Author’s response to reviews

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

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Author’s response to reviews:

Cover letter
Dr Benjamin Ragen 26th October, 2018
Editor, BMC Psychology.

Dear Dr Ragen,

Thank you for the opportunity to improve upon, and resubmit, my manuscript entitled “A New Conception and Subsequent Taxonomy of Clinical Psychological Problems” upon receiving feedback offered by two reviewers.

The feedback was valuable, and has led to several important enhancements to the manuscript. These changes are highlighted in the accompanying revised manuscript, and are detailed in this letter, together with the reviewer comments and recommendations that have triggered them.

A difficulty to be overcome has been that the responses and suggestions of the two reviewers have been very different, with no overlap. Therefore, subsequent amendments are outlined separately.

Reviewer #1:

“Good aspects of this manuscript:

- A sober view on the limitations of current classifications of mental disorders, including the limitations of categorical and dimensionality approaches.

- A concept of functional circles as a unit of analysis, addressing dynamical features of clinical behavioral patterns.

- An attempt to include aspects of an individual’s interaction with their environment (unfortunately, mostly social) into the model.
- Good knowledge of the literature discussing the matter of classifications of mental disorders. However, it is a pity that the recent Theme Issue of the Philosophical Transactions of the Royal Society - Biology, N 373(1744) specifically devoted to taxonomies of psychological individual differences (including mental disorders) passed unnoticed by this paper.

- An appreciation of the complexity of the search for biomarkers of mental disorders, and limitations of findings.

- An understanding of the limitations of the neuroanatomy-focused approach to biomarkers of mental disorders employed by RDoC group.

- A suggestion of therapy considerations as a part of taxonomy.”

These comments did not require alterations to the manuscript.

“Aspects of this manuscript that raised concerns:

1. What about a need for common language? The idea of a separation between taxonomies used in psychiatry and clinical psychology is questionable.”

Thank you for raising this concern for clarification. Comments 1 (a)-(d) express doubts that there is a need for clinical psychology’s own parallel problem taxonomy. Therefore, relevant sections have been bolstered with further supportive references, such as:

Further references, mainly from psychiatrists, outlining the limitations of the DSM in both psychological and psychiatric research and practice appear on page 3. Including: “The search for biological etiology has greatly disappointed (Kapur, Phillips, & Insel, 2012; Kendler, 2012), suggesting that psychiatric diagnosis has oversimplified psychopathology (Kendler, Zachar, & Craver, 2011). DSM and the ICD, meantime, have been poor guides to even psychopharmacological treatment selection (Bostic & Rho, 2006; Mohamed & Rosenheck, 2008), let alone to psychological therapy selection.”

The usefulness of a ‘common language’ and an acceptance that the common language of diagnosed mental disorders will never be supplanted is made clear in the paper, e.g. on page 26: “…greater specificity and description of symptoms at a phenomenological level has at least given us a common language that can be applied with reasonably good interrater reliability (Anand & Malhi, 2011; Brown, Di Nardo, et al., 2001; Hyman, 2010), and on pages 28-29: “It is the clinical utility of the candidate levels of analysis and of intervention that should determine whether a problem is best regarded as a ‘psychiatric problem’ or a CPP.” “Therefore, PMC theory seeks to distinguish problems best regarded as psychological-level, or psychiatric, or neurological, or sociological, and so on, and promotes concept definition and an appropriate problem taxonomy at each relevant level.”
Furthermore, Table 2 incorporates those (Type III) psychological problems that are, and forever should be, regarded as ‘psychiatric disorders’. The language of clinical psychological problems (CPPs) will only ever be additional, but a very useful addition, for Type II psychological problems. This is now made clearer by the following on page 29: “Another consequence is that the common language of ‘mental health’ services is anticipated to remain that of ‘mental disorders’. This conception has dominated for a long time, and is arguably the most appropriate for a substantial portion of people’s problems. PMC theory and its conception of CPPs is likely to remain an additional, concordant, and complementary body of concepts and therapy-relevant taxonomy.”

“a) Both sciences of psychology and psychiatry deal with the same phenomena - clinical and subclinical deviations in human behavior, and behavior regulation follows universal mechanisms, regardless of whichever science studies them. These sciences should have a compatible language and taxonomies since their subjects, behavioral regulation in normal and clinical populations, are based on the same neurophysiological systems and laws of nature which hold for all people. We can only talk about a continuum in the degree of disbalance of biomarkers between healthy (weak disbalance) and mentally ill people (strong disbalance) but the language describing this continuum should be as universal as possible between these two sciences.”

This is a valid point, and although the two sciences “deal with the same phenomena”, they do so at different levels. (From page 4: “The psychological and the biological are different levels of analysis, assessment, and intervention (Gold, 2009), and any alignment of phenomena at these two levels is, by definition, correlational, not causal (Miller, 2010). It is no more likely that all CPPs will be reduced in the future to neurobiological conditions than that the geological study of earthquakes will be reduced to molecular theory (Gold, 2009, p. 508).”)

Although “universal mechanisms” are involved, some are biological-level mechanisms (chemical imbalances and suchlike), and some are psychological-level mechanisms (classical conditioning and suchlike). That there is a neural basis to all behaviour change (see page 28), including to classical conditioning, in no way diminishes the usefulness of analysing and categorising problems at a psychological, behavioural, or conditioning level.

Further, the languages and taxonomies of CPPs and of mental disorders will remain entirely compatible. The following has been added on page 34: “Communicability will be enhanced not only among clinical psychologists. The medical model and PMC theory are compatible and complementary. Already, clinical psychologists accept referrals of diagnosed mental disorders, understand what is meant, do their own functional analyses and case formulations, and often feed these back to the psychiatric or physician referrer, who understands them perfectly well, and may even appreciate a PMC code summary of this formulation.”

“b) In practice, both of them [psychiatry and psychology] use psychotherapies based on DSM/ICD-based diagnoses. To ensure efficient collaboration between practitioners in these sciences/practices (i.e. psychiatry and clinical psychology) it makes sense for them to speak the same language, facilitating an exchange of their findings in terms of
assessments and therapy approaches. In the practice of psychological treatment, communication between psychiatrists prescribing medications or providing consultation notes and psychotherapists providing psychological therapy is rather common. Both specialties use each other’s opinions on diagnosis and occasionally - course of treatment. This is one of the arguments in favour of uniting and not separating psychiatric and psychological taxonomies.”

As described in the article, it is true that most outcome research has used diagnosed groups, e.g. ‘An RCT of CBT for Social Anxiety Disorder’. But this methodology of mixing diagnosed groups with formulation-based treatments has had very limited success. In practice most clinical psychologists will base their intervention on their functional analysis/case formulation, not merely on the diagnosis the client presents with.

Hence, advancement in clinical psychology has been retarded by the imposition of a less developed and conceptually mismatched problem taxonomy. Therefore, on page 5, the following addition has been made: “The sluggish pace of discovery in psychiatry has been attributed, in part, to the limited validity and the arbitrariness of traditional diagnoses (Cuthbert & Insel, 2013).”

And on page 6: “When arbitrary categories are forced onto dimensional phenomena like symptoms, then reliability suffers (Chmielewski et al., 2015; Kotov, et al., 2017). Not only have no biological markers for the common mental disorders been uncovered, but this arbitrarily interchangeable collection of criteria for determining their presence means that not a single mental disorder has been established as a discrete categorical entity, as opposed to a dimensional outlier (Haslam, Holland, & Kuppens, 2012; Wright et al., 2013).”

“c) In psychiatry the concept of cycles in the regulation of behavior, as well as the social and functional environment, is as important as in psychology, so there is no reason to separate psychiatric and psychological taxonomies. Capacity and cognitive abilities are commonly assessed in psychiatry without going too deeply into the biology of possible mental illness. Vice versa, the psychology of non-clinical populations often investigates the neurophysiology of psychological phenomena.”

It is agreed that ‘vicious cycles’ can be definitive of problems within all applied, remedial disciplines, psychiatry included, and also in many models/orientations within clinical psychology, forensic psychology, organizational psychology, and medicine. However, the elements in those PMCs differ between disciplines, so the PMC Model within clinical psychology must be a ‘metatheory’ to allow the incorporation of all evidence-based PMCs from any theoretical orientation. Therefore a new section – ‘The PMC Model is a Metatheory’ – has been added at pages 32-33:

“CBT’s substantial evidence base, and its inherent emphasis on here-and-now case formulations and therapeutic disruption of problem maintainers, make it a natural fit for the PMC model of CPPs. However PMC theory is not specific to a CBT view of the world. The Complex Network Model of CPPs, which PMC theory has grown from, “does not involve the acceptance of any particular theory about psychopathology” (Borsboom & Cramer, 2013, p.96). Theorists and
practitioners from a range of orientations have identified and targeted ‘vicious circles’ or ‘vicious cycles’ as a core feature of their conception of CPPs. Only the nature and level of causal elements within their formulated PMCs vary.

For example, the movement toward integration of models of psychotherapy (Gold & Stricker, 1993; Norcross & Goldfried, 1992) has frequently pointed to cyclic processes between internal (psychodynamic) states and external (cognitive-behavioral) events (Wachtel, 1994). Reciprocal, cyclic, self-perpetuating processes have a “pervasive role” in psychoanalytic, cognitive-behavioral, systemic, and experiential models of psychopathology (Wachtel, 2014).

Within the psychodynamic model the type of problem or disorder can be gleaned by content, but the presence of a disorder can be determined by “cyclic psychodynamics” (Machtiger, 1985; Smith, 2008; Wachtel, 1993). Narcissistic personality disorder, for example, can be understood purely intrapsychically and/or as an environmentally-maintained disposition in which “what makes them continue to feel bad is how they go about trying to feel better” (Wachtel, 1994, p.52). Short-term existential interventions have been explained as ways to break vicious circles of emotion (Lantz & Walsh, 2007).

Family systems theorists have long emphasised cyclical formulations with psychodynamic elements (Watzlawick, Weakland, & Fisch, 1974). Indeed they have promoted this focus on ‘circular causal loops’ in problem formulations – as opposed to linear psychodynamic cause and effect explanations – as a ‘new epistemology’ (Hoffman, 1981).

Nor is defining the essence of a clinical problem in terms of vicious circles or PMCs particular to clinical psychology. It has been proposed or enacted within forensic psychology (Alison & Stein, 2001; Weston 1967), organizational psychology (Masuch, 1985), and crossculturally (Kirmayer & Sartorius, 2007). Much of medicine describes vicious circles of organic pathology (fever, organ failure, etc.) treated by intervening in biochemical PMCs. For example, many neurodegenerative diseases are now understood in terms of “cascades” of “proteinopathies” within or affecting neurons (Jellinger, 2009).

Therefore, theorists and researchers from many theoretical orientations are able to contribute, and to utilize, the new taxonomy, only providing that the focus is on problem maintenance, and the relevant PMCs and implied treatments are evidence-based.”

In psychiatry the causal elements can include psychological-level factors, such as behaviours, and biological-level ones, such as neurochemical conditions. Psychologists must seek psychiatric expertise when the latter are critical in a formulation. But when a PMC is comprised entirely of psychological-level elements (as in all of the PMCs in the currently proposed taxonomy), then a ‘mental disorder’ model is less useful than a PMC model, especially in therapy selection. There are no biological markers to seek, and a purely biological intervention (e.g. psychopharmacology) is unlikely to be adequate. These adjacent levels of analysis and intervention overlap, but are not identical.

“d) The author argues in favour of having a taxonomy in clinical psychology which would be different from psychiatric classifications but he gives examples of decidedly
psychiatric illnesses such as depression, anxiety, OCD, addiction, schizophrenia. Moreover, his taxonomy also includes traditional psychiatric disorders. He does not provide a single diagnosis or example related to a "pure psychological" level (if such a level exists) and therefore he did not provide a justification for a separation between CPP and DSM classifications. This contradicts his introductory statement and the main theme of the abstract, and suggests that his taxonomy would like to unify both, psychiatric and psychological individual differences.”

This comment describes the core of the misalignment between the biologically-based individual differences perspective of Reviewer 1, and the psychological-level formulation-based conception and problem taxonomy being proposed. For example, the terms “depression” and “anxiety” do not, in the current context, refer to “decidedly psychiatric illnesses”. They are labels or descriptors of emotional states. ‘Major Depressive Disorder’ is, on the other hand, a putative psychiatric illness.

The use of general grouping terms for problem areas in the taxonomy of PMCs, such as ‘Depression’ or ‘Relationship Problems’, are not at all diagnostic labels. This is discussed on page 31:

“‘Some nomothetic conceptual overlay is necessary as an initial guide to the more in-depth process of individual functional analysis because it defines the domain of interest’ (Bissett & Hayes, 1999, p. 380). Such categories are not formal diagnoses. Denman (1995) has pointed out that case formulations will inevitably have ‘diagnostic elements’ in them, but not in a psychiatric/DSM sense. For example, “the problem is ‘marital in nature’” is a form of diagnosis and a useful label when formulating a person’s problems to determine treatment (Carey & Pilgrim, 2010), so ‘marital’ or ‘relationship’ categories of PMCs would be clinically useful, but not as de facto DSM diagnoses.”

The PMC taxonomy includes traditional psychiatric disorders only in the 12.y.z section (Table 6 in Appendix) where PMCs that frequently occur in association with ‘psychiatric’ Type III problems are listed. Again, a PMC conception and taxonomy will never supplant a ‘mental disorders’ one. It is intended to sit alongside.

If a “purely psychological” level of analysis or intervention does not exist, this could bring into question the very need for a distinct discipline or profession of clinical psychology at all – something that this paper could not contemplate.

The continued use of mental disorder-based terms is inevitable because: (a) The Type III psychological problems of Table 2 occur in the context of mental disorders that will continue to be usefully recognized as such (e.g. schizophrenia); (b) The language of PMC theory is yet to be developed and accepted. Hence the use of “disorders” terminology in Table 1; And (c) due to the respective disciplines’ substantial overlap, it is likely that they will need to recognize and use each other’s concepts and terminology forever (e.g. ‘Panic Disorder’ is a useful shorthand for a handful of similar and frequently co-occurring PMCs). Kotov et al. (2017) similarly admit to using terms from DSM-5 to help communication when HiTOPS dimensions parallel its disorders, though their new system does not include any of the traditional diagnoses.
“e) He provides a rather cartoon version of what psychiatrists do or clinical psychologists do when assessing clients. In reality only the CBT approach is a rather cookie-maker approach but outside of CBT practice both psychiatrists and psychologists always collect personal, medical and employment history to derive a plan for treatment. An individual-based approach in practice and assessment is very common, and often all types of therapies are mixed in the treatment of the same person or the same type of disorders.”

It is conceded that a psychiatric assessment can be flexible, complex, and wide-ranging. But a functional analysis and ensuing psychological-level therapy is of its nature very individualized, and is potentially more so than a DSM diagnosis and prescription. Therefore, the following passage on page 9 has been extended along these lines: “With regard to treatment-relevance and clinical utility, even for psychiatrists the DSM “describes a collection of disorders, not an integrated system of psychopathology” (Clark, Watson, & Reynolds, 1995, p.147). Many existing diagnoses encompass multiple pathological processes (Zimmerman, et al., 2015). DSM’s search for reliability of diagnosis at the cost of theoretical integration and validity (Kendell & Jablensky, 2003) plus its high rates of comorbidity, high frequency of “Other Specified/Unspecified” (previously “Not Otherwise Specified”) diagnoses, and divergent and overlapping criteria sets make for little guidance in choice of treatments (Kingdon & Young, 2007).”

And, on page 33, the way that PMC theory better handles the complexity of people and of their lives is referenced:

“A PMC taxonomy overcomes almost all of the problems clinical psychologists have with DSM. Its only comparative drawback is its relative complexity. But we have seen that, for clinical psychologists, DSM’s simplicity has been gained at an unacceptable cost to its validity and clinical utility. Life is complex. People are complex. And DSM has oversimplified them.”

“2. What about a need for multi-disciplinary approach? Taxonomies in all other sciences were developed as a result of multi-disciplinary analysis, and not just analysis of the phenomena specific to a given discipline. Taxonomies in biology, chemistry, cosmology, medicine, anatomy, particle physics, etc. are based on principles of natural phenomena noticed by boundary sciences (genetic analysis and evolution for taxonomies of biological species; Pauli principles in physics for chemical taxonomies; anatomic and even physiological investigations of healthy people for medical taxonomies, etc). The author states that psychiatry didn't justify its own taxonomy by sets of verified biomarkers, and that RDoC focused on neuroanatomy - implying that the whole approach is therefore wrong. However, psychology and psychiatry are very young sciences, in comparison to these classic disciplines, and it would be a rather ignorant approach for psychology to withdraw from cooperation with psychiatry when it comes to the development of taxonomies of mental disorders, only because they are just at the beginning of their journey. Moreover, the author seems not very knowledgeable in regards to achievements in neuroscience in regards to specific biomarkers. For example, he says nothing about neuro-chemical biomarker research, even though, considering the practice of psychopharmacology, neurochemical biomarkers (neurotransmitters, hormones, neuropeptides, opiod receptors) are likely candidates for consistent individual differences both, in healthy and mentally ill people.”
“…implying that the whole approach is therefore wrong”: This implication is not intended, therefore changes have been made to further avoid this implication, including:

Page 4: Addition: “So even were the RDoC project to improve diagnostic reliability, validity, and clinical utility for psychiatrists, it would still offer no greater attraction to clinical psychology.”

Page 5: “Schizophrenia and a simple reactive dog phobia are also likely to represent different classes of CPP in this light. The former more comfortably rests within a taxonomy of ‘mental disorders’ such as the DSM. A reactive dog phobia, on the other hand, may be more conceptually concordant with clinical psychology’s own parallel purely psychological-level taxonomy of CPPs.”

…”for psychology to withdraw from cooperation with psychiatry…”: At no point is such a withdrawal advocated. The opposite is discussed under ‘psychiatric problems’ on pages 28-29. For example:

““The current classification systems are less controversial for conditions with an identified biological aetiology such as in the fields of neuropsychology, dementias, and moderate to severe learning disability” (DCP, 2013, p.2).”

“It is the clinical utility of the candidate levels of analysis and of intervention that should determine whether a problem is best regarded as a ‘psychiatric problem’ or a CPP. Therefore, are there problems that are better considered – either in therapeutic terms or in explanatory-mechanism terms – as essentially organically-based rather than entirely PMC-driven? Haslam (2010) has argued that there are. He cites as examples autism and schizotypal personality.”

“Medication is considered first-line treatment for schizophrenia (Bradford, Stroup, & Lieberman, 2002), and concurrent psychosocial interventions are “almost always offered adjunctively to pharmacotherapy” (Roth & Fonagy, 2005, P. 292). This treatment-relevance confers some clinical utility upon the DSM diagnosis of schizophrenic disorders (Farmer & Nelson-Gray, 1999).”

“…he says nothing about neuro-chemical biomarker research…”: Few detailed comments are made on this issue because:

(a) The thesis is within clinical psychology, not psychiatry;

(b) The psychiatric literature consulted describes results of research on biomarkers for all mental disorders as disappointing and not clinically useful or reliable yet. Passages outlining this include:

“This ‘nosology of diseases’ (Hyman, 2010) has been problematic for psychiatry itself (Bracken et al., 2012; Frances, 2013; Hyman, 2007; Kingdon & Young, 2007; Kupfer, 2013), let alone for clinical psychology. It has been plagued by such major problems as excessive rates of comorbidity (Cramer et al., 2010; Kessler et al., 2005) which may be an indicator of arbitrary
boundaries between its disorders (Ormel et al., 2015; Teesson, Slade, & mills, 2009), by the broad heterogeneity within its diagnosed groups (Clark, Watson, & Reynolds, 1995; Hyman, 2010; Kim & Ahn, 2002; Zimmerman et al., 2015), and by the fact that none of the putative underlying disease processes have been uncovered in the 35 years of research since DSM-III was published (Andersson & Ghaderi, 2006; Hayes et al., 1996; Kingdon & Young, 2007; Kupfer, First, & Regier, 2002; Sussman, 2009). The search for biological etiology has greatly disappointed (Kapur, Phillips, & Insel, 2012; Kendler, 2012), suggesting that psychiatric diagnosis has oversimplified psychopathology (Kendler, Zachar, & Craver, 2011). DSM and the ICD, meantime, have been poor guides to even psychopharmacological treatment selection (Bostic & Rho, 2006; Mohamed & Rosenheck, 2008), let alone to psychological therapy selection.” … which has been extended and elaborated subsequent to the importance of this issue being pointed out.

And (c) if biomarkers that are reliable and clinically useful are discovered, this is more likely to be of relevance to biological-level interventions than to psychological-level ones.

“3. For taxonomies we need to know the mechanisms and principles of our phenomena. All other sciences settled upon their taxonomies in this way, and their studies into mechanisms continue, leading to upgrades of their models. The same is applicable to psychological-psychiatric taxonomies. For behavioural mechanisms, however, we must "compare the notes" of multiple sciences, and partitioning into "levels", such as biological vs. social, is not helping us. After all, there are multiple examples of how neurochemical systems (for example, hormonal) respond to social and peer interactions. The author suggests that using neuroscience is a reductionist approach but not using it would be an ignorant approach. A proposed in this paper taxonomy has no grounds in mechanisms of behavioral regulation, whether at the level of neuroscience or at the level of cognitive psychology or research in emotionality, and therefore remains speculative. It seems that the author tries very hard to justify a lack of analysis of the neurophysiology of mental disorders in the development of his model. The arguments often sound like a voice of a CBT practitioner saying "We don't know much of biology, and we don't need to know". Yet, we can't develop classifications without knowledge of mechanisms of behavior, and we can't know mechanisms without consulting with neuroscience as one of the sources of our understanding of human behavior.

However, to base the taxonomy solely on one science, whether biological or social, is a general reductionist approach. Behavior is a highly contextual and its generative processes depend on the "here and now" capacities of an individual plus on situational demands, so for our taxonomies we likely have to derive formalisms describing both the capacities of an individual and a classification of environmental contexts (both, social and biological). For multi-disciplinary analysis, therefore, we have to be informed about the reality of processes that we try to classify, considering different perspectives, not only biomarkers of specific behavioral deviations but also environmental factors interacting with these biomarkers. The author's own example of differentiation between schizophrenia and dog phobia show the importance of having a taxonomy that differentiates between different degree of bio-chemical factors in hiven disorders, and for that we need to know these factors at the first place.”
“…this paper taxonomy has no grounds in mechanisms of behavioural regulation,…and therefore remains speculative.”: The psychological-level mechanisms proposed to underlie the PMC taxonomy are the ones most supported by evidence from the last 70 years of research in applied clinical psychology. That is, those based on learning theory, conditioning theory, cognitive mediation processes, etc. The generic model of Figure 1, for example, subsumes such diverse psychological phenomena as operant conditioning, classical conditioning, cognitive mediation, expectancy effects, cognitive dissonance effects, psychoeducation, multimodal therapies, and transdiagnostic therapies. The current neurochemical models, on the other hand, are still entirely speculative, even for schizophrenia and bipolar disorder, let alone for social anxiety problems and relationship problems.

“…we can’t know mechanisms without consulting with neuroscience…”: Psychological-level mechanisms and neurochemical-level mechanisms are distinct and equally valid levels of analysis. Neither should entirely subsume the other. See, for example, page 4: “Mental disorders may be studied at different levels of analysis (e.g. molecular genetics, neurochemistry, cognitive neuroscience, personality, environment), and no level is inherently superior or fundamental to any other” (Deacon, 2013, p.856).

“4. All concepts are social constructs: It can be agreed with the author that mental disorders are psychological constructs but so are all scientific concepts, so it is not a reason to abandon them if there are not yet any better concepts. All sciences start with some "working" concepts and then look for the mechanisms of these phenomena, upgrading their scientific vocabulary. Moreover, despite the criticism of categorical and dimensionality approaches for bringing social constructs, the author also proposes mainly a categorical model based on a rather weak theoretical approach coming from specific type of practices (CBT) and not based on neuroscience.”

“…it is not a reason to abandon them…”: I can reassure that at no point is the abandonment of the social construct of mental disorders advocated. In fact they continue to appear in the new taxonomy as Type III psychological problems.

“…start with some ‘working’ concepts…”: The thesis does not assert the falsehood of the concept of mental disorders, only their limited usefulness as psychological constructs – especially for clinical psychologists – in the face of more useful “better concepts”.

“…based on a rather weak theoretical approach…”: The theories and therapies associated with CBT are the most research-derived, evidence-based, and therapeutically successful of all the psychological-level approaches to CPPs.

However, PMC theory does not rely upon this. As outlined on pages 19 and 32-33, any model of CPPs that recognizes the centrality of maintenance processes (even biological-level ones) can contribute PMCs to the taxonomy, provided they are evidence-based. In this sense, PMC theory is a metatheory.
5. The author clearly treats the CBT justification as a main theory of behavioral regulation, and this approach is not very competent or promising. The author derives specifiers for his model (Table 4) from CBT practices as if CBT was the answer to all treatment or diagnostic considerations. In reality CBT (before it borrowed under its roof almost all possible methods from other types of therapy) is not very efficient in the treatment of most mental disorders and was only moderately efficient for the treatment of OCD and some anxiety disorders. More importantly, it is not a science-based approach but rather a manual from practitioners simplifying psychological science for not psychologically-minded clients. Most behaviour is not driven by cognitions but it is the other way around: a body's state affects cognition and emotions. Embodiment in cognitive phenomena commonly emerges whenever behaviour follows the physical state of the body and habits, and cognition justifies current actions. In Jeffrey Gray's (2001) words, "consciousness comes too late". Those little triangles in the submitted Figures are naive graphs from CBT practitioners, which reflect only some tendencies of regulation between just 3 rather general aspects of behaviour. These graphs, however, miss important subtleties in the regulation of the depicted classes and completely miss other functional aspects of behavioral regulation. The author is too caught up in the CBT paradigm that he sincerely believes is a valid model of behaviour that could substitute knowledge of neuroscience when it comes to work on taxonomies.

The role of cognition in behaviour change has been hotly debated. Empirically, many Cognitive Therapy techniques have been found to be effective. But it is still arguable as to whether an epiphenomenon is being measured. That is, is behaviour changed in some other way, and a later cognitive rationalization is confabulated? However, the current thesis does not rest on these issues. It carefully and consistently distinguishes Cognitive Therapy (CT) from Cognitive-Behaviour Therapy (CBT). The alteration of cognitive triggers and mediations in CT is but a small part of CBT’s proven-effective armamentarium. Whether it is a CT intervention that is applied by a clinician, or a behavioural intervention is chosen, or an emotion-focused strategy, or an environmental manipulation, etc., will depend upon the functional analysis and case formulation that has been developed. A taxonomy of PMCs can assist in this process.

Examples of PMCs, of case formulations, and of specific therapies that are given in the paper are heavily CBT-representative only because this reflects the author’s clinical practice. As described, any orientation can contribute PMCs to the taxonomy, including neuroscientific ones.

6. A suggestion to consider therapy planning when deriving a diagnosis is a good move, especially when discussing PMC, but unfortunately there is no ready therapy "sets" yet that can be considered for such tasks. There is no single therapy that appeared to be efficient, CBT included. "Treatment-reliance" criterion for classifications of mental disorders is still a matter of wishful thinking.

It is true that links between diagnoses and therapy selection recommendations in the literature are currently vague and varied. But links between functional analyses/case formulations and specific interventions are rife, and are largely intuitive, e.g. a panic cycle maintained by hyperventilation will benefit from breath control techniques.
The paper concurs with the desirability of avoiding simplistic ‘cook book’ diagnosis-to-treatment models, and this is emphasized on pages 13-14:

“Research trials have typically treated highly selected groups with a single diagnosis, while in clinical practice patients have many comorbidities and atypical symptom profiles (Persons, 2008; Thompson-Hollands et al., 2014). Clinicians are more likely to apply several interventions, and will base this on the individual case formulation they have developed, on the assumption that each technique is targeting something different.”

“For example, we know that targeting specific mediating cognitive processes in social phobia is more effective than standardized generic cognitive-behavioural treatment (Rapee et al., 2009), because the mental disorder ‘Social Anxiety Disorder’ can encompass a number of (mechanism-defined) CPPs.”

This paper does not regard CBT as a “single therapy”. It is a learning theory-derived assemblage of evidence-based techniques.

With regard to the final point, it is agreed that a treatment-based taxonomy of CPPs would be unwieldy and unhelpful. From page 31:

“If classification within the taxonomy were to be according solely to specific treatment implications, this would result in categories such as “Problems treatable by exposure therapy” or “CPPs for which thought stopping is frequently a useful adjunct”. Such categories would be large, unwieldy, highly overlapping, and counterintuitive. Also, this would be a reversion to the medical model ‘pill for an ill’ or ‘Condition X therefore Treatment Y’ approach, rather than increasing the rate, reliability, and communicability of case formulation and subsequent tailored treatment programs.”

“7. The examples on p. 6 meant to show the overlap between diagnosis but they overlap on the criteria of loss of functionality and distress. These are expected overlaps as these are indeed are the main criteria differentiating behaviour in healthy humans from clinical cases. In this sense there is no surprise that all mental disorders have these universal criteria related to the loss of functionality. However, I don't defend the structure of the DSM and I agree that categories that are currently used for its structure should be revised based on functional aspects of behaviour.”

No response or alterations required.

Reviewer #2:

“I found this submission very interesting. It was beautifully written and rhetorically effective. It is dense, but it is dense with good information and a thoughtful analysis. There is much to like about this paper. I have a few minor suggestions for the author to consider:”
No response or alterations required.

“1) Submission p. 6: The author's treatment of dimensional mental disorder constructs struck me as extreme and poorly justified; compared to other points made in the submission, this position seems weak. There has been a great deal of thinking and writing on dimensional mental disorder (and personality), which many consider different (but not worse) than categorical approaches. I would suggest a clearer formulation of what the authors wants to say on this point. It currently reads like a strawman argument.”

This is fair comment. Dimensional responses to the categorical problems of the medical model have been numerous, evidence-based, clinically useful, and have been developed further during the assembly of this manuscript. Therefore, extensive and substantial updates and elaborations on the benefits, contributions, and limitations of such transdiagnostic models, research, and treatment development (too extensive to be repeated here) have been added on pages 7-8.

And on page 11 has been added:

“It has been argued that dimensional data can lead to actionable ‘diagnoses’ in medicine (Kotov et al., 2017), so why not in clinical psychology? For example diagnoses are determined, and treatments initiated, from blood pressure measurements and fasting glucose levels using indicative ranges of scores. However, even in medicine, this is regarded as second best. It is much preferable to uncover some clear, qualitatively distinct pathology such as an infection or a lesion, than to find that a score looks too high or too low. Is it better to treat every adult person under a height of 4’6” with growth hormone, or to reserve this treatment for people who are not producing their own growth hormone?”

“2) There was what I view as a largely missed opportunity here to focus on developments in transdiagnostic research and classification. While transdiagnostic processes emerge, I would recommend the author look into work by the Hierarchical Taxonomy of Psychopathology (HiTOP) consortium, which is creating an evidence-based classification system of mental disorder classification. While this may be "topographical" in the author's terms, it is nevertheless worth considering for inclusion, and more germane to the current presentation than RDoC. See Kotov et al. (2017) Journal of Abnormal Psychology. See other work as well by Thomas Achenbach, Robert Krueger (one paper is cited), Nicholas Eaton, Kristian Markon, Roman Kotov, David Watson, Lee Anna Clark, and many others.”

A very worthwhile inclusion is recommended here, only neglected because the HiTOP consortium formed during the composition of this manuscript. Therefore, this development has now been included on page 10:

“The taxonomic arm of this empirically-based transdiagnostic movement – the Hierarchical Taxonomy Of Psychopathology (HiTOP) consortium – has been a very recent development (Kotov et al., 2017). Its rich vein of studies (Forbush & Watson, 2013; Kotov et al., 2011; Slade & Warson, 2006) establishing an alternative dimensional organization of psychopathology helps
to overcome such problems with traditional nosologies as the issue of arbitrary thresholds and subsequent loss of information, ensuing reliability problems, diagnostic heterogeneity, theoretically disruptive high comorbidities, and exclusion of undiagnosable people with serious CPPs (Eaton, Rodriguez-Seijas, Carragher, & Krueger, 2015; Kotov et al., 2017). But it suffers from all the problems of a dimensional taxonomy. It can only offer a dimensional elaboration, based on symptom measurements, on top of a categorical ‘disorder’ model, because it still does not implicate proximal causes for, and the 'essence' of, CPPs”

(Comparison with the RDoC movement [pp. 3-4] has been retained despite the above, as Reviewer 1’s comments have been more neuroscientically focused.)

“3) I found the treatment of stigma reduction throughout to be well handled and appreciated.”

No response or alterations required.

“4) The treatment of network models is both timely and behind-the-times. That is, I was very pleased to see it. However, many of the citations are from early network approaches, and the field has grown (and grows) very quickly. I would suggest the author read and cite more recent work, which has shed light on both the benefits and limitations of the network approach (e.g., within- and between-person networks, replicability issues).”

Once again, this is valuable feedback, and very justified. The network approach is the model of CPPs that has come furthest toward a PMC conception and taxonomy. It is only half a step removed. And it is heavily researched and fast developing. So, even during the development of this manuscript, major advances have occurred – especially in the theoretical proposals of Borsboom (2017) and Borsboom et al. (2018). Note that these advances have been in the direction of recognition of “self-sustaining feedback loops” as a critical concept in understanding psychopathology.

Therefore, this omission has been remedied by major updates and additions on page 21, too extensive to duplicate here.

I hope and trust that these changes are satisfactory, and that the manuscript is now suitable for publication in BMC Psychology.

Yours sincerely,

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