

Evaluating Acceptance and Commitment Therapy: An Analysis of a Recent Critique

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Acceptance and commitment therapy (ACT) is a newer psychotherapy that has generated much clinical and research interest in recent years. However, the approach has begun to receive strong criticism from proponents of traditional cognitive-behavioral therapy (CBT). Hofmann and Asmundson (2008) recently compared and contrasted ACT and traditional CBT. They concluded that ACT's criticisms of traditional CBT are inaccurate; both ACT and CBT can be understood using a similar theoretical model; and there is no evidence that ACT represents a "third wave" of behavior therapy, as is sometimes claimed by its proponents. In the current article, I further analyze Hofmann and Asmundson's critique of ACT to determine its evidential merit and to attempt to clarify potential points of misunderstanding between CBT and ACT proponents.

Keywords: acceptance and commitment therapy, cognitive behavior therapy, psychotherapy research

Acceptance and commitment therapy (ACT) is a novel acceptance/mindfulness-based behavioral treatment that has been increasing in popularity in recent years. A detailed description of ACT theory and technique is beyond the scope of the current article, and thus it will only be summarized briefly here. Readers are referred to other books and articles that provide more detailed descriptions (Hayes, 2004a, 2004b; Hayes, Barnes-Holmes, & Roche, 2001; Hayes, Luma, Bond, Masuda, & Lillis, 2006; Hayes & Strosahl, 2004; Hayes, Strosahl, & Wilson, 1999).

ACT stems from a philosophy of radical behaviorism. The approach itself is rooted in a specific theoretical model, called Relational Frame Theory (RFT) (Hayes et al., 2001), which was developed to provide an updated behavior analytic account of language that expands upon the previous work of B. F. Skinner. In general, ACT can be described as combining acceptance and mindfulness strategies with overt behavior change efforts to improve what its creators call psychological flexibility (Hayes et al., 1999). *Psychological flexibility* is defined as "the ability to contact the present moment more fully as a conscious human being, and to either change or persist when doing so serves valued ends" (Hayes, Strosahl, Bunting, Twohig, & Wilson, 2004, p. 5). In other words, healthy psychological functioning is proposed to be related to a person's ability to adaptively respond to changing environmental contingencies. In contrast, psychological *inflexibility* or rigidity is theorized to be the result of what ACT calls cognitive fusion and experiential avoidance. *Cognitive*

fusion is defined as "the tendency of human beings to live in a world excessively structured by literal language" (Strosahl, Hayes, Wilson, & Gifford, 2004, p. 39). For example, when a person is fused with a thought ("I am depressed"), he/she is experiencing that thought literally ("I" = "depression"). This cognitive fusion permits the literal content of thinking to dominate over a person's behavior ("I can't go to work today because I am depressed"). Cognitive fusion also fosters *experiential avoidance*, which is defined as "the attempt to escape or avoid the form, frequency, or situational sensitivity of private events, even when the attempt to do so causes psychological harm (Hayes et al., 2004, p. 27). When engaged in experiential avoidance, the person attempts to avoid or suppress undesirable private material such as thoughts, memories, emotions, and bodily sensations as if they were inherently harmful, even though doing so can paradoxically worsen these problems in the long-run (Wenzlaff & Wegner, 2000). The co-processes of fusion and experiential avoidance result in the narrowing of a person's behavioral repertoire (i.e., psychological inflexibility), which is believed to lead to and maintain a wide spectrum of psychopathological behaviors. ACT targets six core processes for psychological flexibility: promoting acceptance of distressing internal experiences, fostering cognitive defusion so the literal content of thought does not dominate over a person's behavior, practicing awareness of ongoing experience in the present moment, establishing a stable sense of self that is broader than merely its evaluative content, developing personal valued life directions to guide behavior, and committing to actions that are consistent with these personally chosen values.

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Various psychotherapeutic techniques, many of which are inspired by or borrowed from other approaches to psychotherapy (e.g., humanistic, gestalt), are used to address

psychological inflexibility. In particular, ACT makes heavy use of metaphors, logical paradoxes, and experiential exercises, as well as more traditional behavioral techniques (e.g., behavioral activation, exposure). The goal of these strategies is to improve psychological flexibility by fostering acceptance of internal states of distress and cognitive defusion from problematic language-based processes.

Initial research on ACT suggests that: (1) Psychological inflexibility is related to diverse indices of psychopathology as predicted. (2) ACT has been shown to be potentially efficacious for a variety of clinical conditions based on preliminary trials. (3) Many of the specific components of ACT show initial evidence of efficacy in experimental studies. (4) ACT appears to work at least partly through its hypothesized mechanisms of action; although formal statistical mediation has only been demonstrated in a few studies to date (Hayes et al., 2006). The aforementioned research is in addition to numerous experimental studies conducted to date that separately lend support for RFT, the underlying basic science research program that relate to the clinical application of ACT (Hayes et al., 2001).

The ACT studies conducted to date lend support for many important aspects of the approach, but most represent small pilot studies that have methodological limitations. Thus, independent replication trials using larger samples will be necessary to confirm these initial promising findings. However, the evidence to date appears moderately strong in support of ACT at this stage of investigation. Recent meta-analyses of ACT outcomes conducted in a wide-range of clinical populations indicate medium to large effect size differences versus comparison conditions (Hayes et al., 2006; Öst, 2008; Powers, Zum Vörde Sive Vörding, & Emmelkamp, 2009), which is consistent with the broader CBT literature. Contrary to some authors' claims to the contrary regarding ACT's empirical status (see Öst, 2008), the American Psychological Association's Division 12 (Clinical Psychology) recently included ACT for depression on its list of empirically-supported treatments, concluding that it has "moderately strong" empirical support based on published clinical trials¹.

The increasing popularity of ACT in recent years has begun to draw the attention and scrutiny of proponents of traditional cognitive-behavioral therapy (CBT), many of whom have expressed skepticism about the approach. In recent articles and book chapters, some critics have argued that ACT offers relatively minor variations compared with traditional CBT that may not warrant the widespread clinical and research attention that the treatment has been receiving (Corrigan, 2001; Hofmann, 2008a; Hofmann & Asmundson, 2008; Leahy, 2008; Öst, 2008; Velten, 2007). The purpose of the current article is to explore emerging criticisms of ACT's rationale and treatment model from the traditional CBT community. The goal is not to "defend" ACT per se, but instead to compare and contrast the comments of critics with the claims of ACT proponents to determine whether these arguments are logically consistent and empirically justified. Recently, Hofmann and Asmundson (2008) published a comparison of ACT versus CBT and concluded that ACT may be more "old hat" than "new wave." Specifically, these au-

thors: (1) responded to Hayes and colleagues' previously published criticisms of traditional CBT; (2) proposed that Gross' (2001) emotion regulation model can be used to explain both ACT and CBT; and (3) rejected the argument that ACT represents a "third wave" of behavior therapy. Each of these topics is discussed in detail below to better understand the concerns expressed by critics and to attempt to determine their actual evidential merit.

AN ANALYSIS OF HOFMANN AND ASMUNDSON'S REBUTTAL TO HAYES ET AL.'S CRITIQUE OF CBT

In an attempt to differentiate ACT from traditional CBT, Hayes and colleagues (Hayes, 2004a; Hayes et al., 2006; Hayes et al., 1999) have offered critiques of traditional CBT theory and technique. In their own review, Hofmann and Asmundson (2008) attempt to rebut many of Hayes and colleagues' criticisms. First, I present the original criticisms offered against CBT by Hayes and colleagues, followed by Hofmann and Asmundson's rebuttals, and finally my analysis of these issues.

Critique 1: CBT Is a Mechanistic Approach

Hayes et al. (1999) argue that CBT-based approaches hold certain underlying "mechanistic" philosophical assumptions, such that they promote the notion (sometimes implicitly) that internal mental states are directly causal in relation to behavior. In contrast, Hayes et al. (2006) state that ACT is based on a pragmatic philosophy called *functional contextualism*, which "views psychological events as ongoing actions of the whole organism interacting in and with historically and situationally defined contexts" (p. 4). Hayes et al. (1999) further explain:

Some types of cognitive-behavioral therapy, for example, are based on a computer metaphor (as is much of cognitive psychology itself). Like a computer, humans are thought to store, access, and process information. In this view, the task when dealing with an unworkable thought is to change the form of the thought, just as a computer may be changed by replacing memory chips or by changing software. This "out with the bad, in with the good" mechanistic approach is quite different from a contextual perspective wherein the emphasis may be on "seeing the bad thought as a thought, no more, no less." (p. 21)

Hofmann and Asmundson (2008) respond that the computer metaphor of CBT is simply "inaccurate" (p. 7). These authors further argue that CBT is not mechanistic and is not concerned with replacing "bad" with "good" thoughts, but instead is focused on systematically training the individual to produce more realistic and adaptive evaluations though

¹ http://www.psychology.sunysb.edu/eklonsky-division12/treatments/depression_acceptance.html

the modification of underlying information processing biases. Hofmann and Asmundson acknowledge that CBT aims to correct unrealistic cognitions that produce emotional distress, but they further clarify that “if there is good reason to be sad, angry, fearful, worried, and so forth, the CBT therapist will not attempt to change these adaptive responses” (p. 7).

First, is it true as Hofmann and Asmundson (2008) assert that the computer metaphor of CBT used by Hayes et al. (1999) is incorrect? The following is an example of how one cognitive therapy researcher proposed explaining the cognitive processes targeted by CBT:

Each ICS [Interacting Cognitive Subsystem] subsystem has a memory that stores copies of all the patterns of information that it takes as input. It follows that, where the depressive interlock configuration has been operating for some time, the recent sections of the Implicational subsystem’s memory store will contain many representations of depression-related schematic models. Once the distraction task is complete, these models will be easily accessed, effectively “leaking back” into the data stream circulating round the central engine, and so restarting the depression interlock configuration. (Teasdale, 1996, p. 39)

Such writings clearly use computer-related terminology and concepts, which in turn suggest that there are indeed mechanistic assumptions in at least “some types” of CBT, as Hayes et al. claim. To be fair, Hayes et al. could also have noted that there have been attempts to understand CBT from less mechanistic perspectives, such as Psychological Constructivism (Mahoney, 1991).

Hofmann and Asmundson (2008) are on firmer ground, though, when they argue that CBT therapists are concerned with more than simply replacing “bad” thoughts with “good” ones. It is true that descriptions of CBT often focus on changing specific negative thoughts offered by the patient. Hofmann and Asmundson, however, clarify that the *ultimate* aim of CBT is to change information processing biases more broadly so that evaluations better conform to the reality of the situation or are more adaptive in nature. At least in their early writings, Hayes et al. (1999) admittedly present an oversimplified description of CBT when they imply that its therapists only focus on good-bad thought swapping techniques.

Even if we accept these points of clarification, Hayes et al. (1999) would likely still view CBT as implicitly mechanistic from an epistemological standpoint. For example, the Beckian CBT therapist encourages the patient to examine particular “automatic thoughts” associated with an emotionally distressing situation, and then helps him/her to identify and correct the “distortions” (e.g., black-or-white or dichotomous thinking) contained in these cognitions. These distorted automatic thoughts are believed to stem from more general underlying dysfunctional beliefs or rules, and even more basic “core beliefs” or schemas that are largely derived from childhood experiences (Beck, 2008; Beck, 1995; Needleman,

1999). Ultimately, these more basic information processing components are thought to require modification to produce consistently realistic and adaptive appraisals.

As previously discussed, ACT is based on a contextual philosophy that views the whole organism in its historical and situational context as the level of analysis. In contrast, many forms of CBT assume that putatively faulty “parts” (e.g., distorted automatic thoughts, dysfunctional beliefs, or schemas) of a larger “system” (i.e., the person’s mind) can be effectively isolated and directly modified to produce behavior change. Hofmann and Asmundson (2008) emphasize that CBT is truly interested in changing basic information processing processes that are viewed as faulty, such as general information processing biases and schemas (“I’m an incompetent person”), not simply changing specific distorted cognitions (“I’m stupid for not being able to understand this new assignment my boss gave me”). However, this may well be a distinction without a difference from the perspective of ACT. Cognitive constructs such as schemas are in essence, simply larger “parts” of the “whole” that CBT considers to be faulty or maladaptive in particular ways. In contrast, Hayes et al. (1999) explain:

Rather than trying to change the *form* of private experience, ACT therapists attempt to change the *functions* of private experiences by manipulating the context in which some forms of activity (e.g., thoughts and feelings) are usually related to other forms (e.g. overt behaviors).” (p. 24)

In ACT, there is no assumption that a person’s thoughts, beliefs, rules, conditional statements, schemas, attributions, appraisals, evaluations, or any private events *per se* require direct modification efforts by the therapist in order for the patient to achieve his/her desired behavioral changes. Put another way: all of these cognitive constructions would be considered “forms” of private experience from an ACT standpoint.

Furthermore, Hayes et al. (1999) note that mechanistic approaches assume an ontological realism such that: “We can know what *is* because what *is* is real” (emphasis added, p. 20). However, ACT considers itself “a-ontological” in the sense that it assumes that “what is true is what works” (p. 20). In other words, CBT’s focus on identifying and changing putatively *distorted* or *faulty* information processing biases necessarily implies a certain degree of realism and mechanism. One can legitimately argue that CBT may not be as extreme or classic in its mechanism or realism as Hayes et al. seem to imply. However, Hofmann and Asmundson’s (2008) response fails to fully address the implicit mechanistic assumptions in traditional CBT as highlighted by Hayes and colleagues. Of course, it remains an open empirical question whether a mechanistic versus contextual approach is actually more useful from a psychotherapeutic or scientific standpoint.

Critique 2: CBT Is “Symptom” Focused

Hayes (2004a) also argues that CBT is more narrowly focused on targeting identified “symptoms” or what he calls the forms and frequencies of behavior (i.e., “first-order” change). In contrast, he asserts that ACT is more broadly focused on altering the functions and contexts of behavior (i.e., “second-order” change). For example, Hayes writes:

[T]he new behavior therapies carry forward the behavior therapy tradition, but they (1) abandon a sole commitment to first-order change, (2) adopt more contextualistic assumptions, (3) adopt more experiential and indirect change strategies in addition to direct strategies, and (4) considerably broaden the focus of change. (Hayes, 2004a, p. 6)

Hofmann and Asmundson (2008) disagree with Hayes’ premise that CBT is focused on symptom reduction:

The goal in CBT is to reduce or eliminate psychological distress. This goal incorporates symptom reduction. The process to achieve this goal, however, is not through direction modification of the symptoms but, instead, through identifying and modifying dysfunctional cognitions that are causally related to symptom interpretation and related psychological distress. (p. 7)

In other words, Hofmann and Asmundson argue that the aim of CBT is to reduce the emotional distress related to symptoms, not through the direct reduction of symptoms *per se*, but instead by altering the “cognitive distortion and misinterpretation that underlies the emotional distress associated with these [symptoms]” (p. 8).

First, many cognitive therapists would likely disagree with Hofmann and Asmundson’s (2008) premise that the goal of CBT is not to reduce distress through the direct modification of symptoms, but only through the alteration of underlying cognitive processes. Other authors who have research backgrounds and training in CBT as well as ACT also view this as a distinction between the two approaches (Forman & Herbert, 2009). Hofmann and Asmundson fail to provide citations or direct quotations from any primary CBT sources to support this particular radical interpretation of CBT. Thus, it is difficult to precisely determine the origins of these claims. Other prominent CBT theorists, however, seem to contradict Hofmann and Asmundson’s assessment. For example, in their seminal book *Cognitive Therapy of Depression*, A. T. Beck, Rush, Shaw, and Emery (1979) explain:

Since the selection of a focal point takes into account not only what the patient perceives as his crucial realistic problems but also the feasibility of resolving these problems promptly, there are times when consideration of the patient’s more general problems has to be postponed until his disabling symptoms can be alleviated. (p. 167)

A. T. Beck et al. describe the use of symptom alleviation techniques in CBT, including teaching patients “a variety of self-management procedures to control their anxiety” (p. 181). These authors further explain that such techniques are deemed effective because depression can be understood as being comprised of various interacting systems (e.g., affective, cognitive, behavioral, and physiological), and thus targeting one domain will produce changes in the others (again, notice the implicitly mechanistic philosophy of such an argument). Hofmann and Asmundson are correct in pointing out that the primary focus of CBT is to change cognitive biases because they are assumed to be the fundamental dysfunctional processes producing distress. Thus, Hofmann and Asmundson do not see these processes as “symptoms” *per se*. However, this goal is accomplished through a variety of different strategies, and direct symptom alleviation is an important part of that process from both practical and theoretical levels. Most CBT therapists and patients very much appreciate this aspect of the therapy and do not see it as a weakness at all.

But how does one define a symptom? It appears that the crux of the disagreement between Hofmann and Asmundson (2008) and Hayes (2004a) actually comes from their differing conceptualizations of what the “symptom” is versus what the mechanism is that produces the “symptom.” Understandably, Hofmann and Asmundson view dysfunctional cognitions as direct causal agents that affect behavior. However, Hayes and colleagues reject this premise first and foremost from philosophical grounds. Hayes et al. (1999) explain: “From a functional contextualistic perspective, only events external to behavior can ‘cause’ behavior” (p. 55, also see Hayes & Brownstein, 1986). Thus, a cognition, which is considered a type of “private” or internal event, cannot literally determine behavior from this perspective. This premise is, of course, subject to debate and stems from ACT’s roots in radical behaviorism. But from an ACT perspective, strategies to alter cognitive processes directly would be viewed as attempts to change a symptom or what is more broadly considered a “first-order” problem, not the factors actually producing the problem. As previously discussed, ACT offers its own theory for how psychopathology is created and maintained based on RFT (Hayes et al., 2001). Again, Hofmann and Asmundson appear to misunderstand the more fundamental aspects of Hayes et al.’s premise, and thus their response ultimately falls short in addressing the root of the disagreement between CBT and ACT.

Critique 3: CBT Is Weakly Linked to Basic Cognitive Science

Hayes et al. (2006) also argue: “The link between cognitive therapy and basic cognitive science continue to be weak” (p. 3). In contrast, Hayes and colleagues point to ACT’s link to learning theory and their basic research program called RFT. Hofmann and Asmundson (2008) contest this conclusion: “We are surprised that this critique was raised, perhaps because we have been directly involved in a number of exciting studies that directly link CBT and other scientific fields, most notably clinical neuroscience” (p. 8). These authors pro-

ceed to describe work linking exposure therapy for anxiety disorders and the process of extinction learning, which has been associated with certain neuroanatomical regions such as the prefrontal cortex and the amygdala (Myers & Davis, 2007). They also cite review articles recently published by Hofmann (2007, 2008b) that also present similar theoretical connections between cognitive and behavioral therapies and neuroscience.

First, it may be helpful at this point to discuss the context out of which CBT originally developed. CBT originally emerged during the 1960s and 70s, a time that also saw a burgeoning interest in the newer cognitive science fields (Freeman & Reinecke, 1995; Gaudiano, 2008; Hayes, Follette, & Follette, 1995). However, whereas cognitive psychology and neuroscience were developing in the experimental laboratory, CBT was developing in the therapy office. Both cognitive researchers and cognitive therapists shared the viewpoint that thought processes were essential to understanding human behavior, but they had very different aims. Cognitive psychologists and neuroscientists were interested in understanding basic mental processes, whereas cognitive therapists were interested in developing applied techniques to treat psychopathological cognitive processes. Thus, cognitive therapists ended up borrowing certain cognitive science concepts and models (e.g., computer-related metaphors and models), but they did not share the same research methods or goals.

It is curious why Hofmann and Asmundson (2008) attempt to take Hayes et al. (2006) to task for pointing out the historically weak links between basic cognitive science and CBT. They fail to mention that others have been making virtually the same observations for years, including noted individuals from within the CBT community (Ingram & Siegle, 2001; Teasdale, 1996). A recent special issue of the journal *Behavior Therapy* presented emerging research attempting to work toward a cognitive science basis for CBT. Writing a closing commentary on the research studies presented and the state of this literature in general, Matthews' (2006) appears to agree in principle with Hayes et al.'s critique: "But despite having embraced the *terminology* of cognitive psychology, clinicians have not been able to draw on experimental cognitive psychology to develop a scientific basis that would be useful in developing new treatment methods" (emphasis in the original, p. 314).

Cognitive science is a multidisciplinary field, and most relevant to the current discussion, encompasses experimental cognitive psychology (Gardner, 1985). It is unclear why Hofmann and Asmundson (2008) only focus their response on the connection between CBT and clinical neuroscience more narrowly. Regardless, the review article by Myer and Davis (2007) cited by Hofmann and Asmundson does not actually support their argument upon closer examination. This paper discusses exposure therapy specifically, which is the behavioral component of CBT that does not include the direct cognitive modification techniques specific to the full CBT approach. It is true that substantial links between classic behavior therapy and basic science exist, but this does not necessarily support CBT, which contains additional elements.

Furthermore, Hofmann's (2007; 2008) own writings on this topic do not represent novel research linking cognitive science to CBT more specifically. Instead, these papers are literature reviews that propose certain theoretical connections between CBT and clinical neuroscience. In one of these papers, Hofmann (2008) concludes that "exposure therapy is a form of cognitive intervention that specifically changes harm expectancy" (p. 206). However, the issue at hand is not whether behavioral interventions can influence cognitions. Both ACT and CBT camps would probably agree on this point. Rather, the issue is whether CBT theory and practice are firmly linked to what experimental cognitive science tells us about how normal human information processing works, how it becomes aberrant, and how it can be corrected. If, as claimed, Hofmann and Asmundson presented a representative sample of the currently available evidence to empirically link CBT and cognitive science (or even neuroscience more specifically), then it is unclear how one could interpret the cognitive science foundation of CBT as anything but "weak" at this point in time.

Critique 4: CBT Techniques Did Not Emerge from Basic Science Research

Hayes et al. (2006) also note the historical disconnection between the actual techniques used in CBT and basic science research: "Looking at the array of popular techniques developed in CBT, none are known to have emerged directly from the basic science laboratories" (pp. 6-7). Hofmann and Asmundson (2008) respond by providing what they describe as a representative example of how basic science has actually produced these CBT techniques. These authors' present the following line of reasoning:

1. Heimberg and colleagues developed a CBT protocol for treating social phobia in 1991, which was adapted from Beck et al.'s (1979) original CBT for treating depression.
2. In a randomized trial, Heimberg et al. (1998) showed that CBT was as effective as medication, and more effective than placebo for treating social phobia. However, the effect sizes achieved clearly indicated room for improvement.
3. Hofmann and Asmundson cite laboratory research occurring over the past 10 years on the psychopathology of social phobia that has identified certain cognitive constructs that appear to be associated with the disorder (e.g., self-focused attention).
4. They conclude by describing a modified version of CBT that uses innovative techniques such as video feedback to target self-focused attention specifically. A recent RCT of this modified approach conducted by Clark et al. (2003) suggested that the effect size gains might be superior to those achieved using Heimberg et al.'s original treatment.

Clearly some CBT researchers are working diligently to develop techniques stemming more directly from experimental research, and they may ultimately succeed. However, this is a relatively late development. Such efforts do not dispute the fact that the historical links between CBT and cognitive science have always been tenuous at best. A closer examination of Hofmann and Asmundson's (2008) example may actually do more to support rather than refute Hayes et al.'s (2006) original analysis.

1. Heimberg et al.'s (1998) approach is the most popular evidence-based CBT model of social phobia to date. The authors acknowledge that Heimberg et al.'s protocol was adapted from Beck et al.'s (1979) clinically-derived model and techniques for depression, which did not come from basic science research.
2. Hofmann and Asmundson summarize basic psychopathology research on social phobia that has occurred over the past 10 years. However, this research appeared to be conducted mainly in clinical samples and focused on psychopathological processes. There is also a need to connect CBT to basic research in normal cognitive processes, as the aim of the treatment is to return to the person to normal functioning. Hayes et al. would likely see CBT techniques emerging from research conducted in nonclinical samples as the ultimate goal of this line of investigation. It should be noted that RFT (Hayes et al., 2001) is based on behavioral principles applied to language and cognition that apply to pathological as well as nonpathological forms of behavior.
3. Clark et al. (2003) note that they actually developed their modified CBT protocol from a theoretical clinical model originally published in 1995 (Clark & Wells, 1995). The clinical model that inspired Clark et al.'s modified social phobia treatment was formulated before the experimental psychopathology studies that Hofmann and Asmundson cited to support their case were even conducted. Hofmann and Asmundson even acknowledge this at one point: "Clark and Wells' [sic] (1995) formulated a model that correctly predicted many of the maintenance factors and processes that were *later identified* in the aforementioned laboratory studies" (emphasis added, p. 9). This does not suggest an emergence of CBT techniques from basic science, but an attempt to link them after the fact. Although itself a laudable goal, this does not directly contradict Hayes et al.'s original criticism.
4. It also is important to note that the protocol developed by Clark et al. is very consistent with the original Heimberg et al. treatment, especially in terms of the core CBT principles and strategies of both. Thus, it would be more accurate to describe the techniques employed by Clark et al. as incremental improvements at best, rather than unique strategies that are fundamentally different from those employed in traditional CBT for social phobia.

5. Finally, it is misleading to suggest that Clark et al.'s modified treatment, based on one study conducted to date, is more efficacious than Heimberg et al.'s original treatment. The Clark et al. study did not directly compare the two approaches, and comparisons of effect sizes between studies are not definitive given that there are numerous factors other than a treatment's actual efficacy that will affect the results (e.g., sample characteristics). A dismantling study would be needed to test Hofmann and Asmundson's hypothesis.

To reiterate, Hayes et al. (2006) argued that no popular CBT techniques to date directly emerged from basic science (i.e., experimental laboratory research). Hofmann and Asmundson (2008) and Hayes et al. may quibble over the importance of the word "popular" to their respective arguments. Nevertheless, several aspects of Hofmann and Asmundson's example fail to convincingly refute Hayes et al.'s original claim. It is true that in recent years CBT researchers have begun to look to experimental psychopathology research as an inspiration for modifying their treatments with some early promise. However, much more research will be needed before basic cognitive science (especially research on non-pathological cognitive processes) can be convincingly linked to core CBT interventions.

Critique 5: CBT Is Not Supported by Component Analysis Studies

Component analyses or dismantling studies attempt to "unpack" the efficacy of a multi-component treatment by experimentally isolating and systematically testing the effects of its components separately (Kazdin, 1998). Preliminary research on several of ACT's core processes (e.g., acceptance, cognitive defusion) has been generally supportive thus far; although more research will be needed to test all the components of ACT, especially when used together (Hayes et al., 2006). In contrast, Hayes et al. (2006) note: "Component analysis studies have generally failed to find support for the importance of direct cognitive change strategies, which was the common sense lynch pin of CBT" (p. 3). A recent independent review of component analyses of CBT reached similar conclusions (Longmore & Worrell, 2007). Instead of attempting to dispute this conclusion, Hofmann and Asmundson respond by challenging the importance of component analysis studies for CBT. They argue the following:

Briefly stated, our argument is that a component analysis can neither support nor refute the CBT model because cognitions can change without explicitly targeting them in treatment. For example, a spider phobic person who exposes herself to spiders without experiencing any of the feared consequences will show a reduction in harm expectancy, even without any explicit cognitive restructuring techniques. (p. 10)

Originally, Hayes et al. (2006) questioned the importance of direct cognitive change strategies in CBT, and Hofmann

and Asmundson (2008) responded by arguing that the cognitive components are not necessary for cognitive change. However, this point is not connected to Hayes et al.'s original claim. To reiterate, the original criticism was that controlled studies have suggested that the "C" component in CBT has not been shown to be necessary in the vast majority of cases for producing the clinical improvement observed. This is not an irrelevant point as Hofmann and Asmundson argue. The original premise of CBT was that the explicit cognitive interventions would provide additional benefits beyond those already achieved using traditional behavioral methods only (e.g., *in vivo* exposure). For example, A. T. Beck et al. (1979) assert:

The impact of the therapeutic techniques derived from a strictly behavioral or conditioning model is limited because of the restriction to observable behavior and selective exclusion of information regarding the patient's attitudes, beliefs, and thoughts—his cognitions. Hence, even though the behavior therapist induces the patient to become more active, his pessimism, self-disparagement, and suicidal impulses may remain unchanged. (pp. 118-119)

If Hofmann and Asmundson concede the point that cognitive restructuring is superfluous to clinical improvement, then this would seem to undermine one of the central rationales for CBT. Traditional behavior therapy is in many ways a more parsimonious treatment and is less complicated to implement compared with a multi-component treatment like CBT. In trying to defend CBT, Hofmann and Asmundson actually appear to undermine it.

Critique 6: Improvement in CBT Often Occurs Before the Cognitive Interventions

Hayes et al. (2006) further argue that: "The response to traditional cognitive therapy often occurs before cognitive changes techniques have been implemented, a finding that has still not been adequately explained" (p. 3). Hayes et al. are referring primarily to the study by Illardi and Craighead (1994) reporting that 60-70% of the improvement in depression during CBT occurred during the first 4 weeks of the treatment, even though the cognitive restructuring component of the treatment is often first introduced after this point. The early stage of CBT for depression is dominated by behavioral strategies (e.g., activities scheduling). Thus, one interpretation, which seems to be preferred by Hayes et al., is that the earlier instated behavioral components of CBT are actually responsible for the majority of clinical improvement, rather than the cognitive components specific to CBT that occur later on in treatment. Hofmann and Asmundson (2008) respond by pointing out that that these findings have been contested in the literature. For example, Tang and DeRubeis (1999) argue that cognitive strategies actually are introduced during the first 4 weeks of treatment, and their reanalysis of the data suggests more of a dose-response pattern in many cases.

Hofmann and Asmundson (2008) correctly point out weaknesses in Illardi and Craighead's (1994) conclusions, as the literature in this area is sparse and contradictory. To settle this issue, it would be necessary to conduct a study directly testing Hayes et al.'s (2006) hypothesis that the benefit of CBT occurs because of the earlier implemented behavioral components, rather than the later initiated cognitive components. For example, depressed patients could be randomly assigned to receive cognitive restructuring first and then behavioral activation, the behavioral intervention first and then the cognitive strategies, or a control condition that contains neither of these explicit strategies (e.g., supportive therapy). Such a study would help to address two separate questions: (1) how much clinical improvement occurs early on in CBT and (2) what is the relative contribution of cognitive restructuring versus behavioral intervention to the clinical improvement observed in CBT? Unfortunately, such an investigation has not been conducted to date. Thus, Hayes et al.'s argument in this case is open to dispute, and Hofmann and Asmundson legitimately question this conclusion.

Critique 7: CBT's Hypothesized Mechanisms of Change Are Not Well-Supported

Changes in cognitive processes directly targeted during CBT are hypothesized to be primarily responsible for the clinical improvement observed. Of all Hayes et al.'s (2006) criticisms of CBT, the following is perhaps their most important: "Support for the hypothesized mediators of change in CBT is weak (e.g., Burns & Spangler, 2001; Morgenstern & Longabaugh, 2000), particularly in areas that are causal and explanatory rather than descriptive (Beck & Perkins, 2001; Bieling & Kuyken, 2003)" (p. 3). Hayes and colleagues have previously reported on the results of several preliminary studies that appear to support the hypothesized mechanisms of change in ACT based on statistical mediation analyses.

Previously, Hofmann and Asmundson (2008) attempted to defend the lack of positive findings from CBT component analyses by instead proposing that: "The real question is: Do changes in cognitions mediate changes in symptoms?" (p. 10). One would expect these authors then to proceed with their argument by offering strong evidence to support cognitive mediation in CBT intervention effects. But after acknowledging that the seminal paper describing straightforward methods for conducting statistical mediation analyses was published in 1986 (Baron & Kenny, 1986), Hofmann and Asmundson explain: "Very few studies have examined treatment mediation in CBT" (p. 10). Even though modern CBT was developed in the 1970's, these authors further note that most studies on the topic of CBT mediation effects have been conducted over the last 5 years. Based on a review of these studies, they conclude: "It is too early to make any firm conclusions based on this limited amount of research" (p. 10).

Earlier, Hofmann and Asmundson (2008) argued that component analyses were irrelevant, and that mediation studies were the key to determining the scientific basis for CBT. However, these authors then later admit that, although the

methods of determining statistical mediation have been available since the 1980's, few studies have been published on this topic in the CBT literature until very recently. The issue at hand is whether or not cognitive change is the mechanism (or at least one of the primary mechanisms) through which CBT specifically produces its clinical effects. Hofmann and Asmundson cite six studies that they suggest provide some support for cognitive mechanisms of action in CBT. Unfortunately, none technically proves mediation.

1. Wilson, Fairburn, Agras, Walsh, and Kraemer (2002) compared CBT versus interpersonal psychotherapy for bulimia nervosa. However, they found no significant treatment by mediator interaction: "The absence of an interaction is inconsistent with the view that the two treatments worked through different mechanisms" (p. 272).
2. Smits, Powers, Cho, and Telch (2004) randomized patients with panic disorder to CBT versus a waitlist or no treatment control. They found that the cognitive construct "fear of fear" mediated the effect of treatment on outcome. It is interesting to note that the concept "fear of fear" is not particularly consistent with the original cognitive model of psychopathology as it actually reflects more of a meta-cognitive concept more consistent with mindfulness interventions. However, as only a waitlist or no-treatment comparison condition was used, it is impossible to conclude that CBT operates specifically through the reduction of this "fear of fear" construct.
3. Hofmann (2004) reported the results of a randomized trial that examined the efficacy of group CBT, group exposure therapy without cognitive interventions, or waitlist control in a sample with social phobia. However, results failed to show cognitive change as a differential mediator of CBT versus exposure therapy without the cognitive interventions.
4. Smits, Rosenfield, McDonald, and Telch (2006) used a within-subjects design to examine cognitive change associated with fear reduction in patients with social phobia during a brief exposure-based intervention. However, as the study did not randomize patients to a non-CBT comparison treatment, it could not demonstrate cognitive mechanisms of change in CBT for social phobia specifically.
5. Hofmann et al. (2007) reported that cognitive change was associated with symptom improvement in panic disorder patients receiving CBT, but not in patients receiving pharmacotherapy alone. However, CBT was not compared to another psychotherapy condition in this study, and thus could not test the specific mechanisms of this approach. Furthermore, Hofmann and colleagues note the primary limitation of their study: "One must recognize, however, that our results do not conclusively prove that cognitive mediation occurred because we could not establish temporal precedence

of the mediator" (p. 378). Establishing the temporal order of the mediator variable is the *sine qua non* of statistical mediation (Holmbeck, 1997).

6. Finally, Petry, Litt, Kadden, and Ledgerwood (2007) found relatively weak evidence that "coping skills" mediated treatment response in the early part of treatment between CBT and Gamblers Anonymous conditions in a sample of pathological gamblers. However, this study did not specifically examine cognitive mediators, as Hofmann and Asmundson suggest, but instead investigated more generic "coping strategies," which included behavioral skills. The authors of this study actually conclude: "[R]elationships between coping skills and gambling behavior are fairly strong, regardless of treatment received" (p. 1280).

None of the studies cited convincingly demonstrates that cognitive change is specifically related to improvement following CBT, which is the central rationale for the approach. Hofmann and Asmundson (2008) did supply preliminary evidence that cognitive changes mediated overall symptom reduction in some cases. But they failed to demonstrate that CBT specifically works by directly altering these cognitive constructs. There also are many failed attempts at obtaining cognitive mediation reported by researchers in the literature. As a whole, the studies cited actually suggest that this cognitive change may be a nonspecific process unrelated to CBT. This implies that cognitive change may be the product of a different underlying process that is being impacted by any successful psychotherapy.

GROSS' EMOTION REGULATION MODEL AND ACT

As discussed, ACT proposes a comprehensive theoretical model called RFT (Hayes et al., 2001) as the experimental basis for the treatment approach. Hofmann and Asmundson (2008) do not directly challenge RFT as a legitimate account of ACT technique or process. Instead, they propose that a different theoretical model can be used to explain CBT as well as ACT. These authors argue that both ACT and CBT aim to reduce emotional distress, but they simply accomplish this goal using different techniques. Hofmann and Asmundson point to Gross' (2001) emotional regulation model to make their case. First, it is necessary to understand this model in detail. As originally explained by Gross and John (2003) :

This conception holds that an emotion begins with an evaluation of emotion cues. When attended to and evaluated in certain ways, emotion cues trigger a coordinated set of response tendencies that involve experiential, behavioral, and physiological systems. Once these response tendencies arise, they may be modulated in various ways. Because emotion unfolds over time, emotion regulation strategies can be distinguished in terms of when they have their impact on the emotion-generative process. (p. 348)

In the model, there is a distinction made between *antecedent-focused* and *response-focused* emotion regulation strategies. As their names imply, antecedent-focused strategies occur prior to the full activation of the emotion and its associated responses, whereas response-focused strategies occur after the emotion and related responses have already occurred. According to Gross' model, *cognitive reappraisal* (i.e., changing one's interpretation of a situation to make it less distressing) is an antecedent-focused strategy and *expressive suppression* (i.e., trying to hide the display of strong emotional reactions) is a response-focused strategy. In one study, Gross (1998) examined the effects of emotional regulation strategies in subjects who viewed a video of a graphic arm amputation. In the cognitive reappraisal condition, subjects were instructed to think about the film in a way that would prevent them from feeling upset by it. In contrast, subjects were instructed to hide their external emotional reactions to the film in the suppression condition. A third control condition was also included in which subjects simply were instructed to watch the film. Gross found that both reappraisal and suppression reduced the expression of emotion relative to the control condition. Furthermore, reappraisal decreased self-reported levels of disgust and did not affect physiological arousal. However, suppression did not decrease disgust, but did lead to increases in physiological responding. Gross interpreted these findings as suggesting two different emotional regulation processes.

Based on Gross' (2001) emotional regulation model, Hofmann and Asmundson conclude:

In essence, CBT and ACT target different stages in the emotion-generative process: CBT promotes adaptive antecedent-focused emotion regulation strategies, whereas ACT counter-acts maladaptive response-focused emotion regulation strategies. The cognitive restructuring techniques used in CBT are in line with the antecedent-focused emotion regulation strategies, providing skills that are often effective in reducing emotional distress in the long term. Acceptance and mindfulness-based strategies counter suppression and, thereby, alleviate emotional distress. (p. 12)

Based on the earlier discussion of ACT's theoretical model, this is an oversimplification of the approach. Furthermore, contrary to the claims of Hofmann and Asmundson (2008), many ACT strategies can be understood as being consistent with Gross' (2001) antecedent-focused strategies. If an emotion regulation strategy is used before the emotional response is fully activated, it is called "antecedent focused." According to the Gross model, it is the actual timing of the strategy that is of critical importance, not the focus on "cognitive reappraisal" *per se*. Gross (2001) focuses on cognitive reappraisal as one antecedent-focused strategy, but he notes that other strategies are permitted in the model (but are simply yet to be tested). For example, the cognitive defusion component of ACT could be viewed as an antecedent-

focused strategy based on Gross' model. The difference is that in ACT, cognitive defusion strategies are designed to undermine the undesirable *functions* of cognitions, whereas in CBT, direct cognitive modification techniques are used to alter the actual *forms* of the dysfunctional thoughts or information biases (Hayes et al., 1999).

Furthermore, it is important to point out that Hofmann and Asmundson (2008) fail to properly describe the concept of suppression as used in Gross' model. Gross (2001) is actually referring to a specific form of suppression, which is characterized by *expressive inhibition*. He explains: "The term 'suppression' also has a long history. It has been used to refer to inhibiting feelings, behavior, or thoughts. Here I use it to refer to inhibiting emotion-expressive behavior" (p. 219). Gross provides the following example: "An example of response-focused regulation is keeping a poker face while holding a great hand during an exciting card game" (p. 215). However, when Hayes et al. (1999) refer to suppression, they are generally referring to thought suppression more specifically. For example, they summarize the research on thought suppression in this way: "When subjects are asked to suppress a thought, they later show an increase in this suppressed thought as compared with those not given suppression instructions" (p. 60). This rationale is provided for why ACT views avoidance of unwanted thoughts as a counterproductive strategy in the long-run. After correctly understanding Gross' meaning of expressive suppression (versus thought suppression), it is clear that neither CBT nor ACT focus much on this process, and both could be viewed as fostering antecedent-focused strategies. Thus, Gross' model is not particularly helpful in distinguishing between CBT and ACT. Furthermore, Hofmann and Asmundson fail to address Hayes et al.'s claim that RFT provides the better overall theoretical explanation for ACT. Many different models can be adapted or altered to fit the data. Thus, it is most important to compare and contrast various models to identify which one seems to fit the data better overall.

IS THERE REALLY A "THIRD WAVE"?

In recent publications, Hayes (2004b) has claimed that ACT and other mindfulness/acceptance-based therapies can be viewed as representing a "third wave" of behavior therapy—the first wave being classic behavior therapy and the second wave being the "cognitive revolution" that ushered in tradition CBT. He explains:

Grounded in an empirical, principle-focused approach, the third wave of behavioral and cognitive therapy is particularly sensitive to the context and functions of psychological phenomena, not just their forms, and thus tends to emphasize contextual and experiential change strategies in addition to more direct and didactic ones. These treatments tend to seek the construction of broad, flexible and effective repertoires over an eliminative approach to narrowly defined prob-

lems, and to emphasize the relevance of the issues they examine for clinicians as well as clients. The third wave reformulates and synthesizes previous generations of behavioral and cognitive therapy and carries them forward into questions, issues, and domains previously addressed primarily by other traditions, in hope of improving both understanding and outcomes. (emphasis in original, p. 658)

He further states: “What I mean by a ‘wave’ is a set or formulation of dominant assumptions, methods, and goals, some implicit, that help organize research, theory, and practice” (p. 640).

Hofmann and Asmundson (2008) reject this premise, stating: “[W]e are not convinced that ACT or other acceptance-based treatment [sic] are part of a *third wave* of psychotherapy, replacing CBT. There is no data to suggest that it represents an entirely new treatment approach” (emphasis in original, p. 13). To support their case, Hofmann and Asmundson describe personal communications with the creators of other supposed “third wave” therapies. They report that the creators of Metacognitive Therapy (Wells, 2000) and Dialectical Behavior Therapy (Linehan, 1993) view their respective approaches as part of the family of cognitive-behavioral approaches. Hofmann and Asmundson also reiterate their argument that ACT and CBT both could be understood using Gross’ emotional regulation model, making them more fundamentally compatible than incompatible.

First, it is clear from Hayes’ direct quote presented above that he is not arguing that ACT is an “entirely new treatment approach” designed to “replace CBT,” as Hofmann and Asmundson assert. Instead, he is proposing that ACT is in many ways an extension of the behavioral and cognitive traditions, but that it has enough distinctive theoretical elements to make it something that can be legitimately differentiated from these previous approaches. In the end, the rhetoric of “third wave” used by Hayes (2004b) may be best viewed as a matter of opinion and historical perspective (Gaudiano, 2008). Thus, only time will tell whether ACT and other acceptance/mindfulness therapies will be viewed through the lens of history as truly representing novel approaches that deserve this distinct moniker. Hofmann and Asmundson (2008) make a legitimate point when they argue that there is a great deal of heterogeneity among the approaches sometimes lumped by Hayes into this third wave. It may be best to try to understand these newer therapies as falling along a continuum, with some approaches offering relatively minor variations from traditional CBT (e.g., Metacognitive Therapy), compared with others that hold seemingly incommensurate assumptions and principles that make them difficult to neatly reconcile with their predecessors in theory or practice (e.g., ACT). Hofmann and Asmundson suggest that an increased attention to mediation analyses in outcomes studies in CBT and ACT ultimately will be needed to distinguish between these approaches and to identify whether their mechanisms of change actually differ. This is a closing point with which Hayes and colleagues would likely agree.

SUMMARY

In their critique, Hofmann and Asmundson (2008) attempt to provide a rebuttal to Hayes et al.’s (2006) supposed misrepresentations of CBT. They also offer a model of emotion regulation that they claim could account for both CBT and ACT. Thus, it is not surprising that Hofmann and Asmundson fail to find Hayes’ (2004b) argument convincing that ACT represents a truly novel approach that deserves recognition as the “third wave” of behavior therapy. By providing a more detailed analysis of Hofmann and Asmundson’s claims, I attempted to clarify some of these issues. Although these critics understandably attempted to provide a more nuanced understanding of CBT compared with the sometimes oversimplified depictions presented by Hayes et al. (1999), at least as they appeared in early ACT writings, Hofmann and Asmundson ultimately fail to contradict the actual merits of the original critiques of CBT in the majority of cases. To summarize:

1. CBT appears to be more mechanistic in its underlying assumptions and thus more focused on symptom alleviation and changing the “forms” of behavior relative to an approach such as ACT. The strength of this argument depends somewhat upon one’s meaning and use of the term “symptom” and may be more a matter of degree than kind. The relative scientific benefits of a mechanistic versus a contextualistic philosophical approach to psychotherapy are yet to be determined.
2. CBT has historically weak theoretical and practical links to basic cognitive science and only recently has experimental psychopathology research begun to inform its techniques. Little evidence currently exists linking CBT and basic cognitive research focused on nonpathological information processing.
3. Component analyses have generally failed to support the need for multi-component CBT interventions that include an explicit cognitive modification component relative to more parsimonious behavioral approaches.
4. There has been scant evidence to date that the effects of CBT are produced specifically through its distinctive cognitive modification strategies, and a number of studies have produced largely disconfirming results (Longmore & Worrell, 2007), which Hofmann and Asmundson fail to address.
5. Some evidence suggests that a relatively large proportion of improvement occurs very early on in CBT, and thus often prior to the initiation of the core cognitive modification strategies. However, Hofmann and Asmundson correctly note that there are several possible explanations for this phenomenon and more research will be necessary to explain these findings.
6. Finally, a more detailed examination of Gross’ (2001) emotion regulation model appears to weaken Hof-

mann and Asmundson's claim that it can neatly explain account for the effects of both CBT and ACT.

Many of the weaknesses in Hofmann and Asmundson's (2008) arguments may stem in part from a more fundamental misunderstanding of ACT by these authors. For example, Hofmann and Asmundson repeatedly focus their discussion on ACT *techniques* in their article, rather than on its core principles of change. Hayes et al. (1999) explain: "The effective ACT therapist uses ACT as *functionally defined*, not merely as *topographically defined*" (emphasis in original, p. 16). Hayes et al. further explain that, within ACT, "virtually any behavior change technique is acceptable. The difference is that behavior change goals, guided exposure, social skills training, modeling, role-playing, couples work, and so on, are integrated with an ACT perspective" (p. 258). CBT traditionally places more importance on its distinctive set of techniques and strategies. However, ACT eschews this approach, instead arguing for the importance of principles of behavior change, and thus uses whatever techniques further this aim. In this way, ACT has a consistent and specific theoretical foundation rooted in a particular understanding of radical behaviorism, but it is eclectic at a technical level.

In the end, Hofmann and Asmundson's (2008) review would have been more useful if it had focused on a detailed analysis of ACT as it is actually described by its creators, and then presented evidence-based arguments that countered or challenged these claims. Instead, these authors frequently defined ACT in ways that made it easier to argue against. Thus, as Hayes and colleagues can be seen as sometimes presenting CBT in overly simplistic terms to provide a better rationale for ACT, so Hofmann and Asmundson can be seen as falling into a similar rhetorical trap. It is my hope that this review will help to better clarify some of the theoretical differences between ACT and traditional CBT to foster further substantive debate and dialogue on this topic among researchers and clinicians interested in these approaches.

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