Reviewer's report

Title: Building a Theory of Audit and Feedback: A 'Menu of Constructs' Approach

Version: 1 Date: 16 February 2012

Reviewer: MARIE M JOHNSTON

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'Building a Theory of Audit and Feedback: A 'Menu of Constructs' Approach'
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Implementation Science
Debate
Review of Paper

The title of this paper led me to hope for a useful advance of theory relevant to audit and feedback (A&F). There has been such a strong body of evidence using theory in this field, but with a continuing need for providing a stronger and better basis for choosing and developing theoretical approaches. This essay reiterates some of the common critiques and proposes an alternative approach, ‘a menu of constructs’ (MOC). In reviewing this kind of paper, i.e. one directed at ‘debate’, my comments deal both with reviewing the ideas presented and with the manner in which they are presented. I have no specific suggestions about revisions and offer my remarks for the authors to use as they wish. I have numbered comments in square brackets that they might wish to respond to or incorporate.

I generally liked the way that the ideas were presented and offer a few detailed bullet points below. [1] A more major point is that the paper does not refer to other papers that discuss the value of theory such as Eccles (2005 a/b). [2] Some arguments were presented at a somewhat general level and seemed to be fighting a ‘straw man’ e.g. ‘a great deal of work..’, ‘not generally’, ‘the many more ...’ and it is difficult to ascertain which theory or type of theory is being criticised. It would have been more appropriate to challenge some of the better attempts to use theory in KT. [3] In addition, the criteria for judging a theory or theoretical approach to be good or bad were never made clearly explicit and so it is possible to criticise published theoretical accounts by criteria which are then not applied to MOC.

Turning to the ideas presented rather than the manner of presentation, the authors rightly identify some aspects of the value of theory such as ‘substantive justification.... instead (of) past practice and logistical constraints’, ‘transfer from one area of human endeavour to others’, ‘causal mechanisms’, ‘foundation of empirical work’, ‘methodological innovation’ ‘applicability to many contexts’. [4] Other arguments in favour of theory have been presented by many authors including Eccles (2005 a/b).

Equally valid are some of the criticisms such as ‘enormous number’ but ‘focused
....on a very small number of theories’, ‘can miss important information’ ‘complicate ... application’, problems addressed by Michie et al (2005) in simplifying theory into domains which retain the breadth and range.

Concern is also expressed about the potential of theories to predict behaviour rather than intention. [5]They rightly recognise that the same theories that predict intention do in fact predict KT behaviour (Eccles et al., 2006), just to a lesser extent. [6]This argument would suggest that any additional constructs require empirical support as a predictor of behaviour, but this argument is not applied to the proposals they bring forward. The predominance of theory which describes but does not suggest methods of improving KT is also criticised. [7] But surely the authors could then have considered the theories which not only predict behaviour but clearly specify how to change behaviour such as Social Cognitive Theory or Learning Theory. Learning Theory not only predicts KT, we have used it to design an effective intervention (Clarkson et al, 2008). [8]Equally, in criticising the use of correlational designs, it would have been useful if the paper had considered the papers where the authors use experimental designs (e.g. Bonetti et al 2005, Eccles et al 2009) or even RCTs such as Clarkson et al.

The authors consider the alternative approach of ‘context-specific ‘micro theories’” but indicate a preference for their notion of a ‘menu of constructs’ (MOC). This approach involves the selection of constructs from the ‘entire menu, not just those associated with a particular theory’, selecting ‘those constructs that seem relevant to the new context’. I have two major concerns about this proposal. [9]First, the method of selecting constructs for inclusion seems unreliable and likely to be determined by some combination of prejudice and ignorance of the range of possible constructs. The authors refer to the theoretical domains approach (Michie et al 2005 ref 13) which was an attempt to ensure that the full range of theoretical factors was at least considered, but do not appear to require this of the MOC. Nor do they make use of the paper by the major theorists to identify the necessary constructs (Fishbein et al 2001).

[10]Second, in proposing MOC, the authors are suggesting what Bandura has criticised as ‘cafeteria style research’, but deal with none of his criticisms:

‘Proliferation of conceptual models of health behaviour tends to spawn cafeteria style research. Constructs are picked from various theories and strung together in the name of theoretical integration. This practice multiplies predictors needlessly in several ways. Similar factors, but given different names, are included in new conglomerates as though they were entirely different determinants. Facets of a higher-order construct are split into seemingly different determinants, as when different forms of anticipated outcomes of behavioral change are included as different constructs under the names of attitudes, normative influences, and outcome expectations. Following the
timeless dictum that, the more the better, some researchers overload their studies with a host of factors that contribute only trivially to health habits because of redundancy. There is a marked difference between expanding the scope of an integrative theory and creating conglomerates from different theories with problems of redundancy and fractionation of predictors and theoretical disconnectedness.’ (Bandura 1998).

We have also combined constructs from a variety of theories, but based on empirical evidence rather than simply trying to identify those that are relevant (e.g. Eccles et al 2007; Bonetti et al 2006). But that approach should also be criticised as it loses some of the value of theory. Similarly the theoretical domains (TD) approach of (Michie et al 2005) can revert to something of a cafeteria style if the domains are not used to direct attention to the relevant theory. Nevertheless it is not clear how MOC advances on the TD methods.

[11]A major part of the latter criticism of MOC style selection of constructs is that it loses the relationship between constructs in predicting behaviour. For example, in the TPB, one would not expect ‘intention’ to predict behaviour if there was not at least a modicum of ‘perceived behavioural control’, nor vice versa – we do not develop intentions simply because we believe we can do it. A major part of theory is the clear distinction between the critical elements and the propositions about the relationships between them; separation of the elements or constructs from the specification of their relationship is clearly problematic.

The authors argue for theories that ‘explain mechanisms contributing to a behaviour, rather than attempting to account for behaviours in general and acontextually’. [12]I could not understand this as the theories being criticised such as the TPB specifically set out to identify contributing factors and to contextualise them by specified methods that require item development based on beliefs that are contextually salient.

They also argue for theory that explains differences between instances rather than behaviour in general. [13]However they pay little consideration to the fact that most of the theories considered should explain both general and specific instances of KT; for example, Social Cognitive Theory predicts who is most likely to act appropriately and also, for a single person, when they are likely to act appropriately. This is not a critique of theory but about how theories have been applied. The development of improved methods of analysing single or multiple instances e.g. using multi-level modelling or time-series analyses, is beginning to overcome that problem, as this is not fundamentally a problem with the theories per se.

In introducing examples of what the authors propose to add to existing theory, they begin with dual-process approaches. This is an important and developing aspect of KT work. [14]While we and others have investigated both types of process i.e. reflective and automatic/associative (e.g. Walker et al. 2003; Eccles et al. 2007; Bonetti et al., 2006), we have only recently attempted to integrate the two types of model to investigate dual processes simultaneously (Presseau et al., in submission).
The second example, ‘cognitive dissonance and information discounting’, is considered in isolation from the theoretical approach which was designed to overcome some of the deficits of the cognitive dissonance model i.e. the TPB, which deals with dissonance in terms of multiple beliefs which may complement or contradict each other and with information discounting by using the evaluations (outcome evaluations and motivation to comply).

The third example, ‘feedback and feed forward’ also seems interesting. However no reference is made to the work by Gardner et al (2010) who analyse A&F studies using a major ‘feedback’ theory, ie ‘Control Theory’, and demonstrate new and useful insights into the feedback process.

In sum, in general I like the way in which this paper presents arguments. They reiterate many of the good arguments about theory in this field which have been very influential in the development of recent research in KT. However I am not persuaded of their belief (no evidence is presented) that MOC would advance the field. It does not offer a clear advance on the TD approach which can also be criticised for separating theoretical constructs from the integrated theories. The additional constructs they suggest adding to the field are interesting but I think the paper ignores previous papers which have used similar theoretical constructs and they provide little empirical support for the argument that this would advance KT research; in particular I was surprised at the omission of Gardner et al.’s paper ( I include the abstract below) since it both uses relevant constructs and has been applied empirically to A& F. The paper would be greatly strengthened by indicating how the proposed MOC approach would enhance published research that already overcomes some of the criticism e.g. that uses experimental designs and that is based on theory that specifies how improvement in KT can be achieved. So while I value the debate, I am not convinced that MOC would be an advance without a more persuasive comparison with current good theory-based KT research.

References
[I apologise for the predominance of reference to my own and colleagues work – they are simply included as readily available examples]


Clarkson JE , Turner S, Grimshaw JM, Ramsay CR, Johnston M, Scott A, Bonetti


Evidence syntheses are used to inform health care policy and practice. Behaviour change theories offer frameworks for categorising and evaluating interventions and identifying likely mechanisms through which effects are achieved. Yet systematic reviews rarely explicitly classify intervention components using theory, which may result in evidence syntheses and health care practice recommendations that are less than optimal. This paper outlines a method for applying theory to evidence syntheses of behaviour change interventions. We illustrate this method with an analysis of ‘audit and feedback’ interventions, based on data from a Cochrane review. Our analysis is based on Control Theory, which suggests that behaviour change is most likely if feedback is accompanied by comparison with a behavioural target and by action plans, and we coded interventions for these three techniques. Multivariate meta-regression was performed on 85 comparisons from 61 studies. However, few interventions incorporated targets or action plans, and so meta-regression models were likely to be underfitted due to insufficient power. The utility of our approach could not be tested via our analysis because of the limited nature of the audit and feedback interventions. However, we show that conceptualising and categorising interventions using behaviour change theory can reveal the theoretical coherence of interventions and so point towards improvements in intervention design, evaluation and synthesis. The results demonstrate that a theory-based approach to evidence synthesis is feasible, and can prove beneficial in understanding intervention design, even where there is insufficient empirical evidence to reliably synthesise effects of specific intervention components.

"Minor issues not for publication"

Some small points:
• No abstract – but a summary at the end
• Reference 11 (and possibly others) is the wrong reference for the point being made
• Abbreviation A&F introduced without explanation
• The heading ‘Discussion’ seems inappropriate
• ‘Early results suggest’ (p 17) is unreferenced

Level of interest: An article of limited interest

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I publish in this field but I think this is all clear in my review