Author’s response to reviews

Title: A New Conception and Subsequent Taxonomy of Clinical Psychological Problems

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Dear Dr Harris,

I write in response to your request for a revised manuscript subsequent to a second round of review of ‘A New Conception and Subsequent Taxonomy of Clinical Psychological Problems’ (PSYO-D-18-00040R1) by the two reviewers Dr Irina Trofimova and Dr Nicholas Eaton.

I note that neither reviewer has recommended specific changes that would improve the paper. Reviewer 1 (Dr Trofimova) because she does not favour its publication, and Reviewer 2 (Dr Eaton) because he has “no further comments to strengthen this interesting submission”.

Due to the insoluble differences between the responses of Reviewer 1 and of Reviewer 2, I now request an arbitration by a third reviewer, whether this be yourself as Editor, another member of the Editorial Board, or a further outside party.

The thrust of this new conception of clinical psychological problems (CPPs) and its subsequent taxonomic implications has been presented at the 2016 World Congress of Behavioural & Cognitive Therapies, the 2017 Australian Association For Cognitive & Behaviour Therapy, and the 2018 Australian Psychological Society Congress, as well as in therapy manuals and articles which have been used in postgraduate training in clinical psychology. The response from all parties has been very positive, and much closer to that of Reviewer 2 than of Reviewer 1.

Therefore, as you suggest, I will limit myself to addressing Reviewer 1’s comments in detail:

-- “I don't wish to enter into a lengthy polemic with the author as my first review was quite extensive. I will simply point out that for the most part my comments were not addressed.” My previous cover letter with the revised submission addressed both Reviewers’ comments point-by-point, to an extent of 8 pages in the case of Reviewer 1, and 2 pages in the case of Reviewer 2. Some Reviewer comments meant that “no response or alterations [were] required”. Some elicited changes and additions to the manuscript, which were highlighted by Track Changes. And some responses were explanations and clarifications as to why listed changes would not be an improvement. I understand that Reviewer 1 was disappointed in those in the latter category, but all comments were addressed.
“The author simply mentioned that there are many psychiatrists who are unhappy with current taxonomies (no doubt about this), and used it as an argument defending his suggestion for separating psychological and psychiatric taxonomies.” Psychiatrists’ problems with DSM were mentioned in passing -- e.g. in paragraph 2 of the section “Psychiatry is more biological-level…..” -- but the bulk of the paper is about the mismatch between the clinical needs of clinical psychologists, who assess, formulate, and intervene at a psychological level, and a DSM-type psychiatric mental disorders “weak medical model” of clinical psychological problems (CPPs).

“However, he does not make the case for such a separation…” The case for a separate psychological taxonomy rests on the utility of a new separate conception of CPPs, which is outlined on pages 2 – 14 and 20 – 22.

“and does not provide an efficient classification system even for psychological diversity or for "psychological problems". Few changes were made to the taxonomy proposed by this author, and this taxonomy simply repeats categories already used by the current psychiatric classifications (DSM/ICD).” The “subsequent taxonomy” of CPPs of the title of the paper is a secondary proposal, entailed by the new conception of CPPs. The tentatively proposed new taxonomy outlined in Appendix B is admitted in the manuscript to be “nascent”, “embryonic”, the work of one clinician, and in need of an “effort by teams of clinicians and researchers corresponding to some meaningful fraction of the effort devoted to the development of the DSM” (p.32). The classifications (e.g. ‘depression’) in this taxonomy are not critical (see pages 30 – 32). They are meant primarily to be clinically useful. They do not imply radical theoretical or conceptual significance. It is the content of the taxonomy, not its groupings – what is listed, not how – that is of psychological theoretical import. The content comprises CPPs expressed in the form of psychological-level problem-maintaining circles (PMCs), instead of mental disorders of low clinical utility.

“Moreover, the proposed coding, unfortunately, reflects neither the full complexity of psychopathology nor offers a more efficient way of coding psychopathology. The examples given in Table 7 could be easily derived from existing classifications (DSM, ICD) simply by using a Comorbid specifier (for example, Comorbid Anxiety and Depression; Comorbid Depression and Substance Abuse). Shuffling the codes doesn't solve the main flaws of current psychiatric taxonomies such as overlapping diagnoses, insufficient neurophysiological justification for the structure of these taxonomies, and (in the example of the HiTOP model) obsessions with dimensionality approach.” As described above, Reviewer 1 has been distracted by the groupings of the PMCs in the appended nascent taxonomy proposal, and away from what is being listed. Table 7, for example, “could [not] be easily derived from existing classifications” because it does not list separate or even paired ‘mental disorders’. From page 63: “The interactive problems listed in Table 7 do not represent separately diagnosed mental disorders in the DSM sense. They represent clusters or fields within the symptom matrix of the network model (Cramer et al., 2010).” It is fully agreed that “shuffling the codes doesn’t solve the main flaws of current psychiatric taxonomies”. What is proposed is to code psychological-level, dysfunctional or undesired cyclic processes instead of, or as well as, putative latent mental illnesses.
“It seems that the only difference, in the author's opinion, between psychological problems and clinical psychological problems is the chronicity of symptoms (Table 1). If so, what is wrong with just using a specifier for the chronicity of the diagnosis (chronic, acute) rather than not complicating the classification with new sets of categories?” PMC theory does not assert that chronicity is key. In fact the point is made that normal grief reactions, which can be quite prolonged and hence ‘chronic’, are not CPPs, because psychological-level cyclic maintenance processes are not implicated. This is made clear in the section “Fast and slow PMCs”: “The PMC model of CPPs replaces the seven-or-more criteria for the existence of a mental disorder found in DSM, with one criterion: Has a psychological-level PMC formed? The nearest DSM criterion to this is duration of the problem. However, this is an inadequate criterion because the complexity of life means that some PMCs can form and warrant intervention in days, and some do not cement for months, even years” (p.25).

-- “In the Table 2 the author considers normal psychological reactions under "general psychological problems" (are they problems or are they not?)” Yes, they are problems in that they can be aversive, negative, or undesired, but they are still ‘normal’ and not ‘clinical’. Feeling stressed by an exam or sad at a loss are negative states, but still normal, probably functionally useful, and do not warrant clinical intervention. So they are psychological-level problems, but not clinical psychological problems (CPPs).

-- “and grief reaction as something transient (this is hard to agree with)” That the vast majority of grief reactions are transient is an empirical claim supported by overwhelming research evidence. E.g. Bonanno et al., 2002; Greer, 2010; Parkes, 1993; Schut & Stroebe, 2005; Windholz et al., 1985; Worden, 2008.

-- “Moreover, the author implies that the diagnoses listed in the column with Clinical Psychological Problems are rather stable states. In reality, patients suffering from these psychiatric disorders have "good days and bad days", so the criterion of their persistence is rather unfruitful.” The word “stable” is not used in Table 2. The word “persistent” is. Persistence of a PMC creating a CPP does not exclude “good days and bad days” any more than the assumption of a persistent underlying psychiatric disorder does. Also, the list referred to in Table 2 is of CPPs, not of “psychiatric disorders”. This is clear from the inclusion of relationship problems and of sleep problems.

-- “Moreover, one of the author's arguments for having a special PMC taxonomy is that psychiatric diagnoses somehow refer to different behavioural aspects rather than psychological ones, and relate more to neuropsychiological imbalances than psychological cycles. Yet, his list of psychological problems, that should be in his view a subject of psychological taxonomies, includes problems that mostly arise from psychophysiological problems - pain, acute stress, binging, tantrums, diet. In brief, the author did not make a good case for a separation between psychological and psychiatric taxonomies, or for the use of the PMC concept.” The key phrase in this argument is “problems that arise from psychophysiological problems – pain, acute stress, binging, tantrums, diet”. As CPPs, none of these areas of problem arise from psychophysiology, except in the trivial and irrelevant sense that all psychological processes have an inevitable physiological substrate. This universal neurochemical substrate is acknowledged in the notes attached to the general model of Figure 1. The critical issue is: At what level – physiological,
psychological, sociological, anthropological,….. – is the PMC or the dysfunction occurring? With schizophrenia and epilepsy it is easy to argue for a neurochemical or physiological dysfunction, responding best to a physiological intervention. On the other hand, the difference between a single tantrum and a CPP involving frequent tantrums, or between one binge on chocolate and a bulimic problem, is unlikely to be a neurochemical psychiatric illness, but very likely to be a psychological-level maintaining PMC, breakable by psychological-level intervention.

-- “In my previous review I advised the author to limit the paper just to underlining the need for using a "cycles" concept in developing new taxonomies. His diagrams (eg. Figure 2) are not very sophisticated or novel, but just remind the reader of the most well-known cycles in psychopathology. It seems that the author didn't follow my advice, and kept the main body of the paper untouched, only expanding it by a discussion of the dimensional approach.” Reviewer 1 does not favour the proposed new conception, and rather than advise on how to improve the arguments and evidence base of the paper, she has effectively recommended that the proposal be abandoned, and that I write something about cycles in psychiatric conditions. This has been done in the past. Such a paper would add little if anything to the corpus of clinical practice or knowledge.

-- “The paper became simply unreadable as it has too many comments about too many issues.” Whereas Reviewer 1 regarded the paper as “unreadable”, Reviewer 2 described it as “beautifully written and rhetorically effective. It is dense, but it is dense with good information and a thoughtful analysis”.

-- “The author also over-estimates the effect of CBT, considering only evidence in favour of using this type of therapy.” On page 33, PMC theory is described as a metatheory because any therapeutic orientation and model of CPPs can be the source of the elements in a therapy-guiding PMC formulation, provided they are mainly psychological-level and evidence-based.

Because the response to this proposed new conception of CPPs, and the style of taxonomy which can ensue, has been so positive from all parties, bar Dr Trofimova, I have undertaken this next step of submitting a major article for peer review.

I am therefore also optimistic regarding the seeking of an arbitrating further opinion, and because neither reviewer has suggested specific changes to improve the paper’s arguments, I have therefore re-submitted an unchanged manuscript.

Yours sincerely,

Gary Bakker.