COMMENTARY

Mindfulness and Psychological Process

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The author reviews the articles in the Special Section on Mindfulness, starting from the assumption that emotions evolved as signaling systems that need to be sensitive to environmental contingencies. Failure to switch off emotion is due to the activation of mental representations of present, past, and future that are created independently of external contingencies. Mindfulness training can be seen as one way to teach people to discriminate such "simulations" from objects and contingencies as they actually are. The articles in this Special Section show how even brief laboratory training can have effects on processing affective stimuli; that long-term meditation practitioners show distinct reactions to pain; that longer meditation training is associated with differences in brain structure; that 8 weeks' mindfulness practice brings about changes in the way emotion is processed showing that participants can learn to uncouple the sensory, directly experienced self from the "narrative" self; that mindfulness training can affect working memory capacity, and enhance the ability of participants to talk about past crises in a way that enables them to remain specific and yet not be overwhelmed. The implications of these findings for understanding emotion and for further research is discussed.

Keywords: mindfulness, meditation, Darwin, reactivity, mode of processing

In his book *The Expression of the Emotions in Man and Animals,* Darwin reports the following experiment on himself at the London Zoo reptile house.

> I put my face to the thick glass-plate in front of a puff adder in the Zoological Gardens, with the firm determination of not starting back if the snake struck at me; but as soon as the blow was struck, my resolution went for nothing, and I jumped a yard or two backward with astonishing rapidity. My will and reason were powerless against the imagination of a danger which had never been experienced. (Darwin, 1872, p. 40)

Much research on human emotion traces its origin to this seminal work*,* which signaled a major change in the way people conceived emotion and its expression. Darwin's intention was to present evidence for his theory of evolution and its central principles of inherited variation and natural selection. Now that his theories are broadly accepted, however, we do not use these inherited reactions to bolster evidence of evolutionary theory. Rather we use them to tell us about the phenomenology and underlying mechanisms of emotions; in particular which aspects are automatic, and which are more "controlled" or "strategic."

It is now commonplace to understand emotion as based on automatic processes, reactions elicited by situations that are of signifi-

cance for a person in terms of imminent threat (fear), loss of attachments or of status (sadness), blocked goal pursuit (anger), exposure or ingestion of unpleasant substances (disgust), and success in goal pursuit (happiness). The neurobiological mechanisms underlying such automatic reactions are well-understood, and this helps us see the way that people differ, one from another, in the magnitude, conditionability, and speed of extinction of such reactions.

Such basic emotional reactions are said to have a signaling function (Oatley & Johnson-Laird, 1987, 1996), with the negative emotions signaling a change in processing priorities for the person him or herself, and to others. But to be effective as signals, emotions have to be exquisitely sensitive to external contingencies. That is, they have to possess two necessary qualities. First they have to *turn on* when the external world demands it. Without this, our ancestors would not have jumped back from snakes, and we would not be here to read this journal. But second, and just as important, emotion has to *turn off* when circumstances in the external world change. Emotions evolved to be temporary—to be sensitive to the onsets and offsets of the dangers of the external world.

Darwin wanted his readers to see that emotions evolved long before hominids roamed the planet. We can easily see that bottom-up reactivity to threats and losses provides a good explanation of why emotions in animals and humans switch on. It does not do such a good job of explaining why emotions in humans are *maintained*. For this, we need to look at how humans evolved the capacity to build mental models and symbolic processing that released them from sensitivity to current contingencies (cf. Hayes, Luoma, Bond, Masuda, & Lillis, 2006). Although such symbolic representations of past and future give humans great advantages in problem-solving capabilities, it is this same capacity to work "off-line" that means emotions do not switch off. For current low mood can reactivate recollections of past loss and humiliation (see Lau, Segal, & Williams, 2004, for

review), and anxiety can simulate future terrors (see Schacter, Addis, & Buchner, 2008 for review). Problems arise when our "simulations" are treated by the evolutionary primitive neural pathways as real threats and real losses to be dealt with now and with a high degree of priority.

Clinically, we see the effect of such simulations—mental model building—as maintaining factors in many emotional disorders. These have been well-described in depression, where situations are typically interpreted in a negative way ("My lower than expected grade on this test means I will fail in *everything* I ever attempt"; Beck, Rush, Shaw, & Emery, 1979). It is important that simulation processes can be activated by the evidence of one's own mood: "This feeling sad means I am a failure" leading to depression about depression; Teasdale, 1983, 1988). As one patient said in an early session of a recent mindfulness-based cognitive therapy (MBCT) class "I can see that these negative thoughts mean I am depressed. I can see that. But then I say to myself: 'I'm getting depressed again, what an idiot I am.'" Similar "reactions to reactions" can be found in post traumatic stress disorder ("I should be over this by now; these flashbacks mean I am weak"; Ehlers & Clark, 2000). An essential aspect of cognitive treatment is to focus not only on the primary emotional expressions, but the mental models that are built around such expressions, give extra meaning to them, and so maintain the disorder after the Darwinian expression might otherwise have extinguished (see Figure 1).

Why do these mental models not switch off so readily? Because such model-building is an essential aspect of the symbolic processes that work so well for smooth pursuit of most tasks, problems, and situations we encounter (e.g. understanding language,
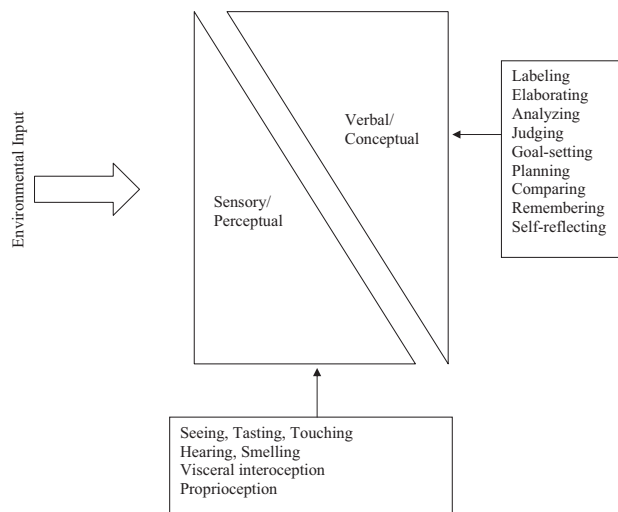
Figure 1. Two modes of mind. This shows a schematic representing the relative probability of two modes in which the mind operates: conceptual (language-based) processing versus sensory-perceptual processing. In every waking moment we are receiving sights, sounds, tastes, smells, touch: stimuli from the external and internal world, but these are generally ignored in favor of spending most of our attention in conceptual mode: thinking, planning, daydreaming, analyzing, remembering, comparing, judging, analyzing, and so forth. Attentional training in mindfulness programs cultivate the ability to shift modes as an essential first step to being able to hold *all* experience (sensory and conceptual) in a wider awareness that is itself neither merely sensory nor conceptual.

Sanford & Garrod, 1994). To be efficient and effective, these simulations need (a) to come on automatically, (b) to be taken to represent reality as a default option, (c) to use conceptual associative structures in memory, (d) to use anticipation of the future and recollection of the past to inform present problem solving, and (e) to include (and retain in working memory) what is the current goal to be selected and what should currently be de-selected, avoided, or inhibited (Duncan, 1993; Duncan, Emslie, Williams, Johnson, & Freer, 1996). This is the "doing" mode of mind (see Williams, 2008 for recent discussion) and, far from being an enemy that treatment has to overcome, such a mode is so essential for everyday living that damage to the operation of the doing mode has devastating consequences, clearly seen in those with dementia.

What has this to do with mindfulness? It shows first that psychopathology arises from the overuse, in some contexts, of common and quite normal psychological processes without which we could not function. It is using the doing mode to suppress or elaborate emotional expression that can tragically backfire, for it reduces attentional control and capacity, and further increases the emotional disturbance and helplessness it was intended to fix. Mindfulness training is not about getting rid of these processes or clearing the mind, but about coming to see where natural, automatic reactions stop and the simulation, elaboration, and avoidance processes begin.

Mindfulness training aims to cultivate an alternative ("being") mode through meditation practices that teach people how to pay open-hearted attention to objects in the exterior and interior world as they unfold, moment by moment. Attention is paid not only to the objects themselves but to our *reactions* to them, particularly reactions of wanting positive states to last, negative states to end, and neutral states to be less boring. In this way, the meditation practitioner begins to see clearly the difference between Darwinian reactivity, and the overlearned simulation processes that construct mental models and imbue objects and situations with extra implications. Figure 2 illustrates how one commonly used meditation practice, the Body Scan, is structured to (a) increase sustained attentional focus, (b) teach the difference between thinking *about* sensations versus *experiencing* them directly, and (c) teach participants to see clearly and relate differently to mental states such as boredom and restlessness. Participants have many opportunities in such practices to practice seeing their own "simulations," simply and nonjudgmentally, as mental events in the field of awareness, rather than truths that should be taken personally and require urgent action.

What is the effect of such mental training? Does it have effects that can be seen in imaging of the brain? Can we analogize this practice in the laboratory? Does it affect mood, sensitivity to pain, and working memory? And, in the end, does it cultivate a decentered perspective in which thoughts and feelings are related to as mental events, rather than as representing reality? All these questions are addressed by the articles in this Issue.

The Articles in This Issue

Together, the current articles demonstrate the diversity in designs that can be used to investigate mindfulness. Let us start by reviewing these designs.

First the articles demonstrate differences in how to define and "capture" the independent variable of central interest: mindfulness.
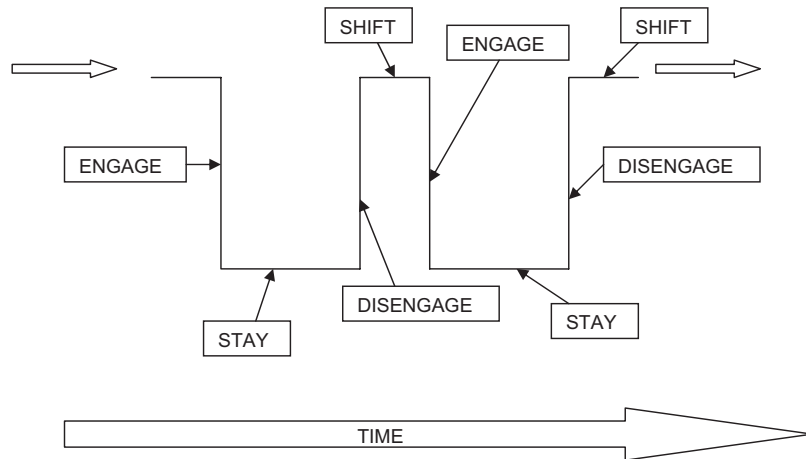
Figure 2. The attentional processes during one mindfulness meditation practice (the Body Scan). The attentional training involved in one meditation practice, the Body Scan, is shown in Figure 1. It shows the attention engagement−disengagement cycle that is repeated around 50 times during a Body Scan practice. Each cycle involves four intentional components: (a) shifting attention from one region of the body to another, (b) engaging attention at this site, (c) staying at this place to explore what sensations may be discovered here, (d) disengaging attention before repeating (a)− shifting to the next region of the body. During this practice, participants have additionally to hold in working memory two metaintentions: (a) to notice mind-wandering and return attention to the intended focus, and (b) to explore sensations—and acknowledge mind-wandering—with an attitude of friendly curiosity and compassion rather than comparison, analysis or judgment.

Between them, mindfulness is studied, first, in its "dispositional" form (Way, Creswell, Eisenberger, & Lieberman, 2010), that is, as an individual difference variable in relatively naïve participants. Second, it is studied in expert versus novice practitioners (Grant, Courtemanche, Duerden, Duncan, & Rainville, 2010; Perlman, Salomons, Davidson, & Lutz, 2010). Third, its effects are studied before and after an 8-week mindfulness training course (Farb et al., 2010; Goldin & Gross, 2010; Hargus, Crane, Barnhofer, & Williams, 2010; Jha, Stanley, Kiyonaga, Wong, & Gelfand, 2010). Finally, it is studied in brief laboratory interventions either in naïve participants (Erisman & Roemer, 2010), or in order to contrast naïve with expert participants (Perlman et al., 2010).

Second, the studies illustrate the range of designs that can be used to "challenge" the system and examine what effect mindfulness might have in moderating the impact of such challenges. These challenges included brief exposure to affective versus neutral laboratory stimuli (both verbal—Goldin & Gross, 2010; and images—Way et al., 2010), longer mood inductions using film material (Erisman & Roemer, 2010; Farb et al., 2010), exposure to physically painful stimuli (Grant et al., 2010; Perlman et al., 2010), exposure to intensive military training (Jha et al., 2010), and, finally, asking participants for descriptions of emotionally painful events (Hargus et al., 2010).

Third, these studies also illustrate the range of dependent measures that can be used: structural magnetic resonance imaging (MRI; Grant et al., 2010) functional MRI (Farb et al., 2010; Goldin & Gross, 2010; Way et al., 2010), self rating of affect, and of stimulus intensity and pleasantness (Erisman & Roemer, 2010; Perlman et al., 2010), working memory capacity (Jha et al., 2010), and independent ratings of transcripts of patients' descriptions (Hargus et al., 2010).

In examining each study, it is interesting to notice how the different designs approach the same basic question: does mindfulness have an impact and, if it does, how does it do so? Let us examine each contribution in turn.

Erisman and Roemer (2010) show that a brief (10 minute) mindfulness-type intervention or control intervention could change affective reactions to film material. As the authors themselves point out, the interventions may have been too brief in too few participants to yield definitive conclusions. Yet it nicely raises the general point about "how much is enough." Mindfulness teachers report that novice practitioners often have great difficulties in the first few weeks of a mindfulness-based program (Segal, Williams, & Teasdale, 2002), so how can we expect a 10-minute manipulation to have any effect whatsoever? This is an important question.

One answer to it is that the authors are not trying to analogize a full mindfulness treatment program, but to bring about very short-term changes that would need much longer to consolidate if they were going to bring about long-term benefit, but which can and do affect mood and other aspects of functioning in interesting ways (e.g., Arch & Craske, 2006). Another answer is that we know brief laboratory manipulations (e.g., analytic/ruminative vs. experiential/concrete processing of self-related material) can have large effects on dependent variables such as problem solving or autobiographical memory that are known to have important clinical impacts (Watkins & Teasdale, 2002, 2004), so why would brief mindfulness interventions not also be amenable to study in this way?

Bringing mindfulness under experimental control in the laboratory is only part of the picture, but it is an important aspect of understanding how the cognitive system incorporates new information or procedures and what effects such change has. Whether to call such laboratory procedures "mindfulness" is another matter. We could relabel them as brief "acceptance-based processing" interventions (or other similar labels that refer more precisely to the specific subcomponent of mindfulness treatment that is being

modeled in each study). This might avoid confusion with mindfulness in its longer, multicomponent, clinically based training. The field will doubtless find its own way of negotiating the terms it uses.

Way et al. (2010) build on previous work on dispositional mindfulness using Brown and Ryan's (2003) scale, which assesses "automatic pilot" and the effects of inattentiveness on day-to-day functioning. This article shows to what extent such inattentiveness is associated with chronic overreactivity of the limbic system, which then gives rise to hyper-reactivity to affective stimuli. It reminds us that inattentiveness is not merely a neutral, mildly inconvenient state of mind. Rather this state of constantly being "drawn away" from moment-to-moment experience by self-related concerns is closely related to stress and affective reactivity. Thus the "lack of concentration" we see in depressed patients is not simply a "symptom" of depression, an epiphenomenon that will right itself with effective pharmacological or psychological treatment, but may itself be a critical maintaining factor, the treatment of which may have important impacts on other aspects of depression.

Farb et al. (2010) showed how participants demonstrate a marked difference in neural activity during experimentally induced sadness, before and after mindfulness-based stress reduction (MBSR). Their results show how a mindfulness program shifts regulation strategies from those brain regions supporting cognitive−affective representations of the self to those that process viscero-somatic information (from conceptual to sensory processing in Figure 1). As we have seen earlier, humans need the conceptual (simulation) mode for many tasks, but the fact that this mode assumes that the representations (images and thoughts) are valid indicators of reality is highly problematic when the content of the thoughts is global, negative, self-related material.

This study's results also have other implications. Note that participants both before and after mindfulness training reported the same degree of sadness following a sad film. This was an important feature of their study. They needed the "raw emotion" to be the same pre- and posttraining so they could examine the neural differences that might underlie the differences in the way such sadness was processed in the brain. Had there been differences in sadness, any neural difference may have reflected this different intensity of affect. But we can contrast this with the prediction of self-reported *differences* in affective reaction in the Erisman and Roemer (2010) study. It raises the important question of when we would expect to see differences in affective reaction and when we would not. Mindfulness training does not claim to reduce initial affective reaction, but rather the longer-term consequences of such reaction on avoidance and elaboration or enmeshment. Mindfulness is not about "not feeling" or becoming detached from affect. Decentering is not the same as dissociation or even distancing. It is rather seeing something *as* it is, without further elaboration: for example, seeing thoughts *as* mental events, or seeing physical sensations *as* physical sensations, rather than seeing them as having meaning for the integrity of self.

This means that, following mindfulness training, we might even expect stronger reactions in the short term, but for these to extinguish when the stimulus is no longer present (see Goldin and Gross, 2010, later, for one example of this). That is, we expect emotions following mindfulness training to be restored to their proper signal function: switching on when necessary and switching off when no longer needed. This raises the question of what time course of reactions is predicted, and therefore what is the appropriate time over which such reactivity should be assessed in our research studies. We are a long way from knowing the answers to this question, or knowing which are the most appropriate measures to assess such a time course.

Perlman et al. (2010) raise a similar issue of measurement. They showed no difference between novices and long-term practitioners in their responses to a question about the *intensity* of pain during a "focused attention" condition, but the long-term practitioners rated the pain as less unpleasant during an "open presence" condition.

As the authors themselves agree, it is not straightforward to ask questions about subjective states, even physical sensations experimentally controlled. For example, we do not know whether experienced meditators are using the scales in the same way as novices. If we ask an experienced meditator how much a painful sensation bothers them, they may choose to say how much they are able to see clearly the extent to which it is unpleasant (i.e., the immediate "feel of things" that is an automatic "read-out" of the pleasantness of objects of awareness), or the extent to which they can see clearly their own aversion to it (aversion refers to the subsequent tendency to escape or avoid a stimulus, over which there is a greater degree of choice).

These data therefore raise again the issue of what measures are best suited to pick up the differences we would expect to see, and the issue of over what time-line we might expect to see them. This issue is shown to be important because there is an apparent discrepancy with the other paper on pain in this Special Issue.

Grant et al. (2010) found that long-term meditators had greater pain tolerance, and also thicker gray matter in brain regions relevant to pain processing. Further, the more experienced the practitioner, the thicker the gray matter, and pain tolerance was increased in those who had brains with thicker gray matter (anterior cingulate, secondary somatosensory cortex, and right insula).

What might explain the difference between the results of this study and those of Perlman et al. (2010)? Might it be a trivial difference such as using the left calf (Grant et al.) or the inside of the left wrist (Perlman et al.) to apply the pain stimuli? Or could it be that the Grant study assessed pain sensitivity by recording at what temperature participants rated the stimulus as 6–7 on a scale of pain of 0–10, whereas Perlman et al. *predetermined* the level at which a temperature on the wrist would get rated as 8 on a 0–10 scale and then used that temperature to examine ratings of subjective intensity when presented later? Only a study that manipulates both site of pain and method of assessment in the same experiment will disentangle these questions.

This disparity should not distract us however, from the more interesting aspect of Grant et al.'s findings: that greater experience in the practice of meditation was associated with thicker gray matter in areas associated with pain sensitivity. At first one might suppose that the thicker gray matter simply arises from the extensive experience that long-term meditators have in sitting and thereby learning to deal skillfully with intense sensations. Grant et al. offer a more intriguing possibility: that it is not the pain that is important, but *attending* to the affected region of the body in an evenhanded way. This is important because, as they point out, chronic pain is associated with *thinner* gray matter in some of these brain regions. This suggests that one problem in chronic pain

is not only the pain itself, but the "turning away" from, the averting of attention from the regions that give rise to painful sensations, either through deliberate distraction, or by thinking *about* the pain (conceptually) rather than experiencing the sensations directly (see Figure 1). If this can be substantiated it would have important implications for the neural substrates of *mental* pain, and the neural changes associated with learning the willingness to experience it.

Jha et al. (2010) examined military personnel as they prepared for active military service in a war zone before and after an MBSR program, dividing participants into those who practiced meditation a lot versus those who practiced only a little. Results showed that, at posttest, those who had practiced more had maintained both their working memory capacity and their positive mood, both of which had deteriorated in those who had not practiced as much.

The study is important in that it examines the effect of mindfulness on what might normally be thought of as a "cold" cognitive skill: working memory. However, it turns out that this mental capacity is not so cold. Impaired executive control in depression is common and disabling (for reviews, see Burt, Zembar, & Niederehe, 1995; Hartlage, Alloy, Vazquez, & Dykman, 1993) and working memory capacity is a critical determinant of specificity of recall in autobiographical memory (Dalgleish et al., 2007), which in turn predicts whether people exposed to trauma will develop posttraumatic stress disorder (Bryant, Sutherland, & Guthrie, 2007).

The fact that Jha et al. (2010) found that more practice of mindfulness in the period leading up to exposure to a trauma (the war zone itself) prevents the deterioration in working memory capacity that might usually ensue is potentially important. Of course (and as the authors themselves point out), it might be a third as-yet-unknown factor that predicts both lower enthusiasm/ persistence in practicing mindfulness *and* predicts deterioration in capacity with training, but we'll need studies that explicitly examine this issue experimentally to see precisely what is going on here.

Goldin and Gross (2010) found that MBSR (which they found to produce positive clinical benefits in social anxiety) was associated with important changes in functional (f)MRI, including a reduction in the pretreatment tendency for anxious sentences to produce activation in areas associated with self-reflection (ventro- and dorso- medial prefrontal cortex) and increase in activation of areas associated with attention (parietal and occipital regions). They also found a different pattern of amygdala activation following treatment, with faster recruitment of amygdala after exposure to the sentences, but also faster extinction of this response.

Of particular interest here is their suggestion that the new pattern of amygdala activation/deactivation might be due to a switch from an effortful attempt to control anxiety, to a more automatic shift. We saw earlier how neuroscience research across a number of domains is revealing that attention focused toward one object naturally incurs some de-selection or inhibition of alternative objects (Bishop, Duncan, Brett, & Lawrence, 2004; Duncan et al., 2008; Tipper, 2001, 2003). Although these naturally occurring processes involve "inhibition," this is not the type of inhibition that causes psychopathology. Indeed, as we have seen, *insufficient* inhibitory control is likely to predict goal neglect, resulting in attention being too easily "hijacked" by current concerns. Goldin and Gross's discussion sets us this challenge: what is the neural marker of the difference between the *adaptive* balance between excitatory and inhibitory processing that needs to be strengthened,

and the *maladaptive* suppression of unwanted material (worries, memories, imaginings, etc.) that simply results in greater elaboration and further avoidance?

Mapping these de-selection or inhibitory processes, and distinguishing between adaptive inhibitory balance and maladaptive suppression of unwanted material may turn out to be the most important challenge for fMRI research over the next decade. We can hypothesize that maladaptive suppression, whatever else it involves, will always involve activation of the medial anterior to posterior (midline) structures that are activated when comparison between a current and "wished for" state of the self is invoked. I suggest that this will turn out to be the case whatever the implications of the notion that such midline structures actually underlie more general inferential and memory processes beyond the "self" (Legrand & Ruby, 2009) since the self-related suppression we are attempting to map is part of a mode of mind that involves the indirect, representational information that is the "currency" of both inference and memory.

Hargus et al. (2010) examined whether mindfulness training actually make a difference to the way people relate to their own experience. Previous research had shown that lack of specificity in recalling past events is associated with both rumination and avoidance, and can lead to impaired problem solving (Williams et al., 2007), and that poor meta-awareness skills increases risk of recurrence of major depression (Teasdale et al., 2002). So, before and after treatment in a randomized controlled trial of MBCT, they assessed the specificity and meta-awareness with which depressed participants described a previous crisis. They showed that specificity decreased over time in the group that did not have treatment but was maintained in the MBCT group, and that the MBCT group showed significantly greater capacity to describe their crisis from a metaaware perspective following treatment compared to the control group.

These results have important clinical implications. Patients need to be able to learn from past crises and to see warning signs of a new crisis, but this involves being able to recall previous negative events in sufficient detail without becoming overwhelmed or overenmeshed in the often real tragedy they represent. These results are the first to show that mindfulness can bring about *changes* in decentering, using a procedure that does not depend on self-report. But we need to be cautious: no outcome trial has yet compared mindfulness treatment with a comparable active treatment. We need to remind ourselves that other active treatments may turn out to have identical outcomes, even possibly mediated by identical processes.

Together, these articles point to many unanswered questions for future work to address. First, we need trials, large and small, that compare mindfulness training with equally plausible active treatments, both to compare efficacy, as well as to examine what the moderators and mediators of effective treatment are.

Second, we need to acknowledge that effect sizes in mindfulness trials are not yet large enough for us to think this is the treatment of first choice for a large proportion of our patients. What should our response be? The past 30 years of research in cognitive therapy has shown how discovering the specific processes that maintain different types of psychopathology with greater and greater precision has resulted in more effective approaches. The field of mindfulness will need to investigate the extent to which the same will apply. Can we continue to train skills to deal with universal

vulnerabilities (as is necessarily done in classes that include people with a range of problems) and also expect the same training to address the specific vulnerabilities we find in participants who come for classes designed for people with the same diagnosis (e.g., depression or social anxiety). Just as generic relaxation training turned out to be insufficiently helpful with different anxiety disorders, so mindfulness approaches may turn out to require skilful adaptation to different contexts and conditions.

Third, we need research that identifies the neural signature of maladaptive suppression, distinguishing it from the moment-by-moment adaptive inhibition of irrelevant stimuli that allows normal attention to function relatively effortlessly. Once we know more about the neural signatures that distinguish adaptive from maladaptive processes, we will need to test the hypothesis that the neural changes we see following mindfulness training (e.g., the uncoupling of "narrative" self-related areas from viscero-somatic areas) actually predict important clinical outcomes in the long-term.

Finally, we need laboratory manipulations that produce reliable effects that analogize components of mindfulness training without being overconcerned about how closely they match the whole complex of procedures and practices that mindfulness training involves in the clinical situation. We should feel free to manipulate the putative underlying processes with some precision (as in the experiential vs. analytic manipulation of self-focus of Watkins & Teasdale, 2002, 2004) rather than try to get a close match to mindfulness practices used in the clinic.

Concluding Remarks

This commentary has assumed that emotion is a signaling system that has evolved to be sensitive to environmental contingencies. Many emotional problems arise from a failure to switch off emotional systems once they have been activated. This may be partly due to individual differences in basic, genetically determined extinction processes, but we have focused here on the extent to which the failure to switch-off is due to the habitual activation of mental models or simulations, representations (images and thoughts) of present, past, and future that are created independently of the external contingencies, and continue to affect the evolutionary older systems that, unless trained to do so, fail to discriminate the simulation from things as they actually are. The articles in this Special Section amplify this account. Short trainings in acceptance-based processing can have effects in the lab, in long-term practitioners' reactions to pain, and in naïve participants' reaction to films. Longer meditation training changes the structure of the brain: long-term practitioners are different in understandable ways from novices, yet even 8 weeks' mindfulness practice brings about changes in the way emotion is processed that shows that participants can be trained to uncouple the sensory, directly experienced self from the "narrative" self-representation that contains the memories and inferences from which our sense of "self" is constructed and then defended. Mindfulness training has effects on working memory capacity, and on the ability of participants to narrate past crises in a way that enables them to remain specific and yet not be overwhelmed.

Research into the psychological and biological processes associated with mindfulness training has reached the end of its first, "ground clearing" stage. We have seen in this Special Issue that there is a range of experimental strategies that can be used to investigate this most ancient and yet most modern of approaches. Each of these strategies carry risks, but the benefit of adding to our knowledge will in the end by judged by the quality of the treatment and the benefits to patients it enables.

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Correction to Leach and Spears (2009)

In the article "Dejection at In-Group Defeat and Schadenfreude Toward Second- and Third-Party Out-Groups" by Colin Wayne Leach and Russell Spears (*Emotion*, 2009, Vol. 9, No. 5, pp. 659-665), the authors affiliations were incorrectly listed.

Colin Wayne Leach was affiliated with his current institution, the Department of Psychology, University of Connecticut, and Russell Spears was affiliated with his current institution, the School of Psychology, Cardiff University, Wales, U.K. at the time of writing.

The research was conducted while Russell Spears was at the University of Amsterdam and Collin Wayne Leach was a visiting scholar at the University of Amsterdam and supported by the *Nederlandse Organisatie voor Wetenschappelijk Onderzoek*.