How useful are the stages of change for targeting interventions? randomized test of a brief intervention to reduce smoking

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Randomized Test of a Brief Intervention to Reduce Smoking

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We would like to thank Kondylia Koulouri for her help with collecting and entering the data.

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Abstract

Objectives. To see whether the stages of change are useful for targeting a brief intervention to reduce smoking based on implementation intentions. A second objective was to rule out demand characteristics as an alternative explanation for the findings of intervention studies based on the transtheoretical model and implementation intentions. *Design*. Participants (N = 350) were randomized to a passive control condition (questionnaire only), active control condition (questionnaire-plus-instruction to plan to quit) or experimental condition (questionnaire, plan to quit, form an implementation intention). Their behavior and psychosocial orientation to quit were measured at baseline and 2-month follow-up. *Main Outcome Measures*. Theory of planned behavior variables, nicotine dependence and quitting. *Results*. Significantly more people quit smoking in the experimental condition than in the control conditions, and the planning instructions changed intention to quit and perceived control over quitting, but not behavior. Stage of change moderated these effects such that implementation intentions worked best for individuals who were in the preparation stage at baseline. *Conclusion*. Harnessing both motivational and volitional processes seems to enhance the effectiveness of smoking cessation programs, although further work is required to clarify inconsistencies in the literature using the stages of change.

Key Words: IMPLEMENTATION INTENTIONS; MOTIVATION; VOLITION; STAGES OF CHANGE; SMOKING; THEORY OF PLANNED BEHAVIOR

How Useful are the Stages of Change for Targeting Interventions?

Randomized Test of a Brief Intervention to Reduce Smoking

Smoking is regarded as the most important source of preventable morbidity and mortality. In the United Kingdom alone, it is estimated that 106,000 people died from smoking-related causes in each year between 1998 and 2002 (Twigg, Moon, & Walker, 2004). The serious consequences associated with smoking has led to concerted efforts to encourage people to quit, and the prevalence of smoking declined from 45% in 1974 to 26% in 2003 (UK National Statistics, 2006). However, despite this significant decrease in the prevalence of smoking, the rate of decline since the early 1990's is generally regarded as being too slow to meet the prevalence target set in the UK Department of Health's Public Service Agreement, to have 21% or fewer adult smokers by 2010 (UK Department of Health, 2004). The implication is that further research into the factors that might enhance smoking cessation programs in the population at large is required. An important means of achieving this goal is through efficient targeting of broad cross-sections of the population with theory-based interventions.

The transtheoretical model (Prochaska & DiClemente, 1983) has been used extensively to try and encourage smoking cessation and is arguably the dominant psychological model in this field. The transtheoretical model consists of at least 14 individual components, including the stages of change (precontemplation, contemplation, preparation, action and maintenance), decisional balance, self-efficacy/temptation, and ten processes of change. As a tool for promoting smoking cessation, the transtheoretical model has typically been used to develop individually tailored communications (see Velicer, Prochaska, & Redding, 2006). On the basis of participants' responses to questionnaires, up to 20,000 unique reports tailored by each part of the transtheoretical model can be delivered and have thus far produced smoking abstinence rates of 22-26% (Velicer et al., 2006). However, transtheoretical model-based tailored

communications have only been tested against relatively passive control conditions (e.g., no treatment, self-help manual), which is potentially problematic because it is: (a) unclear which parts of the interventions are causing the significant effects; and (b) plausible that receiving additional materials or increasing the attention paid to participants could partially account for the positive findings. There are also several practical constraints associated with the transtheoretical model-based tailored communications; for example, participants must be screened prior to receiving the interventions and the materials comprise 3-4 page booklets (Velicer et al., 2006), which require commitment on behalf of the participant to read and engage in them.

One theory-based health behavior change technique that circumvents these practical difficulties is Gollwitzer's (1999) concept of implementation intention that was developed in the experimental social psychology literature and has been shown to be effective in changing health behavior (Gollwitzer & Sheeran, 2006). Work in the laboratory shows that implementation intentions work by making critical situations for action salient and by automatizing a linked behavioral response (Gollwitzer & Sheeran, 2006). In contrast with techniques found in cognitive behavioral therapy such as goal-setting (which generally focus on helping a client achieve a goal using self-reward), implementation intentions are often self-directed and are focused on identifying responses that will lead to goal attainment and on anticipating suitable occasions to act. Thus, participants are asked to specify cues in the environment that will guide their future behavior (e.g., being tempted to smoke), and link them with behaviors they will enact when they encounter those cues (e.g., engaging in physical activity). The idea is that specifying where and when one will act ensures that the behavior will be triggered at the appropriate time in the future without affecting people's motivation (Gollwitzer & Sheeran, 2006). Across 94 independent studies in both laboratory and field settings, Gollwitzer and Sheeran's (2006) meta-analysis showed that implementation intention had an average effect size of d = .65. Although

none of the 94 studies reviewed by Gollwitzer and Sheeran (2006) investigated smoking cessation, recent studies suggest that implementation intentions are effective in encouraging quitting. For example, Armitage (2007) showed that an implementation intention intervention caused 11.63% (5/43) of smokers to quit as opposed to 2% (1/47) who quit in the control group ($p < .05$, see also Armitage, in press).

However, implementation intention intervention studies are not without their limitations. First, consistent with the transtheoretical model-based tailoring studies, most implementation intention interventions have been tested against a passive control condition: Participants in the experimental group are typically asked to change their behavior and to form an implementation intention whereas participants in the control group are typically asked to do neither (e.g., Armitage, 2007; for exceptions see Armitage, in press; Murgraff, White, & Phillips, 1996). This is important because the use of passive control groups may have exaggerated the effectiveness of implementation intention-based interventions and it would be valuable to tease apart the effects of asking participants to change their behavior from forming an implementation intention.

Second, a debate has arisen surrounding two studies by Jackson and colleagues (Jackson et al., 2005; Jackson et al., 2006), which failed to replicate the effects of implementation intentions in relation to fruit and vegetable consumption and adherence to antibiotics in clinical populations. In explaining their findings, Jackson et al. (2005, 2006) argue that students may be particularly susceptible to implementation intention interventions because they are more compliant with task demands. Consistent with Jackson et al.'s (2005, 2006) interpretation of their data, Gollwitzer and Sheeran (2006) showed that the effects of implementation intentions were greater in student samples $d = .65$ ($N = 6{,}855$) than in the general public ($d = .58$, $N = 1{,}076$). The implication is that further research into implementation intention-based interventions needs to be conducted on general population – as opposed to student – samples.

Third, there is evidence to suggest that the effectiveness of implementation intention interventions might be enhanced if they were targeted. Of particular relevance to the present research, a study by Armitage (2006) tested whether social cognitive variables and an implementation intention intervention could explain movements between Prochaska and DiClemente's (1983) stages of change in relation to eating a low-fat diet. Consistent with Gollwitzer's (1999) theory of action phases, Armitage's (2006) findings showed that proportionately more people progressed from the preparation (preparing oneself and one's social world for a change in behavior) stage than the precontemplation (unaware or under-aware of the risks associated a health behavior) or contemplation (considering changing one's behavior in the next month) stages. The implication is that targeting implementation intentions at smokers in the preparation stage might enhance the effects of the implementation intention interventions. However, the results of a recent cross-sectional survey have questioned the validity of the staging algorithm for smoking. Herzog and Blagg (2007) tested the stage of change algorithm against several measures of motivation to quit and concluded that the stage of change algorithm underestimated motivation to quit smoking. Although Herzog and Blagg's (2007) study raises important questions about the validity of the stage of change measure, it is worth noting a number of potentially important differences between Herzog and Blagg's (2007) study and the present research.

First, common among most of the supporting studies they cite, Herzog and Blagg (2007) tested the stage of change algorithm against single item measures of motivation (see also Etter & Sutton, 2002; Kraft, Sutton, & Reynolds, 1999; Sciamanna, Hoch, Duke, Fogle, & Ford, 2000), raising questions about the reliability of the motivational measures used to question the validity of the staging algorithm. Second, it is plausible that Herzog and Blagg's (2007) participants may have been more motivated than smokers in the population at large because they recruited

smokers through “newspaper advertisements and flyers distributed at community events” (p. 224) and paid participants $25 for completing the questionnaire as opposed to proactively recruiting participants from the community (cf. Velicer et al., 2006). In fact, several studies cited by Herzog and Blagg (2007) include smokers who may have been more motivated to quit than the average: Etter and Sutton (2002) collected their data over the internet using a sample that consisted mainly of people who had previously taken part in an online smoking cessation program; Quinlan and McCaul (2000) used students who were paid $15 or received course credit and were entered in a lottery; and Sciamanna et al. (2000) examined hospitalized smokers. The present study adopts a more proactive approach to recruitment and uses standard reliable measures (derived from Ajzen’s, 1991, theory of planned behavior) to assess motivation.

Herzog and Blagg’s (2007) study also seems to challenge work showing strong linear relationships between motivation and the stages of change (e.g., Armitage, 2006; Armitage & Arden, 2007). From a conceptual point of view, linear differences in motivation across the stages of change are problematic, as they imply a “pseudo-stage model” (e.g., Sutton, 2000). However, from a practical perspective, measures of motivation do not lend themselves to aid in the targeting of interventions because they are continuous and therefore not readily amenable to audience segmentation. In contrast, the staging algorithm provides clear cut-off points and a potentially powerful way of targeting relatively discrete groups. Given that the stages of change are linearly related to a variety of social cognitive variables from several theoretical perspectives (e.g., Armitage, 2006; Armitage & Arden, 2007), it seems preferable to target interventions on the basis of the stages of change as opposed to the demographic variables (e.g., income, age, gender) favored by social marketers (e.g., Knott, Muers, & Aldridge, 2007).

Rationale and Study Aims

The research reviewed above provides the following rationale for the present study. First, there is a need to develop effective behavior change interventions that can be administered efficiently to smokers in the general population. Second, both transtheoretical model-based and implementation intention-based interventions have employed passive control groups, which may have exaggerated their effectiveness. Third, several studies seem to question the validity of the stages of change algorithm, but these conclusions have been drawn on the basis of comparisons with potentially unreliable measures of motivation and in samples of smokers who might reasonably be expected to be highly motivated to quit. Fourth, there is some evidence to suggest that the effects of implementation intentions may be most pronounced when they are targeted at people preparing to change their behavior – and it would be valuable to verify this claim.

On this basis, it was therefore hypothesized that: (a) implementation intentions would significantly reduce smoking over and above the effects of both active and passive control conditions; (b) consistent with the idea that implementation intentions are volitional strategies, the effects of the intervention would be independent of motivational variables derived from the theory of planned behavior (Ajzen, 1991); (c) the stage of change algorithm would show a linear relationship with reliable measures of motivation in a sample recruited proactively; and (d) the stages of change, derived from Prochaska and DiClemente's (1983) transtheoretical model would moderate the effects of the implementation intention intervention such that more quitting would occur in participants who formed implementation intentions while being in the preparation stage.

Method

Participants and Procedure

Workers employed by a company (N = 1,600) in the south of England were screened to see if they currently smoked cigarettes. Four hundred and thirteen smokers were identified and

invited (with no incentive) to complete a "Cigarette Smoking Questionnaire". Three hundred and fifty consented to participate (Figure 1).

The sample consisted of 177 women and 173 men aged 36 years on average (M = 36.20, SD = 14.30) who were from a range of academic backgrounds and occupations. Fourteen percent (n = 49) had no formal qualifications and 50.8% (n = 178) participants finished full time education by the age of 18 or younger. The majority of the sample (70.3%) smoked 11 or more cigarettes per day.

In order to assess the potential generalizability of the findings, the study population was compared with the UK population as a whole (Source: UK National statistics website: www.statistics.gov.uk). Consistent with the sampling frame, the employed (100% versus 74.9% in the UK) and people of typical working age (99% aged 16-74 years versus 72.3% in the UK) were overrepresented. However, the proportion of people holding degrees was roughly equivalent (19.0% versus 16.3% in the UK), as was the proportion of women in the sample (50.6% versus 51.3% women in the UK). Crucially, the number of smokers in the worksite (25.8% versus 26.0%), and the number of cigarettes smoked (70.3% smoking 11 or more per day) were directly comparable with figures for the UK population as a whole (72.2% smoking 10 or more per day – note that the Fagerstrom tolerance questionnaire specifies 11 cigarettes per day as a cut-off whereas the UK government uses 10 cigarettes per day as its cut-off).

Following screening to ensure the sample consisted solely of smokers, a 3-by-2 randomized controlled design was implemented. The independent variables were condition (experimental versus active control versus passive control) and time (baseline versus follow-up). The manipulations were presented on the last page of a questionnaire designed to measure behavior and beliefs associated with cigarette smoking, meaning that once the questionnaires had been sorted into random order (on the basis of a web-based randomizer) the individual who

distributed the questionnaires to participants was blind to the conditions. Anonymity was ensured through the generation of a personal code. Participants were informed they could withdraw from the study at any time without question.

All three groups received identical-looking questionnaires (the passive control group received the questionnaire alone). In addition to the questionnaire, participants randomly assigned to the active control condition were asked to plan to quit smoking in the next two months: "We want you to plan to quit smoking during the next 2 months. You are free to choose *how* you will do this, but we want you to formulate your plans in as much detail as possible". Participants randomized to the experimental condition were given the same instructions with an additional sentence that asked them to: "Please pay particular attention to the situations in which you will implement these plans". Thus, participants in the experimental group received standard implementation intention instructions (cf. Gollwitzer & Sheeran, 2006) that specifically asked participants to link their plans with critical situations.

Participants' subsequent cognition and behavior were measured at follow-up, two months post-baseline. At follow-up, all participants were given identical confidential questionnaires (minus the implementation intention intervention where appropriate), which were matched on the basis of the self-generated personal code. Two hundred and eighty-three (80.9%) participants completed both the baseline and follow-up measures and provided sufficient information to match their responses on the basis of their self-generated personal code (Figure 1). The data were analyzed on an intention to treat basis, meaning that those lost to follow-up were treated as "no-changers".

Measures

Stage of change. Stage of change for smoking was assessed using an abridged version of DiClemente, Prochaska, Fairhurst, Velicer, Rossi, and Velasquez's (1991) algorithm because

nonsmokers (including people in action and maintenance) were screened out prior to the study commencing. Thus, participants were first asked how many times they had quit smoking for at least 24 hours (to which they responded in open format) and second whether they were seriously thinking of quitting smoking. The latter item provided participants with a checklist of three options. Participants were categorized as being in the preparation stage if they were seriously thinking of quitting smoking within the next 30 days and if they had made at least one 24-hour quit attempt in the past year. Participants who were seriously thinking of quitting smoking within the next 30 days but had not made at least one 24-hour quit attempt in the past year were deemed contemplators. Participants were also regarded as being in the contemplation stage if they were seriously thinking of quitting smoking within the next 6 months. If participants indicated that they were not thinking of quitting, they were allocated to the precontemplation stage.

Motivation. Variables derived from Ajzen's (1991) theory of planned behavior - chosen because it is a model of human motivation that has been shown to be predictive of health behavior in general and smoking cessation in particular (e.g., Armitage, 2007) - were used to measure motivation. Each variable was measured by averaging responses to three standard items on 7-point scales. The perceived control scale included the item: "My quitting smoking in the next 2 months is/would be…*difficult-easy*", Cronbach's α at baseline was .89 and .92 at follow-up. The behavioral intention scale (e.g., "I intend to quit smoking in the next 2 months *definitely do not-definitely do*") also possessed good internal reliability at baseline (α = .94) and follow-up (α = .95), as did the attitude scale (e.g., "My quitting smoking in the next 2 months is/would be… *bad-good*", Cronbach's αs = .94 and .95 at baseline and follow-up, respectively), and the subjective norm scale (e.g., "People who are important to me want me to quit smoking in the next 2 months *very unlikely-very likely*", Cronbach's αs = .73 at baseline and .78 at follow-up.

Behavior. Smoking status was assessed on the basis of whether people reported quitting (at follow-up only), and using a biologically-validated measure of nicotine dependence. Based on the Fagerstrom tolerance questionnaire (see Fagerstrom & Schneider, 1989, for a review), the revised tolerance questionnaire is a 10-item measure of nicotine dependence that has been validated against expired alveolar carbon monoxide (see Tate & Schmitz, 1993). It includes items such as, "How many cigarettes a day do you smoke?" and "How often do you smoke when you are sick with a cold, the flu, or are so ill that you are in bed for most of the day?", all of which are measured on 5-point Likert scales anchored with *10 or less* to *26 or more* and *never* to *always*, respectively. Cronbach's α indicated that the revised tolerance questionnaire possessed good internal reliability at both baseline ($\alpha = .78$) and follow-up ($\alpha = .81$).

These measures were chosen because of their demonstrable reliability and validity, and because biological validation techniques (e.g., continine assessments) are not appropriate for trials with fewer than 15,000 participants (Department of Health and Human Services, 1990; Velicer, Prochaska, Rossi, & Snow, 1992).

Results

Randomization Check

The success of the randomization procedure was checked using MANOVA. The independent variable was *condition* with three levels (experimental group versus the two control groups), and the dependent variables were age, gender, nicotine dependence, age of first cigarette, theory of planned behavior variables, and stage of change. Unfortunately, the multivariate test was significant, $F(18, 678) = 2.19$, $p < .01$, $\eta_p^2 = .05$. Decomposition of this effect showed that participants in the active control condition were more likely to be in the precontemplation stage at baseline, $F(2, 349) = 5.06$, $p < .01$, $\eta_p^2 = .03$, and were less likely to

intend to quit at baseline, $F(2, 349) = 3.50$, $p < .05$, $\eta_p^2 = .02$. Where appropriate, the following analyses statistically control for the possible effects of these differences on the findings.

Relationships Between Stages of Change and Motivation

Table 1 presents the descriptive data for theory of planned behavior variables and nicotine dependence across the stages of change. As predicted, across each stage of change, theory of planned behavior variables increased in value, whereas nicotine dependence decreased. Polynomial-based orthogonal contrasts (linear and quadratic) with an adjustment for unequal sample sizes (Rosenthal & Rosnow, 1991, p. 473) were used to test for discontinuity patterns across the stages of change (see Table 1; Sutton, 2000). For each variable, the data show a clear statistically significant linear pattern across the stages of change: For example, on average, people in the precontemplation stage did not intend to quit smoking ($M = 1.82$, $SD = 0.92$), whereas people in contemplation reported more positive intentions with respect to quitting and scored around the midpoint of the intention scale ($M = 3.96$, $SD = 1.87$). In contrast, people in preparation had very positive intentions toward quitting, scoring close to the end of the scale on average ($M = 6.12$, $SD = 0.96$). Despite being measured on different scales, the zero-order correlation between stage and intention was .78 ($p < .01$). The one exception to this pattern of findings is for nicotine dependence, but the effect size associated with the significant quadratic trend is much smaller than that associated with the linear trend (Table 1). In sum, the cross-sectional data support the construct validity of the stages of change measure.

Effects of the Implementation Intention Intervention

The effects of the implementation intention intervention on the principal outcome measures were tested initially using a series of mixed ANCOVAs, controlling for the effects of baseline stage of change and baseline intention, which had been shown to differ between groups. *Condition* (experimental versus active control versus passive control) was the between-

participants factor, and *time* (baseline versus follow-up) the within-persons factor. Quitting, nicotine dependence, and variables from the theory of planned behavior were the dependent variables. The data presented in Table 2 show there were four significant condition *x* time interactions, for quitting, nicotine dependence, intention and perceived control. These effects were decomposed in three steps. First, within-participants differences between baseline and follow-up were tested using independent repeated measures ANOVAs. Second, between-participants differences in the dependent variables at follow-up were tested using ANCOVAs controlling for the dependent variables at baseline. Third, any differences between the three conditions were clarified using simple contrasts.

Within-participants analyses demonstrated significant levels of quitting in the experimental condition, $F(1, 114) = 15.80$, $p < .01$, $\eta_p^2 = .12$, but very little quitting in the passive, $F(1, 114) = 1.00$, $p = .32$, $\eta_p^2 = .01$, or active control conditions, $F(1, 119) = 2.02$, $p = .16$, $\eta_p^2 = .02$. Consistent with this, ANCOVA of follow-up quitting showed significant differences, $F(2, 349) = 10.47$, $p < .01$, $\eta_p^2 = .06$, with simple contrasts showing that the experimental condition differed significantly (ps < .01) from both control conditions (the two control conditions did not differ significantly from one another, $p = .77$). Thus, there was significantly more quitting in the experimental group (Figure 2). More specifically, 12.17% (14/115) who received the experimental manipulation quit, as opposed to 1.67% (2/120) in the active control and 0.87% (1/115) in the passive control group.

Consistent with the preceding analyses, nicotine dependence did not change significantly across time in the passive control group, $F(1, 114) = 1.00$, $p = .32$, $\eta_p^2 = .01$. There were, however, significant decreases in nicotine dependence in both the active control, $F(1, 119) = 18.31$, $p < .01$, $\eta_p^2 = .13$, and experimental conditions, $F(1, 114) = 20.87$, $p < .01$, $\eta_p^2 = .15$. These potential differences between conditions were clarified using ANCOVA, which was

significant, $F(2, 349) = 12.32$, $p < .01$, $\eta_p^2 = .07$, with significant differences between the experimental group and both the passive control ($p < .01$) and active control ($p < .01$) conditions. Thus, although there were significant decreases in nicotine dependence across the course of the study, the effects were most pronounced in the experimental condition (Figure 2).

The behavioral intentions of participants in the passive control group actually declined over time, albeit not statistically significantly, $F(1, 114) = 1.25$, $p = .27$, $\eta_p^2 = .01$. In contrast, intention to quit increased significantly in the active control, $F(1, 119) = 10.39$, $p < .01$, $\eta_p^2 = .08$, and experimental conditions, $F(1, 114) = 13.58$, $p < .01$, $\eta_p^2 = .11$. ANCOVA of potential differences between conditions at follow-up was significant, $F(2, 349) = 6.09$, $p < .01$, $\eta_p^2 = .07$, with simple contrasts showing that participants in the experimental group had significantly greater intentions than participants in the passive control condition ($p < .01$) but that the experimental and active control conditions did not differ significantly from one another ($p = .73$, Figure 2). Thus, intention to quit significantly increased across the course of the study to a similar extent in both the active control group and experimental condition, but not in the passive control condition.

Perceived control marginally decreased over time in the passive control group, $F(1, 114) = 2.25$, $p = .14$, $\eta_p^2 = .02$, and marginally increased in the active control group, $F(1, 119) = 1.69$, $p = .20$, $\eta_p^2 = .01$. In contrast, the perceived control of participants in the experimental condition significantly increased between baseline and follow-up, $F(1, 114) = 6.97$, $p < .01$, $\eta_p^2 = .06$. Consistent with these analyses, ANCOVA of potential differences between conditions at follow-up was significant, $F(2, 349) = 5.61$, $p < .01$, $\eta_p^2 = .03$, with simple contrasts showing that participants in the experimental group had significantly greater perceived control at follow-up

than participants in the passive control condition ($p < .01$) but only marginally stronger perceived control than participants in the active control condition ($p = .08$, Figure 2).

The preceding analyses show that the experimental manipulation significantly increased quitting, intention and perceived control, and significantly reduced nicotine dependence compared to the passive control condition. In contrast, the only significant differences between the experimental and active control conditions were with respect to quitting and nicotine dependence. The implication is that asking participants to form a plan significantly increased intentions and perceived control, but that only by additionally forming an implementation intention was behavior actually changed. Content analysis of the statements revealed that all the people who subsequently quit wrote out implementation intentions but these did not differ noticeably from those who wrote out implementation intentions but didn't subsequently quit. One difficulty is that the field setting precludes testing the hypothesized automatic operation of implementation intentions.

Effects of the Implementation Intention Intervention: Moderating Effects of Stage of Change

The potential moderating effects of stage of change on the effects of the implementation intention intervention were tested using a series of mixed ANOVAs. *Condition* (experimental versus active control versus passive control) and stage of change (precontemplation, contemplation, preparation) were the between-participants factors, and *time* (baseline versus follow-up) was the within-persons factor. The dependent variables were quitting, nicotine dependence, behavioral intention and perceived control (cf. Table 2). The data presented in Table 3 show there was just one significant three-way interaction, where quitting was the dependent variable, $F(4, 341) = 3.27$, $p < .05$, $\eta_p^2 = .04$. These data show that, of the people who received the implementation intention intervention 35.29% (6/17) quit if they had been in preparation at baseline, compared with 7.69% (4/52) who had been in precontemplation at

baseline and 8.70% (4/46) who had been in contemplation at baseline. ANCOVA, controlling for baseline, showed these differences were significant, $F(2, 115) = 5.33$, $p < .01$, $\eta_p^2 = .09$. Simple contrasts confirmed that the implementation intention intervention was more potent for people in preparation at baseline, compared with either precontemplation or contemplation (*p*s < .01). The implication is that although implementation intentions are effective regardless of stage of change, they are most effective for people who are preparing to change.

Discussion

The present study was designed to examine whether merely planning to change one's behavior could account for the effects of previous implementation intention studies, to ensure that such interventions could work in nonstudent samples, and to see whether the stages of change could moderate the effects. There were three key findings. First, implementation intentions were effective in encouraging people to quit. Second, the study showed that the effects of implementation intentions could not be explained by planning alone: Planning increased intention and perceived control but did not increase quitting. This is consistent with Gollwitzer's (1999) theory that motivation and volition are independent. Third, the effects of the implementation intention intervention were moderated by stage of change: 35% (6/17) of smokers who were initially in the preparation stage subsequently quit, compared to 8% (4/52) in the precontemplation stage and 9% (4/46) in the contemplation stage. The following discussion considers the conceptual and practical implications of this work.

Taking the sample as a whole, 12% (14/115) quit smoking in the experimental condition compared with 1% (3/235) of smokers who quit in the two control groups and the question arises as to how these effects compare with transtheoretical model-based tailored approaches. Based on Velicer et al.'s (2006) review, it was possible to compute relative benefit ratios for each of the studies. The best relative benefit ratio was 2.27 (.25 ÷ .11) for the expert system over a relevant

control condition (Prochaska et al., 1993). Comparing the present experimental condition with the active control condition in the present study produced a relative benefit ratio of 7.30 (relative benefit ratio = 14.00 for experimental over passive control). Although it is important to highlight the disparity in terms of length of follow-up between the present study and those reviewed by Velicer et al. (2006; 18 months versus 2 months in the present study), the present findings are encouraging. Moreover, in contrast with studies reported in Velicer et al.'s (2006) review, the present intervention was tested against an equivalently active control group, meaning that several potential demand characteristics can also be ruled out. Crucially, participants in the present active control condition were asked to plan to change their behavior, but were not asked explicitly to link critical situations with appropriate behavioral responses. The implication is that the impressive effects obtained in implementation intention intervention studies to date (see Gollwitzer & Sheeran, 2006) cannot be attributed to planning effects, but the specific linking in memory of critical situation with appropriate behavioral response.

In contrast with the effects on smoking behavior, the present study showed no differences in cognition between the active control group and the experimental group. Thus, compared with the passive control condition, asking people to make plans to change their behavior increased their intentions and perceived control but these effects did not lead to a change in behavior. This is consistent with the idea that motivation and volition are independent of one another (cf. Gollwitzer & Sheeran, 2006), and implies that motivation may not be sufficient to engender health behavior change.

The present findings also cast doubt on Jackson et al.'s (2005, 2006) claims that implementation intention effects are attributable to the use of student samples that are more compliant with task demands. The present study showed that implementation intentions were effective in a sample that was broadly representative of the smoking population. The implication

is that other factors potentially explaining Jackson et al.'s (2005, 2006) null effects must be explored. For example, the participants in both Jackson et al. (2005, 2006) studies were highly motivated and it is therefore arguable as to whether their participants were experiencing any breakdown in self-regulation that implementation intentions could deal with.

Consistent with Armitage's (2006) study into dietary change, the proportion of smokers quitting was greater still for those who were in preparation at baseline (35%, 6/17) as opposed to those in precontemplation (8%, 4/52) and contemplation (9%, 4/46). The implication is that implementation intention-based interventions can be administered to large populations of smokers, but will be most effective if resources first allow for targeting. The present study shows that the stages of change may be important in terms of audience segmentation, in other words, in targeting intervention efforts at those who are most likely to change. Further work that uses the stages of change as a tool for segmenting audiences is required. However, it is worth noting the valid concerns of Herzog and Blagg (2007) regarding the stage of change algorithm. Clearly, Herzog and Blagg's (2007) data show a different pattern to ours and it would be valuable to clarify where these differences lie in future research, for example, between: US versus UK smokers, passive versus proactive recruitment of participants, single-item versus multiple-item measures of motivation, and samples with 68% women versus 51% women (see also Etter & Sutton, 2002; Kraft et al., 1999; Quinlan & McCaul, 2000; Sciamanna et al., 2000).

Conceptually, the present study extends laboratory-based research in two important respects. First, by demonstrating that implementation intentions were able to overcome a habit-forming behavior such as smoking in a field setting, the present study adds to a growing body of laboratory work showing that implementation intentions possess the key properties of habits (see Gollwitzer & Sheeran, 2006). Second, by controlling for the effects of the planning instruction, the implication is that the "active ingredient" within an implementation intention is the linking of

critical situation with appropriate behavioral response. This means it is possible to distinguish implementation intentions from other related planning exercises such as goal setting or action planning: The key to an effective intervention seems to be the explicit linking of critical situation with appropriate behavioral response, and it would be valuable to establish whether other related techniques can be augmented using this "active ingredient".

More generally, further work might fruitfully look at guided implementation intentions, whereby a health professional, in consultation with a client, identifies the appropriate specification for their implementation intention. Indeed, considering the present findings in conjunction with Velicer et al.'s (2006) implies that interactive elements (e.g., feedback) in conjunction with implementation intentions might further enhance the effectiveness of both types of intervention.

Potential Limitations

A number of potential criticisms of the study should be noted. First, it would be valuable to replicate the present findings over a time period greater than two months because the maintenance of behavior is commonly defined as commencing six months after initial performance of the health behavior (e.g., Prochaska & DiClemente, 1983). However, there are grounds for cautious optimism. First, although 6 months is used as the norm for establishing maintenance of behaviour, this criterion is arbitrary. In fact, empirical evidence supports the view that maintenance can come sooner, particularly in the case of behaviors that have high frequency of performance in stable contexts (see Armitage, 2005). Second, there is a large body of evidence demonstrating that greater initial health behavior change is associated with large improvements in ongoing health behavior change (e.g., Jeffery, Wing, & Mayer, 1998) meaning that maximizing early treatment gains is a key area for further research. Third, other studies have shown that the effects of implementation intentions on behavior persist over much longer

periods of time, and so it seems plausible that the present findings would persist beyond the two months allowed by the employers in the present study (Gollwitzer & Sheeran, 2006).

A second possible limitation concerns the measure of behavior, which was self-reported. While it would be desirable to have a more objective measure, there are grounds for confidence in the present findings. First, there was a clear dissociation between self-reports of behavior and self-reports of cognition: Whereas self-reported cognition changed as a result of the planning instructions, self-reported behavior did not. If reporting biases did account for the present findings, it is unclear why these would affect behavioral outcomes and not psychological outcomes. Second, one of the principal outcome measures (the revised tolerance questionnaire) has demonstrable reliability and validity when compared with biological measures of smoking (e.g., Fagerstrom & Schneider, 1989; Tate & Schmitz, 1993) and the Surgeon General's Report of 1990 (Department of Health and Human Services, 1990) concludes that biological validation techniques (e.g., continine assessments) are unnecessary for most smoking cessation trials, particularly in studies with fewer than 15,000 participants (see also Velicer et al., 1992). Third, the effects of implementation intentions have been demonstrated in relation to objectively verifiable outcome measures (e.g., Gollwitzer & Sheeran, 2006), providing further confidence in the present findings.

Conclusions

Although previous research on transtheoretical model-based and implementation intention-based interventions can be criticized on grounds of having used passive control groups, the present study found little evidence to suggest that active control groups affect rates of quitting. In addition, there was further support for the idea that the stages of change may prove a valuable tool in deciding where to direct resources. Further research is needed to establish ways in which to further enhance the effectiveness of implementation intention-based interventions.

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Table 1: *Cross-Sectional Analysis of the Stages of Change*

Dependent Vartiables	Precontemplation (n = 181) *M* (*SD*)	Contemplation (n = 110) *M* (*SD*)	Preparation (n = 59) *M* (*SD*)	*F* for Linear Contrast	*F* for Quadratic Contrast
Nicotine Dependence	3.19 (0.69)	2.70 (0.59)	2.55 (0.65)	56.84**	4.44*
Intention	1.82 (0.92)	3.96 (1.87)	6.12 (0.96)	537.48**	0.01
Perceived Control	2.97 (1.44)	4.39 (1.36)	5.25 (1.02)	153.56**	2.86
Subjective Norm	5.07 (1.49)	5.81 (1.20)	6.43 (0.95)	53.88**	0.13
Attitude	2.45 (1.30)	4.51 (1.62)	6.07 (0.87)	371.77**	2.27

Note. Degrees of freedom associated with the *F* tests are 2, 347.

*$p < .05$. **$p < .01$.

Table 2: *Comparisons of Experimental and Control Groups Between Baseline and Follow-up*

Dependent Variables	Conditions	Baseline		Follow-up		F[a]
		M	*SD*	*M*	*SD*	*df* = 2, 345
Quitting[b]						10.25**
	Experimental	1.00	0.00	1.12	0.33	
	Active Control	1.00	0.00	1.02	0.13	
	Passive Control	1.00	0.00	1.01	0.09	
Nicotine Dependence						10.22**
	Experimental	2.84	0.74	2.60	0.84	
	Active Control	2.93	0.60	2.83	0.54	
	Passive Control	3.01	0.78	2.99	0.78	
Intention						5.88**
	Experimental	3.44	2.13	3.79	2.12	
	Active Control	2.82	1.81	3.22	1.84	
	Passive Control	3.42	2.22	3.33	2.09	
Attitude						2.15
	Experimental	3.80	1.94	4.24	1.87	
	Active Control	3.55	1.90	3.98	1.79	
	Passive Control	3.77	2.00	4.01	1.92	
Subjective Norm						2.25
	Experimental	5.63	1.48	5.57	1.60	
	Active Control	5.34	1.44	5.09	1.33	
	Passive Control	5.64	1.30	5.41	1.21	
Perceived Control						4.09*
	Experimental	4.00	1.79	4.19	1.69	
	Active Control	3.57	1.48	3.67	1.36	
	Passive Control	3.84	1.58	3.74	1.54	

Note. All analyses control for stage of change and intention at baseline. [a]*F* refers to the condition *x* time interaction. [b]Quitting is scored 1 = not quit, 2 = quit. Given that quitting is a binary outcome variable with a 90:10 split thereby potentially threatening the robustness of ANCOVA, these data were reanalyzed using a series of nonparametric tests. The findings of the nonparametric texts are identical to those reported here. The ANCOVAs are preferred because: (a) nonparametric tests cannot control for the baseline differences attributable to randomization problems, (b) nonparametric tests cannot test for interactions, (c) effect sizes can be computed for all analyses, and (d) it allows comparability across the analyses.

*$p < .05$. **$p < .01$.

Table 2: *Comparison of Experimental and Control Groups Between Baseline and Follow-up*

Dependent Variables	Conditions	Stage	*n*	Baseline		Follow-up		F^a
				M	*SD*	*M*	*SD*	*df* = 4, 341
Quitting[b]								3.27*
	Experimental	Precontemplation	52	1.00	0.00	1.08	0.27	
		Contemplation	46	1.00	0.00	1.09	0.28	
		Preparation	17	1.00	0.00	1.35	0.49	
	Active Control	Precontemplation	76	1.00	0.00	1.00	0.00	
		Contemplation	30	1.00	0.00	1.03	0.18	
		Preparation	14	1.00	0.00	1.07	0.27	
	Passive Control	Precontemplation	53	1.00	0.00	1.00	0.00	
		Contemplation	34	1.00	0.00	1.00	0.00	
		Preparation	28	1.00	0.00	1.04	0.19	
Nicotine Dependence								0.09
	Experimental	Precontemplation	52	3.15	0.69	2.91	0.86	
		Contemplation	46	2.69	0.67	2.47	0.74	

		Preparation	17	2.31	0.59	2.03	0.65	
	Active Control	Precontemplation	76	3.03	0.60	2.93	0.55	
		Contemplation	30	2.64	0.52	2.57	0.47	
		Preparation	14	3.02	0.58	2.82	0.50	
	Passive Control	Precontemplation	53	3.45	0.75	3.46	0.69	
		Contemplation	34	2.77	0.53	2.77	0.55	
		Preparation	28	2.46	0.61	2.37	0.65	
Intention								0.78
	Experimental	Precontemplation	52	1.88	0.98	2.34	1.46	
		Contemplation	46	4.17	1.93	4.49	1.74	
		Preparation	17	6.22	1.07	6.35	1.25	
	Active Control	Precontemplation	76	2.01	0.98	2.42	1.44	
		Contemplation	30	3.36	1.85	4.07	1.57	
		Preparation	14	6.05	0.98	5.81	1.01	
	Passive Control	Precontemplation	53	1.50	0.65	1.51	0.66	
		Contemplation	34	4.20	1.74	4.22	1.53	

		Preparation	28	6.11	0.91	5.68	1.26	
Perceived Control								0.05
	Experimental	Precontemplation	52	2.96	1.66	3.20	1.61	
		Contemplation	46	4.59	1.46	4.73	1.32	
		Preparation	17	5.56	0.98	5.75	0.73	
	Active Control	Precontemplation	76	3.15	1.39	3.30	1.33	
		Contemplation	30	4.11	1.42	4.09	1.28	
		Preparation	14	4.73	1.19	4.73	0.84	
	Passive Control	Precontemplation	53	2.73	1.26	2.67	1.12	
		Contemplation	34	4.36	1.14	4.20	1.19	
		Preparation	28	5.32	0.89	5.18	1.09	

Note. [a]*F* refers to the condition *x* stage *x* time interaction. [b]Quitting is scored 1 = not quit, 2 = quit.

*$p < .05$.

Figure 1

Flow Diagram of Participant Progress Through the Phases of the Randomized Trial

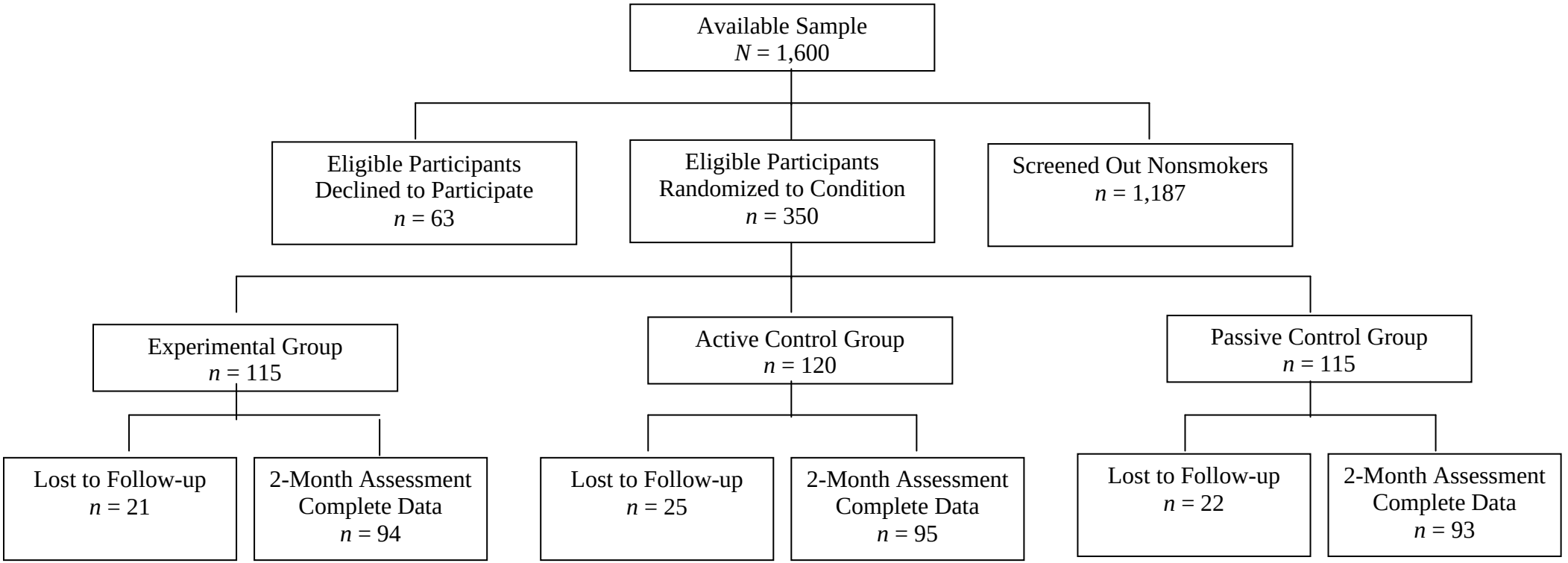
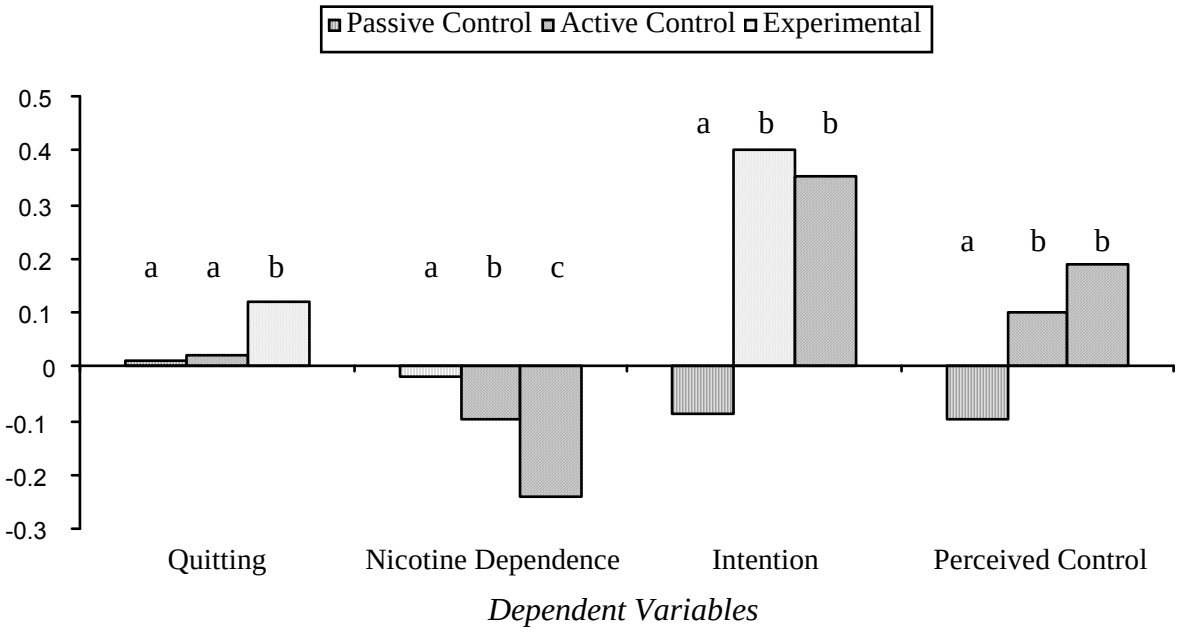


Figure 2

Significant Effects of the Manipulations on Changes in Cognition and Behavior



Note. For ease of presentation the data are raw difference scores (follow-up minus baseline), although the analyses are based on planned contrasts controlling for baseline. Letters above the columns denote significant differences; columns not sharing a letter differ significantly ($p < .05$).

Appendix

PAPER SECTION and topic	Item #	Description	Reported on page #
TITLE & ABSTRACT	1	How participants were allocated to interventions (*e.g.*, "random allocation", "randomized", or "randomly assigned").	1
INTRODUCTION			
Background	2	Scientific background and explanation of rationale.	2-6
METHODS			
Participants	3	Eligibility criteria for participants and the settings and locations where the data were collected	6-8
Interventions	4	Precise details of the interventions intended for each group and how and when they were actually administered.	7
Objectives	5	Specific objectives and hypotheses.	5-6
Outcomes	6	Clearly defined primary and secondary outcome measures and, when applicable, any methods used to enhance the quality of measurements (*e.g.*, multiple observations, training of assessors).	8-10

Sample size	7	How sample size was determined and, when applicable, explanation of any interim analyses and stopping rules.	6, 29
Randomization: sequence generation	8	Method used to generate the random allocation sequence, including details of any restriction (*e.g.*, blocking, stratification).	7
Randomization: allocation concealment	9	Method used to implement the random allocation sequence (*e.g.*, numbered containers or central telephone), clarifying whether the sequence was concealed until interventions were assigned.	7
Randomization: implementation	10	Who generated the allocation sequence, who enrolled participants, and who assigned participants to their groups.	7
Blinding (masking)	11	Whether or not participants, those administering the interventions, and those assessing the outcomes were blinded to group assignment. When relevant, how the success of blinding was evaluated.	7
Statistical methods	12	Statistical methods used to compare groups for primary outcome(s); Methods for additional	10-14

		analyses, such as subgroup analyses and adjusted analyses.	
RESULTS			
Participant flow	13	Flow of participants through each stage (a diagram is strongly recommended). Specifically, for each group report the numbers of participants randomly assigned, receiving intended treatment, completing the study protocol, and analyzed for the primary outcome. Describe protocol deviations from study as planned, together with reasons.	29
Recruitment	14	Dates defining the periods of recruitment and follow-up	7
Baseline data	15	Baseline demographic and clinical characteristics of each group.	6
Numbers analyzed	16	Number of participants (denominator) in each group included in each analysis and whether the analysis was by "intention-to-treat". State the results in absolute numbers when feasible (*e.g.*, 10/20, not 50%).	8, 10-14
Outcomes and	17	For each primary and secondary outcome, a	10-14

estimation		summary of results for each group, and the estimated effect size and its precision (*e.g.*, 95% confidence interval).	
Ancillary analyses	18	Address multiplicity by reporting any other analyses performed, including subgroup analyses and adjusted analyses, indicating those pre-specified and those exploratory.	10-14
Adverse events	19	All important adverse events or side effects in each intervention group.	N/A
DISCUSSION			
Interpretation	20	Interpretation of the results, taking into account study hypotheses, sources of potential bias or imprecision and the dangers associated with multiplicity of analyses and outcomes.	14-19
Generalizability	21	Generalizability (external validity) of the trial findings.	6, 14-19
Overall evidence	22	General interpretation of the results in the context of current evidence.	14-19