

PCI Registered Reports – Review

Optimising performance using a synergistic mindsets intervention

This registered report excellently describes a very interesting and impressive study, and meets most of the Stage 1 criteria to a high degree (i.e., logic, rationale, and plausibility of proposed hypotheses; soundness and feasibility of the methodology and analysis pipeline; and consideration of sufficient outcome-neutral conditions). That said, as I outline in my more specific feedback below, I believe some improvements are possible in relation to a couple of the criteria (i.e., scientific validity of the research question; clarity and degree of methodological detail). For instance, the registered report would benefit from a clearer and stronger theoretical underpinning (e.g., biopsychosocial model of challenge and threat), greater alignment between underlying theory and methods (e.g., choice of self-report measures of challenge and threat), and a more vivid description of, and stronger justification for, some methodological elements (e.g., cardiovascular data recording, self-report measures). Overall, I enjoyed reading this registered report and hope the authors find my feedback useful.

Major feedback

- The introduction, and study outlined in the registered report more broadly, would benefit from a clearer and stronger theoretical underpinning. Indeed, at present, the authors briefly describe aspects of numerous theories (e.g., work of Blascovich, Lazarus, etc.), and the introduction would benefit from a clearer overview of the framework(s) most strongly informing the study (i.e., biopsychosocial model of challenge and threat).
- Methodologically, the measure of situational appraisals currently described does not align well with the conceptualisation of challenge and threat appraisals included in the introduction (i.e., balance of evaluated situational demands vs. personal coping resources). Greater coherence is therefore needed and so I would recommend the authors use an alternative measure (e.g., items adapted from the cognitive appraisal ratio combined into a demand resource evaluation score). Indeed, there have been calls in the challenge and threat literature for more homogenous methods to be used across studies (see Hase et al., 2019 for a review). Furthermore, linked to this point, I would encourage the authors to use timescales with their cardiovascular reactivity data that better align with previous research (e.g., final minute of resting baseline and last minute of pre-match period).
- Similarly, if the authors wish to assess an individual's propensity to appraise all potentially stressful situations as more of a challenge or threat, I recommend that they use the Appraisal of Challenge or Threat Scale developed by Tomaka et al. (2018), as it better aligns with how they are currently conceptualising challenge and threat in the introduction.

Minor feedback

- The title is punchy, but could better reflect the study outlined in the registered report. For example, a key outcome, challenge and threat (or stress appraisals), is missing.
- The abstract offers a neat summary of the study described in the registered report, including background, methods, and hypotheses. Areas for improvement include:
 - Greater conceptual clarity via more accurate terminology. For instance, challenge and threat are appraisals of motivated performance situations that can be inferred via cardiovascular responses and are theorised to have downstream effects on affective responses (e.g., emotions), performance outcomes, etc. Thus, referring to

- ‘challenge versus threat affective responses’ is not conceptually accurate. This is a recurring issue that could be resolved throughout the registered report.
- More methodological information could be provided. For example, it might be clearer precisely when measurements of challenge and threat will be taken.
 - The introduction excellently covers relevant and recent literature (e.g., Yeager et al., 2022) to ‘set-up’ the study described in the registered report. Areas for improvement include:
 - Greater flow between paragraphs and subsections (e.g., opening paragraph into the subsection entitled ‘how appraisals influence performance’).
 - More theoretical content. For instance, key components of pertinent theory are missing such as clear definitions of concepts such as ‘motivated performance situations’, ‘task engagement’, ‘cardiac output’, and ‘total peripheral resistance’ which are central to the biopsychosocial model of challenge and threat.
 - Extra criticality. For example, while challenge and threat appraisals are thought to lead to different emotional responses (see the predictions of the theory of challenge and threat states in athletes by Jones et al.), the empirical evidence supporting this assertion, at least in the sport psychology literature, has been relatively mixed.
 - Figure 1 presents challenge and threat appraisal in a different way conceptually to the text (i.e., harm and control vs. situational demands and personal coping resources), and so greater conceptual clarity and alignment is needed. Indeed, as noted above, a clearer overview of underpinning theory is needed (e.g., Blascovich’s BPSM).
 - Given the inclusion of stress mindsets in the study described in the registered report, I would suggest briefly summarising this literature somewhere in the introduction.
 - The method provides an excellent and relatively detailed summary of how the authors plan to collect and analyse their data. Areas for improvement include:
 - It is unclear why physiological markers of challenge and threat are included in the primary analyses, but self-report measures are included in exploratory (or secondary) analyses, particularly given the former are thought to objectively reflect the latter. I would encourage the authors to reflect on this and consider if self-report measures of situational appraisals should be included in the primary analyses. If so, the effect sizes reported in previous research linking challenge and threat, measured via self-report, with performance could be useful in informing the sampling plan (see Hase et al., 2019 for a review).
 - More information is required in terms of how randomisation will be achieved. In addition, the authors should consider other criteria that are commonly used to assess the methodological quality of randomised controlled studies and what they might do to ensure their proposed study satisfies these criteria. For example, will researchers assessing outcomes (i.e., challenge and threat) be blind to group allocation? How will missing data be kept to a minimum? Etc.
 - The authors might want to consider assessing, and/or controlling for, interoceptive ability or awareness as this might impact the effectiveness of some elements of the synergistic mindset intervention (e.g., components based on arousal reappraisal).
 - More information is needed on how the authors will ensure and assess task engagement in the laboratory-based performance tasks. Additionally, how will it be ensured the tasks represent personally relevant motivated performance situations? Linked to this, I would strongly encourage the authors to assess HR as well as PEP as a marker of task engagement. Indeed, they could follow the work of Seery et al. and combine HR and PEP into a single index to simplify analyses, etc.
 - It could be made clearer to the reader precisely where the intervention content and/or materials will be stored to enable replication (e.g., OSF)?

- It seems like two-minute baseline and recovery periods are to be used around e-sports matches. Is this sufficient to enable cardiovascular markers to return to baseline? You commonly see 5-minute periods of recording used in prior research.
- How precisely will the synergistic mindsets group report adherence and progress with the intervention? More details are needed to enable replication.
- From Figure 2, it seems that the emotion recall task used in stage 3 (i.e., post-intervention) is not being used in stage 1 (i.e., pre-intervention). Why? I would have thought pre- to post-intervention changes in appraisals, emotions, etc. would be vital in evaluating the effectiveness of the synergistic mindset intervention.
- Scoring information (e.g., sum totals or mean values), as well as more details relating to the validity and reliability of each self-report measure, is needed. Also, for some measures (e.g., situational affect regulation), different scales will be used pre- and post-intervention. This seems a little unusual and it is currently unclear why. Indeed, stronger justifications are needed relating to the self-report measures. For example, why have the authors decided to use the stress mindset measure over other relevant questionnaires (e.g., instrument developed by Keech et al.)?
- After identifying outliers, how will they be dealt with (e.g., excluded, winsorized, etc.)? More information is needed to enable replication.