Editor’s note (J. R. S.)

Volume 17, issue 5 of *Health Education Research* was devoted to the work of the Behavior Change Consortium (BCC) in the United States. Because the BCC approach constitutes one perspective on improving the health of the public, we asked for a Commentary on the BCC approach by Dr Jim Connelly of Leeds University, UK. Below is his perspective on the BCC approach, as well as two responses from BCC members to his critique. We invite commentary, as Letters to the Editor, on this important discussion.

COMMENTARY

The Behavior Change Consortium studies: missed opportunities—individual focus with an inadequate engagement with personhood and socio-economic realities

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Introduction

The papers and a supportive Commentary in the Behavior Change Consortium (BCC) theme issue of *Health Education Research* (Volume 17 number 5, 2002) make large claims about the importance of the National Institutes of Health (NIH)-funded BCC studies they report. We are told that not only will the results of these studies supply behavioral change methods with tested interventions, but they will allow us to understand the relative effectiveness of the various underlying theoretical frameworks which will, in turn, allow practitioners to discard redundant theories in favor of what is of proven worth. These claims are of such importance that it may be sensible to ignore some obvious problems and issues in the studies. However, as it turns out, the number and substance of these problems and issues is so extensive that they require a necessarily brief critical discussion.

This critique is not written to represent any particular faction. It is not based upon a UK (or European) stance on health promotion or health education and it is not written to convey any particular professional view. Instead, the comments I shall make arise from a number of differences in the way I conceive the appropriate responses to the problem behaviors targeted by the NIH BCC. However, whilst these remarks are my own, I think that they will be shared by many who accept the reasoning behind them and that it is likely that such people will share with me a WHO conception of health promotion—it is to this extent that my comments might be said to reflect a particular standpoint.

Critical issues

What is a WHO conception of health promotion? It may be summed up by its founding documents and declarations [see (Tones and Tilford, 2001)], and by an open acknowledgement of the social, economic and political influences on health
(Kickbusch, 1986; Milio, 1986). Briefly, this conception emphasizes the social and economic conditions which allow all behaviors, including unhealthy behaviors, to originate, develop and reproduce themselves over the life-course and to the next generation (Kuh and Ben-Shlomo 1997; Marmot and Wilkinson, 1999). Such an analysis highlights the first critical comment on the BCC studies, that they conceive behaviors as overly individually chosen, maintained and changeable. Far from reflecting a diversity approach, it is clear that the overwhelming perspectives here are an individualistic focus and a cognitive/construct psychology. Partial exceptions to this are the studies by Coday et al. (Coday et al., 2002) which engages with low incomes; Resnick et al. (Resnick et al., 2002) who well-specific social mediating factors; Williams et al. (Williams et al., 2002) who use ‘Self-Determination Theory’ on the basis that autonomy responds to empowerment rather than manipulation; Dwewaltowski et al. (Dzewaltowski et al., 2002) which pays serious attention to a settings approach and to the realistic effectiveness of interventions in context; and Borrelli et al. (Borrelli et al., 2002) which also pays serious attention to the underlying mechanism for changes derived from a contextual social theory and adopts an appropriately sophisticated analysis.

Unfortunately, the paper on theory comparison by Nigg et al. (Nigg et al., 2002), whilst acknowledging this wider perception of health promotion, fails to adequately trace the missed opportunities for a community-wide and policy-change research agenda. Their commentary suggests that although such wider considerations were known, they were consciously discounted in favor of what are presented as more specific scientific goals. However, this faint acknowledgement will not suffice, more is required. Why was the overwhelmingly individual focus for change not contrasted with a collective focus? Why was change in more affluent groups not compared and contrasted with that in less affluent groups? Such contrasts would supply useful information on the social and economic realities in which the ‘problem behaviors’ are lived. Although the introductions to the majority of these studies ably document the epidemiology of the problem behaviors, describing the strong socioeconomic patterning of sedentary habits, smoking and being overweight, the causal reasons and mechanisms behind these consistent observations are not discussed or seen as a basis for intervention. Rather, with the partial exceptions noted, the problem behaviors are taken as ‘given’ and their reduction (and in some cases its sustainability) is the only aim. Other approaches were possible (Puska, 1985; Samuels 1993; Wilkinson 1996). Even within a cognitive psychology paradigm the individual focus was not the only option (Wallak et al., 1993; Bandura, 1997), but this collective/community perspective is only marginally represented in these studies. Indeed, Nigg et al. (Nigg et al., 2002) themselves provide additional references to community/collective and policy-change studies; it appears that if this road was not followed, it was not followed deliberately.

Turning to the conception of health psychology in these studies it is regrettable that truly different conceptions were not sought or funded. The approach is relentlessly that of a ‘subpersonal’ cognitive psychology (Harre, 1984; Edwards and Potter, 1992). Even though open to quantification, the constructs of most cognitive models are not conceptually or empirically well defined, indeed at least some are part of an operationalized tautology, i.e. a putative construct is ‘measured’ and by a type of methodological behavioralism this fact of measurement is then presented as evidence for the actual existence of what was measured. Early on, Cummings et al. (Cummings et al., 1980) noted that there was considerable overlap between the various constructs contained within social cognition models of health behavior and suggested that where differences appeared this represented merely a difference in labeling rather than a real difference (Conner and Norman, 1996). The constructs of ‘self-efficacy’ and ‘autonomy’ or ‘self-determination’ might appear to be more ‘integral’ and reflect a ‘person-centered’, even a ‘holistic’ conception of the human mind, but they too oversimplify, probably for reasons of methodological behaviorism, the elaborated complexity, dynamism
and crucially discursive and emergent nature of the root conception of personhood ‘self’ (Harre, 1998). What is at stake here is the appropriateness and scientific status of the outdated metaphor of the computer which still underpins much of cognitive psychology. The centrality of rule-following rather than discourse, behavior rather than meaning and ends-means calculation rather than integrity of personhood over time (narrative biography) are evident in the construct, and overwhelmingly, cognitive psychology adopted in the BCC studies. Again, more than one alternative approach was available (Edwards and Potter, 1992; Harre, 1998).

The applicability of the critique of the computer metaphor contained in the philosopher John Searle’s ‘The Chinese Room’ argument is called to my mind (Searle, 1981). In particular, even when the partial correlations and structural equations (promised in some analyses) are computed, I think we will not know what motivated the changed behaviors that are observed and measured. Surely it was possible for the NIH to have funded qualitative research to include in-depth interviewing of the people who did and did not participate in these various studies. This qualitative research would have explored the personal (and social and economic) reasons for success or failure. Such research is necessary because agreement of persons with cognitive psychologists over the meaning and content even of ‘constructs’ (the building blocks of cognitive psychology) cannot be assumed (Conner and Norman 1996). Moreover, as Oakley (Oakley, 2000) reminds us, even given the (safety blanket) formalism of the randomized controlled trial (RCT), behavioral results cannot be simply read-off from the characteristics of the researcher-identified intervention (Garcia et al., 1985). Disregard of emotional influences, absence of the personal generation of salient health beliefs (rather than using modal results from small pilots) and lack of adequate ‘within-subject’ analyses are also problems for many BCC studies which may be identified as arising even within a cognitive construct psychology (Conner and Norman, 1996).

Will these studies and the collaboration they have enabled deliver unambiguous knowledge about the comparative effectiveness of different theories? The short answer is that this is unlikely. Behavioral outcomes and cognitive features as measured by construct scales are likely to be even more under-determined by theory than is usually the case; reasonable accounts of behavior can and no doubt will be derivable from different theories with the recourse to ‘unmeasured variables’ always at hand to make or complete one or another post hoc explanation. In particular, despite what appears in some cases as a rather tokenistic recourse to randomization, these studies do not even come close to the ideal of the ‘explanatory’ trial (Schwartz and Lellouch, 1967). They are better viewed as a series of ‘pragmatic’ trials and even here there are particular difficulties in that it is at least a possibility that there will be subgroup or even main effects attributable to ‘therapist characteristics’ which will remain, in these trials, of only speculative importance as they were not measured or included in the randomization. This and other problems (see below) make the clear-cut description of the intervention packages that are being used far from simple and this practical difficulty alone makes for a severe problem in generalizability.

In psychotherapeutic interventions (cognitive or not), the control condition is particularly important and is usually far from ‘inactive’. Control conditions (therapist influences apart) can evoke and meet either positive or negative expectancies (Crow et al., 1999). Unsurprisingly, the effects of control conditions are best explained as being due to the interpretation and meanings that persons actively construct and believe about these conditions. Oakley provides a useful discussion of the classic ‘positive’ Hawthorne effects [(Oakley, 2000), pp. 182–184] and its converse, the ‘resentful demoralization’ [(Oakley, 2000), p. 282] of those ‘excluded’ by randomization along with other subjective effects of the control condition.

The correct classification of the BCC studies as pragmatic rather than explanatory trials is important for other reasons. Firstly, in an explanatory trial the finding that there is no difference between the ‘theory-positive’ and ‘theory-negative’ arms
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usually leads to a conclusion of ‘no effect of the theory positive’ and, at least sometimes, claims that the tested theory was wrong or at least inadequate. However, if both arms, in fact, show useful levels of positive change it is important to emphasize this and further explore its practical importance; such results should not be discarded because they did not distinguish between theories. Consequently, given that there will inevitably be ‘active’ control conditions it is most unwise to a priori set the comparison of standardized effect sizes as the way to compare trials.

To fully make use of the likely BCC studies’ results the purpose of the RCT must be interrogated. Why insist on a control condition and randomization if, as is stated, these studies are about understanding the usefulness of theoretically based interventions? The emergence of other methods for exploring complex interventions in a complex and irredeemably open system (society) should have allowed funding success for designs that eschewed the ‘imprimatur’ of the RCT (Pawson and Tilley, 1997; Lipsey and Cordray, 2000).

Conclusions

Despite the inadequacies of the RCT design, it may still be possible in some cases to use a context-sensitive realistic analysis which takes the espoused theoretical frameworks seriously (Pawson and Tilley, 1997; Connelly, 2001). Briefly (and approximately) this would mean a thorough ‘within-subjects’ analysis which relates behavioral outcomes to a priori identified individual (or group)-level mediator and moderator ‘generative mechanisms’ (Pawson and Tilley, 1997). Proponents of the RCT would see this recommendation as just another call for subgroup analysis and are likely to point out the severe limitations of subgroup analyses for producing statistically unbiased knowledge (Assmann, 2000). However, from a realist perspective, the waste of information which can and is produced by a simple aggregation of results (looking at mean or median or modal values) is unsupportable (Connelly and Worth, 1997). Even though many of the BCC protocols state that their analysis ‘will explore the influence of mediator variables’, this requires a much more detailed and a priori specification.

Taken together these trials reflect the dominance of particular types of explaining social action. They eschew any sociologically realistic engagement with the day-to-day structural determinants of health and health-related behaviors in favor of a deeply positivist behavioral and cognitivist psychology. Their individualistic focus, with some exceptions, reflects a wider cultural myth of freedom of choice over consumption without financial restraint (so that healthy food choices are seen as affordable and accessible to everyone). Personhood, empowerment and collective action comprise key alternative concepts that appear to have been left unconsidered or unattractive to grant awarding agencies.

Finally, it is not clear what ethical understanding of the place of behavioral therapy within public health is assumed in these studies. Volunteering for life-changing studies when empirical research on behavior change has not adequately established increased length or quality of life amongst those at risk (but who have not actually had a disease) requires further justification. One does not have to subscribe to the existentialist ethics which valorize ‘authenticity’ above conformity to have some unease at the seemingly untrammeled enthusiasm for ‘healthy’ behaviors as both individually desirable and, implicitly, correct. There are many values other than ‘health’ and being other than ‘oneself’ does not logically, or empirically, imply an improved quality of life or an increase in the flourishing of the self as a member of a community (Collier, 1999).

References


Connelly, J. (2001) Critical realism and health promotion:


