).

Several factors suggest that attrition in the present study did not bias conclusions about treatment effects. First, participants most frequently cited life events or lack of time, rather than lack of improvement, as reasons for dropping out. Second, demographic characteristics (e.g., ethnicity), clinical characteristics (e.g., diagnosis), and pretreatment baseline scores on all assessments did not predict dropout. Third, we used a mixed models approach to estimate treatment effects using all available data from all participants. When data are missing at random, mixed models accurately estimate treatment effects, even in the presence of high dropout (Gallop & Tasca, 2009). We conducted pattern-mixture analyses to confirm that data were missing at random. For all outcome variables, and all but one of the ACT process variables, missing data patterns (completion vs. dropout) were not informative, and did not significantly impact treatment effect estimates. The only exception was for self-compassion. Although this specific result based on pattern-mixture analyses should be interpreted cautiously, treatment effects for self-compassion appeared larger for completers relative to dropouts, though remaining statistically significant for both groups.

It is certainly the case that attrition should be minimized as much as possible, and enhancing compliance with self-help resources may itself be a target of future work. However, it may also be the case that higher attrition rates are inevitable in this research area and potentially preferable to lower attrition rates achieved through research-motivated retention strategies (for a discussion, see Amico, 2009). Furthermore, attrition might provide useful information about real-life problems self-help users encounter (Eysenbach, 2005; Williams & Whitfield, 2001). Attrition in this study implies that self-help bibliotherapy is a cost-effective, widely available, and efficacious intervention, but also one that some users might have difficulty completing without external sources of encouragement. Yet, this does not mean that self-help bibliotherapy is not useful. Rather, self-help books might need to explicitly address treatment ambivalence, perhaps by incorporating motivational interviewing (Miller & Rollnick, 2013) as done in some face-to-face transdiagnostic treatments (e.g., Barlow et al., 2011). Despite identifiable limitations, low-cost/low-resource interventions are needed to reach people who lack access to care (Kazdin & Blasé, 2011). The present work represents a step toward this larger goal.

There are several other implications of the present work. Most notably, results suggest ACT might be effective for anxiety disorders when delivered in a self-help format without therapist contact, coaching, or support. This conclusion, though preliminary, is important for three reasons. First, most people likely use self-help books without therapist guidance (McKendree-Smith et al., 2003). Second, an expansion of evidence-based treatment for anxiety, beyond modalities involving therapists, is needed to meet the high demand for services, which far outweighs current resources (Kazdin & Blasé, 2011). Cost-effective, widely applicable methods of treatment dissemination, such as the present workbook and ACT-based self-help more generally, might be one method to address these needs. Third, the gap between demand and available services is a global problem, and the present study suggests ACT-based self-help might be effective in an international population. Future research could build on this work to further evaluate the impact of ACT in self-help contexts.

Indeed, despite the availability of self-help books, few are evidence-based and even fewer have been subjected to empirical evaluation (Malouff & Rooke, 2007; Redding et al., 2008). The relative lack of support is unfortunate, and researchers have recently argued that ACT self-help books specifically might lack necessary evaluation to support claims of their usefulness (see Rosen & Lilienfeld, 2015). Thus, efforts such as the current study are sorely needed and directly address such concerns. Future research should continue to evaluate popular commercially available self-help books, perhaps comparing their use to face-to-face therapy and other self-help formats.

Relatedly, the few self-help books that have received empirical support tend to target specific, narrowly defined problems (e.g., social anxiety,

panic disorder). Arguably, such a focused effort is necessary during early stages of treatment development and dissemination. However, there is a need to expand the scope of this work to address transdiagnostic concerns and processes that might account for high rates of emotional disorder comorbidity (Brown & Barlow, 2005). Along these lines, the present study is unique in showing a broad ameliorative effect of self-help bibliotherapy using a transdiagnostic approach targeting the entire spectrum of anxiety disorders within a clinically distressed anxious sample. Indeed, the workbook focused exclusively on ACT processes thought to underlie anxiety difficulties, without offering readers anxiety reduction as a goal or outcome. Nonetheless, the results suggest the workbook produced improvements in anxiety symptoms, depressive symptoms (a problem area that was never addressed specifically), and quality of life. These outcomes speak to the broad applicability and potential utility of ACT when delivered as a self-help intervention. Self-help interventions that focus on core processes transecting various forms of psychological suffering might, in turn, increase the likelihood of meaningful changes that are both specific and broadly applicable to many areas of functioning.

Although the present findings are promising, they should be interpreted in light of several potential study limitations. First, this study used a wait-list controlled crossover treatment design. On the one hand, wait-list designs are useful as an early step in treatment and dissemination research (Carroll & Nuro, 2002), provide control for traditional threats to internal validity (e.g., time; Mohr et al., 2009), and are common in self-help research (see Hirai & Clum, 2006). Consequently, we chose to focus on dissemination in an international sample and on the impact of workbook use on ACT processes, consistent with the development strategy of ACT (Hayes, Levin, Plumb-Vilardaga, Villatte, & Pistorello, 2013; cf. Muto et al., 2011). On the other hand, wait-list designs are susceptible to several confounds (Cunningham, Kypri, & McCambridge, 2013). One method of mitigating potential confounds is to employ a crossover design, which was done in this study. Results showed the wait-list group improved after, but not before, using the workbook, which allows for stronger causal inference compared to a standard no-treatment control condition. As empirical support emerges for other transdiagnostic self-help workbooks, we recommend using active comparison conditions (e.g., other self-help books, face-to-face treatment).

Additionally, the international sample, despite its geographic diversity, was predominantly White and highly educated. Furthermore, assessments were limited to self-report. While the nature of the study prevented direct observation, it might have been helpful to track participant behavior within the study website (e.g., monitoring log-in frequency). In addition, although assessed at the beginning of the study, we did not track use of concurrent medication, therapy, or other treatment during the trial. Finally, as addressed above, a cost of our naturalistic design was the relatively high attrition rate.

As a whole, the present results support using the *MAWA* as a self-help intervention in an international sample struggling with anxiety and depressive concerns, and support the effectiveness of ACT for anxiety disorders more generally. As such, the present findings add to growing evidence that ACT is efficacious and effective for anxiety disorders (for a review, see Swain et al., 2013). Additionally, workbook use produced improvements in ACT treatment processes (e.g., psychological flexibility, defusion; see Arch, Wolitzky-Taylor, Eifert, & Craske, 2012; Forman et al., 2012). Although not a substitute for formal mediation, it is encouraging that workbook use was linked with change in these processes.

Though we are encouraged by these findings, more work is clearly needed to adequately address unmet behavioral health care needs, perhaps by exploring ways to motivate self-help users without using therapist contact. As self-help is increasingly being disseminated using not only books but also the Internet and mobile devices, we likely will be better positioned to address a pressing global problem related to the underutilization and lack of access to effective behavioral health care. This study is just one small step in that direction, and in renewed research efforts to develop intervention technologies that can be readily adapted by end users to both prevent and alleviate suffering.

Conflict of Interest Statement

The authors declare that there are no conflicts of interest.

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