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### Interventions to change health behaviours: evidence-based or evidence-inspired?

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## INTERVENTIONS TO CHANGE HEALTH BEHAVIOURS: EVIDENCE-BASED OR EVIDENCE-INSPIRED?

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This critical review assesses whether evaluation studies can answer three key questions about behaviour change interventions: ‘Do they work? How well do they work? How do they work?’ Reviews of intervention evaluations are examined, particularly those addressing decreasing unprotected sexual intercourse and smoking. Selection of outcome measures and calculation of effect sizes are discussed. The article also considers the extent to which evaluation reports specify (i) discrete intervention techniques and (ii) psychological mechanisms that account for observed behavioural change. It is concluded that intervention descriptions are often not specific about the techniques employed and that there is no clear correspondence between theoretical inspiration and adoption of particular change techniques. The review calls for experimental testing of specific theory-based techniques, separately and in combination.

*Keywords:* Intervention; Behaviour change; Effectiveness; Trials; Health behaviour; Health promotion

What we do affects our health and longevity (Belloc and Breslow, 1972; Wingrad *et al.*, 1982). Persuading people to cease smoking, to take exercise, to eat a healthy diet and to avoid unprotected sexual intercourse has the potential to improve the health of a population. Moreover, Kaplan (1990) has argued that behavioural change *per se* is often the aim or “central outcome” of health care. Consequently, key questions for health psychologists are: Which behaviour change techniques work? and How do they work?

Evaluations of behaviour change interventions can answer three questions about an intervention. First, *does it work?* Demonstrating that an intervention has produced measurable improvement relative to an appropriate control group is prerequisite to

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investment in subsequent trials or adoption in health care practice.<sup>1</sup> Second, *how well does it work?* The effect size generated by a successful trial indicates the impact that the intervention is likely to have at an individual or population level. This has implications for cost effectiveness. Third, *how does it work?* Evaluation replication and intervention adoption (Bartholomew *et al.*, 2001) depends upon specification of the critical techniques or procedures responsible for behaviour change. Improving intervention effectiveness and transferring change techniques from one behaviour to another requires an understanding of the causal processes and mechanisms, that is, the underlying psychological changes that account for observed behaviour change. Without such understanding, the application of behaviour change technologies is likely to be slow, with “wheels” being re-invented rather than re-applied.

When behaviour change interventions consist of techniques based on empirically-supported theory, then that theory provides an explanation of how the intervention works. For example, in the case of operant conditioning, the behaviour change techniques involve manipulation of environmental contingencies. If an unwanted behaviour is maintained by rewarding consequences then withdrawing that reinforcement and reinforcing alternative behaviours should reduce the frequency, duration, and/or intensity of the unwanted behaviour. Fordyce (1976) demonstrated that withdrawing social interaction and approval following pain behaviours (e.g., moaning, wincing, and avoiding action) and making such reinforcement contingent on other activities reduced the frequency of pain behaviour. Techniques involving identification of reinforcers and changing the consequences of behaviour have been widely and successfully applied across behaviours (e.g. Walker, 1984; Sarafino, 2001). Similarly, specific techniques have also been developed to enhance self-efficacy (Bandura, 1986), including verbal persuasion, modelling (including self-modelling), selective attention to past successes and sequential mastery experiences. These techniques have also been widely applied and found to generate behaviour change (Bandura, 1992, 1997, 1998). In general, however, theories identifying psychological determinants of health behaviours (see Abraham *et al.*, 1998 for a review) have not generated behaviour change interventions based on well-specified, empirically-supported change techniques (cf. Hardeman *et al.*, 2002).

The aim of this review is to assess the extent to which available evidence supports the development of effective, theoretically-understood behaviour change techniques across behavioural domains. We discuss selected reviews of intervention evaluations and consider whether interventions have successfully changed behaviour, how effective they have been, how they have achieved their effects, and what particular psychological behaviour change techniques have been identified. We argue that experimental research and increased theoretical and methodological clarity could accelerate the identification of effective behaviour change techniques and the development of evidence-based practice in health psychology. We suggest that improving the quality of evaluations and standardising intervention evaluation reports could facilitate this process.

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<sup>1</sup> Behaviour change only indicates intervention success when naturally occurring change is controlled. Therefore, non-randomised studies using “before and after” designs provide weak evidence of change. We use the term “behaviour change” to mean behavioural change over and above that is observed in a matched control group that cannot reasonably be attributed to measurement inaccuracy. Note that “behaviour change” does not imply use of change scores (that may compound measurement error over observations). Baseline behaviour rates may be controlled statistically or through randomisation.

## DOES IT WORK?

Behaviour change interventions are effective to the extent that they make a difference in terms of appropriately measured behavioural outcomes in properly controlled experiments or trials. If a trial is methodologically flawed then, even if the intervention appears to be effective, it will require further testing. Consequently, minimum quality standards for research methods in evaluations are essential. Oakley *et al.* (1995) argued this point cogently in a methodological review of sexual health education interventions. Drawing upon the principles developed by the Cochrane Collaboration (1994), Oakley *et al.* identified four main indicators of methodological rigour, namely, (1) employing a randomly allocated or matched control group, (2) reporting pre-intervention and post-intervention data, (3) reporting “intention-to-treat” analyses or controlling for differential attrition, and (4) reporting on all outcomes indicated by the aims of the intervention. In addition to these core criteria, Oakley *et al.* also noted that providing an intervention description that allows replication and ensuring adequate statistical power are crucial (see also Jepson, 1999).

Further criteria are required to judge the external validity of intervention evaluations (i.e., the generalisability of results). In a review of controlled interventions to change diet, physical activity, and smoking cessation in health care settings, Glasgow *et al.* (2002) discuss five such criteria (the RE-AIM evaluation framework). These are (i) “reach” i.e., participation rate and representativeness and investigator exclusion rate, (ii) “effectiveness” i.e., intended and unintended consequences (e.g., side effects), (iii) “adoption” i.e., the extent to which an intervention is adopted by implementers, (iv) “implementation” i.e., whether components of an intervention are delivered as intended, (v) “maintenance” of behaviour change (requiring long term follow-up). Glasgow *et al.* found that published evaluation reports often omitted information about these criteria, limiting the possibility of assessing external validity.

Choice of outcome measures is critical to any trial. It can be argued that if interventions are designed to change behaviour in order to promote health, then health outcome should be the index of success (cf. Stephenson *et al.*, 2000). For example, if an intervention is designed to promote adherence to a prophylactic medication regimen then, perhaps, reduced infections rather than increased adherence should be the critical outcome. This approach is limited, however, because factors other than targeted behaviour change affect health status. Multiple lifestyle characteristics, including a range of behaviours and contextual factors, from socio-economic status to disease prevalence (e.g., Alder *et al.* 1994), may moderate the effect of a particular behaviour on a specified health outcome. Consequently, the effectiveness of an intervention in altering a targeted behaviour may be missed when evaluations focus on health, rather than behavioural, outcomes. Of course, if observed behaviour change does not affect health outcomes, then questions arise regarding the appropriateness of the targeted behaviour, its measurement and assumptions about its causal links to health.

A particular behaviour change may also affect a range of health indices. For example, adherence in placebo conditions appears to be associated with a wide variety of health outcomes (Epstein, 1984). Thus, the effectiveness of behaviour change techniques may be underestimated by assessment of specific health outcomes. Consequently, objective measures of behaviour are likely to be the most informative outcomes when evaluating behaviour change interventions.

A variety of methodological issues arise regarding the reliability and validity of behavioural measures and the interpretation of observed change, particularly in the case of self-report measures (e.g., Catania *et al.*, 1990). Change in reported behaviour may reflect different underlying change processes (e.g., Norman and Parker, 1996). Behaviour may have changed (alpha change) or a response shift may have occurred such that respondents have re-calibrated the measurement dimension (beta change) or re-conceptualised the behaviour (gamma change). For example, intervention participants may become aware that they have been overestimating health behaviours so that post-intervention realistic reporting may suggest a decrease in behaviour when, in fact, no change has occurred (beta change). Beta and gamma change can be identified and controlled for statistically (Vandenberg and Self, 1993).

Reported behaviour change can also occur in the absence of actual behaviour change due to social desirability effects. The perceived appropriateness of behaviour may be affected by respondents' perceptions of the research and researchers (e.g., Davies and Best, 1996). For example, in the case of treatment trials, participants may emphasise their problems at the beginning of treatment and seek to acknowledge their appreciation at the end, thereby exaggerating apparent intervention effectiveness. Hence the importance of placebo control conditions.

## HOW WELL DOES IT WORK?

Estimates of effect sizes are becoming more frequent as systematic reviews and meta-analyses are increasingly used to assess intervention success. Various effect sizes are available: for categorical data, the measure of choice is the odds ratio<sup>2</sup> (Haddock *et al.*, 1998) and for interval data,  $d$  is a key indicator of change<sup>3</sup> (Hedges and Olkin, 1985). These indicators allow comparison of the effectiveness of different interventions and enable researchers to estimate how much difference the intervention is likely to make if applied in practice. For example, Hunter and Schmidt (1990) note that effective psychological interventions typically have effect sizes (i.e.,  $d$  values) of 0.20–0.40. Thus effect sizes should be included in intervention evaluation reports.

Adequate statistical power is important, because underpowered evaluations are inconclusive if no effect is found. However, evaluations can also be overpowered in the sense that observed (statistically significant) differences may be judged to be too small to be of practical relevance (e.g., Family Heart Study Group, 1994; OXCHECK Study Group, 1994). Thus, specification of a minimum effect size indicating that a change could be of clinical relevance is an important part of power analysis.

Effect sizes, by themselves, do not reveal the importance of an observed behaviour change in terms of clinical significance or public health impact. This requires a broader assessment of the relationship between targeted behaviours, health, functioning and quality of life (Kaplan, 1990). Effect sizes and assessments of the impact of successful behaviour change interventions are required to undertake economic analyses of

<sup>2</sup> Odds ratio =  $p_i(1 - p_c) / p_c(1 - p_i)$  where  $p_i$  is the proportion which changes successfully in the intervention group and  $p_c$  is the proportion which changes successfully in the control group (Haddock *et al.*, 1998).

<sup>3</sup>  $g$  (also referred to as  $d$ ) =  $(m_i - m_c) / sd_{ic}$  the difference between the means of the intervention and the control group, divided by the pooled standard deviation (Hedges and Olkin, 1985).

cost effectiveness. These, in turn, determine the applied value of an intervention and influence implementation policy.

## HOW DOES IT WORK?

Understanding why an intervention works requires identification of the change-generating techniques that comprise the intervention. By “techniques”, we mean a concrete description of the procedures used by those delivering the intervention in sufficient detail to enable exact replication. Although this may seem obvious, evaluation reports rarely provide descriptions of intervention procedures that allow replication and these are not always available on request. If pressure on journal space precludes the inclusion of such detail, editors could require authors to post appropriate details on a cited website.

Without clear specification of critical techniques, replications of purportedly identical interventions may unwittingly vary in critical respects, making inconsistencies in outcome data difficult to explain. A further complication is that interventions often involve combinations of techniques. Optimal combinations of techniques can only be identified through experimental testing of techniques separately and in combination. This implies, for example, that, what Campbell *et al.* (2000) refer to as “modelling” in their framework for the evaluation of complex interventions, may itself involve multiple evaluation studies in order to identify components or techniques that are critical to intervention effectiveness.

Atheoretical interventions fail to build on existing knowledge. In the absence of a theoretical underpinning, intervention design is derived from implicit theories that may omit psychological processes central to behaviour change and so fail to optimise effectiveness. The generalisability of such interventions is also limited if change mechanisms are poorly understood. Theory has been variously defined. The Oxford English Dictionary (online), implies that a theory is a system of ideas, confirmed by observation or experiment, that explains a group of facts or phenomena. We suggest that interventions can be regarded as theory-based when the theory used to interpret findings has two characteristics. First, it provides a clear account of hypothesised mechanisms or causal processes that generate behaviour change. In other words, it describes psychological processes accounting for the initiation, re-direction or cessation of behaviour achieved by the intervention. Second, the theory is supported by independent experimental work. This implies that new theories should be subjected to empirical tests before being applied in behaviour change interventions.

In practice, the popularity of a theory may not depend on empirical support. A theory that corresponds to common sense ideas about behavioural change may be more easily accepted than one based on evidence. For example, Prochaska and DiClemente’s (1983) transtheoretical model (TTM; Prochaska *et al.*, 1992, 1993) highlights psychological ‘readiness for change’ by identifying preparatory psychological changes that precede behaviour change. This is an attractive basis for health promotion because it has the potential to identify intervention success in promoting readiness, rather than behaviour change itself, and because it appears to explain why some people respond to interventions while others do not. TTM has been applied to smoking cessation (see Sutton, 2000 for a review) and to other behaviour change

programmes (e.g., see Harlow *et al.*, 1999, for an application to condom use promotion). It has also been used as a basis for training health promoters. Yet the evidence for the stages of change proposed by the model remains contentious. Sutton (2000) concludes that the distinctions between stages are “logically flawed” and based on “arbitrary time periods” (pp. 209–211). For example, the “preparation” involves having made a recent quit attempt. Thus a smoker cannot pass through this stage when they make an attempt to quit for the first time. Sutton (1996, 2000) also notes there is little evidence supporting transition through these specified stages over time. For example, Prochaska *et al.* (1991) report that only 16% of participants progressed from one stage to the next without reversals over a two year period and that 12% moved backwards during the same period. Bandura (1998), Sutton (2000), and Weinstein *et al.* (1998) conclude that there is little evidence supporting the use of TTM, stage-specific interventions. Thus, despite the popularity of this model, the psychological processes described by TTM are not well supported by available data.

TTM proposes that movement between stages depends on a number of change processes (e.g., Prochaska *et al.*, 1992, p. 1108). These include cognitive changes (as in the case of “consciousness raising”, “self re-evaluation” and “environmental re-evaluation”) and changes in environmental contingencies (e.g., “counterconditioning”, “stimulus control” “reinforcement management” and “social liberation”) and may involve interaction with others (as in the case of “dramatic relief” and “helping relationships”). Thus, movement through TTM stages is determined, at least in part, by processes specified by social cognitive and learning theory models of behaviour change. If psychologists aspire to establish a range of theoretically-understood, and evidence-based behaviour change techniques then the key research question is – do specified change processes reliably generate behaviour change? – not – do people move through discrete changes during behaviour change? There is little research into TTM-specified change processes but Segan *et al.* (2002) found that most of these processes did not predict initiation (i.e., quit attempts) or maintenance (i.e., abstinence) of smoking cessation.

A number of social cognition models specify how potentially modifiable beliefs shape behaviour (see Conner and Norman, 1996; Abraham *et al.*, 1998 for reviews). If these theories are to contribute to understanding behaviour change, then cognition-changing techniques need to be specified and the mediation of behaviour change outcomes by theory-specified cognition change must be demonstrated (Baron and Kenny, 1986). Therefore, including measures of proposed mediators in intervention trials will facilitate the development of psychological models of behaviour change.

Cognitive constructs specified by social cognition models, other than social cognitive theory (Bandura, 1998), have not been widely applied to behaviour change interventions. Some successful interventions have been based on the health belief model (Janz and Becker, 1984; Jones *et al.*, 1987; Strecher *et al.*, 1994) and the theory of planned behaviour (TPB; Ajzen, 1991; Hardeman *et al.*, 2002). However, techniques designed to change specific cognitions correlated with health behaviour change have not been systematically tested. For example, a recent review of applications of the TPB to behaviour change found a diverse literature with few studies that were explicit about how the theory had been applied (Hardeman *et al.*, 2002). Of the 12 identified studies that used the TPB to develop a behaviour change intervention, four were found to change behaviour and none investigated whether behaviour change was mediated by the psychological changes proposed by the theory. Thus, although numerous studies demonstrate that

the TPB can predict health behaviours (e.g., Godin and Kok, 1996), the theory has not been systematically evaluated as an explanation of behaviour change.

A variety of techniques have been used in intervention studies drawing on the TPB, including verbal persuasion, goal setting, rehearsal of skills, modelling, and planning (Hardeman *et al.*, 2002). Verbal persuasion techniques have been tested in studies of belief and attitude change (e.g., Cooper and Fazio, 1984; Petty and Cacioppo, 1986) and applied to health behaviours. Similar techniques have also been used to create cognitive dissonance. For example, Stone *et al.* (1994) demonstrated how inducing cognitive dissonance can be applied to the promotion of condom use. However, available data do not allow a mapping of specific intervention techniques onto change mechanisms, i.e., theory-specified psychological changes explaining the effects of behaviour change interventions. Yet this is what is required if psychology is to develop a scientifically-based, theory-specified technology of behaviour change.

An example of the systematic evaluation of a specific theory-based technique is Gollwitzer's work on implementation intentions. Gollwitzer proposed that once an intention is formed it is more likely to be enacted if a person also decides when and where to act (e.g., I will go swimming at the town-centre pool at 6.00 pm on Tuesday evening). Prompting this type of cognitive rehearsal was found to be effective in a series of laboratory studies (Gollwitzer, 1993, 1999) and has since been evaluated in applied interventions (e.g., Michie *et al.*, under review; Orbell *et al.*, 1997; Sheeran, 2002).

The specification of psychological change mechanisms underlying the effectiveness of a particular technique highlights the need for process evaluation in which theory-derived antecedents of behaviour change are measured and techniques are evaluated separately. For example, Milne *et al.* (2002) found that an exercise-promoting intervention based on protection motivation theory resulted in predicted cognition changes but did not promote behaviour change. However, when those receiving the intervention were also prompted to form implementation intentions they were more likely to exercise than those who did not make such specific plans. Thus, in this case, implementation intention formation can be identified as the critical technique. Such studies show how the measurement of psychological antecedents and experimental manipulation can help develop a theory-based and evidence-based science of behaviour change.

In summary, we have argued that (i) observable behavioural change is the best criterion of effectiveness for behavioural intervention, (ii) it is important to be able to compare intervention effectiveness using effect sizes, (iii) a technology of change must be founded on clearly described intervention techniques derived from explicit theoretical models that have received independent empirical support, (iv) mediational analyses of the relationship between theoretically-predicted psychological antecedents of behaviour change and intervention-induced behaviour change can clarify underlying mechanisms, and (v) experimental examination of particular techniques, separately and in combination, is required to identify the critical change-generating processes in a successful intervention.

## REVIEWS OF BEHAVIOUR CHANGE INTERVENTIONS

These arguments are illustrated through examination of recent reviews of interventions designed to change two health behaviours: the frequency of unprotected sexual



intercourse and the frequency of smoking. These are different types of behaviour: the first is a social behaviour, biologically driven, and with lower frequency than the second which is an individual, addictive behaviour. It is not our purpose to review intervention effectiveness in these domains but to reflect on the way in which intervention effectiveness has been tested, explained, and reviewed. Reviews have been selected because they are comprehensive or because they illustrate issues relating to reviewing procedures or the development of a theory-based technology of behaviour change. We shall examine the manner in which the reviews establish the adequacy of evaluations, the outcome measures they consider, and their use of effect sizes. We shall also discuss the extent to which they identify discrete intervention techniques and seek to establish whether or not evaluations test psychological theories purporting to account for observed behavioural change.

### **Decreasing the Frequency of Unprotected Sexual Intercourse**

Unprotected sexual intercourse is a difficult behaviour to research because objective measures are not normally available. However, Gerrard *et al.* (1989) found good agreement between couples' reports of condom use and methodological recommendations for maximising the accuracy of measures of sexual behaviour are available (Catania *et al.*, 1990). Fisher and Fisher (1992) reported a comprehensive review of evaluations of interventions in this area. They found few randomised control trials (RCTs) and observed outcome measures ranging from reported attitudes and intention change to reported condom use at one year follow-up. The review reported significant differences in specified outcomes (e.g., pre-intervention, post-intervention differences) but not effect sizes. Brief descriptions of interventions were abstracted from the original reports (e.g., "single small group session offering AIDS information, behavioural skills training, and motivation to change behaviour") and interventions were categorised in terms of whether their content was derived from "formal" or "informal" theory.

Nine interventions (18%) were classified as based on formal theory and, although a few were found to have successfully changed reported behaviour, limitations in evaluation methodology cast doubt on the reliability of findings. For example, commenting on interventions for gay and bisexual men, the authors noted that "despite the apparent success of these interventions, methodological limitations prevent any of them from being regarded as definitive" (p. 456). Fisher and Fisher concluded that few effective interventions had been identified, primarily because "AIDS reduction efforts that have been based on formal conceptualisations of any kind are exceedingly rare" (p. 463). Thus, evaluation studies conducted over eight years were, with few exceptions, found to be of little relevance to the development of a theoretical understanding of the processes involved in behaviour change. Moreover, this work was, in general, no more than suggestive in relation to future intervention development because weaknesses in method limited conclusions regarding effectiveness. This constitutes a major loss in terms of the potential theoretical and applied implications of this body of work.

Fisher and Fisher's (1992) conclusions were confirmed by Oakley *et al.* (1995) who identified 65 outcome evaluations of sexual health interventions for young people. Only 20 (31%) were RCTs and only 12 (18%) met the four core methodological criteria listed above. Outcome measures varied, including increased knowledge and self-reported condom use. The review provided brief descriptions of interventions and noted significant differences but not effect sizes. Three of 12 "methodologically sound" interventions

were judged to be effective but only one of these (Rotheram-Borus *et al.*, 1991) measured behaviour at follow-up (condom use over the next six months). Interestingly, this intervention was judged by Fisher and Fisher (1992) to have an “informal” theoretical basis. One of the 12 methodologically sound interventions was judged to be “harmful” because young men in the intervention condition were likely to report having had their first sexual experience earlier than those in the control group. This intervention sought to promote premarital sexual abstinence through highlighting sexual risks and developing sexual decision-making (Christopher and Rosa, 1991).

Recent reviews have been more encouraging. Kalichman *et al.* (1996) examined 12 controlled trials of interventions designed to induce sexual behaviour change (including the one effective behavioural change study identified by Oakley *et al.*, 1995). Kalichman *et al.* argue that, in general, these interventions were inspired by social learning theory and social cognition models and, in particular, by the work of Bandura (1986, 1997) and colleagues. It is unclear whether particular theoretical propositions were used to design these interventions and whether the evaluations examined the extent to which changes in theory-specified measures could account for observed behaviour change (see Hardeman *et al.*, 2002). Nonetheless this is a useful review, including descriptions of the evaluations including the design, sample sizes, effect sizes (*ds*), a brief indication of the nature and duration of the intervention and the type of control group. All studies had positive outcomes, with effect sizes ranging from small to moderate with an average of  $d=0.25$ . Although this is a small effect size, Kalichman *et al.* noted that it is of similar magnitude to that observed for a number of routinely-employed health care interventions. This evidence suggests that such interventions do work, but it does not provide guidelines on what techniques are essential to this success. Kalichman *et al.* also noted that few evaluations monitored maintenance of behaviour change over time (beyond post-intervention measures).

Kalichman and Hospers (1997) identified 19 RCTs of interventions designed to induce sexual behaviour change. Findings were described in relation to a range of target groups but effect sizes were not calculated. The authors emphasise the importance of social cognitive theory and note the development of HIV-specific cognition models, including the AIDS Risk Reduction Model (Catania *et al.*, 1991) and the Information Motivation Behaviour model (Fisher and Fisher, 1992) which is closely related to the TPB. Kalichman and Hospers noted that such models “specify four fundamental intervention activities”, namely,

- “1. Instruction: providing an explanation and rationale for learning the new skill.
2. Modelling: providing an example of effective enactment of the behaviour provided by a credible model.
3. Practice: role-playing potential risk-inducing situations to practice the new behaviour; and
4. Feedback: using feedback on performance from group leader and fellow group members to support and reinforce behaviour changes” (Suppl. p. 192).

This is a helpful list but it does not provide empirical evidence clarifying whether or not these techniques are critical to intervention success. As such, it remains “evidence-inspired” rather than evidence-based.

Stephenson *et al.* (2000) reviewed seven RCTs of sexual behaviour interventions that used laboratory-assessed rates of sexually transmitted infections (STIs) as outcomes. Two interventions were judged to be effective while five reported non-significant

differences between intervention and control groups. Effect sizes were not reported. The two effective interventions were based on the theory of reasoned action (Fishbein and Ajzen, 1975) a precursor of the TPB (Kamb *et al.*, 1998) and the AIDS Risk Reduction Model (Shain *et al.*, 1999). This supports the conclusions of Kalichman and colleagues (Kalichman *et al.*, 1996; Kalichman and Hospers, 1997) that such social cognitive models provide a good basis for intervention design (see also Sheeran *et al.*, 1999). However, Stephenson *et al.* noted that it is difficult to assess the extent to which such theories have been faithfully translated into intervention techniques.<sup>4</sup> Again, it is unclear whether particular theoretical propositions were used to design these interventions and whether mediational analyses were conducted.

Six of the seven studies identified by Stephenson *et al.* reported intervention-induced behaviour change but only two reported effects on STI rates. The two successful interventions were based on preliminary research into the needs and perceptions of the target populations, suggesting that this may have contributed to their success (see also Ajzen and Fishbein, 1980; Fisher and Fisher, 1992). These data also emphasise that the relationship between sexual behaviour change and STI rates is complex. No measures of behaviour are available from participants' partners and changes in number of sexual partners, regardless of risk behaviour, could have an important impact on STI-risk. Thus, assessment of multiple behaviours may be required if we are to account for how sexual behaviour change is, or is not, translated into changed disease risk.

Jemmott and Jemmott (2000) examined the effectiveness of 36 controlled interventions designed to reduce HIV-risk amongst young people. Effect sizes were calculated and supported the findings of Kalichman *et al.* (1996) with average *ds* for interventions measuring condom use and unprotected intercourse as outcomes of 0.28 and 0.19, respectively. Interestingly, Jemmott and Jemmott examined whether interventions had effects on theoretically prescribed cognitive mediators of such behaviour change, including knowledge, beliefs, intention, and self-efficacy. They divided interventions into those that had a small effect ( $ds = 0.04 - 0.19$ ,  $N = 11$ ) and those that had large effects on such cognitive mediators ( $ds = 0.21 - 0.87$ ,  $N = 9$ ). Those with stronger effects on cognitive mediators had a larger effect on condom use ( $ds = 0.15$ ) for interventions with a small effect on mediators *versus* 0.41 for interventions with larger effects on mediators abstinence ( $ds = 0.02$  *versus* 0.20, respectively). This is consistent with previous reviews highlighting the utility of social cognitive theories. It also underlines the value of measuring potential mediators of intervention effects. Jemmott and Jemmott conclude that carefully designed interventions based on theory, and taking account of the population characteristics and cultural context, can generate positive changes in adolescents' HIV-risk behaviour. They noted, however, that the external validity and boundary conditions for such effectiveness require clarification.

Overall, reviews of evaluations of interventions designed to change sexual behaviour suggest that such programmes can be effective in comparison to randomised controls.

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<sup>4</sup> Stephenson *et al.* noted that the study applying the AIDS Risk Reduction Model takes no account of the stages identified by the model. However, Catania *et al.* (1990) presented the AIDS Risk Reduction Model "as a heuristic device intended to facilitate the conceptual organization of research on individual behaviour change, and intervention development and evaluation" (p. 55) and did not envisage movement through "stages" as unidirectional.

However, reported behaviour change may not translate automatically into reductions in STI rates (Stephenson, 2000). Theory-based interventions appear to be effective, especially those based on social cognition theories such as social cognitive theory, the theory of reasoned action and the AIDS Risk Reduction Model (Kalichman *et al.*, 1976; Fisher and Fisher, 1992; Kirby *et al.*, 1994; Stephenson *et al.*, 2000). However, there is no systematic evaluation of whether intervention content (as opposed, for example, to outcome measures) is theory-based and no systematic testing of whether changes in theory-specified measures can account for behaviour change. Reviews also indicate that preliminary research into the determinants of the target population's sexual behaviour enhances the likelihood of success.

This body of work provides a basis for systematic trials that could evaluate the use of particular theory-based techniques (and combinations of these techniques). At present, however, despite considerable investment in this area, it is difficult to identify particular intervention techniques that have been found to reliably promote safer sexual behaviour (such as condom use) amongst any particular population, although skill development techniques (e.g., instruction, modelling, role play, and feedback) and a focus on sexual negotiation are likely to be important (Bandura, 1997; Kalichman and Hospers, 1997). Moreover, the lack of systematic mediational analysis means that we can only tentatively suggest psychological changes that facilitate reported behaviour change.

Quality, theory-driven interventions can be identified. For example, Fisher *et al.* (1996) report an evaluation of a group-based, randomised control trial of an intervention designed to change antecedents of HIV-preventive behaviour specified by the Information-Motivation-Behavioral Skills (IMB) model. The IMB model was developed by Fisher and Fisher (1992) and integrates knowledge of HIV/AIDS, HIV-relevant health beliefs (e.g., perceived susceptibility), cognitive constructs specified by the theory of reasoned action (e.g., attitudes, social norms, and intentions regarding condom use; Fishbein and Ajzen, 1975) as well as the cognitive prerequisites of skilled performance such as self-efficacy in relation to preparatory actions such as buying and carrying condoms and negotiating condom use. Theoretical relationships between these various antecedents are precisely specified (see Fisher and Fisher, 1992). Fisher *et al.* (1996) provided students in the intervention group with three two-hour educational sessions. Theory-based measures of students' knowledge, motivation, and skills were assessed pre- and post-intervention and HIV-preventive behaviour (e.g., condom use) was assessed using self report measures 2–3 months after the intervention. The intervention employed techniques that targeted theoretically-specified cognitive antecedents of condom use. For example, persuasive communication was used to promote positive norms and undermine negative attitudes towards condom use. During small group discussions, peer educators elicited examples of the increasing normativeness of condom use and expressed their own approval of condom use. Peer educators also acknowledged negative attitudes (that had been observed in previous research with this population) and invited participants to identify ways of avoiding negative outcomes that some students associated with condom use. Intervention participants watched safer sex negotiation being modelled on video and by peer educators and practiced putting a condom on a model of a penis. Results showed that compared to the control group, the intervention group had greater knowledge, more positive attitudes towards condom use, and higher self-efficacy in relation to performing HIV-preventive behaviours at post-intervention assessment. At follow-up, the intervention group

reported significantly more HIV-preventive behaviour, including acquiring keeping condoms accessible and using condoms during intercourse. No formal mediation analyses were reported. Nonetheless, this intervention illustrates how a clearly-specified theoretical model can be directly linked to behaviour-change techniques designed to affect particular antecedents of behaviour and how both potentially-mediating and behavioural measures can be included as outcome measures.

### Smoking Cessation

Guidelines for the conduct of smoking cessation interventions have been published within the UK (Department of Health, 2000). These are derived from a review commissioned by the Health Education Authority which concludes that “intensive support” (for example, a smokers’ clinic) combined with nicotine replacement therapy (NRT) increases the percentage of smokers who are abstinent after six months by 16% (Raw *et al.*, 1999). These guidelines include the following recommendations for the content of smoking cessation interventions: (1) five weekly evening group sessions of about an hour each over four weeks, with follow-up sessions offered at various times up to 12 months from the beginning of the course; (2) ensure NRT is distributed during the first session and a quit date is set; (3) use subsequent sessions to discuss group members’ experiences and difficulties and to facilitate mutual encouragement and support; (4) adopt a client-oriented, rather than didactic approach to group sessions; and (5) use groups of 15–25 people. These guidelines specify intervention procedures that have been found to be effective across studies. This is unusual in the behaviour change literature and welcome from a practice perspective.

The evidence underpinning these recommendations is taken primarily from two systematic reviews, one from the UK (Silagy *et al.*, 1999) and one from the US (Fiore *et al.*, 1996). The first, a systematic review of 87 randomised controlled trials evaluating the effectiveness of different forms of NRT, produced an odds ratio of 1.72 for abstinence from smoking for at least six months with NRT compared to control (Silagy *et al.*, 1999). Interventions comprised gum, patches, spray, or inhalers and were compared with placebo or a control with no NRT. Where possible, interventions were categorised as low-intensity additional support (routine care) or high-intensity additional support (a first consultation of at least 30 min or, alternatively, more than three consultations overall). The probability of quitting was higher with high intensity support. However, there was no clear difference between high and low intensity interventions when the behavioural outcome was abstinence, as defined by not smoking during at least 6 months follow-up. Thus, in relation to abstinence, intervention intensity was not critical to effectiveness.

The second review examined the relationship between 12 major assessment or treatment strategies and outcome, defined as at least five months abstinence (Fiore *et al.*, 1998). Fifty six studies met the selection criteria for comparisons among various intensity levels of person-to-person contact. “Minimal-contact” interventions were defined as involving 3 min or less, “brief” as 3–10 min, and “counselling/psychosocial” as more than 10 min. Interventions were more effective as intensity level of person-to-person contact increased (ORs of 1.2, 1.4, and 2.4 for minimal, brief, and counselling/psychosocial, respectively). In addition, the greater the number of weeks over which person-to-person counselling or treatment is delivered, the more effective it is. Four to seven sessions appeared to be especially effective.

Fiore *et al.* did not restrict themselves to studies of NRT. They also considered the content of interventions, that is, what health professionals do with the time they spend with smokers. The reviewers found that the evidence of the effectiveness of particular, broadly defined, techniques (for example, problem solving, skills training, relapse prevention, stress management) was weak, for two main reasons. First, interventions tended to use a mixture of behaviour change techniques so that efficacy could not be attributed to any particular technique. Second, there were confounds between the behaviour change technique used and other intervention characteristics (e.g., intensity and duration of intervention). Neither review discussed theoretical explanations of psychological processes that might be involved in smoking cessation.

Similar points were made in a meta-analytic review of smoking cessation for pregnant women (Kelly *et al.*, 2001). This review categorised interventions into two groups. The first aimed to enhance threat perception by emphasising the risks of smoking during pregnancy and the potential severity of the outcomes. The second focussed upon cognitive preparation to quit, for example, by engendering positive attitudes towards quitting. Ten threat-perception interventions and 21 cognitive preparation interventions were identified and the latter were found to yield significantly greater effect sizes (2.2 *versus* 6.5% average difference in proportions quitting between intervention and control groups, respectively). However, there was a notable heterogeneity of effect sizes within the cognitive preparation interventions, probably reflecting a range of procedures and designs. The results highlight the lack of correspondence between theoretical explanations of behaviour change and the techniques used in interventions.

Kelly *et al.* (2001) observed an overall odds ratio of 1.78 for 36 smoking cessation interventions and found, unsurprisingly, that follow-up contact enhances effectiveness. Interestingly, they noted that individual counselling is not associated with success and may reduce intervention effectiveness amongst pregnant women. However, individual counselling could not be defined precisely because different interventions use the term “counselling” to refer to a variety of intervention techniques. This review emphasises the problems inherent in identifying critical intervention techniques in the absence of systematic design and evaluation of intervention techniques. Particular combinations of techniques are effective but it remains unclear how and why.

Quality, theory-driven interventions do exist in this area. For example, a self-help intervention designed to increase quitting amongst smokers was evaluated by Dijkstra and DeVries (2001). In seeking to investigate *why* such an intervention can be effective, it addressed two main questions: do specific types of information in health education self-help materials lead to specific cognitive changes, and do cognitive changes lead to behavioural changes? Both questions were conceptualised according to Bandura’s Social Cognitive Theory (Bandura, 1986, 1997). The targeted cognitions had been found in previous empirical work to predict intention to quit and smoking cessation (Dijkstra *et al.*, 1999).

Participants were randomly assigned to one of four theoretically-based information conditions, receiving either outcome information or self-efficacy enhancing information or both or neither. A detailed description of these types of information was provided. The targeted cognitions were assessed using validated measures and the distinctiveness of the constructs was verified. Four types of positive outcome were assessed (short- and long-term physical outcomes, expected social consequences and self-evaluative consequences) and two types of negative outcome (loss of positive functions and

expected withdrawal symptoms). Social incentives were also assessed, as were three types of self-efficacy (emotional, social, and habitual). The behavioural outcome was assessed by self-report and a justification for this provided.

The results showed that information designed to enhance self-efficacy led to increased self-efficacy, but not positive outcome expectations. Information about outcome led to an increase in positive outcome expectations but it also generalised, leading to increased self-efficacy. Changes in these cognitions predicted quitting activity, especially increases in self-efficacy to cope with negative emotions. Only the interventions that included self-efficacy enhancing information were more effective than no information, supporting Social Cognitive Theory. This study provides an example of good practice in that the interventions were based on theory, the main postulates of that theory were tested directly and procedures were reported with sufficient specificity to allow replication.

Government guidelines provide detailed instructions on how to change smoking behaviour but the evidence base for this is not clear. For example, in relation to intensity and delivery, it is not clear why one-hour sessions with 15–25 people are recommended. More importantly, available evidence does not allow specification of effective techniques or identification of underlying change processes. For example, the “techniques” suggested by the UK guidelines are “discussion of difficulties” and “encouragement”. These guidelines could be translated into theory-based techniques as follows:

1. Specify realistic goals, that is, ask participants to describe the target behaviour (e.g., quitting smoking today).
2. Use persuasive communication to promote positive attitudes towards smoking cessation, i.e., emphasise valued positive outcomes.
3. Enhance self-efficacy by inviting participants to identify barriers and evaluate means to overcome these.
4. Develop action plan to avoid triggering stimuli (e.g., schedule social outings to non-smoking environments), develop competing activities (e.g., join a gym) and identify rewards (e.g., spending money saved on treats).
5. Draw up a behavioural contract stating that the participant will (i) practice the behaviour, that is, stop smoking over the coming week, (ii) monitor her behaviour by keeping a diary of her performance, and (iii) report back in one week’s time.
6. One week later, ask participants to describe and analyse how they performed and review the action plan accordingly.
7. Recycle through the above steps refining the action plan and eliminating further barriers.

The current literature suggests that this combination of techniques may promote smoking cessation, particularly if used in conjunction with NRT and repeated over seven weekly sessions. However, the likelihood of success cannot be assessed with certainty (Fiore *et al.*, 1998). The mechanisms responsible for observed smoking cessation could be assessed by including measures of the cognitive antecedents they are intended to affect (e.g., attitudes and self-efficacy) and by conducting mediation analyses. Experimental work evaluating the techniques separately and in combination could establish critical techniques and combinations. Other psychological change techniques might also be employed to promote smoking cessation. For example,

TABLE I Effect sizes by category of exercise-promotion intervention Dishman and Buckworth (1996)

<i>Intervention type</i>	<i>N</i>	<i>Weighted effect (r)</i>	<i>Non-weighted effect (r)</i>
Behaviour modification	141	0.92	0.56
Cognitive-behaviour modification	35	0.10	0.25
Health education/ risk appraisal	51	0.10	0.25
Exercise prescription	59	0.21	0.23
P.E. curriculum	42	0.21	0.36
Combination	117	0.11	0.24

modelling, reinforcement, and promotion of self attributions for success. It is helpful and pragmatic to develop clear intervention guidelines, but the present guidelines appear to be evidence-inspired rather than evidence-based.

### Changing Diet and Increasing Activity

Our analysis is not dependent on consideration of condom use and smoking cessation. Similar conclusions can be drawn from reviews of evaluations of interventions to promote weight loss through diet and increased physical activity. For example, Dishman and Buckworth (1996) categorised exercise promotion interventions in terms of change procedures and found that effect sizes were at least twice as large for behaviour modification interventions, compared to other types (see Table I).<sup>5</sup> However, this taxonomy is a mixture of theoretical and atheoretical categories. For example, “behaviour modification” has a clear theoretical underpinning (classical and operant conditioning and modelling/vicarious learning) whilst “exercise prescription” and “P.E. curriculum” specify forms of delivery. Moreover, the specified techniques do not map onto the intervention categories in a one-to-one manner, with 25% of the interventions involving combinations of categories. Thus, again, intervention characterisation is confounded in terms of theoretical underpinning, techniques used, delivery, intensity, and duration.

Tang *et al.* (1998) categorised nutrition advice in terms of intensity (e.g., number of hours of counselling) without defining the techniques used (e.g., what counselling involves). Contento *et al.* (1995) categorised interventions on the basis of their content, distinguishing between “communications and educational strategies”, “behavioural change strategies”, “environmental strategies”, and “community activation and organisation”. These reviewers concluded that the most effective interventions were those that used “behavioural change strategies”. However, as with Dishman and Buckworth’s taxonomy (1996), this categorisation is a mixture of theoretically-based techniques (e.g., behavioural change strategies) and forms of delivery (e.g., community activation and organisation). Moreover, particular intervention techniques were

<sup>5</sup>Weighing by sample size can obscure important effects observed in studies of smaller groups of people that may prove to be good estimates of population effects upon further sampling. This is because of sampling bias in the relationship between effect size and sample size.



included in more than one category. For example, “reinforcement” was included in both communications and educational strategies and behavioural change strategies.

### Identification of Effective Intervention Techniques

A five-step “Intervention Mapping” procedure for detailed planning of intervention design and evaluation has been advocated by Bartholomew *et al.* (2001). This involves (1) specifying intervention objectives, (2) selecting theories relevant to the behaviours to be changed and their context, (3) designing and organising the intervention, (4) specifying adoption and implementation plans, and (5) generating an evaluation plan. This is a useful framework that emphasises the importance of theory-based design and the documentation of planning, development, and evaluation processes. However, if Stages 2 and 3 are to be evidence-based, rather than evidence-inspired, we need to develop taxonomies of theory-based behaviour-change techniques found to be effective in previous interventions.

Evaluating the effectiveness of theory-based behaviour change techniques separately and in combination with appropriate measures of predicted mediators will require a major investment in behaviour change research over decades. Yet, without this research, intervention design will remain an art rather than a science, with designers choosing from a variety of partially supported theories, using different theories to justify the same technique and developing different techniques to implement the same theory.

So what theory-based behaviour-change techniques have been found to be effective? We have noted the well-documented work on changing behaviour through manipulating environmental contingencies and enhancing self-efficacy. In both cases a variety of specified techniques are available to instigate change (e.g., Walker, 1984; Bandura, 1997). We have also considered cognitive change techniques used in research into persuasive communication, including the induction of cognitive dissonance. The attitude change literature contains recommendations on the construction of strong arguments and the creation of contrasts between current beliefs and desired states (e.g., Petty and Cacioppo, 1986; Stone *et al.*, 1994). The TTM highlights 10 change “processes”, including some derived from learning theory (e.g., reinforcement management) and the attitude change literature (e.g., self-evaluation) and also indicates some techniques that may generate these change processes (e.g., schedules of self-reward for behaviour change and clarification of values) (Prochaska *et al.*, 1992).

Since the 1960s, studies have found that behaviour does not change merely as a result of presenting fear-inducing health messages (Leventhal *et al.*, 1965, 1967; Witte and Allen, 2000). However, if such messages are combined with an action plan, i.e. a detailed plan as to when, where, and how to carry out a recommended health behaviour within everyday activities, the likelihood of the behaviour occurring increases. In two studies, one of uptake of tetanus inoculation (Leventhal *et al.*, 1965) and one of smoking cessation (Leventhal *et al.*, 1967), behaviour changed in the desired direction following the combination of a threatening health message and an action plan. More recently, Gollwitzer (1993, 1999) has demonstrated that consciously planning when and where to act enables faster and more reliable prompting of action by a specified context (see also Sheeran, 2002). Others have shown how unconscious priming can shape subsequent action (e.g., Bargh and Chartrand, 1999), although it remains unclear if unconscious priming can lead to lasting behaviour change (see e.g., Macrae and Johnston, 1998).

Different techniques are required to prompt initiation of new behaviour (e.g., cognitive rehearsal, guided imagery, skills development, modelling, self-instruction, and manipulation of environmental contingencies and action planning) *versus* breaking established habits (e.g., self-monitoring, behavioural contracts) and maintaining and generalising behaviour (e.g., reinforcement, experimenting with a new behaviour across contexts, and attributing successful behaviour change to the self). Additionally, behaviour change may sometimes depend upon emotional self-regulation which may be supported by, e.g., relaxation training, stress management, or anger management techniques.

A detailed taxonomy of behaviour change techniques, their theoretical basis and the available evidence for their effectiveness with specified behaviours and populations would be an invaluable resource for health promoters. Hardeman *et al.* (2000) listed 19 behaviour change “methods” commonly used in behaviour change programmes. These vary from the specific (e.g., “contingencies or incentives for behaviour”, “modelling by others”) to the general (e.g., “stress management”, “increasing skills: problem solving, decision making”). Hardeman *et al.* (2000) also identified four “models” of change, that is, the health education model, social learning theory, the social psychological model, and the behavioural model. These models were not mapped onto the listed change methods but it is interesting to note that they overlap with the four “fundamental intervention activities” identified by Kalichman and Hospers (1997), i.e., instruction, modelling, practice, and feedback. These are useful foundations for more detailed work on cataloguing the psychological change techniques that have been found to be effective in generating measurable behaviour change.

## CONCLUSION

Identifying interventions that promote behavioural change does not necessarily clarify which *psychological change techniques* incorporated in those interventions are responsible for generating behaviour change, nor how they generate change. Intervention descriptions are often not specific about the techniques employed. Hardeman *et al.* (2002) reported that many of the 24 interventions they reviewed did not describe the behaviour change methods used and others described them in insufficient detail to allow classification of the techniques employed. In addition, tests of theory-specified psychological changes used to explain behavioural change, that is, mediational analyses, are the exception rather than the rule in evaluations of behaviour change interventions. Well-designed RCTs can help us understand what type of intervention promotes particular behaviour change. However, a new approach to evaluating theory-based techniques, both in isolation and in combination, will be required if we are to progress towards a theory-based technology of behaviour change.

In summary, then, we draw six conclusions regarding the literature on behaviour change interventions.

1. Many intervention evaluations are inconclusive due to research design limitations, despite available guidelines. This represents a loss of valuable research resources and effort.
2. Nonetheless, there is evidence that psychological interventions are effective in changing health behaviours such as condom use, smoking, exercise, and diet.

3. It is difficult to identify particular techniques that are critical to intervention effectiveness because these are confounded with each other and with other intervention characteristics, including form of delivery, intensity, and duration.
4. Therefore, recommendations regarding intervention design tend to be based on imprecise characterisations of effective interventions rather than on trials that have isolated techniques critical to intervention effectiveness. As such, they are evidence-inspired rather than evidence-based.
5. There is a need for experimental studies of techniques, forms of delivery and target populations to identify the combinations that are critical to intervention effectiveness.
6. Intervention reports should describe the techniques they employ in enough detail that they can be exactly replicated and they should include measures of theory-based antecedents of behaviour change so that mediation analysis can be used to explore change mechanisms.

Collectively, these suggestions set a challenging agenda for health psychology research. In particular, they emphasise the need for experimental methods, clear descriptions of intervention techniques and theoretical specificity.

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