Mindfulness and clinical science

Bergljot Gjelsvik, Alice Tickell, Ruth Baer, Chris O’Neill and Catherine Crane call for more rigour and less hype

In 2015 Time magazine declared that we were seeing a ‘mindful revolution’. The range of claims for the effects of mindfulness – some sound, some premature or inflated, others just plain wrong – suggests that there is a significant schism between the popular psychology accounts of mindfulness, and the evidence base. Expectations about the benefits of mindfulness have arisen in areas, and with a confidence, that far outstrips the current state of rigorous scientific investigation. There is emerging evidence that mindfulness training is not the panacea it is sometimes portrayed to be. Sometimes, results from one area are used to justify the use of mindfulness training in other contexts for which the scientific evidence simply does not exist. And researchers such as Willoughby Britton (Lindahl et al., 2017) have recently been exploring the possibility that mindfulness meditation might even be harmful to some.

So has hype preceded evidence? As with anything becoming popular very quickly, a backlash is predictable. Some critiques reflect the pendulum swinging to the other extreme, with Miguel Farias and Catherine Wikholm asking in a 2016 article ‘Has the science of mindfulness lost its mind?’ Yet the current climate of scrutiny represents a welcome opportunity to critically appraise the evidence for mindfulness-based programmes and their realistic potential, with a 2017 review led by Nicholas van Dam simply encouraging us to ‘Mind the hype’.

It seems timely to ask, then, what is the status of the clinical science of mindfulness? Are we mindful of the scientific evidence that exists, and its limitations? What is required in order for the field to mature and refine its scientific credentials? Here, we will explore the evidence base for mindfulness-based programmes (MBPs) that exist within the healthcare realm. We will outline three principles by which the overall scope and cogency of clinical mindfulness science might be evaluated. We focus on mindfulness-based cognitive therapy (MBCT) for depression, where the evidence base is most developed, but our points apply to any mindfulness-based programme for which claims of therapeutic effects are made.

Commitment to scientific evidence

The principle that clinical practice should be evidence-based is widely accepted within medicine. However, those interested in ‘mindfulness’ are a broad and varied
group. For some, mindfulness practice is a spiritual endeavour associated with Buddhist teachings (the Dharma). For others mindfulness training is viewed as a secular healthcare intervention. Still others regard ‘mindfulness’ in relatively non-specific terms, as part of the general zeitgeist or an irritating fad. Indeed, when different people refer to ‘mindfulness’, what they are talking about may be located anywhere along a continuum from spiritual practices for which the existence of an evidence base is less relevant, to health care, where it is critical. In the middle of this continuum, scientific knowledge arising from and relevant to MBPs delivered in healthcare settings is taken as justification for a much broader array of activities and products for which the evidence is often lacking (see Stephany Tlalka's blog post ‘The trouble with mindfulness apps’).

Despite the widespread buzz, the clinical science of mindfulness is in its early stages. Bad or misguided mindfulness science is like any bad science – unlikely to reveal anything of significance, and a potential source of misunderstandings and unsubstantiated claims. Thus, it is not enough to distinguish popular accounts from research findings; we also need to critically appraise the quality of the research on which our claims are based. In order for MBPs to be adopted in healthcare settings, they need to show hard evidence of efficacy; not anecdotal evidence of transformed lives, or experts’ claims of benefits. Thus, the field critically needs not only to ensure high research output, but the consistent use of high-quality designs with the potential to maximise the impact of findings.

In a 2015 piece on ‘Prospects for a clinical science of mindfulness-based intervention’, Sona Dimidjian and Zindel Segal argued that the durability and public health impact of MBPs depends critically on our ability to make these programmes accountable to ongoing scientific inquiry, and suggested that the translational model of treatment development of the US National Institutes of Health (NIH) should be a starting point. We’ll highlight three key questions in this ‘translational circle’.

**Is MBCT well grounded in a theory?**
A key motivation for research into mindfulness-based interventions, just as for clinical science in general, is to develop ‘maximally potent and implementable interventions’ (Onken et al., 2014). In the case of MBCT this development has followed a research process in which the interventions are ‘translated’ from theoretical models of what processes may instigate and maintain a clinical problem or change process, into interventions that target these problems and processes directly.

The differential activation hypothesis suggests that what keeps individuals vulnerable to recurrent depression is the tendency to react to temporary low mood with a ‘depressive interlock’ characterised by maladaptive responses such as depressive rumination and toxic thoughts (‘I’m a failure’), which maintain and exacerbate symptoms. The selection of systematic training in mindfulness practices as a therapeutic tool to target this depressive interlock was not random – it was based on the hypothesis that mindfulness skills support people in identifying distressing thoughts, feelings and body sensations, responding to them in less maladaptive ways, and cultivating acceptance and self-compassion, skills that together break up associative depressive networks and offset the risk of relapse (see Segal’s work with Mark Williams and John Teasdale).

**Where is the intervention located?**
Development of efficacious therapies relies critically on a sound theory and initial piloting. However, this alone is not enough. Any new intervention would need to be ‘well winnowed’ by passing through a cumulative series of stages that test its feasibility and efficacy. In short, this entails testing the intervention in controlled research settings and then community settings to see whether it works as anticipated in the real world. If findings are positive, then attention can turn to finding out how to implement it more widely and refine it – as knowledge of its mechanisms increases – to boost its potency and reduce any potential associated negative impacts or harms.

Early randomised controlled trials of MBCT for relapse prevention in recurrent depression, often led by John Teasdale, suggested that MBCT led to lower rates...
of depressive relapse than ‘treatment as usual’ control conditions. However, MBCT is a complex intervention including generic factors – such as group setting, welcoming atmosphere, expectation effects – as well as the specific factors such as the mindfulness practice itself. Some of the more recent trials have compared MBCT to maintenance antidepressants and active psychological interventions. The most comprehensive systematic review of MBCT for relapse prevention in depression to date, published in 2016 by Willem Kuyken and colleagues, suggests that across the nine RCTs included, MBCT is effective in preventing relapse in recurrent depression when compared with the various control conditions employed across the studies. However, there is no robust evidence at present that MBCT is more (or less) effective than maintenance medication or any other active psychological treatment (Crane & Segal, 2016).

Scientific scrutiny can also help to discern for whom MBCT might be beneficial, and for whom it is not more useful than other approaches. Findings such as those of Mark Williams and colleagues’ Staying Well After Depression Trial, which showed that MBCT for people with recurrent, predominantly suicidal, depression was not superior to a closely matched active psychological control treatment or usual care overall, but was superior for people with a history of childhood trauma, demand that we further scrutinise and elaborate our understandings of risk mechanisms and the extent to which they are targeted by MBCT in particular clinical groups.

This also reveals an ethical dimension of an ongoing scientific scrutiny of MBPs. When research shows findings we were not expecting – for example a recent trial led by Suzanne Chambers, showing that mindfulness meditation for prostate cancer made no difference – this prevents vulnerable people from going through a programme that might not help them, and invites rethinking. The broader work on the identification of cognitive and neural mechanisms of action underpinning the beneficial change produced by mindfulness programmes in clinical and non-clinical populations is still in its early stages (see a 2016 review from Richard Davidson). This work may yet yield important insights that can be fed into the treatment development process.

In parallel to demonstrating efficacy in highly controlled randomised trials, we need to understand more about what happens in the real world once a treatment becomes a recommended part of routine clinical practice. Treatments may not always be delivered only to the patient group for whom the evidence base exists or in the format that has been tested, and a range of factors may facilitate or introduce barriers to effective implementation. For MBCT these implementation issues have been studied as part of the ASPIRE Project, led by Jo Rycroft-Malone and Willem Kuyken, as suggested in the final stage in the NIH stage model’s translational approach. By understanding how effective dissemination and implementation take place, we can increase the likelihood that interventions will remain efficacious as they move into routine clinical practice, and will become available to those people who might benefit.

What is the ‘hard science’?
Several meta-analytic studies have been published in recent years, focused on evaluating the efficacy of MBPs for various outcomes, including anxiety and depression (Hofmann et al., 2010), and psychological stress and wellbeing (Goyal et al., 2014). There are quite a lot of studies addressing other problems, and the studies are extremely variable in scope, scale and quality. Mapping the existing evidence for MBPs, Dimidjian and Segal (2015) found that the majority of clinical mindfulness research to date involves small-scale studies typically developing or applying MBPs to new populations and problems (e.g. ‘Would an MBP help problem X?’).

This pattern is common in the early development of new treatment approaches. Dimidjian and Segal estimate that, within the field of mindfulness research, only 30 per cent of all research has moved beyond Stage 1, and only 1 per cent of research has moved beyond research contexts. Thus, very little clinical mindfulness research reaches the final three stages of the NIH model, leaving most research in the field at a comparatively preliminary level. Yet when we consider specifically MBCT as a treatment approach for relapse prevention in recurrent depression, this has gone from being tested in research settings to being examined in a relatively large number of randomised controlled trials, including pragmatic trials testing its effectiveness in community settings. It may now be legitimate for researchers to consider its potential usefulness with different populations, and for different purposes. Of course, any new project must demonstrate not only its own theoretical rationale, but also how this builds upon the theoretical rationale it has borrowed from a prior ‘well-winnowed’ area of research.

Key sources


Full list available in online/app version.
Pushing the envelope

There are now a substantial number of smaller-scale studies investigating mindfulness-based interventions in school settings. Meta-analyses (e.g. Klingbeil et al., 2017; Maynard et al., 2017; Zenner et al., 2014; Zoogman et al., 2015) show that these varied interventions have small but significant effects on relevant outcomes such as mindfulness, emotional health and attentional control. However, there is an absence of large-scale robust trials of MBPs in this age group, particularly over longer follow-up periods. There is also very little work exploring mechanisms of action, differential responding based on factors such as age or vulnerability to mental health problems, or issues of implementation and dissemination. A large, Wellcome-funded research programme (My Resilience in Adolescence – MYRIAD), led by Willem Kuyken, is currently examining the effects of a school-based mindfulness curriculum, derived from MBCT, but delivered as a universal intervention to children aged 11 to 16, as well as conducting studies looking at mechanisms of change, including in comparison to an active psychological control intervention, and at facilitators and barriers to implementation in UK schools.

Other mindfulness approaches are, in contrast, at a much earlier stage of development, or have not been formally evaluated at all. One such example is the use of MBCT for health anxiety. In this case a theoretical account of the use of MBCT in this population has been developed and elucidated (Surawy et al., 2015), and several studies, including a small-scale uncontrolled trial (Lovas & Barsky, 2010) and a larger randomised controlled trial (McManus et al., 2012) have been conducted. However, none of these interventions have gone through the full translational development from theory to testing in research settings to being tested and disseminated in community settings. This is an example of interventions being on an ‘intervention cliff’ (Weisz et al., 2014).

Still not well understood

Despite the widespread claims for the effects of mindfulness training, we cannot extend the evidence base from recurrent depression to other areas uncritically. We must define clearly what we mean by a mindfulness-based programme – Crane and colleagues’ 2017 paper in which the ‘DNA’ of mindfulness-based programmes is laid out is helpful in this respect. We must accurately define the problem being targeted, and test these assumptions in well-designed and sufficiently powered studies. Only then can careful adaptation begin.

The underlying basic mechanisms of action of MBPs in general, or of MBCT, are still not well understood. Alan Kazdin’s remark from more than a decade ago, whilst originally aimed at psychotherapy research at large, is highly relevant for mindfulness research today: ‘… research advances are sorely needed in studying the mediators and mechanisms of therapeutic change. It is remarkable that after decades of psychotherapy research we cannot provide an evidence-based explanation for how or why even our most well-studied interventions produce change.’ Understanding how an intervention works is critical in deciding which components of an intervention are key, and which are not. Van der Velden and colleagues’ 2015 systematic review of mechanisms underlying MBCT, and Alsubaie and colleagues’ 2017 systematic review of mechanisms of action in MBCT and MBSR in people with physical and/or psychological conditions, are important first steps towards increasing that understanding.

So we find ourselves at a pivotal point in the development of mindfulness-based programmes in healthcare settings and beyond. The tendency to extrapolate from scientific evidence in one area, such as MBCT for recurrent depression, to justify the use of mindfulness for other problems and in other settings, is problematic. Enthusiastic popular psychology accounts of mindfulness must be distinguished from and tempered by scientific understanding of the developing evidence base. In addition, scientific inquiry into the effects of mindfulness-based programmes would benefit from following the translational model of treatment development, that has already supported the development, refinement and widespread implementation of evidence-based cognitive-behavioural approaches to a range of common mental health conditions in the UK. The clinical science of mindfulness is in its early stages – the number of trials is still fairly modest, and we do not yet have a full understanding of the mechanisms underlying treatment effects, even in the areas for which there is stronger evidence of clinical efficacy. A combination of healthy scepticism, rigorous, theoretically informed adaptation and taking promising interventions through the whole translational treatment development arc is likely to be key as the field progresses.

Bergjot Gjelsvik
Alice Tickell
Ruth Baer
Chris O’Neill
Catherine Crane
are at the Oxford Mindfulness Centre, Department of Psychiatry,
University of Oxford
bergjot.gjelsvik@psych.ox.ac.uk

“It is remarkable that after decades of psychotherapy research we cannot provide an evidence-based explanation for how or why even our most well-studied interventions produce change”