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DOI
10.1037/hea0000346

Publication date
2016

Document Version
Final published version

Published in
Health Psychology

License
Article 25fa Dutch Copyright Act

Health Psychology
2016, Vol. 35, No. 8, 870–880

0278-6133/16/$12.00 http://dx.doi.org/10.1037/hea0000346

A Randomized Controlled Trial of Web-Based Attentional Bias Modification to Help Smokers Quit

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Objective: To assess the efficacy of a multiple-sessions Web-based Attentional Bias Modification (ABM) self-help intervention in 434 smokers who made a quit-attempt. ***Method:*** Respondents were randomized to receive 6 sessions of ABM- or placebo-training in a period of 2 weeks. Smoking-related cognitions (e.g., self-efficacy and intention to quit) and cognitive biases (i.e., attentional and approach bias) for smoking-cues were assessed before training (pretest). Primary outcome-variable was continued abstinence, 6 months after baseline. Bias reduction at the posttraining assessment was the secondary outcome. A 2 × 2 mixed analysis of variance (ANOVA) and logistic regression analyses were conducted using the whole sample ($N = 434$) as well as subsamples of light to moderate smokers (<15 cigarettes, $N = 115$) and heavy smokers (15 or more cigarettes, $N = 319$). Conservative analyses (coding drop-outs as smokers) as well as complete case analyses were conducted. ***Results:*** The ABM training had no significant effect regarding bias reduction and no behavioral effects in the whole sample of smokers. Subsample analyses revealed a significant positive effect on continued abstinence in heavy smokers only (complete case analyses: odds ratio [OR] = 3.15; $p = .02$; confidence interval [CI] = 1.24–7.99; conservative analyses: $OR = 2.49$; $p = .02$; CI = 1.13–5.48). ***Conclusion:*** Web-based ABM training is ineffective in fostering cognitive bias reduction and continued smoking abstinence. However, the positive effects in heavy smokers—as indicated by exploratory subsample analyses—warrant further research into the potential of multiple sessions ABM training to foster continued smoking abstinence in heavy smokers who make a quit-attempt.

Keywords: smoking cessation, relapse prevention, attentional bias modification, implicit cognitive processes

Supplemental materials: http://dx.doi.org/10.1037/hea0000346.supp

Smoking is one of the main preventable causes of premature death and disability with a high economic and social burden (CDC, 2008; Gelder, Poos, & Zantinge, 2011). Although most smokers are aware of the negative consequences of smoking and want to quit smoking, only 3–5% manage to remain abstinent for six months or longer (Hughes, Keely, & Naud, 2004). Smoking relapse prevention programs—mainly using behavioral techniques and skill trainings—have shown only modest effects (Hajek, Stead, West, Jarvis, & Lancaster, 2009). This underlines the need for new interventions to support smokers who want to quit smoking.

Dual-process models (e.g., Bechara, 2005; Deutsch & Strack, 2006; Strack & Deutsch, 2004; Wiers et al., 2007; Wiers & Stacy, 2006) propose that behavior is determined by the joint outcome of reflective cognitive processes on the one hand, and impulsive cognitive-motivational processes, on the other hand. Impulsive processes are fast, associative, and triggered automatically. The latter processes are sensitive to the incentive salience of cues and result in cognitive biases such as attentional bias (Field & Cox, 2008; Robinson & Berridge, 2003; Robinson & Berridge, 2008) and approach bias (Field, Kiernan, Eastwood, & Child, 2008; Robinson & Berridge, 2008; Wiers, Rinck, Dictus, & van den

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Ethical approval was obtained from the Medical Ethics Committee of the Maastricht Academic Hospital and Maastricht University (MEC 08-3–003; NL21414.068.08). The study has been registered in the Dutch Trial Register (NTR2312). This study was funded by the Dutch Organization for Health Research and Innovation (grant 6130-0030). Reinout W. Wiers is supported by VICI grant 453-08-001 from the Dutch National Science Foundation (N.W.O). We thank Wouter Boendermaker for his valuable contributions to the development and piloting of the Train Yourself Smoke-free program materials. We would also like to thank Stivoro for their help in recruiting respondents, as well as the respondents for their participation in the study.

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Wildenberg, 2009) for cues related to the addictive behavior. In the past decade, many studies have assessed these cognitive biases and their relation to behavior (meta-analysis; Rooke, Hine, & Thorsteinsson, 2008). An attentional bias is observed when someone's attention is selectively drawn by a category of cues (e.g., addiction-related cues). An approach bias is observed when someone demonstrates an automatically triggered action-tendency to approach addiction-related cues rather than a tendency to avoid them. Both biases have been demonstrated in smokers (e.g., Cox, Fadardi, & Pothos, 2006; Mogg, Bradley, Field, & De Houwer, 2003; Waters, Shiffman, Bradley, & Mogg, 2003; Watson, de Wit, Hommel, & Wiers, 2012).

More important, implicit cognitive processes cannot only be assessed, but they can also be directly manipulated, using techniques collectively referred to as Cognitive Bias Modification (CBM). In their pioneering study, MacLeod and colleagues adapted a version of the Visual Probe Task (VPT)—a measurement of attentional bias—to modify attentional bias for threat-related stimuli in students with medium levels of anxiety, either toward or away from threat stimuli, and found effects both on the bias and on behavior (MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). The first studies in the domain of addiction tested the effects of a single-session of Attentional Bias Modification (ABM) and showed that an attentional bias for alcohol (Field et al., 2007; Schoenmakers, Wiers, Jones, Bruce, & Jansen, 2007) and smoking (Attwood, O'Sullivan, Leonards, Mackintosh, & Munafo, 2008; Field, Duka, Tyler, & Schoenmakers, 2009) could be modified toward or away from the substance-related cues. However, training effects were only observed on trained pictures: no generalization was found, neither "close generalization" (i.e., decreased bias toward untrained substance-related pictures when measured with the same task), nor more distant generalization (e.g., other cognitive biases or behavior). Distant generalization effects of one specific training to other cognitive biases are a current topic in CBM research (see for a review Wiers, Gladwin, Hofmann, Salemink, & Ridderinkhof, 2013a). These generalization effects are important because behavior is mostly determined by multiple cognitive biases—as mentioned above both an attentional bias and an approach bias have been related to smoking—and it is an important question whether these biases can be targeted with a single type of training or if separate training-strategies are warranted. The most important generalization of training is to behavioral outcomes.

More encouraging than the effects of the single-session interventions, the first two studies on multiple-session ABM training for problem drinkers (Fadardi & Cox, 2009) and alcohol-dependent patients in treatment (Schoenmakers et al., 2010) did result in a decreased bias when measured with untrained stimuli (i.e., close generalization effects) and in reduced drinking (Fadardi & Cox, 2009) and later relapse (Schoenmakers et al., 2010). In the smoking domain, however, a recent study showed no baseline bias and no effects of multiple sessions of ABM on either attentional bias or smoking behavior (Begh et al., 2015). In contrast, another study in the smoking domain found that three sessions of ABM reduced attentional bias, but did not result in changes with regard to smoking outcomes (Lopes, Pires, & Bizarro, 2014). Comparable findings from a study by Kerst and Waters (2014) pointed out that three sessions of ABM provided on mobile devices significantly changed the attentional bias for smoking, but had no effects on behavior. The latter study was conducted among smokers without a desire to change their behavior, supporting the idea that motivation to quit could be an important condition for behavioral effects (cf. Wiers et al., 2013a, 2015). For this reason it was judged crucial to preselect smokers who actually made a quit attempt in the present study.

A final topic concerns the delivery mode of ABM training. The Internet has shown to be a promising delivery mode for health-promoting interventions (see, for instance, Brug, Campbell, & van Assema, 1999; Elfeddali, Bolman, Candel, Wiers, & de Vries, 2012; Te Poel, Bolman, Reubsaet, & de Vries, 2009; Vandelanotte, Spathonis, Eakin, & Owen, 2007; Walters, Wright, & Shegog, 2006). However, recent studies on social anxiety found no effects of Internet-delivered ABM training (Boettcher, Berger, & Renneberg, 2012; Boettcher et al., 2013; Carlbring et al., 2012), which contrasted with more positive findings for ABM strategies delivered in a clinical setting for the same disorder (Amir, Beard, Burns, & Bomyea, 2009; Schmidt, Richey, Buckner, & Timpano, 2009). Further testing of the efficacy of Internet-delivered ABM interventions is important, both for theoretical reasons (can a disorder-related attentional bias be successfully modified over the web) and for practical reasons, especially for smoking, where relatively few people receive formal treatment.

Based on the foregoing discussion, the present study aimed to test whether six sessions of ABM training delivered over the Internet would result in reduced attentional bias and induce more distant generalization effects regarding approach bias and success in quitting. It was hypothesized that, compared with placebo-training, the ABM-training would: (a) result in a significantly larger reduction of attentional bias (assessed both with trained stimuli, and with untrained stimuli); (b) generalize to influence an approach bias for cigarettes (distant generalization); and (c) foster continued smoking abstinence. Because an attentional bias has been associated with the quantity and frequency of use, with some studies revealing significantly greater bias among heavy users (Field, Mogg, Zetteler, & Bradley, 2004; Mogg & Bradley, 2002; Townshend & Duka, 2001) and others reporting greater bias among lighter users (e.g., Hogarth, Mogg, Bradley, Duka, & Dickinson, 2003; Mogg, Field, & Bradley, 2005; Waters et al., 2003), it was considered necessary to further explore this by means of subsample analyses. The baseline bias and the effects of the training were, therefore, also explored for light to moderate and heavy smokers separately, with heavy smoking defined as smoking at least 15 cigarettes per day (following Rodu, Plurphanswat, & Fagerström, 2015; Waters et al., 2003; Wilson, Parsons, & Wakefield, 1999).

Method

Sample Size Calculation

Power calculations (power .80; $p < .05$) were based on the assumption to find an abstinence rate of 5% in the control condition and 15% in the ABM condition, and suggested that 132 smokers per condition should be recruited. Assuming an attrition rate of at least 20%, we aimed to include 325 smokers in our sample.

Recruitment and Inclusion

Recruitment started in December 2009 with ads on online national news pages referring to a website. The website explained that we had developed a training program to help smokers remain smoke-free after a quit-attempt and that the efficacy of the training was being tested (vs. a placebo program). The respondents were notified that they had a 50% chance to be assigned to the placebo training. The website also explained the inclusion criteria.

Inclusion criteria included giving consent for participation, being 18–65 years, reporting smoking on a daily basis for at least 1 year and not having made a quit-attempt yet (thus, respondents who did not smoke in the previous week and/or the previous 24 hr were excluded from analyses). Furthermore, participants needed to select a quit-date at baseline. Participants met the exclusion criteria when they did not understand (ability to read and write) Dutch well, when they consumed more than 16 standard units of alcohol per week (10 g of pure alcohol in the Netherlands), when they were unable to refrain from alcohol use during a whole day, when they regularly used drugs or calming medicines, when they reported having a psychological disorder or dyslexia and when they were color blind and/or vision impaired (even with glasses). The criteria were assessed with yes/no questions (e.g., "Are you color blind?"; "Do you have a psychological disorder (e.g., a depression, psychosis, schizophrenia, etc.)?"; "Do you use soft or hard drugs?"). The question about the ability to read and write Dutch ("What would be your evaluation of your knowledge (i.e., ability to understand, read and write) of the Dutch language?") had five answering categories (very bad, bad, average, good, or very good). Participants also needed to fill in the baseline questionnaire and the VPT task (because randomization took place after entering this task). Participants further needed to confirm their quit-attempt after their quit-date; otherwise, they were excluded from further participation. Furthermore, respondents who participated in none of the ABM/placebo sessions were excluded from analyses; so doing had no substantive impact on the findings reported below. The enrollment and inclusion of respondents is presented in Figure 1. The final analytical sample consisted of 434 respondents.

Design and Procedure

The website contained a link to the program where respondents could sign up for participation by creating their own user account. The account was used for all sessions, the sessions were Web based and invitations and reminders were sent by e-mail. The study was a parallel-group randomized controlled trial with two conditions: an ABM training condition and a control condition (placebo-training, which consisted of continued assessments). The study flow was as follows:

1. The respondents started with a pretraining assessment with a self-report questionnaire in which they were also asked to select a quit-date, a manikin-task to measure approach-avoidance biases and a VPT to measure attentional biases (see below for details). The respondents were randomized into one of the two conditions by means of a computerized randomization mode after entering the VPT;
2. The respondents needed to confirm their quit-attempt after the chosen quit-date (they were asked if they had quit for 24 hr on their chosen quit-date and could answer: [a] yes; [b] no, but I am still planning to quit; or [c] no, and I am not planning to quit). Respondents could select a new quit-date when they chose Option 2; they were excluded from further participation when they chose Option 3.
3. Respondents who confirmed the quit-attempt (by choosing Option 1) started with the VPT-based training or placebo sessions. These training/placebo sessions started 18 hr after the respondents entered the previous session and closed after 60 hr; this time frame was used for all of the training and placebo sessions.
4. A posttraining assessment (i.e., manikin and VPT tasks) followed the intervention sessions.
5. An Assessment of continued smoking abstinence followed 6 months after baseline.

Measures

Self-report questionnaires. *Demographic and smoking-behavior* related information was gathered by assessing age, gender, educational level, the number of cigarettes smoked per day, and the number of years one has been smoking and previous quit-attempts. *Level of dependence* was measured by six items based on the abbreviated Fagerström Test for Nicotine Dependence (FTND; Fagerstrom, Heatherton, & Kozlowski, 1990; Heatherton, Kozlowski, Frecker, & Fagerstrom, 1991). *Craving* was assessed (cf. Dijkstra & Borland, 2003) with six items (e.g., "I desire smoking a cigarette") rated on a 5-point scale from 1 = *never* to (5) *very often*.

Based on previous studies (Dijkstra & de Vries, 2000; Hoving, Mudde, & de Vries, 2006; Mudde, Willemsen, Kremers, & de Vries, 2006; Te Poel et al., 2009) we assessed *intention to quit smoking* by one item ("how strong is your intention to quit smoking?") rated on a 10-point scale from (1) *very weak* to (10) *very strong*; and *self-efficacy* by 14 items asking whether the respondent thought to be able to refrain from smoking in various high-risk situations (e.g., at a party or when being angry), rated on a 7-point Likert scale from (1) *totally disagree* to (7) *totally agree*.

The Attention Control Scale measured two components of attention (attention focusing and attention shifting) with 20 items (e.g., "even when I am working very hard, I get distracted by things that happen in my environment") rated on a 4-point scale from (1) *almost never* to (4) *always*.

Continued abstinence after 6 months was defined as not smoking at all after the quit-attempt (cf. Hughes et al., 2003). The respondents were asked whether they had smoked since their chosen quit-date and could answer (1. yes; 2. no).

The mean scores were included in the analyses and all scales had a good reliability, Cronbach's α >.80.

Materials. We used eight sets of 12 matched smoking—related (e.g., smoking people, cigarette [packages], etc.) and neutral (e.g., nonsmoking people, pencils [packages], etc.) picture pairs for the VPT: Sets 1 and 2 in the pretraining, Sets 2–8 in the training/placebo sessions, and Set 1 (untrained pictures) in the

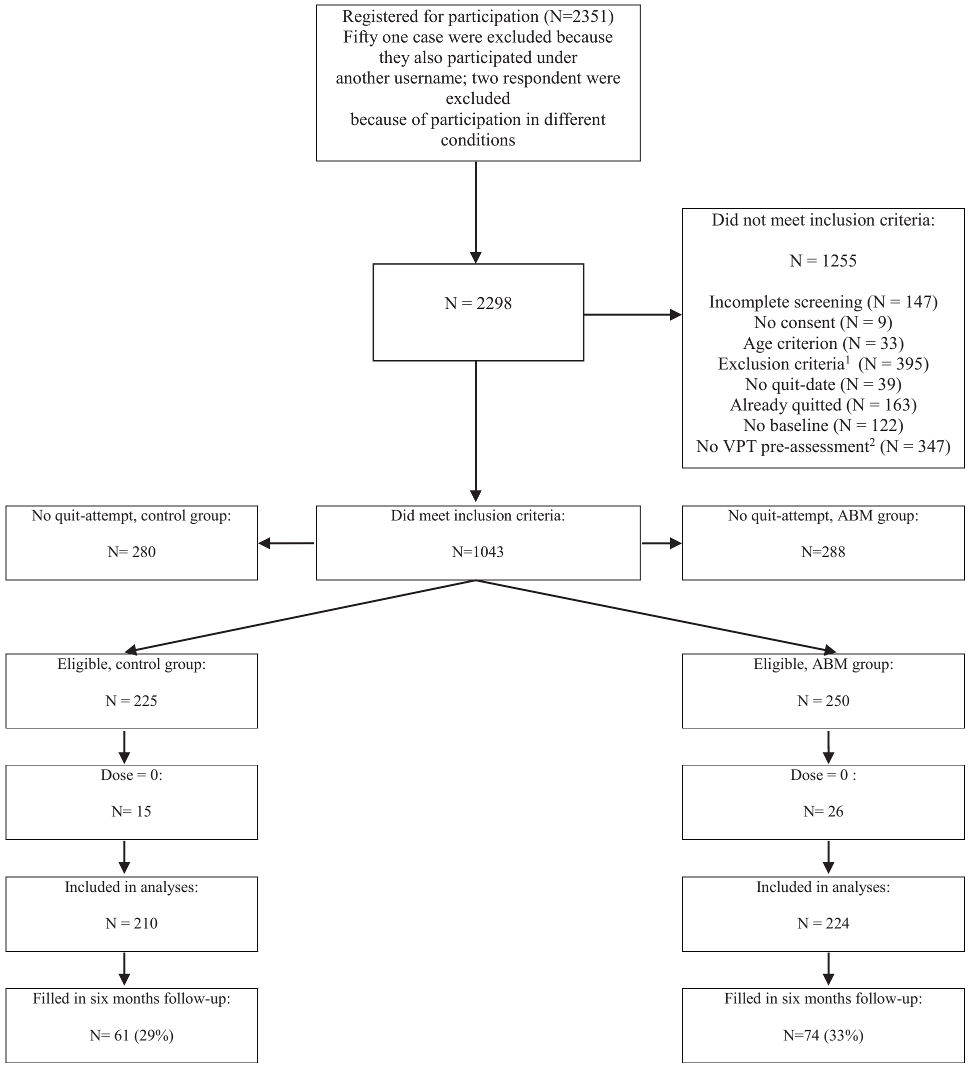

Figure 1. Enrollment and inclusion of respondents. [1]These criteria were: not smoking on a daily base, being a smoker for less than 1 year, not understanding (ability to read and write) Dutch well, consuming more than 16 standard units of alcohol per week being unable to refrain from alcohol use during a whole day, regularly using drugs or calming medicines, reporting a psychological disorder or dyslexia, or being color blind and/or vision impaired (even with glasses). [2]Respondents were randomized only after entering the Visual Probe Task (VPT).

posttraining. Eight neutral/neutral picture pairs formed practice/filler trials. Each picture had a dimension of 425 × 282 pixels.

For the manikin-task we used 14 matched smoking-related and neutral picture pairs: another four picture pairs formed practice trials. Each picture had a dimension of 352 × 288 pixels. The manikin-task consisted of other pictures than the pictures used in the VPT.

Reaction time paradigms. Approach-avoidance bias was assessed with a manikin task (De Houwer, Crombez, Baeyens, & Hermans, 2001; Mogg et al., 2003). Smoking-related and neutral pictures were presented one by one on the computer screen, with a manikin appearing above or below (50–50%) the pictures. The respondents were instructed to either move the manikin toward (in the "approach block") or away (in the "avoid block") from the smoking-related picture. Respondents were instructed to respond quickly, to make as few mistakes as possible, and to use the letters 'I' (for moving up) and 'V' (for moving down). The eight practice trials after the instruction were followed by an approach and avoid block; the order of the blocks was counterbalanced across respondents. A correct response was followed by the next picture (with an intertrial interval of 1,500 ms); an incorrect response was followed by a red cross and a repetition of the trial to ensure serious task performance (cf., Greenwald, Nosek, & Banaji, 2003). Each block presented 28 trials, all pictures were presented once in each block. Response times (RTs) were logged for all respondents.

Attentional bias was assessed with the VPT (MacLeod, Mathews, & Tata, 1986). Critical trials (actual measurement trials) presented smoking/neutral pictures pairs, while practice trials (for practicing with the keys) and filler trials (for reducing the frequency with which critical trials appear in the task) were formed by neutral/neutral pairs. The VPT-trials started with a fixation cross for 500 ms followed by a matched picture pair, after 500 ms a probe (in this case an arrow) replaced one of the pictures. Respondents were instructed to identify the direction of the arrow as quickly and correct as possible by using the letters S (pointing up) and L (pointing down). Error responses were followed by an identical trial. The pretraining assessment started with eight practice trials (that were repeated until a correct response was given in all trials) followed by two blocks both consisting of 12 filler trials and 48 critical trials (repeating two sets of 12 pictures four times). The arrow appeared equally often behind the smoking-related and neutral pictures. The posttraining procedure was identical to the pretraining procedure.

Intervention

The training and placebo-training were both based on the VPT and included six sessions, each with eight practice trials and six blocks; 48 assessment trials in Block 1, 48 critical trials (actual training trials) and 12 intermeasure trials in Block 2–5 and 48 critical trials in Block 6. The assessment trials in the intervention sessions were included to reduce the frequency with which critical trials appeared in the task; they were not included in the analyses (cf., MacLeod et al., 2002).

In the placebo-training (control condition), the arrow replaced the smoking-related and neutral pictures equally often (continued assessment). For training, a contingency was introduced with the arrow replacing the neutral picture in all critical trials, in 50% of the Block 1 trials (continued assessment) and in 50% of the intermeasure trials; thus, the arrow replaced the neutral picture in 92% of the trials over Blocks 2–6 (cf., MacLeod et al., 2002). Respondents were instructed to identify the direction of the arrow as quickly and accurately as possible by using the letters S (pointing up) and L (pointing down). The number of trials included was based on a previous multiple—session ABM training for abstinent alcohol-dependent patients (Schoenmakers et al., 2010).

Data Analyses

Baseline differences were assessed using χ^2-tests for categorical variables and *t* tests for continuous variables. Attrition analyses were conducted by means of logistic regression analyses. VPT-data were used to calculate median RTs (cf., MacLeod et al., 2002; Schoenmakers et al., 2007) when the probe replaced smoking-related pictures and when it replaced neutral pictures (per respondent). Attentional bias was calculated by subtracting the median RTs for probes replacing smoking-related pictures from the median RTs for probes replacing neutral pictures, a positive score indicated a bias toward smoking-related pictures. An approach-bias for smoking related cues was calculated by subtracting the median approach RTs from the median avoidance RTs, a positive score indicated an approach bias.

One sample *t* tests were conducted to assess pretraining biases. Cognitive bias scores were subjected to a 2 (time: pre or posttest) $\times$ 2 (condition: ABM or control condition) mixed analysis of variance (ANOVA) with time as the within subjects factor and condition as the between subjects factor. Logistic regression analyses—complete case analyses as well as conservative analyses (including drop-outs as relapsers)—were conducted to assess behavioral effects after 6 months. The hypotheses were tested at the two-sided 5% significance level.

Results

Sample Characteristics

Pretraining demographic and smoking-related characteristics are presented in Table 1. The majority of the respondents were women (69%), the average age was 41 years ($SD = 11.04$) and the average consumption was 18 cigarettes per day ($SD = 7.63$). The majority (92%) of the respondents had made previous quit-attempts.

Adherence and Attrition

On average, the respondents ($N = 434$) participated in 3.8 sessions ($SD = 1.95$); 194 respondents (45%) participated in 1–3 sessions and 240 respondents (55%) participated in 4–6 sessions. Among respondents who filled out the posttraining measurement ($N = 210$) the average dose was 5 sessions ($SD = 1.11$); 18 (9%) participated in 1–3 sessions and 192 (91%) participated in 4–6 sessions. Dose-response associations could not be assessed because of the imbalance between the "low-dose (1–3 sessions)" and "high-dose (4–6 sessions)" group. The number of sessions followed did not differ significantly between the control and ABM condition ($p > .20$).

The study drop-out rate after six months in the control group (71.0%) was not significantly different from drop-out in the ABM group (67%); drop-out was related to being younger, lower craving levels, and being male.

Data Preparation

Data were excluded because of (a) technical errors/closing tasks (VPT data: 0.4% of the data, manikin task: 1.2%); (b) respondents filling out the measurements multiple times (VPT: 4.1%, manikin task: 4.1%, data from the premeasurement for which the quit-attempt was confirmed were included); and (c) error trials, identical trials after error trials and trials with RTs <200 ms and >2,000 ms (note that trials from the manikin task were excluded when the RTs >3,000 ms; VPT: 7.0%; manikin task: 12.6%). Five respondents had an outlying (>3 *SD*s different from the group's mean) attentional bias score at baseline; their baseline data were excluded from the baseline analyses and the ANOVA (note that the exclusion of these respondents did not result in a different pattern of significant results). One ABM respondent had a posttraining attentional bias score of more than 7 *SD*s lower than the other respondents in the ABM group; these data were not included in the ANOVA (note that they would have biased the results in favor of the training). At the pretraining assessment of the manikin task, 1 respondent had no pretraining data, 8 respondents had valid RTs on less than half of the trials (<7 trials) for the approach or avoid assignment, and 11 respondents had outlying bias scores (>3 *SD* from the group's mean). Additionally, one respondent had an outlying bias score at the postmeasurement.

Cognitive Biases

Cognitive biases at the pretraining assessments. The baseline attentional bias was 3.46 ms ($SD = 30.62$) and the baseline

Table 1
Demographics and Smoking-Related Characteristics Per Condition

Characteristics	Overall (*N* = 434)	Control (*N* = 210)	ABM (*N* = 224)
Gender			
% Female	68.9%	70.5%	67.4%
Age			
Mean age (*SD*)	40.76 (11.04)	40.54 (11.44)	40.96 (10.68)
Educational level			
% Low educated	8.5%	8.6%	8.5%
% Medium educated	52.8%	53.3%	52.2%
% High educated	38.7%	38.1%	39.3%
Cigarettes smoked per day			
Mean consumption (*SD*)	17.78 (7.63)	18.09 (6.88)	17.49 (8.27)
Years one had been smoking			
Mean number of years (*SD*)	25.25 (11.19)	24.81 (11.63)	25.67 (10.78)
Previous quit-attempts			
% Yes	91.7%	92.9%	90.6%
FTND score			
Mean level (*SD*)	4.30 (2.33)	4.54 (2.13)	4.08 (2.48)
Craving			
Mean level (*SD*)	3.034 (.69)	3.05 (.67)	3.02 (.72)
Intention to quit			
Mean level (*SD*)	8.98 (1.30)	8.89 (1.40)	9.06 (1.19)
Attentional control			
Mean level (*SD*)	2.77 (.41)	2.77 (.40)	2.77 (.42)
Attentional bias			
Mean level (*SD*)	3.46 (30.62)	3.45 (31.81)	3.46 (29.53)
Approach bias			
Mean level (*SD*)	45.77 (200.85)	40.08 (200.22)	50.98 (201.75)

Note. ABN = Attentional Bias Modification; FTND = Fagerström Test for Nicotine Dependence.

approach bias was 45.77 ($SD = 200.85$), indicating that the sample of smokers was faster in reacting when the arrow replaced the smoking related pictures and when the smoking related pictures needed to be approached. A t test against zero revealed that the bias differed significantly from zero (attentional bias: $t(428) = 2.34$, $p = .02$; approach bias: $t(413) = 4.64$, $p < .001$). Subgroup analyses showed that this significant bias was only found in the group of heavy smokers (attentional bias: $t(314) = 2.65$, $p = .08$; approach bias: $t(301) = 5.18$, $p < .001$), while the bias was nonsignificant in light to moderate smokers. Attentional bias did not differ significantly between light to moderate and heavy smokers, $t(427) = -1.32$, $p = .19$, while approach bias was significantly stronger among heavy smokers, $t(412) = -2.28$, $p = .02$.

Effects of the ABM training on reducing cognitive biases. The mixed 2 × 2 ANOVA (conducted for trained pictures as well as for untrained pictures) showed no significant main effects of time nor condition on attentional bias and no significant interaction effect condition*time ($p > .15$), indicating no significant changes in attentional bias scores in the whole sample from pre- to post-training. Subsequent exploratory subsample analyses among light to moderate and heavy smokers also revealed no significant effects ($p > .15$).

With regard to approach bias the ANOVA revealed a significant main effect of time in the whole sample and in the subsample of heavy smokers (whole sample: $F(1, 205) = 4.77$, $p = .03$; heavy smokers: $F(1, 138) = 6.70$, $p = .01$, a nonsignificant ($p > .15$) main effect of condition (in the whole sample as well as the subsamples) and a nonsignificant ($p > .15$) condition*time interaction effect (in the whole sample as well as the subsamples).

Continued Abstinence 6 Months After the Pretraining

In the whole sample, the effect of the ABM training (adjusting for the effects of age, gender, FTND score, and craving levels) had a nonsignificant effect on abstinence (complete cases: odds ratio [*OR*] = 1.86; $p = .11$; confidence interval [CI] = 0.87–3.96; conservative analyses: $OR = 1.60$; $p = .14$; CI = 0.86–3.00). The interaction term condition by smokers' group that was added in the second block was significant (complete cases: $OR = .19$; $p = .04$; CI = .04–.95; conservative analyses: $OR = .24$; $p = .04$; CI = .06–.92), indicating that the effect of the training was significantly different between light to moderate and heavy smokers.

Simple slope analyses conducted to explore the subgroup effects showed a significant positive effect of the ABM training on abstinence in heavy smokers (complete case: $OR = 3.15$; $p = .02$; CI = 1.24–7.99; conservative analyses: $OR = 2.49$; $p = .02$; CI = 1.13–5.48), with no significant effects in light to moderate smokers. To assess if the effects were robust to different definitions of heavy smoking, additional subgroup analyses–in which the definition of heavy smoking was based on the median consumption (median = 19 cigarettes per day, with heavy smoking defined as smoking at least 19 cigarettes per day)–were conducted: ABM had a (borderline) significant effect in this subsample of heavy smokers (complete cases: $OR = 3.91$; $p = .02$; CI = 1.20–12.71; conservative analysis: $OR = 2.43$; $p = .07$; CI = .93–6.37), but not in the subsample of light to moderate smokers. Mediation of the behavioral effects by change on implicit cognitive biases was not assessed, because of the lack of significant effects regarding bias reduction. Table 2 presents the abstinence rates, stratified by smokers' group.

Table 2
Abstinence Rates After 6 Months Using Complete Cases as Well as the Whole Sample, and Stratified for Light and Heavy Smokers

Conditions	All smokers		Heavy smokers		Light-moderate smokers	
	N	Abstinent *N* (%)	*N*	Abstinent *N* (%)	*N*	Abstinent *N* (%)
Complete cases						
Control	61	18 (29.5%)	43	10 (23.3%)	18	8 (44.4%)
ABM	74	29 (39.2%)	47	22 (46.8%)	27	7 (25.9%)
χ^2	1.38		5.44		1.67	
p-value	.28		.03		.22	
Conservative analysis						
Control	210	18 (8.6%)	164	10 (6.1%)	46	8 (17.4%)
ABM	224	29 (12.9%)	155	22 (14.2%)	69	7 (10.1%)
χ^2	2.15		5.79		1.28	
p-value	.17		.02		.27	

Note. ABM = Attentional Bias Modification.

Discussion

This study evaluated the effects of six sessions of Web based, self-help ABM-training in supporting smokers with their quit-attempt. Training effects were tested in relation to attentional bias, approach bias (distant generalization) and 6 months continued smoking abstinence. The study further explored the effects of the ABM training in the subsamples of light to moderate smokers and heavy smokers, separately.

First, in line with other studies (for review see Cox et al., 2006), our results demonstrated a statistically significant attentional bias toward smoking-related pictures among smokers. It should be noted, however, that this bias was modest, as it comprised only a couple of milliseconds. However, previous studies have indicated comparable bias levels among heavy smokers (Begh et al., 2015; Waters et al., 2003). While the bias was reliable in the study by Waters et al., it was nonsignificant in the study of Begh et al. The lack of significance in the latter study may be related to the sample size of the study (that was smaller when compared with the study by Waters et al., and our present study). Exploratory subsample analyses further revealed that attentional bias was present in heavy smokers but not in light to moderate smokers. Moreover, the multiple-session ABM training did not result in a significant reduction of attentional bias compared with the control group. This may be related to the low reliability of the dot probe task (Schmukle, 2005), or it could indicate that attentional bias for smoking stimuli is hard to change. The latter is also supported by a recent study on multiple sessions of ABM, which showed no significant effect with regard to attentional bias; this study, however, found no baseline bias (Begh et al., 2015). Another explanation could be related to the design we used. Until now, most smoking-related ABM studies were not Web based but lab-based. One of these studies failed to find a significant decrease in attentional bias when compared with a control group (McHugh, Murray, Hearon, Calkins, & Otto, 2010), while two other studies resulted in a significant decrease in bias (Attwood et al., 2008; Lopes et al., 2014). It should be noted that the studies by McHugh et al. and the study by Attwood et al. used a single session of retraining. Moreover, in the study by Attwood et al., approach training was compared with avoidance training instead of a continued assessment control group; this creates a large difference between the two conditions and increases the possibility of finding an effect on the bias (cf., Wiers et al., 2006). Furthermore, the bias was assessed directly after the training session, while our measurement was at least 18 hr after the start of the last training session. Hence, it is possible that stronger effects would have been found when assessed directly after the training. We did not do this, however, because this assessment could have interfered with the training effect (cf., Eberl et al., 2013; Wiers et al., 2011).

Second, the results indicated that Web based, multiple-session ABM training was ineffective in fostering continued abstinence in the whole sample of smokers, while exploratory subgroup analyses revealed the potential for heavy smokers only (with no significant effects in light to moderate smokers). The overall behavioral findings in our sample are in line with those of Begh and colleagues (2015) who tested the added effect of five sessions of ABM (or control-training) as an adjunct of smoking treatment in 119 heavy smokers and found no effects of ABM on smoking outcomes. Lopes, Pires, and Bizarro (2014) tested 89 smokers in four conditions: (a) no training, (b) three sessions of placebo-training, (c) one ABM session and two placebo-training sessions, and (d) three ABM sessions. The smokers did show an attentional bias for smoking, which was subsequently reduced in a dose-dependent fashion by ABM (longer lasting changes with more active ABM). However, no effects on smoking behavior were found. We note that our study included a larger sample of smokers per condition compared with the latter study, which may have resulted in a stronger statistical power to detect effects in the subsample of heavy smokers. Further, the combined effects are generally in line with moderation hypotheses of CBM, which state that an effect can only be found when there is a bias to begin with, a pattern of results observed in anxiety (Clarke, Notebaert, & MacLeod, 2014; Kuckertz et al., 2014), and in alcoholism (Eberl et al., 2013). From a clinical perspective, it would be tempting to only assign patients to a specific form of CBM when they score high on that particular cognitive bias. However, assessment of these biases is far from suited for this purpose at the moment (see Wiers, Gladwin, & Rinck, 2013b, for further discussion).

Third, our study confirmed that smokers also have a significant approach-bias for cigarette-related stimuli (see for a review Watson et al., 2012). The ABM training had no significant effect on modifying this approach bias, suggesting that ABM does not generalize to an approach bias and that the inclusion of other training strategies such as approach-bias retraining may be needed to ameliorate the clinical outcomes. In this context, a study from the alcohol-domain may be relevant, which found no correlation between attentional bias and approach bias after placebo, but did find a strong correlation after two drinks (Schoenmakers, Wiers, & Field, 2008). Perhaps also in smoking, attentional and approach bias get synchronized only in a craving-induced state and that only then generalized effects from the one training to the other can be reached. Furthermore, the manikin task has—when compared with the Approach Avoidance Task (AAT)—a more explicit character, because respondents are instructed to react on the contents of the pictures, while in the AAT, they react to a contents-irrelevant feature of the picture (e.g., the format, landscape or portrait; Wiers et al., 2009). Although the manikin task has a good reliability (see Wiers et al., 2013b), the AAT task may have yielded different results with regard to approach bias. However, a recent study using a novel task to assess both attentional and approach bias for alcohol, found that both predicted unique variance in drinking (Sharbanee, Stritzke, Wiers, & MacLeod, 2013), adding to the idea that the two processes are distinguishable and should be retrained separately (see also Sharbanee et al., 2014). In alcohol-dependent patients, retraining an approach bias for alcohol has generated positive effects, including increased abstinence 1 year after treatment discharge (Eberl et al., 2013; Wiers, Eberl, Rinck, Becker, & Lindenmeyer, 2011). Note that these effects were on top of clinical "treatment as usual" in an abstinence-oriented program. In the smoking domain, Wittekind and colleagues (2015) found positive effects in comparison with a no-training control group, and Kong and colleagues (2015) found in a placebo-controlled study that adding AAT-training to a cognitive–behavioral therapy program for smoking cessation in adolescents produced better 7-day point prevalence abstinence at statistical trend level, compared with placebo-training. Hence, there are some first hints that AAT-training might also have some effect in smoking cessation, but this has yet be confirmed.

Although the present study yielded interesting results, it is also subject to limitations. First, the many inclusion and exclusion criteria may impede the generalizability of the results (Britton et al., 1998). However, exclusion criteria such as excessive alcohol use, psychological disorders and vision impairments may be necessary, because of their possible influence on RTs. Second, the stimuli used were matched based on visual matching, while more elaborate standardization and validation of the stimuli before task development may have been a more suitable approach, because features of stimuli, such as complexity, can influence the cognitive bias (Miller & Fillmore, 2010). Third, we only used a 500 ms stimulus onset asynchrony in the present study, which does not allow us to differentiate between speeded detection and delayed disengagement. This approach was chosen because Schoenmakers et al. (2010) only found a training effect for 500 ms, and we did not want to make the Internet-based training overly long. Fourth, smoking status was based on self-report, which may have introduced bias. However, the SRNT (*Society for Research on Nicotine & Tobacco*; Benowitz et al., 2002) subcommittee on Biochemical Verification has stated that biological verification is not required in large-scale population-based studies with limited face-to-face contact and studies where the data are collected by mail, telephone, or Internet.

A fifth limitation is that we did not assess whether respondents were aware of the condition to which they were assigned. Previous studies (Beard, Weisberg, & Primack, 2012; Schoenmakers et al., 2010) showed that the large majority of respondents both in the active and in the control ABM conditions think that they have been assigned to a control group, which consequently may impede their motivation to participate. The latter may also be related to the high drop-out rates that also formed a limitation in the present study. However, high drop-out rates are common in Internet trials (Etter, 2005; Eysenbach, 2005; Tate & Zabinski, 2004; Te Poel et al., 2009), and drop-out did not differ between the conditions (indicating that respondents may not have been aware of the condition they were assigned to). Nevertheless, drop-out can bias the results of analyses and future studies on how to prevent drop-out in Internet-trials are recommended. This could be done by adding a more explicit module to the intervention to motivate the participant to plan the quit-attempt and to make coping plans (cf., Wiers et al., 2013a). Alternatively, training may be made more attractive by introducing game-elements (Boendermaker, Prins, & Wiers, 2015). Sixth, the effects in heavy smokers were based on exploratory subsample analyses. Subsample analyses are subject to biases and caution is needed with interpreting the results. Moreover, the sample size of light smokers ($N = 115$) and the heavier smokers ($N = 319$) was unbalanced, which may have influenced the results for instance by means of power issues in the light smokers group. However, the results can help us to direct future research to test the effects of ABM for heavy smokers.

Another limitation is formed by the definition of heavy smoking (at least 15 cigarettes per day), because this definition does not cover all definitions used in the literature (see Schane, Ling, & Glantz, 2010, for a review). Additional analyses in which the definition of heavy smoking was based on the median consumption (at least 19 cigarettes per day) indicated that the effects may be robust to different definitions because they remained significant in the complete case analyses (note that they became of borderline significance in the conservative analyses). More research on the effects in heavy smokers is, however, recommended. Finally, respondents who did not make a quit-attempt were not included in the main behavioral analyses, while analyzing by intention-to-treat entails including all respondents as randomized regardless of whether or not the intervention was received (Heritier, Gebski, & Keech, 2003; Hollis & Campbell, 1999). The latter approach is, however, a conservative approach that may be susceptible to Type II errors, because it also includes respondents who did not receive the intervention (Heritier et al., 2003; Hollis & Campbell, 1999). Therefore, a modified approach has been used more frequently in the last decades (Abraha & Montedori, 2010), because it seems to be more suitable to gain insight into the effects of interventions among those who actually followed them.

In conclusion, this study suggests that Web based multiple-session ABM training is unsuccessful in changing cognitive biases, but that the training may have potential to help heavy smokers who made a quit-attempt to reach continued abstinence. Because the most promising conclusions were based on exploratory subsample analyses, further confirmatory research is needed to draw firm conclusions on the effects of ABM training for heavy smokers. Moreover, challenges for future studies are to gain further insights into how the assessments and training over the Internet can be improved, especially with regard to increasing motivation to participate, to prevent drop-out, and to successfully reduce maladaptive cognitive biases.

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Received March 17, 2015
Revision received December 7, 2015
Accepted December 18, 2015 ■