

# Time for a change: putting the Transtheoretical (Stages of Change) Model to rest

## INTRODUCTION

The Transtheoretical Model of behaviour change, known to many as the Stages of Change (SOC) model, states that with regard to chronic behaviour patterns such as smoking, individuals can be characterized as belonging to one of five or six 'stages' (Prochaska *et al.* 1985; Prochaska & Goldstein 1991; Prochaska & Velicer 1997). Stage definitions vary from behaviour to behaviour and across different versions of the model but in the case of smoking: 'precontemplation' involves an individual not thinking about stopping for at least 6 months; 'contemplation' involves an individual planning to stop between 31 days and 6 months, or less than 31 days if they have not tried to quit for 24 hours in the past year; 'preparation' involves the individual having tried to stop for 24 hours in the past year and planning to stop within 30 days (it has been accepted by the proponents of the model that having tried to stop should perhaps be dropped from this stage definition); 'action' involves the individual having stopped for between 0 and 6 months; 'maintenance' involves the individual having stopped for more than 6 months. In some versions of the model there is also a 'termination' stage in which the individual has permanently adopted the new behaviour pattern.

The model further proposes that individuals progress through stages sequentially but usually revert to prior stages before achieving maintenance and then termination (Prochaska & Velicer 1997). The model also proposes that different self-change strategies (the so-called 'processes of change') are involved in moving between different stages (Prochaska & Velicer 1997) and that the different stages are associated with different beliefs (assessment of the 'pros' and 'cons' of the behaviour and self-confidence in ability to change the behaviour). It argues that interventions to promote change should be designed so that they are appropriate to an individual's current stage (Prochaska & Goldstein 1991). Moving an individual from one stage to another is purported to be a worthwhile goal because it will increase the likelihood that this person will subsequently achieve the termination stage (Prochaska & Goldstein 1991). Proponents of the model have argued that the model has revolutionized health promotion, claiming that interventions that are tailored to the particular stage of the individual improve their effectiveness (Prochaska & Velicer 1997) (for a

readily accessible outline of the model and the assessment tools that accompany it see: <http://www.uri.edu/research/cprc/transtheoretical.htm>).

There are serious problems with the model, many of which have been well articulated (Etter & Perneger 1999; Bunton *et al.* 2000; Whitelaw *et al.* 2000; Sutton 2001; Etter & Sutton 2002; Littell & Girvin 2002). However, its popularity continues largely unabated. This editorial does not seek to revisit the plethora of empirical evidence and conceptual analysis that has been ranged against the model. It simply argues that the problems with the model are so serious that it has held back advances in the field of health promotion and, despite its intuitive appeal to many practitioners, it should be discarded. It is now time for a change. A replacement is needed that more accurately reflects observations about behaviour change, is internally consistent, and generates useful ideas and predictions. It needs to provide a way of describing how people can change with apparent suddenness, even in response to small triggers. It needs to be a stimulus to research that will go beyond a simplistic decision-making model of behaviour and produce genuinely novel insights. However, even in the absence of a new theory, simply reverting to the common sense approach that was used prior to the Transtheoretical Model would better than staying with the model. In that approach people were asked simply about desire to change and ability to change and it was recognized that these were affected by a range of personal and situational factors including addiction.

This editorial draws primarily from research in smoking. It is in this area that the model was first developed and where much of the research relating to it has been carried out. To give some idea of the extent of the dominance of smoking, of 540 articles found in PubMed using the search phrase 'stages of change', 174 also had 'smoking' in the abstract or title, 60 had 'alcohol', seven had cocaine, two had 'heroin' or 'opiate' and one had 'gambling'.

## WHAT IS WRONG WITH THE TRANSTHEORETICAL MODEL

First of all the model is flawed even in its most basic tenet, the concept of the 'stage'. It has to draw arbitrary dividing

lines in order to differentiate between the stages. This has to mean that these are not genuine stages. For example, an individual who is planning to stop smoking is in the preparation stage if this is within the next 30 days (provided that the smoker has made a quit attempt that lasted 24 hours in the past 12 months) but only the contemplation stage if it is in 31 days' time (Sutton 2001). Boundaries between so-called 'stages' are therefore simply arbitrary lines in the sand and statements of the kind 'xx per cent of smokers are in the "contemplation stage"' have little useful meaning. They should not be taken to mean, as they so often are, that 'xx per cent of smokers are thinking about stopping smoking'.

Secondly, this approach to classifying individuals assumes that individuals typically make coherent and stable plans. People responding to multiple-choice questionnaires are compliant and will generally try to choose an answer, but this does not mean that they think about things in the terms set by the response options. Apart from those individuals that set a specific occasion or date for change (e.g. in a New Year's resolution), intentions about change appear to be much less clearly formulated. In what appears to be the first study of its kind, Larabie (in press) found that more than half of reported quit attempts in a general practice sample involved no planning or preparation at all—not even going so far as to finish the current packet of cigarettes. Another recent study found considerable instability in intentions to stop smoking over short periods (Hughes *et al.* in press). A high level of instability in stages has also been found in other domains (De Nooijer *et al.* 2005).

Thirdly, it has been pointed out by others that the stage definitions represent a mixture of different types of construct that do not fit together coherently (e.g. time since quit, past quit attempts and intention) (Etter & Sutton 2002). It is not, as some of those using the model would like it to be, a statement of 'readiness' to change. Readiness or even preparedness is not actually assessed.

Fourthly, the model focuses on conscious decision-making and planning processes and draws attention away from what are known to be important underpinnings of human motivation. It neglects the role of reward and punishment, and associative learning in developing habits that are hard to break (Baumeister *et al.* 1994; Mook 1996; Salamone *et al.* 2003). Much of the problem of behaviour change arises from the fact that unhealthy habit patterns become entrenched and semi-automated through repeated reward and punishment (Robinson & Berridge 2003). These processes operate outside conscious awareness and do not follow decision-making rules such as weighing up costs and benefits. There is little or no consideration of the concept of addiction which is clearly a crucial consideration when it comes to behaviours such as smoking.

Where the model makes predictions beyond those that could be made from common sense it has been found to be incorrect or worse than competing theories (Farkas *et al.* 1996; Herzog *et al.* 1999; Abrams *et al.* 2000). Strong claims have been made for the model (Prochaska & Velicer 1997) but the main body of evidence given in support of the theory is that individuals who are closer to maintenance at any one time are more likely to have changed their behaviour when followed up (e.g. Reed *et al.* 2005). The relationship is often not strong, and by no means all studies find it (Hernandez-Avila *et al.* 1998; Littell & Girvin 2002) but the fact that it is present is given as evidence for the model. However, this says no more than that individuals who are thinking of changing their behaviour are more likely to try to do so than those who are not, or that individuals who are in the process of trying to change are more likely to change than those who are just thinking about it. Put that way, it is simply a statement of the obvious: people who want or plan to do something are obviously more likely to try to do it; and people who try to do something are more likely to succeed than those who do not.

Surprisingly, the proponents of the model appear not to report findings showing that the model is better at predicting behaviour than a simple question such as 'Do you have any plans to try to . . . ?' or even 'Do you want to . . . ?'. However, where others have made the comparison (e.g. SOC versus a simple contemplation letter that preceded it), little difference has been found (Abrams *et al.* 2000), or a simple rating of desire has been found to be better (Pisinger *et al.* 2005). There have also been problems in the reliability of the assignment to categorical stages, as one might expect given that these are designated arbitrarily (Hodgins 2001). One might imagine that a scientific model would need to show an improvement at least on this kind of simple assessment.

Proponents of the model may point to the fact that at least it has drawn attention to the fact that many people are not ready for interventions and progress can be made by moving them in the direction of changing their behaviour. However, in the years that the model has been in use there appears to be no convincing evidence that moving an individual closer to action actually results in a sustained change in behaviour at a later date. In fact, the history of behaviour change research is littered with studies that have succeeded in changing attitudes without accompanying changes in behaviour. Where interventions have been developed that are based on the model these have not proved more effective than interventions which are based on traditional concepts. A recent review comparing stop smoking interventions designed using the SOC approach with non-tailored treatments found no benefit for those based on the model (Riemsma *et al.* 2003). Another review of the effects of applying the

model to primary care behaviour change interventions has similarly found no evidence for a benefit (van Sluijs *et al.* 2004) and nor has there been found to be a benefit of applying the model in promotion of physical activity (Adams & White 2005). By contrast, there is good evidence that tailoring interventions in other ways, including triggers and motives are more effective than untailored approaches (Lancaster & Stead 2002).

### **WHY THE MODEL SHOULD BE ABANDONED**

The model has been little more than a security blanket for researchers and clinicians. First, the seemingly scientific style of the assessment tool give the impression that some form of diagnosis is being made from which a treatment plan can be devised. It gives the appearance of rigour. Secondly, the model also gives permission to go for 'soft' outcomes such as moving an individual from 'precontemplation' to 'contemplation' which is of no proven value. Thirdly, it provides scientific labels to categorise people who would otherwise have to be described using phrases that any non-expert would understand: an individual is a 'precontemplator' not 'someone who is not planning on changing'. Appealing as this may be, it is not founded on evidence and arguably has been damaging to progress.

The model tends to promote the wrong intervention strategy. For example, precontemplators tend to be provided with interventions aimed at 'moving them along' the stages, for example by attempting to persuade them about the benefits of changing. However, if their apparent lack of interest in changing arises from their addiction, these individuals may respond favourably to the offer of a new and promising treatment as appears to have happened when the drug Zyban was launched as a smoking cessation aid (e.g. Zwar & Richmond 2002).

The model is likely to lead to effective interventions not being offered to people who would have responded. There is now evidence in the case of smoking cessation that help should be offered to as wide a group as possible (Pisinger *et al.* 2005a; b), but the SOC model can be taken as giving permission to those attempting to promote behaviour change to give weak interventions or no intervention to 'precontemplators'. This approach fails to take account of the strong situational determinants of behaviour. Behaviour change can arise from a response to a trigger even in apparently unmotivated individuals.

It is common in the case of psychological theories for which there is accumulating evidence that they are not proving helpful, to argue that better measurement is needed or that the theory has not been applied properly. This particular model is no exception (e.g. DiClemente

*et al.* 2004). In the end one is often forced to acceptance that fundamental precepts of the theory are misplaced and arguably that is the case here.

### **WHAT TO DO NOW?**

A better model of behaviour change is clearly needed. There are of course many other decision-making models, such as the Health Belief Model (Garcia & Mann 2003) and the Theory of Planned Behaviour (Garcia & Mann 2003). What is needed is one that operates at the same level of generality as the SOC model and encompasses decision-making processes and motivational processes that are not necessarily accessible to conscious awareness. The model needs to take account of the fact that the behaviours concerned reflect the moment-to-moment balance of motives. At a given time an individual may 'want' to do one thing (e.g. smoke a cigarette) but feel they 'ought' to do something else (e.g. not smoke it)—but these feelings and beliefs are not present most of the time—they arise under specific circumstances. A model of change needs to describe what these circumstances are and how an individual's desires and values are shaped and changed. The model needs to consider the difference between desire and value attaching to a specific behaviour (smoking a cigarette) vs. a label (being a smoker). Lasting behaviour change relies on the balance of motivational forces regarding the specific behaviour consistently favouring the alternative whenever the opportunity to engage in it arises. The model of change needs to describe and explain how this occurs. It is apparent that self-labelling plays an important role in generating this consistency (Kearney & O'Sullivan 2003). An individual who is committed to being a 'non-smoker' is motivated to exercise restraint when temptation to smoke arises. A 'state of change' model is needed which provides a coherent account of the balance of motivational forces that operate on habitual behaviours, and how these need to change for a different pattern of behaviour to emerge. It needs to consider 'state', not as an outcome but as a measurable characteristic (possibly a self-label) that can help to stabilize a new behaviour pattern. It is worth noting, finally, that many practitioners already regard the SOC model as a state of change model in that they informally consider it to represent the state of readiness to change.

In the course of researching this editorial, I have been forced to think about the kind of comprehensive model that is required and have proposed a draft of a model (West in press). It remains to be seen how far this can form a more scientifically sound basis for analysis of behaviour change. In the meantime, when it comes to intervening to promote behaviour change, health profes-

sionals should adopt the approach that worked well in Russell *et al.*'s seminal study of GP advice (Russell *et al.* 1979) and has been found to be effective more recently as well (Pisinger *et al.* 2005), which is to encourage change in, and offer help to, all-comers (except those who are clearly resistant). They should do this respectfully but firmly and with the offer of support and assistance. When it comes to assessing motivation to change, it would be better to revert to simple questions about desire to change that were in place before the SOC model was developed.

ROBERT WEST

*Health Behaviour Unit*

*Department of Epidemiology, Brook House*

*University College London*

*2–16 Torrington Place*

*London WC1E 6BT*

*E-mail: robert.west@ucl.ac.uk*

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### WHEN POPULARITY OUTSTRIPS THE EVIDENCE: COMMENT ON WEST (2005)

Robert West (2005) has taken the bold step of asserting that the Transtheoretical Model (TTM) is so flawed that it should be discarded. Whether one agrees with West's conclusion or not, his editorial should stimulate long-overdue debate about the TTM.

A sharp divide of opinion about the TTM has surfaced in recent years. On one side the model enjoys substantial popularity in the form of a voluminous research literature and a large following among clinicians. On the other side there is discontent among many scholars (e.g. Sutton 2001) who have closely scrutinized the scientific rigor of the model. There can be no questioning the popular success of the TTM: it is established fact. But the scientific merit of the model can be questioned, and West has furthered the debate with his provocative editorial. Consistent with West, this commentary will focus on smoking as the model addiction because smoking has been the primary focus of the TTM.

The TTM became popular because it brought attention to the intuitive notion that some smokers are more ready to quit than others. For this, the originators of the TTM deserve credit, though as West and others (e.g. Bandura 1998) have pointed out, the observation that some smokers are riper for change than others is a confirmation of the obvious. As a scientific model, however, the TTM got off to an inauspicious start. The lynchpin of the TTM is, of course, the stages of change. Thus it would seem essential to take great care in formulating how the stages were to be conceptualized and measured. However, there has never been a peer-reviewed account of the developmental research that led to the creation of the stages of change algorithm. In fact, it is not clear that any systematic developmental research took place at all, and the consequences of this omission plague the model to the present time. Instead, the authors of the TTM essentially decreed that readiness to quit smoking should be measured using the stages of change. For the most part, the

addictions research community then adopted the stages of change, with few questions asked.

There would be little consequence to the omission of developmental data on the stages of change if the stages had subsequently been proved to be valid and effective. However, as West (2005) and others (e.g. Etter & Sutton 2002) point out, this is not the case. The stage of change algorithm is a magpie collection of questionnaire items that do not cohere particularly well. Two of the questionnaire items rely on arbitrary timeframes and binary yes-no response options. One need not be an expert in questionnaire development to detect potential problems with this instrument.

After a time the stages of change became something analogous to a 'brand name.' Virtually all addictions researchers became familiar with the stages of change model, and it became common to include the stages as a basic sample characteristic in studies. A pattern emerged in the TTM literature whereby success was declared on behalf of the TTM regardless of research outcomes. Exuberant interpretations of modest results became commonplace. Grandiose conclusions were extrapolated from unremarkable findings. The popularity of the TTM had come to outstrip the scientific evidence.

The popularity of the TTM came at the cost of reduced scientific and clinical progress. Alternative models of motivation to quit smoking were not pursued because the TTM had 'cornered the market' on the topic. Research on tailored interventions for smoking cessation became dominated by the TTM, which diverted resources and attention that could have been devoted to more promising methods of tailoring.

Twenty years after the introduction of the TTM, West (2005) has concluded that the TTM should be discarded, and this commentator concurs. However, the larger topic of motivation to quit should not be abandoned. Rather, researchers should renew efforts to understand and measure motivation to quit smoking. West has done precisely this by introducing his new model of behavior change. There's much to be learned from the case of the TTM.

First and foremost, researchers should insist that scientific models are judged according to the standards of scientific merit and not by popular trends or intuitive appeal.

THADDEUS A. HERZOG  
*Tobacco Research and Intervention Program*  
*H. Lee Moffitt Cancer Center and Research Institute*  
 4115 East Fowler Avenue,  
 Tampa  
 FL 33617  
 USA  
 E-mail: [herzogta@moffitt.usf.edu](mailto:herzogta@moffitt.usf.edu)

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## THEORETICAL TOOLS FOR THE INDUSTRIAL ERA IN SMOKING CESSATION COUNSELLING: A COMMENT ON WEST (2005)

With over 1400 citations (according to <http://www.scopus.com> [accessed 12 April 2005]), Prochaska's paper summarizing the transtheoretical model is one of the most widely cited papers in the psychological literature (Prochaska, DiClemente & Norcross 1992). Astonishingly, this success was achieved in spite of the early recognition of major problems affecting this model (Davidson 1992).

The concept of 'stage of change' is a haphazard mixture of current behaviour, intention to change, past quit attempts and duration of abstinence. As West (2005) points out, stages are defined by setting arbitrary cut-points on continuous variables (time and intention) and, contrary to what is often believed, this theory says nothing about the time people spend in the first three stages. Furthermore, there is little empirical evidence of progression through the entire stage sequence (Littell & Girvin 2002). It is therefore hard to believe that these stages reflect reality. It makes little sense to classify in the same category (e.g. precontemplation) people with different levels of dependence, or people who have never tried to quit with those who achieved long periods of abstinence. Similarly, using only abstinence criteria to define Action

is too reductive, and neglects important steps that people take on their way towards abstinence (e.g. cutting down, non-daily smoking). Finally, the model does not take into account dependence level, withdrawal symptoms and other key determinants of smoking, in particular environmental and social factors.

The core of the transtheoretical model is a description of associations between variables, in particular between stages and self-change strategies that are supposed to be used in a sequential pattern, 'doing the right thing at the right time' (Segan *et al.* 2004). However, it has never been convincingly shown that distinct strategies are needed to progress across distinct stages (Herzog *et al.* 1999; Segan *et al.* 2004). In fact, stage mismatched interventions, where all smokers, including precontemplators, receive action-oriented advice may be as effective (Dijkstra *et al.* 1998) or even more effective than stage matched interventions (Quinlan & McCaul 2000), which seriously questions the basic tenets of this model.

This model has been widely used to guide interventions and determine who gets what treatment. However, interventions based on this model have not been consistently proven to be more effective than control interventions or than no intervention (Riemsma *et al.* 2003; van Sluijs *et al.* 2004). Worse, labelling people 'precontemplators' (an awkward jargon) is stigmatizing and may lead clinicians to deprive patients of effective treatments. The risk of excluding precontemplators from effective treatments is a major liability of this model.

Valid measurement is the foundation of good science, but measurement of the model's constructs is problematic. There are many, incompatible ways of measuring stages of change, and questionnaires measuring the model's other core constructs (processes of change, self-efficacy and 'pros and cons') were published 15–20 years ago and have not been revised since. A constant development and adjustment of scales is nevertheless required to achieve the best possible measurement in each population subgroup, and theory should be subsequently developed according to empirical findings. But the transtheoretical model is far too rigid, it has not evolved much in the past two decades, and proponents of the model have been reluctant to take into account external criticism and to develop their theory accordingly. Rather, one has the impression that the model is often used rigidly, almost religiously.

There is however, a need for an integrative, comprehensive theory on which interventions can be based. Such a theory is needed in particular for computer-tailored programs, which can reach huge numbers of smokers over the internet. For many smokers, the internet is the only source of information and counselling. One-to-one counselling is not cost effective on the internet, but computer-tailored programs can provide effec-

tive, individually tailored advice to large audiences (Etter 2005). In fact, the transition from one-to-one counselling in clinical settings and telephone helplines to mass level, individually tailored counselling on the internet is comparable to the industrial revolution, when craftsmen working in small shops were replaced by huge plants managed by engineers. To successfully handle this transition, the field needs a theory that tells us what to measure, how to measure it, what type of advice should be given to each category of smokers, when to give each recommendation, and what outcome should be expected in each subgroup. Interventions should be explicitly derivable from this new theory, in contrast with the loose and questionable links between the transtheoretical model and interventions. Researchers should join in a collaborative effort to develop a theory that reflects reality better than the transtheoretical model, and to assess whether interventions based on this theory are more effective than existing interventions.

JEAN-FRANÇOIS ETTER

IMSP-CMU

1 rue Michel-Servet

Geneva 4

CH1211

Switzerland

E-mail: Jean-Francois.Etter@imsp.unige.ch

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## WEIGHING THE PROS AND CONS OF CHANGING CHANGE MODELS: A COMMENT ON WEST (2005)

West (2005) challenges researchers and clinicians to abandon the transtheoretical model (TTM) and to revert to common sense ideas about motivation for change or to develop new models that better account for the complexities of the change process. Part of West's argument is based upon the continuing popularity of the model despite the healthy debate about its theoretical and empirical shortcomings that has occurred in this journal and others. As Whitelaw *et al.* (2000) point out, the need for critique is the greatest at the point that an idea becomes 'accepted' and the TTM has gained this accepted status. An example of this acceptance is the influence of the model on service delivery. There is little empirical evidence that TTM stage-based interventions lead to superior outcomes over non-stage based interventions (Riemsma *et al.* 2003; van Sluijs, van Poppel & van Mechelen 2004; Adams & White 2005). Nonetheless, and despite our pledge toward evidence-based practise, many jurisdictions are developing and implementing stage based interventions in a variety of problem areas despite the lack of a strong evidence-base. Such interventions have the false appearance of being evidence-based because they are based upon scientific models although, in reality, the appeal of TTM for this purpose appears intuitive.

A related unfortunate effect of the wide acceptance of the model's validity is the potential that decisions about who gets what type of service are made on stage of change assessments (Piper & Brown 1998). People assessed as precontemplators might be excluded from service or offered less action-oriented interventions than those deemed to be more ready to change. The evidence, at least in the area of smoking cessation, is inconsistent with this stance. In a recent study, smokers, regardless of their stage of change, were offered a smoking cessation group (Pisinger *et al.* 2005). Only 16% of those who were ultimately successful had serious intention to quit prior to the intervention. The authors argue that cessation support needs to be offered to all smokers regardless of their stage of change. Clearly, intentions or readiness are highly fluid (Hughes *et al.* 2005).

For these ethical reasons, I agree with one aspect of West's argument. A moratorium on the uncritical use of the TTM model in clinical service delivery is warranted. However, I disagree that the flaws of the model are so large that it should be entirely abandoned. The purpose of model building is to stimulate testable hypotheses that will lead to enhancements of our understanding of complex phenomena. The sheer bulk of the research conducted on the TTM is evidence that it provides such a focus. The model can continue to direct interesting research queries that presumably will lead to model refinement. For example, one facet of the model that has empirical support is the predictive validity of the stages—people who are further along the continuum are more likely to have changed their behaviour at a future follow-up point than those who are at an earlier stage. It is reasonable to hypothesize that therapeutic efforts that result in forward stage movement will result in a greater likelihood of future change than interventions that do not result in forward movement. This feature of the TTM accounts for part of its appeal to clinicians working with addictive behaviours—if I can't get action from my clients then at least I can improve my clients' readiness for change... Alas, this hypothesis has not been empirically tested. We don't know whether these 'soft outcomes', as West describes them, are ultimately helpful for our clients (perhaps clients shift back to their initial stage post intervention) or whether these outcomes simply help the therapist cope with the limited success we have getting actual behaviour change from our clients. Surely researchers need to attend to this gap in our understanding before the TTM is abandoned.

Another interesting but understudied clinical development has been the use of the model as therapeutic tool with clients. Littell & Girvin (2002) note that the model is based upon a rational actor assumption of behaviour change, the notion that change is based upon a rational cognitive self-examination by the individual. This feature is undoubtedly another part of the model's appeal to clinicians. Many of us have started to provide our clients with a description of the stages of change portion of the model with the request that clients determine where they fit. Perhaps this process of education and self-staging provides a helpful change schema for our clients that helps organize their ambivalent thoughts about and actions toward change. West argues that self-labeling is an important aspect of maintaining behavioural change. Perhaps, a cognitive understanding of change as a process is important in initiating the change.

In short, the model continues to be an important stimulus to theory and practise development and it will ultimately be usurped by reformulated models. I look forward to learning more about West's alternative model (West, in

press), a model that he admits has been inspired by the current body of research and critique of the TTM.

DAVID C. HODGINS

*Department of Psychology  
Program in Clinical Psychology  
University of Calgary  
2500 University Drive NW  
Calgary  
Alberta  
Canada T2N 1N4  
E-mail: dhodgins@ucalgary.ca*

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## ANOTHER NAIL IN THE COFFIN OF THE TRANSTHEORETICAL MODEL? A COMMENT ON WEST (2005)

In a series of publications (Sutton 1996; 2000a, 2000b, 2001, 2005), I have critically examined the transtheoretical model (TTM) and its associated assessment instruments and evaluated the evidence for the model. I came to the conclusion that the TTM cannot be recommended in its present form and that we need to go 'back to the drawing board' (Sutton 2001). Thus, I reached a similar

conclusion to West (2005) but by a somewhat different route.

Although I endorse West's conclusion, I disagree to some extent with his analysis of what the Rhode Island group calls stage effects (Prochaska *et al.* 2004) and with what he says about intervention studies.

## STAGE EFFECTS

A stage effect is observed when initial pre-action stage of change predicts being in action or maintenance at follow-up: those in the preparation stage at baseline are more likely to be in action or maintenance at follow-up than those in contemplation, and those in contemplation at baseline are more likely to be in action or maintenance at follow-up than those in precontemplation. We need a meta-analysis to quantify these effects, but my reading of the literature on the TTM is that stage effects are a highly consistent finding. Of course, this is not surprising, because the pre-action stages are defined in terms of intentions and past behaviour, and there is ample evidence that these predict future behaviour. Stage effects mean that stage measures may be of practical value, for example in measuring progress towards smoking cessation. This may or may not be 'common sense', but it is not trivial. However, as West points out, other measures may do better.

It is important to appreciate that stage effects do not necessarily provide strong evidence for a stage model because 'pseudostage' models may yield similar effects. For example, continuous measures of intention predict future behaviour and if such an intention measure were categorized into, say, three categories, one would expect to find a (pseudo)stage effect. Stage models make specific predictions about the probabilities of different stage transitions that can be tested using longitudinal data (Weinstein *et al.* 1998; Sutton 2000a).

## INTERVENTIONS

West states that 'Where interventions have been developed that are based on the model these have not proved more effective than interventions which are based on traditional concepts'. Unfortunately, the systematic reviews of stage-based interventions that have been published to date (e.g. Riemsma *et al.* 2003; van Sluijs *et al.* 2004) have included studies that were not proper applications of the TTM. For an intervention to be labelled as TTM-based, it should (1) stratify participants by stage and (2) target the model's 'independent variables' (pros and cons, confidence and temptation, and processes of change), focusing on different variables at different stages. There is a

need for more focused reviews of TTM-based intervention studies.

The interventions that come closest to a strict application of the TTM are those developed by the Rhode Island group. The group's studies of smoking cessation interventions (e.g. Prochaska *et al.* 1993, 2001a, 2001b; Palonen *et al.* 1998)—none of which were cited in West's (2005) editorial—have yielded mainly positive findings. However, adaptations of these interventions evaluated by research groups in the UK and Australia have yielded mainly negative results (Aveyard *et al.* 1999, 2001, 2003; Borland *et al.* 2003; Lawrence *et al.* 2003).

Process analyses showing that TTM-based interventions do indeed influence the variables they target in particular stages and that forward stage movement can be explained by these variables have not been published to date. There have also been few experimental studies of matched and mismatched interventions, which could potentially provide the strongest evidence for or against the model (Weinstein *et al.* 1998; Sutton 2005).

West states that 'the model is likely to lead to effective interventions not being offered to people who would have responded'. This consequence would not be in the spirit of the model. The TTM implies that everyone, regardless of which stage they are in, should receive the appropriate stage-matched intervention designed to move them to the next stage; this includes precontemplators. The issue of whether health professionals should deliver interventions to 'all-comers' or to subgroups selected on the basis of higher risk, greater motivation or some other criterion is a complex one that deserves much more detailed consideration than could be given to it in the editorial.

## ALTERNATIVE MODELS

Discarding the TTM does not necessarily mean abandoning the idea that behaviour change, including smoking cessation, involves movement through a sequence of discrete stages. Two promising alternatives to the TTM are the precaution adoption process model (Weinstein & Sandman 2002; Sutton 2005) and the perspectives on change model of smoking cessation (Borland, Balmford & Hunt 2004). In contrast to the TTM, both these theories are based on a thoughtful analysis of the process of behaviour change, but neither has been tested extensively.

Among existing non-stage models, the theory of planned behaviour (Ajzen 1991, 2002) has several attractive features: (1) it is a general theory; (2) it is clearly specified; (3) there exist clear recommendations for how the constructs should be operationalized; (4) it has been widely used to study health behaviours as well

as other kinds of behaviours; and (5) meta-analyses of observational studies show that it accounts for useful amounts of variance in intentions and behaviour (Sutton 2004). The theory is able to capture the moment-to-moment balance of motives mentioned by West and to take account of situational influences (because different sets of beliefs may be salient in different situations). It is also consistent with the idea that the processes involved in the formation and modification of beliefs, attitudes and intentions may be largely automatic (Ajzen & Fishbein 2000). However, there have been few experimental tests of the theory (Sutton 2002) and few intervention studies (Hardeman *et al.* 2002), and it has not been widely applied to smoking cessation.

### Concluding comment

The TTM has proven remarkably resilient to criticism. The Rhode Island group has not so far responded to my own critiques of the model or to those by Carey *et al.* (1999), Joseph *et al.* (1999), Littell & Girvin (2002) and Rosen (2000) among others. The model is still accepted uncritically by many in the health promotion field. I hope that West's (2005) editorial does finally put the model to rest, but I am not optimistic.

STEPHEN SUTTON  
 University of Cambridge  
 Institute of Public Health  
 Forvie Site  
 Robinson Way  
 Cambridge CB2 2SR  
 E-mail: srs34@medschl.cam.ac.uk

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### **A PREMATURE OBITUARY FOR THE TRANSTHEORETICAL MODEL: A RESPONSE TO WEST (2005)**

The editorial by Robert West eulogizing the Transtheoretical Model (TTM) offers a provocative perspective (West 2005). However, it is not clear why Dr West feels the need to bury something that still has life or why he cannot create a new model from his insights that would make the old one obsolete, dying a natural death rather than what I consider a premature interment. Although his critique offers some valid concerns, essentially he repeats ongoing criticisms that have been addressed previously in this journal and in more recent publications (Prochaska & DiClemente 1998; Connors *et al.* 2001; DiClemente & Velasquez 2002; DiClemente 2003; DiClemente, Schlundt & Gemell 2004) and continues to overreact to exaggerated claims that have been made about the utility and scope of the model. I would agree that some claims have been exaggerated and that there are challenging data and anomalies that need to be examined, explored and explained. However, the basic premise of the editorial is flawed. A balanced assessment would be more useful

for advancing our understanding of the human change process and for exploring both the stage and state aspects of this process.

Dr West's critique is really a criticism of the stages of change and not the entire model. As is true of many previous critiques, he focuses on assessment of the stages of change and issues about time frames and labels. Although they are closely related, it is important not to confuse construct with assessment and confound operationalizing a construct with the phenomenon that the construct is supposed to help explain.

The dimensions of the Transtheoretical Model offer a framework that makes explicit elements of a human intentional behavior change process and answers the question: what does it take for individuals to accomplish successfully sustained behavior change? In contrast to a prior view of change as an on/off phenomenon (unmotivated or motivated; action or inaction), the original insight underlying stages of change was that there appear to be a series of identifiable and separable tasks involved in changing a specific behavior. Stages were a way to segment the process into meaningful steps related to critical tasks, namely concern about the problem and consideration of the possibility of change, risk reward analysis and decision making, planning and prioritization, taking action and revising action plans, and integration of the behavior change into the person's life-style.

The terms 'precontemplation', 'contemplation', 'preparation', 'action' and 'maintenance' were an attempt to identify specific steps or stages in the process and isolate subsets of people who had similar tasks to accomplish as they move forward in the process of change. Stages have always been considered states and not traits so they are quite unstable and individuals can move between them quickly, engaging and abandoning some of these tasks even within a single session of intervention. The exception seems to be the action to maintenance shift, which appears to need the passage of time for the task of consolidation of change. Individuals can also become stuck in a task, such as considering change for long periods of time before taking action. The labels and attempts at making stages operational for assessment were thoughtful but arbitrary ways of labeling these sets of tasks and subgroups of people. Early work with the model followed large numbers of smokers for 2 and 3 years with and without interventions tracking their progress or lack of progress through the process of change. This extensive research supported stage differences and the importance of processes of change in the transitions from one stage to the next (DiClemente & Prochaska 1998).

Making a concept operational so that one can assess the phenomenon is always arbitrary, and simply an attempt to create a dividing line that could be useful in isolating a concept or construct. This is true for all our

psychological concepts such as stress, depression, anxiety, addiction, etc. Constructs always differ from the phenomenon and operationalizing and assessing are always challenging. Although few would deny the existence of anxiety or depression, there are many different ways of operationalizing these constructs, assessing individuals who may or may not have these conditions, and understanding the phenomenon. Problems of operationalization make it more difficult to study the phenomenon but should not be confused with the value of the concept or construct. The objective of the TTM and the research examining the model has been to enhance our understanding of the process of change and our ability to intervene in this process and not to identify a rigidly defined set of stages and prove the existence of those stages.

The key questions are whether these tasks are definable and separable to some degree, whether it is helpful to separate these tasks in order to better conceptualize and manage change, and whether we can identify and assist individuals or groups who seem to be engaged in these similar tasks. These separate tasks are not unique to the TTM and have been identified in many current theories of health behavior change. Both the health belief model and Bandura's social cognitive theory describe decisional considerations and self-evaluations that precede taking action (Bandura 1986). The theory of planned behavior identifies implementation planning as an important dimension of change that precedes action. Early action appears to be different from successfully sustaining change, as is discussed in Marlatt's relapse prevention model (Marlatt & Gordon 1985). What the stages do is to organize these tasks into a logical sequence of activities that seem to build upon one another. Individuals uninterested in change or unconcerned about a current behavior should differ from those convinced of the need to change and preparing for action both in their view of change and what they are doing to create change. However, simply because specific tasks can be identified as distinct does not mean that they are discontinuous and dichotomous. These tasks are part of a larger process of change and build on one another. Critical stage tasks need to be completed in a 'good enough' fashion to allow the individual to move forward but in reality stage tasks are not completely accomplished until successfully completed change is achieved. It seems obvious that someone can move into action without having completed the proper decision-making, planning or prioritization needed to make the change successful. Stages are not boxes from which individuals jump, one to the next, in order to make change, but represent tasks that can be accomplished to a greater or lesser degree. In fact, relapse seems to be related to the quality of the accomplishment of the stage of change tasks and not simply whether one

takes action. Recycling through the stages and the multiple attempts that individuals make in order to successfully recover from addiction seem to support the role of successive approximation in completing the decision making, the commitment, the preparation, the plan and the implementation in such a manner that can support successfully sustained change.

Dr West contends that this view obstructs the view of the role of 'moment to moment balance of desire versus value' and the role of circumstances. I would argue the contrary, that the stages offer a way of viewing and studying how the momentary and the circumstantial interact with the larger process of change. There are implicit and explicit cognitions and a host of normative comparisons and self-evaluations that are operative in the process of change. Motivations are often momentary. Change attempts can be very spontaneous looking and responsive to specific events. I remember my days as a smoker when I would wake up and say to myself that this is it and throw away the cigarettes, only to search for them later that morning and abandon my attempt. Certainly there are momentary influences and actions, but they seem part of a larger process of change. Not until I was able to be convinced and convicted about smoking cessation, created a plan that could work for me and was able to stick with that plan did I successfully quit smoking. Momentary events are not contrary to a process perspective, but complement it.

There is ample evidence of significant differences among subgroups of individuals classified into different stages that do not simply mirror 'common sense' differences between people actively changing and those who are not as was indicated by Dr West. Across multiple behaviors (smoking, dietary behaviors, physical activity, alcohol consumption and drug abuse) there are interesting and consistent differences among subgroups on meaningful process of change dimensions. In longitudinal studies there have been consistent findings that individuals in earlier stages have less success in sustaining behavior change. Dr West ignores these data.

The model has also assisted in exploring interesting phenomena, has contributed to changing how treatment professionals approach individuals referred to treatment and challenged the field to think in a more differentiated and complex manner about health and addictive behavior change (Stotts *et al.* 1996; Carbonari & DiClemente 2000; DiClemente *et al.* 2003). The claim that the model is hindering advances and exploration seems clearly erroneous. Practitioners have made interesting and creative changes in the way they offer services and approach clients. Individuals who are in the process of change have told us repeatedly that the model seems to reflect their experiences of changing a health behavior. There is increased emphasis on early interventions and how inter-

ventions can influence even people who seem to lack motivation. Although often problematic in how stages are assessed and simplistic in approach, research studies into the process of change have grown exponentially. In a recent presentation at the meeting of the Association for the Advancement of Behavior Therapy, colleagues and I examined measures of stage and process of change in a dually diagnosed sample of seriously mentally ill with cocaine dependence to see if measures looked the same and were influenced by neurocognitive status. This led to an interesting discussion of whether seriously mentally ill people accessed an intentional process of change or were more influenced by current considerations and social influences. Dr West's contention that the model must be jettisoned before alternative views can be explored is simply not true, or is true only for those treating the model as a religion and not a heuristic model to explore the change process.

Interment of such a provocative and heuristic model seems premature and unnecessary. It would be a mistake to return completely to a state model resembling the on/off views of the past. Readiness to change is not a single construct but a compilation of tasks and accomplishments that can produce both momentary change and sustained change. Pitting state against stage does a disservice to the process of intentional behavior change. In fact, this process incorporates and can elucidate many of the issues that Dr West identifies in his closing paragraph. There is clearly much more to understand about the process of change and how individuals go about creating and stabilizing a new behavior or abandoning one that is well established. The process involves biological, psychological and social/environmental determinants that are momentary and more stable. However, the process of change appears to be a very productive way to try to integrate these dimensions (DiClemente 2003). Hopefully, a dialogue on how the model does or does not fit the process of change and how various new discoveries challenge the model or make it obsolete can promote a more complete and scientific understanding of human intentional behavior change.

CARLO C. DICLEMENTE

*Department of Psychology  
University of Maryland  
Baltimore County  
MD  
USA*

*E-mail: diclemen@umbc.edu*

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## WHAT DOES IT TAKE FOR A THEORY TO BE ABANDONED? THE TRANSTHEORETICAL MODEL OF BEHAVIOUR CHANGE AS A TEST CASE

I am grateful to my fellow researchers for their thoughtful comments on my editorial arguing for abandonment of the Transtheoretical Model of Behaviour Change (West 2005).

Herzog (2005) notes that the popularity of the model does not seem to derive from a close analysis of its scientific merits. Etter's (2005) commentary elaborates on some of the major conceptual problems with the TTM. Hodgins (2005) notes limitations in the evidence base in support of the model and argues for a moratorium on uncritical use of the model. However, he argues that the model has been and could continue to be a useful stimulus to research. I have regretfully to demur for the reasons given in the editorial.

Sutton (2005) has argued for many years that the model cannot be recommended and my editorial does lit-

tle more than reinforce that view. However, he takes issue with me on two counts. First, he argues that the finding of 'stage transitions' which represents the main evidence in favour of the model, though common sense, is not trivial. Yet surely showing that individuals who at time A are thinking about doing something, or indeed trying to do something, are somewhat more likely to have done it at time B is not a major contribution to understanding of motivation.

Secondly, Sutton argues that the failure to show that TTM-based interventions are more effective than more traditional interventions might have resulted from not having used the model appropriately. This kind of argument is deployed a great deal when empirical studies fail to support theories in psychology. It cannot be ruled out as an explanation in this case. The problem is that it can never be ruled out. How far should we go in allowing this kind of 'get out clause'? There is no simple answer, but if a model has conceptual flaws and counterexamples ranged against it and if, after many years of research, independent reviews conclude that it has not led to development of more effective interventions, perhaps that is enough.

DiClemente (2005) acknowledges that there are valid concerns with the model and rightly points out that my editorial says little of substance that is new. However, he believes that the basic premise of the editorial is flawed. He argues that stages have always been considered states and not traits and they are quite unstable, even changing within a session. But he also argues that the model aimed to segment people into those that could benefit from different types of intervention. These two statements seem at odds because there would not be much purpose in segmenting people on the basis of a state that is in any event unstable.

He also argues that my editorial confused the operationalisation of stages with the underlying concept. In behavioural research operationalisations are needed when we cannot measure something directly; the question here is what exactly is it that we are trying to measure and whether it has any reality beyond the method of measurement. It is obvious that some smokers who, when they think about it, have some kind of intention to stop at some point in the not too distant future while others do not. It is also obvious that some smokers (we do not know how many) will make more definite plans to stop on a particular date which they may or may not put into effect. If one wishes to go beyond this and posit 'stages' that represent some more abstract and stable entity, the operationalisation must specifically address the core definition of a stage which involves (1) stability and (2) discontinuity. The use of arbitrary dividing lines does not do this. Using arbitrary dividing lines to put people's motivational condition into artificial categories encour-

ages people to make statements such as 'xx% of smokers are in the contemplation stage' as though the figure referred to some real quality attaching to those individuals when it does not.

DiClemente argues that the stage approach provides a framework into which momentary influences and states can be understood and harnessed. He cites his own experience of stopping smoking in which the formulation of a plan to which he was committed was a precursor for lasting change. I doubt whether anyone would dispute a statement that making a plan to which one is committed *can* (but does not always) help to combat momentary wishes and urges. Put in those terms, it seems, again to be a statement of the obvious. Put in more rigid terms—that making a plan to which one is committed is *needed* for lasting change—it is contradicted by counterexamples.

DiClemente says that I have ignored data showing that people in earlier stages are less likely to sustain behaviour change. But I did not ignore this data; I and other commentators have merely said that this kind of test is too weak and that where stage assessment has been compared with a simple rating of desire or an addiction model, it has been less successful. The onus is very much on the proponents of the TTM after many years and hundreds of research papers to point to evidence that their approach predicts actual behaviour better than simple alternatives.

It seems like a very negative thing to call for the abandonment of something into which so much has been invested. However, I think there is an onus of those of working in the field to be rigorous and objective in our evaluation of theories and models. In the end it is self-defeating to persist with ideas that are misconceived. The problem of theories that are developed and pursued without adequate regard to counter-examples and to whether they provide better prediction of behaviour than existing accounts is endemic in behavioural research. Perhaps the time has come for a root and branch review of our methods of theory development, testing and application.

Here is a simple common sense account of health-related behaviour. It contains no insights derived from formal study but is merely an articulation of what any intelligent lay person might propose:

'Engaging in or failing to engage in health-protective behaviour patterns depends on opportunities afforded by the one's social and physical environment and the balance of motivations when those opportunities are present. These motivations include desires, urges, needs, habits, evaluations and level of commitment to any prior resolutions. The specific problem for health-promoting behaviours is that they are often less pleasant than the alternatives on offer at the time. Inter-

ventions to reduce chronic unhealthy behaviour patterns involve reducing opportunities for unhealthy behaviours, increasing opportunities for healthy behaviours, reducing the actual or perceived attractiveness of or need for the unhealthy option, increasing the actual or perceived attractiveness of the healthy option, and prompts to make and keep to resolutions to make lasting changes.

Addiction presents special problems because the addictive behaviour (usually a drug) causes changes to the addict's CNS and/or social and physical environment which undermine desires to remain abstinent and/or heighten desire to continue with the activity. This means that more powerful and sustained interventions to bolster motivation to abstain and to reduce motivation to engage in the addictive behaviour are needed.'

Any supposedly scientific model of behaviour change should be able to do demonstrably better at prediction and development of interventions than an operationalisation of this model than this. It should not be hard.

ROBERT WEST  
 Health Behaviour Unit  
 Department of Epidemiology, Brook House  
 University College London  
 2-16 Torrington Place  
 London WC1E 6BT  
 E-mail: robert.west@ucl.ac.uk

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