BJPsych | The British Journal of Psychiatry (2018) 212, 246–248.

Correspondence

Edited by Kiriakos Xenitidis and Colin Campbell

Contents

Processing confusing procedures in the recent re-analysis of a cognitive bias modification meta-analysis

Those worried about the cognitive bias modification (CBM) field being affected by ever-moving goal posts may have thought their concerns confirmed by Grafton and colleagues' re-analysis of the meta-analysis by Cristea and colleagues.[1,2] The paper concludes with the suggestion that we should only call CBM CBM if it is successful. To provide a treatment-inspired analogue: 'This? No, this is just water, it's only homoeopathy if it works'.

It seems that we witness an almost prototypical disagreement between experimentalists and treatment evaluationists about which question to ask and which data to include. Importantly, the two author groups appear quite agreed that the answer to the question 'whether assigning an anxious individual to engage in a CBM procedure will result in direct symptom reduction' would be 'not likely'. Perhaps Grafton and colleagues had better direct their critical attention towards the work by 'field insiders' in which CBM is quite consistently touted as a treatment, not to mention the apparent push for clinical dissemination and premature commercial exploitation. Thus, the question meta-analysed by Cristea and colleagues, authors specialising in meta-analytical evaluation of (proposed) treatments, appears perfectly legitimate.

Grafton and colleagues' exposé on the correct question to meta-analyse, reads uncomfortably like a perceived-damage-containing mission. The discomfort is aggravated by the presented re-analysis, applying dichotomising and partly mystifying criteria to distil a subset of eligible studies from those selected for the original meta-analysis. Specifically, the requirements for a study to pass 'Criterion 3: effect size computed by Cristea *et al* reflects legitimate emotional vulnerability assessment' (Table 1, p. 268) remain unknown, as do the rules governing the final dividing criterion 'intended CBM procedure successfully induced the process of bias modification' (p. 268).

One could attempt to reconstruct the criterion rules from the tables provided, but it matters little. The analysis by Grafton and colleagues is flawed in a manner that must have escaped the attention of authors, reviewers and editors alike, even after Cristea and colleagues pointed it out in their commentary.[3] To be very explicit: Grafton *et al* meta-analysed the study effect-size estimates calculated by Cristea *et al*.

In their original paper,[1] Cristea and colleagues state clearly that (a) for studies reporting multiple symptom outcomes, these were averaged into a single effect-size estimate (p. 8), and (b) effect-size estimates reflect symptoms assessed post-training, excluding assessments following a stressor procedure (p. 9).

Based on the narrative, it appears that criterion 3 has to do with each study either (a) assessing symptoms on trait (rather than state) measures, yet effect-size estimates averaging across state and trait measures were analysed, or (b) employing a post-training stressor procedure, yet symptoms assessed preceding such stressor procedures were analysed. Surely, we are not to assume reliable retro-active impact of unannounced stressors, nor that excluding studies with state measures only, results in adjustment of state measures retained for other studies. Therefore, we must conclude that this small yet crucial detail has gone unnoticed.

A meta-analysis by Grafton and colleagues, assessing evidence for their hypotheses, could perfectly exist alongside the meta-analysis by Cristea and colleagues. The currently presented re-analysis, however, does not convince.

1 Cristea IA, Kok RN, Cuijpers P. Efficacy of cognitive bias modification interventions in anxiety and depression: meta-analysis. *Br J Psychiatry* 2015; **206**: 7–16.

2 Grafton B, MacLeod C, Rudaizky D, Holmes EA, Salemink E, Fox E, et al. Confusing procedures with process when appraising the impact of cognitive bias modification on emotional vulnerability. *Br J Psychiatry* 2017; **211**: 266–71.

3 Cristea IA, Kok RN, Cuijpers P. Invited commentary on … Confusing procedures with process in cognitive bias modification research. *Br J Psychiatry* 2017; **211**: 272–3.

Anne-Will Kruijt, Postdoctoral fellow, Department of Psychology, Stockholm University, Stockholm, Sweden; **Per Carlbring**, Professor, Department of Psychology, Stockholm University, Stockholm, Sweden. Email: anne-wil.kruijt@psychology.su.se

doi:10.1192/bjp.2018.41

Authors' reply: Kruijt & Carlbring misrepresent the position conveyed in our commentary,[1] wrongly attributing to us the suggestion 'that we should only call CBM CBM if it is successful'. Our actual points are: (a) it cannot be claimed that cognitive bias has been modified when assessment data reveal that no modification of cognitive bias has taken place; and (b) the emotional impact of modifying cognitive bias cannot be determined from studies that fail to modify cognitive bias. Also, incorrectly, they describe our commentary as an 'exposé on the correct question to meta-analyse'. We highlight the need to distinguish two quite different questions, without claiming that either is 'correct', and emphasise the resulting problems when meta-analyses fail to do so.

Our position adheres to the tenets of experimental medicine.[2] The first step in experimental medicine is to identify a target *mechanism* that plausibly contributes to the *dysfunction* of interest. For example, high blood pressure represents a *mechanism* that may contribute to the *dysfunction* of elevated stroke risk, and attentional bias to threat represents a *mechanism* that may contribute to the *dysfunction* of anxious disposition. Step two involves developing a candidate *intervention* intended to manipulate this *mechanism*. This could involve a drug intended to reduce blood pressure, or a computer procedure intended to reduce attentional bias to threat. Step three involves delivering the *intervention* to determine: (a) whether the *intervention* impacts the *mechanism*, as intended; and if so (b) whether this impact on *mechanism* therapeutically attenuates *dysfunction*. Should the drug fail to reduce blood pressure, with no observed reduction in stroke risk, it cannot be concluded that reducing blood pressure has no impact on stroke risk. Likewise, should the computer procedure fail to modify attentional bias to threat, with no observed reduction in anxious disposition, it cannot be concluded that modifying attentional bias to threat has no impact on anxious disposition. If the drug sometimes reduces blood pressure, and whenever this occurs stroke risk also decreases, this suggests that blood pressure reduction attenuates stroke risk. Likewise, if the computer procedure sometimes reduces attentional bias to threat, and whenever this occurs anxious disposition also decreases, this suggests that attentional bias modification alters anxious disposition.

https://doi.org/10.1192/bjp.2018.41 Published online by Cambridge University Press