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Invited commentary

Invited commentary on . . . Confusing procedures with process in cognitive bias modification research†

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Summary
The notion that cognitive bias modification should be appraised exclusively on the basis of trials where its postulated mechanisms were successfully changed starkly contradicts the standards of evidence-based psychotherapy. In the laboratory or as a treatment, cognitive bias modification cannot continue to eschew the rigorous scrutiny applied to other interventions.

Declaration of interest
None.

Selective exclusion of studies or outcomes from meta-analyses based on *post-hoc* criteria or applied without transparency or a solid theoretical justification is a pernicious practice that distorts results,[1] thus usually considered a grave error. Yet Grafton and colleagues seem to do exactly that with our meta-analysis.[2] They misconstrue it as investigating whether cognitive bias modification (CBM) alters 'emotional vulnerability', a vague concept of uncertain clinical relevance. Instead, as evident throughout, we examined post-intervention anxiety and depression outcomes. Using our data for anxiety outcomes at post-test, they employ three arbitrary filters to selectively exclude studies and subsequently perform a strictly qualitative and unclear classification of the remaining ones.

The most conspicuous filter is the exclusion of ten studies for measuring 'resting mood state' instead of 'emotional vulnerability'. Eight of these used the State–Trait Anxiety Inventory (STAI)-State, one a specific phobia inventory,[3] another social anxiety measures.[4] Hence, we are at a loss as to what the authors mean, as all of these measure symptoms of 'anxiety'. Moreover, all our effect size calculations were, as described, at post-test. If for instance authors of a trial would use the STAI-State at both post-test and after a so-called stressor task, we only considered the former. So, in this sense everything was 'resting mood'. Furthermore, if Grafton *et al* deemed state anxiety measures as improper, they should have also excluded them from effect size calculations for the other included trials. Their analysis nonetheless retained five other studies that solely used the STAI-State.[5,6] Finally, Grafton *et al* do not substantiate their reanalysis with any actual data analysis, except for tallying findings as yes or no. We undertook this task for them. Presuming they intended to exclude state anxiety, we recalculated effect size for the 12 remaining studies measuring anxiety. The eight where bias change occurred resulted into a small Hedges *g* of 0.38, virtually identical to our original findings[2] (Duval-Tweedie publication bias adjusted $g = 0.28$). We conducted meta-regression analyses combining bias change with other significant moderators of outcome.[2] Bias change no longer predicted outcomes (Table 1).

More generally, the claim that CBM should be assessed for effectiveness *only* in the presence of change in its postulated mechanisms conflicts with the current standards for evaluating psychotherapies. For instance, the effectiveness of cognitive–behavioural therapy[7] is not restricted to trials where dysfunctional thoughts were successfully changed. Process variables are commonly conjectural, unclear, multiple, confounded with outcome measures, assessed in miscellaneous ways, and produce contradictory results. Even when a hypothesised process changes in a trial, it does not follow this is indeed a mechanism of change.[8] For CBM, the nature and direction of bias change needed to engender symptom change have been targets of speculation and debate,[9,10] although posited as self-evident facts by Grafton *et al*.

Table 1 Meta-regression analysis for bias change, alone and in combination with other significant predictors of outcome[2]

Bias change			
Alone	And participant compensation	And delivery	And impact factor
$b = 0.42$, $P = 0.032$	$b = 0.37$, $P = 0.18$	$b = 0.25$, $P = 0.50$	$b = 0.24$, $P = 0.38$

Ultimately, CBM researchers should decide at which table they want to sit. If CBM is cast as a laboratory development, encouraging but as yet inconsequential for clinical practice, exploring procedures to modify assumed processes is an adequate goal. Conversely, if – as repeatedly claimed[10] – CBM is a promising psychotherapy for use on patients and in clinical trials, it should comply with the same standards as *all* psychotherapies. These standards involve evaluating effectiveness on clinically relevant outcomes, using all available evidence, as we did,[2] and cannot hinge on whether or not purported processes have changed. Grafton *et al* summarily gloss over other serious problems we evidenced, such as lack of effects for clinical samples, pervasive publication bias and low study quality. Vague and debatable distinctions qualitatively applied *post-hoc* to a subset of the available data cannot substitute for modest if extant symptom change.

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extra

Can psychotherapy replace the void left by religion in a modern post-religious society? First-person account

Kathrin Hofert

Having experienced some difficulties in my life, I found myself at the receiving end of cognitive–behavioural therapy. I have never been a particularly religious person, but like every human being I have a deeper, ingrained instinct of hope and wanting to believe in something even if it does not have a name.

The experience of psychotherapy was overall very positive and I could not help myself wondering whether this is becoming 'a thing'. As a young professional I have started to share my deepest feelings and emotions, looking for support in our turbulent society; and I was not the only one. Looking around the waiting room, I saw people of different ages, religions and social backgrounds.

A couple of centuries ago, the generation of my parents and grandparents would have found this halt in religion; praying and confessing to share the burden and pressures that rested on their shoulders. You would pour your soul out to a representative of your religious choosing, which helped to soothe the pain and suffering.

Ellis and Beck pioneered cognitive therapy in the 1960s and the development of behaviour therapy can be traced back to the early 20th century. The merging of cognitive and behaviour aspects of psychotherapy constituted the last, 'third' wave of CBT during the 1980s and 90s and formed the foundation of CBT as we know it today.

As a young female doctor I am not very religious. I am too scared to call myself an atheist but at the same time I am not actively believing. I found that my therapy sessions have filled a void, leaving me wondering whether this would have been filled by religion in the past. 50 years ago others would have looked at you funny if you said you would go to the gym – 'What is a gym?', 'Why would you want to run on the spot?' Today, going to the gym is a thing. I can imagine 'seeing a therapist' becoming a thing too. Keeping your mental health fit.

Just like a priest guiding a lost member of his congregation, can a therapist signpost us in the right direction with our mental health? This question is not aimed to belittle anyone who is religious and finds a halt in their religion. I look around and see young professionals in a very similar position to myself. I wonder whether psychotherapy can create an environment outside of religion for people to cope better with their problems without the fear of being judged as sinful. I remain a little more startled now than before I engaged in this thought process. I am not sure whether I am right or wrong or whether there is a right or wrong answer.

The following words of William James, an American philosopher and psychologist who also trained as a physician, capture my thoughts perfectly: 'The greatest discovery of my generation is that human beings can alter their lives by altering their attitudes of mind.'

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