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Self-Efficacy and Smoking Cessation: A Meta-Analysis

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Abstract

According to relapse models, self-efficacy (SE), or confidence in one's ability to abstain, should predict the outcome of an attempt to quit smoking. We reviewed 54 studies that prospectively examined this relationship. The relationship between SE and future smoking depended upon the population studied and the timing of the SE assessment. The relationship between SE and future smoking was modest when SE was assessed prior to a quit attempt; SE scores were .21 standard deviation units (SD) higher for those not smoking at follow-up than for those who were smoking. The relationship was stronger (.47 SD) when SE was assessed post-quit. However, this effect was diminished when only abstainers at the time of the SE assessment were included in analysis (.28 SD). Controlling for smoking status at the time of SE assessment substantially reduced the relationship between SE and future smoking. Although SE has a reliable association with future abstinence, it is less robust than expected. Many studies may overestimate the relationship by failing to appropriately control for smoking behavior at the time of the SE assessment.

Keywords

self-efficacy; smoking cessation; relapse; meta-analysis

Relapse is still the modal outcome of attempts to quit smoking, even when behavioral and/or pharmacological treatments are utilized (Fiore et al., 2000; Piasecki & Baker, 2001). To improve smoking cessation interventions, it is necessary to better understand the factors that underlie relapse. Abstinence self-efficacy (SE), or confidence in one's ability to abstain from smoking, has received much attention as a predictor and possible determinant of smoking cessation outcome (e.g., Baer & Lichtenstein, 1988; Condiotte & Lichtenstein, 1981; Gwaltney et al., 2001; Gwaltney, Shiffman, Balabanis, & Paty, 2005).

Several social-learning models of relapse posit that low SE is a central cause of relapse to smoking. For example, the 'dynamic regulatory feedback model' (Niaura, 2000; Niaura et al., 1988) suggests that SE acts as a final common pathway to relapse, mediating the effects

of other risk factors on smoking. The Transtheoretical Model suggests that SE increases, along with changes in other constructs, such as perceived temptation and use of self-regulatory strategies, are associated with the likelihood of initiating and maintaining changes in smoking behavior (e.g., DiClemente, Prochaska, & Gilbertini, 1985). The relapse prevention model (Marlatt & Donovan, 2005) hypothesizes that lapses result, in part, from acute decreases in SE when a smoker is exposed to high-risk situations (such as seeing others smoking). SE is also an important component of general models of behavior change, such as the theory of planned behavior (Ajzen, 1991) and the health-belief model (Rosenstock, Strecher, & Becker, 1988). Although the mechanisms linking SE to success in cessation have not been well documented, it is proposed that low SE undermines smokers' ability to initiate or maintain efforts to cope with high-risk situations that may lead to smoking (e.g., negative affect states, exposure to smoking cues) or reduces smokers' likelihood of recovering from lapses once they do occur (Bandura, 1997; Marlatt & Donovan, 2005). Because of the importance assigned to SE in influential models of smoking cessation and relapse, increasing self-efficacy is a goal of most cognitive-behavioral smoking cessation treatments (Abrams et al., 2003; Marlatt & Donovan, 2005).

In most studies of SE and smoking cessation, SE is measured at one point in time and these ratings are used to predict who will and who will not abstain from smoking at some future time point (cessation outcome). A qualitative review of 21 studies examining predictors of long-term (>6 months) cessation outcome found that self-efficacy predicted relapse among both self-quitters and treated smokers (Ockene et al., 2000). As the authors note, "probably one of the most consistent findings across the maintenance phase is the importance of self-efficacy in predicting maintained abstinence or alternatively relapse" (Ockene et al., 2000; p. 20). Although the knowledge that SE is a consistent, statistically significant predictor of cessation outcome is useful, this does not provide information about the magnitude of the relationship, which is essential to evaluating its theoretical importance and clinical utility. In this paper, we use meta-analysis to assess the magnitude of the association between SE and cessation outcome.

Synthesizing the SE literature is complicated by variability in study designs. For example, the SE assessment may be administered while participants are smoking regularly, before the onset of a quit attempt, or it may be administered after a quit attempt. This difference could substantially influence the magnitude of the SE-outcome relationship. SE assessments that are administered after a quit attempt may be more strongly related to cessation outcome, because the smoker can consider how difficult it is to maintain abstinence in the presence of obstacles like craving and withdrawal when making their SE judgments (Baer & Lichtenstein, 1988; Gwaltney et al., 2001). Judgments about SE made before quitting may be influenced by bravado or by unrealistic expectations. For example, Lowenstein (1996) has shown that people have limited ability to project how they will feel or act in circumstances different from their current ones. Additionally, because SE judgments may change over time (Gwaltney et al., 2005), SE assessments that are completed closer in time to outcome assessments may yield stronger associations. If postquit measures are more closely related to outcome, this may have implications for when SE measures should be administered, to maximize prediction of cessation outcome. In this analysis, studies examining prequit SE assessments were examined separately from studies using postquit measures, to account for this important difference in study designs.

Even studies examining an SE assessment administered after a quit attempt include participants who are smoking (because they lapsed or failed to attain initial abstinence). To understand the relationship between SE and *maintenance* of smoking cessation, it is necessary to isolate those participants who are abstinent at the time the SE assessment. Therefore, we also examined these participants in a separate analysis.

The participants' smoking behavior at the time of SE assessment is not just another source of variability, it is an important confound to consider when assessing the SE-cessation outcome relationship: Because smokers can recognize the implication of their failing to be abstinent, SE ratings may be strongly influenced by concurrent smoking, which is itself likely to be a strong predictor of future smoking. Therefore, SE may only reflect, not predict, smoking behavior. If SE were found to simply be a marker for concurrent smoking, it would be of little theoretical or practical interest. Therefore, to isolate the "effect" of SE on cessation outcome, it is important to control for smoking at the time of assessment. Analyses that fail to implement this control may grossly distort the SE-cessation outcome relationship. We hypothesized that studies in which smoking was uncontrolled would yield significantly higher effect sizes than studies in which smoking was controlled.

A wide variety of SE measures have been used in smoking cessation studies. One dimension that differentiates the questionnaires is the number of items. The most commonly used measures incorporate SE ratings across multiple contexts or high-risk situations. For example, Velicer and colleagues assessed SE across 20 contexts that mapped onto Marlatt and Gordon's (1985) relapse situation taxonomy. This resulted in a 3-factor structure, including latent domains of positive affect/social situations, negative affect situations, and habit/addictive (e.g., craving) situations (see also Gwaltney et al., 2001). The factors could be used individually or, as is seen most often in the SE-smoking cessation literature, aggregated to form a single, global score. However, single item measures have also been used (e.g., Brandon, Herzog, Juliano, Irvin, Lazev, & Simmons, 2003; Drobles, Meier, & Tiffany, 1994). From a psychometric standpoint, assessments with more items should be more reliable and exhibit more between-person variability, allowing for stronger correlations with other measures. (The reliability coefficient represents the upper bound of any correlation with other variables.) In addition, multi-item measures may force smokers to consider specific high-risk contexts when making SE judgments, which may increase the accuracy of these judgments, yielding stronger relationships with outcome. This information could also be useful clinically: If multi-item assessments are more accurate predictors of outcome, the extra time and effort needed to complete them would be warranted.

The purpose of the present study was to conduct a systematic review of studies using SE to prospectively predict smoking cessation outcome. Meta-analysis was used to establish a quantitative index of the relationship between SE and cessation outcome. In general, we hypothesized that individuals who ultimately quit smoking would have higher baseline SE than those who fail to quit smoking. As the relationship may be influenced by the timing of the SE assessment, we examined prequit and postquit assessments separately. Additionally, the relationship between SE and cessation outcome among individuals who achieved initial abstinence was highlighted. We hypothesized that the relationship between SE and outcome would be substantially inflated by the inclusion of studies that did not control for concurrent smoking at the time of the SE assessment. Other moderators (length of time between SE assessment and outcome assessment, number of items) were also expected to influence the SE-outcome association, but less strongly than concurrent smoking. By quantifying the magnitude of the association between SE and cessation outcome, it may be possible to gauge more accurately the importance of assessing and enhancing SE in smoking cessation treatments.

Method

Identification and Selection of Studies

We conducted a computerized literature review using the PsycInfo (1887–2005) and Medline (1966–2005) databases. In the literature review, we located articles by crossing the keywords *self-efficacy* or *self-confidence* with *smoking* or *smokers* and *relapse* or

abstinence or outcome or cessation. A manual search was also performed by scanning reference lists of reviewed articles, to identify other articles that were not selected in our computerized search. In sum, the literature review returned 253 articles.

The criteria for inclusion in the meta-analysis were: (a) publication in English and (b) SE used to prospectively predict smoking cessation. We excluded nonpeer reviewed studies (book chapters, dissertations) and studies that claimed to assess SE, but actually measured another construct, such as intention to quit smoking. Further, we did not include analyses using change scores (e.g., change in SE during treatment) to predict cessation outcome. Of the 253 studies located, 54 were included in the meta-analysis, based on independent assessment by CG and JM and resolution of disagreements through discussion. Several studies used both global SE scores and situation-specific SE factor scores (e.g., SE in negative affect contexts, in social contexts). When both types were included and the global score was a composite of the situation-specific scores, only the global score was included in analysis. If the global score was independent of the situation-specific measures, both were included. Five of the reviewed publications met inclusion criteria, but lacked enough information to allow for determination of the effect size (e.g., no measure of SE variability); in two of these cases, contact with the study authors yielded the required information.

Study Characteristics Coded

Timing of the SE assessment (prequit vs. postquit) was coded and used to group the studies and the individual analyses extracted from each study. Additionally, the following study characteristics were coded for use as moderators in analysis.

Smoking status at the time of SE assessment—(a) Smokers included, with no statistical control for smoking rate or status; (b) Smokers included, but control for smoking rate or status; (c) Abstainers only included. These codes reflect smoking status at the time of the SE assessment, not the outcome assessment. For example, the SE assessment may be administered 1 day after the initiation of the quit attempt and include only those individuals who were abstinent on that day.

Number of days between the SE assessment and the outcome assessment—Coded as a continuous variable.

Number of items included in the SE assessment—(a) Single item; (b) Multiple items.

CG and JM independently coded and then jointly reviewed all of the studies included in the meta-analysis. Discrepancies were resolved through discussion and review of the study. Inter-rater reliability was calculated based on ratings before joint review. However, as all studies were jointly reviewed and discussed, reliability is mostly useful as an indicator of the level of difficulty involved in extracting each code from the studies. For the continuous effect size indices (e.g., means, standard deviations, correlations), the intraclass correlation (ICC) between the ratings was 0.99. The average ICC for sample size codes (total, abstainers, smokers) was 0.96. The ICC for number of days between the SE assessment and the outcome assessment was .99. For the categorical moderators, kappa was used as a measure of agreement. The weighted kappa for smoking status at the time of the SE assessment was .86, for the timing of the SE assessment it was .84, and for the number of items included it was .90. Each of these values suggests excellent agreement between the raters.

Computation and Analysis of Effect Sizes

For ease of interpretation, we used the standardized mean difference statistic, d , as the effect size (ES; Cohen, 1988) estimate.¹ In this context, d is interpreted as the standardized difference in baseline SE scores between individuals who are smoking at follow-up vs. those who are not smoking at follow-up.² A negative d indicates higher baseline self-efficacy scores for those who are not smoking at follow-up. For example, a d of -0.50 indicates that nonsmokers scored .5 standard deviation units higher on the baseline SE measure than smokers. If group means and standard deviations for smoker and nonsmoker groups were not available, the ES estimate was calculated directly from t test, chi-square, odds ratios, or correlation statistics using the Comprehensive Meta-Analysis software, Version 2.2. In total, we coded 189 analyses from the 54 papers. If study results were described as nonsignificant and not enough information was reported to calculate an ES ($n=33$; 17% of analyses), a conservative ES of zero was assigned (Cooper, 1998; Rosenthal & DiMatteo, 2001).³ To ensure that ESs in pooled estimates were independent, ESs from multiple analyses within one study (e.g., multiple follow-up points, multiple SE measures) were averaged to the study level.

Analyses using a prequit SE measure and analyses using a postquit measure were analyzed separately. As a single study could include multiple analyses of SE and outcome, it was possible for a study to contribute to both subgroups. Within each subgroup, the summary ES (d_s) was calculated as a weighted linear combination of study ESs, weighted by sample size (Hall & Rosenthal, 1995). A d_s of .20 indicates a small ES, a d_s of .50 indicates a medium effect, and a d_s of .80 indicates a large effect. A random effects model was used to calculate the pooled ES estimate, while a mixed effects model was used to examine the influence of moderators on the ESs (where the moderator is treated as a fixed effect). In contrast to a fixed effect model, a more conservative random effects model incorporates an estimate of between-studies variability into error variance estimates in addition to the within-study variance. As a result, findings are more generalizable to the population of studies that examine the self-efficacy-smoking cessation relationship and use potentially different designs, outcome measures, and other parameters than the original studies included in this meta-analysis (Hedges & Vevea, 1998; Rosenthal & DiMatteo, 2001).

Heterogeneity among the ESs was assessed using the Q_w statistic. To examine possible publication bias, a funnel plot, which plots a measure of sample size (standard error, in this case) against the study ES, was reviewed (Light & Pillemer, 1984). If there is no publication bias, the ESs should be distributed evenly around the pooled ES. Publication bias or the 'file drawer effect' is indicated when the ESs from the larger studies are symmetrically distributed around the pooled ES, but the individual ESs from smaller studies are not. This may indicate that studies with smaller sample sizes are more likely to be published if they have larger than average effects, which makes them more likely to be statistically significant.

We next examined whether the moderators influenced the magnitude of the pooled ES. For categorical moderators, we calculated ESs at each level of the moderator and assessed their equivalence using the Q_b statistic.⁴ For number of days between the SE and outcome

¹SE is typically not experimentally manipulated in smoking cessation studies; its relationship with outcome is based almost exclusively on correlational designs. In using the term "effect size" here, we only refer to a summary index of the relationship between SE and cessation outcome; we do not imply any causal relationship between SE and relapse.

²We define nonsmokers as participants who are either abstinent or not relapsed at the time of follow-up assessment.

³The ES could be calculated for several nonsignificant analyses ($n=54$). We also substituted the average ES from these analyses ($r=-.06$) in nonsignificant analyses where the ES could not be calculated, in addition to substituting an ES of 0. This strategy produced similar results.

assessments, we ran a meta-regression analysis to determine if this continuous moderating variable was linearly related to the ES (Rosenthal, 1991).

Results

Study Characteristics

Characteristics of all analyses included in the meta-analysis are summarized in Table 1. The number of coded analyses in each study ranged from 1 to 26, with a mean of 3.5 ± 4.4 analyses per study. On average, 40% of participants were not smoking at follow-up in each study. To assess floor and ceiling effects, we divided the average SE scores by the scale range. The resulting ‘percent of scale range’ score is close to 100 when the average scores approximate the maximum score possible and close to 0 when the average scores approximate the minimum score possible. The average score was 60% of the scale range suggesting a moderately high level of SE. Thus, there was little evidence for floor or ceiling effects. Smoking cessation treatment was provided in 72% of the studies (either all participants received treatment or some received active treatment and others received control treatment). The populations used in the studies varied, but most commonly included adults seeking smoking cessation treatment. Complete abstinence (either point prevalence or continuous) was the most commonly used outcome.⁵ ESs for each study are shown in Figure 1.

Comparisons of Prequit and Postquit SE Assessments

SE assessments completed after the onset of a quit attempt ($d_+ = -.47$) were more strongly associated with outcome than assessments completed before the quit attempt ($d_+ = -.21$), $Q_b(1) = 13.5$, $p < .001$. This supports the decision to analyze these two groups of studies and analyses separately.

Analyses of Prequit SE Assessment

There were 87 analyses examining a prequit SE assessment (Table 1). The ES for assessments completed before the quit attempt was $d_+ = -0.21$, $SE = .04$, 95% CI = -0.28 through -0.14 . In other words, those who were not smoking at follow-up scored .21 standard deviation units higher on the baseline SE measure than smokers. According to Cohen’s guidelines, this is a small effect. Significant heterogeneity was observed among the analyses, $Q_w(33) = 65.6$, $p < .01$. Two outliers (encompassing three analyses; Schnoll et al., 2003; Woodby et al., 1999) with standardized residuals—defined as the standardized difference between d_i and d_+ with d_i omitted from d_+ —greater than 2.0 (Hedges & Olkin, 1985) were dropped from analysis. Subsequently, the heterogeneity among the estimates dropped to a nonsignificant level, $Q_w(31) = 40.2$, ns. The overall ES estimate was attenuated, but still significantly different from zero, $d_+ = -0.17$, $SE = .03$, 95% CI = -0.23 through -0.11 .

After removing the outliers, we examined a funnel plot of the ESs, which plots the standard error associated with each study (reflecting the sample size) against the ES for each study. In the absence of publication bias, the ESs should be distributed evenly around the pooled ES. The funnel plot suggests little evidence of publication bias among the prequit studies (Figure 2A). However, to further examine the effect of any possible bias on the observed results, a

⁴Studies may contribute multiple data points to the moderator analyses. For example, a study may have single- and multiple-item SE assessments and use these measures to predict cessation outcome. Therefore, the same study may be represented in multiple subgroups in the moderator analyses.

⁵In 13 of the 189 analyses examined, smoking rate or % of baseline smoking rate was used as the dependent variable. The ES for these analyses did not significantly differ from the ES for other analyses, $Q_b(1) = 2.8$, ns. Therefore, we retained them in the meta-analysis.

fail-safe N was calculated (Rosenthal, 1979; Orwin, 1983). It would require 365 missing studies with an ES of 0 for the pooled ES to be reduced to a level not significantly different from 0.

Concurrent smoking effects—We divided the sample of analyses using prequit SE measures according to how smoking at the time of the SE assessment was treated in analysis. By definition, prequit measures were administered while participants were smoking. Therefore, analyses that controlled for smoking rate were contrasted to those that did not. Controlling for smoking rate significantly moderated the relationship between SE and outcome, $Q_b(1) = 12.0, p < .01$. As expected, the strongest relationship between SE and outcome was observed in analyses where smoking rate at the time of the SE assessment was uncontrolled in analysis, $d_+ = -0.26, SE = .04, 95\% CI = -0.35$ through -0.18 . The magnitude of the ES, though still significantly different from 0, was substantially reduced when concurrent smoking rate at time of assessment was statistically controlled ($d_+ = -0.09, SE = .03, 95\% CI = -0.14$ through -0.04). Controlling for smoking partially explained the variability among the ES estimates. In analyses that statistically controlled for concurrent smoking there was no significant heterogeneity among the ES, $Q_w(12) = 12.5, ns$. (Indeed, the two outlier studies from the global prequit analysis did not control for smoking.) However, there was significant heterogeneity observed among analyses that did not control for smoking, $Q_w(26) = 48.7, p < .01$.

Time between assessment and outcome—There was no significant relationship between the ES estimate and length of time between the SE and outcome assessments, change in $d_+ = -.00005, SE = .00004, ns$.

Number of items—The relationship between the number of items included in the SE assessment and the magnitude of the ES was not significant, but reached trend level, $Q_b(1) = 3.8, p < .06$. The pooled ES for analyses using a single item measure was $d_+ = -0.31, SE = .08, 95\% CI = -0.46$ through -0.16 , and for multiple item measures, the ES was $d_+ = -0.15, SE = .04, 95\% CI = -0.22$ through -0.08 . If anything, the ES was stronger for single-item measures. There was significant heterogeneity among the ESs from single-item measures, $Q_w(9) = 22.2, p < .01$, but not with the multi-item measures, $Q_w(24) = 34.0, ns$.

Analyses of Postquit Day SE Assessment

There were 105 analyses examining a postquit day SE assessment (Table 1). The ES for assessments completed after a quit attempt was $d_+ = -0.47, SE = .06, 95\% CI = -0.59$ through $-0.35, p < .0001$. According to Cohen's guidelines, this is a medium effect. Those who were not smoking at follow-up had SE scores that were .47 standard deviation units higher at baseline than those who were smoking at follow-up. Significant heterogeneity was observed among the analyses using a postquit assessment, $Q_w(30) = 129.5, p < .001$. Removing three outlier studies (encompassing four analyses; Borrelli & Mermelstein, 1994; Condiotte & Lichtenstein, 1981; Sperry & Nicki, 1991) with standardized residuals greater than two greatly reduced the variability, but it was still statistically significant, $Q_w(27) = 70.2, p < .001$. The overall ES estimate was attenuated, but still significantly different from zero, $d_+ = -0.37, SE = .05, 95\% CI = -0.46$ through -0.27 .

A funnel plot of the ESs and standard errors did suggest some level of publication bias: The ES increased noticeably as the standard error increased (Figure 2B). Because of the evidence for publication bias, we used the trim-and-fill procedure to estimate the ES of potentially missing studies and then recalculated the pooled ES. The estimated ES when these hypothetical 'studies' were included was dramatically reduced, but statistically significant, $d_+ = -0.27, 95\% CI = -0.37$ through -0.17 , suggesting that the association of ES and

outcome is not entirely because of publication bias. To further examine the effect of this possible bias on the observed results, a fail-safe N was calculated (Rosenthal, 1979; Orwin, 1983). It would require 930 missing studies with an ES of 0 for the pooled ES to be reduced to a level not significantly different from 0. However, only 11 missing studies with an ES of 0 would be required to reduce the ES below $-.20$, the cutoff for a small ES (and roughly the ES observed in the prequit studies).

Abstinent smokers—Only three studies statistically controlled for smoking at the time of SE assessment. Therefore, they were excluded from analysis. Studies that did not control for concurrent smoking ($n = 15$ studies) were contrasted to studies including only those abstinent at the time of SE assessment ($n = 20$). Smoking status significantly moderated the relationship between SE and outcome, $Q_b(1) = 15.0, p < .001$. The ES was dramatically stronger among studies that did not control for smoking: The pooled ES for uncontrolled studies was $d_+ = -0.79, SE = .12, 95\% CI = -1.03$ through -0.55 and the ES for studies using abstinent smokers was $d_+ = -0.28, SE = .05, 95\% CI = -0.37$ through -0.18 . Substantial heterogeneity was still observed among the ES estimates, even after splitting them into subcategories based on smoking status: no control for smoking status, $Q_w(14) = 69.0, p < .001$, abstinent only, $Q_w(14) = 38.0, p < .01$.

Time between assessment and outcome—We next examined the length of time between the completion of the SE assessment and the outcome assessment as a moderator. There was a significant relationship between the ES estimate and length of time between the SE and outcome assessments, change in $d_+ = -.001, SE = .0002, p < .001$. The SE-outcome relationship decreased by .001 with each additional day between the SE and outcome assessments or about .10 for every 100 additional days.

Number of items—The relationship between the number of items included in the SE assessment the magnitude of the ES was not significant, $Q_b(1) = 1.0, ns$.

Discussion

Abstinence self-efficacy has been widely studied as a predictor of smoking cessation outcome and is generally considered to be an important mechanism through which abstinence is achieved and maintained (e.g., Marlatt & Donovan, 2005; Ockene et al., 2000). In this meta-analysis, we calculated a quantitative index of the relationship between SE and outcome from a large published literature. As hypothesized, the average ES indicated that individuals who abstain from smoking report significantly higher SE at baseline than those individuals who resume or continue smoking. This finding supports social-learning theories of smoking cessation, (e.g., Witkiewicz & Marlatt, 2004; Niaura, 2000). However, the magnitude of the relationship was much smaller and more unstable than expected.

The most striking finding in the meta-analysis was the importance of controlling for concurrent smoking behavior when examining the SE-outcome relationship. Analyses in which smoking was uncontrolled produced substantially stronger ES estimates than analyses where only abstinent participants were included or smoking was controlled in analysis. This was particularly true in the postquit analyses. The relationship between SE and outcome appears large in magnitude (according to Cohen guidelines) when smoking is uncontrolled. However, when only abstinent smokers are included, the relationship drops to small-to-medium in magnitude. When smoking is uncontrolled, SE may simply be serving as a marker for lapsing, which is already known to be a strong predictor of subsequent relapse. It is not surprising that smokers who have already lapsed (or failed to quit in the first place) would have lower SE, recognizing the relevance of their current “failure” to future prospects. Analyses of abstinent smokers likely provide the purest assessment of the

relationship between SE and maintenance of cessation. These analyses suggest that the relationship between SE and subsequent smoking is reliable and statistically significant, but small, accounting for only 2% of the variance in outcome (d_+ of $-0.28 = r$ of $-.14 = R^2$ of .02).

The relationship between SE and outcome was substantially lower among studies examining a prequit SE measure. This was particularly true when analyses that failed to control for concurrent smoking rate were excluded. In fact, the relationship did not even reach the level of a small effect when these ‘uncontrolled’ analyses were excluded. It is possible that as the quit attempt commences, smokers gain a better understanding of the challenges they face (e.g., craving, withdrawal symptoms, exposure to smoking cues), as well as the resources available to them for maintaining abstinence (e.g., coping skills, social support, effect of pharmacotherapy), leading to more informed and accurate SE judgments (Loewenstein, 1996). In any event, these results suggest that, to maximize predictive validity, SE measures should be administered after the quit attempt has begun. Prequit measures, after accounting for concurrent smoking, may add little to the prediction of future smoking behavior. Even among the postquit analyses, however, the relationship between SE and outcome was only small to medium in magnitude.

We hypothesized that latency between the SE and outcome assessments would be inversely associated with the ES (shorter latency = larger ES). This pattern emerged only among the analyses using postquit measures. This supports the theory that SE judgments vary over time and, therefore, should predict proximal behavior better than distal behavior. Latency may only influence the relationship during the postquit period, because, as previously mentioned, SE is changing in response to challenges to abstinence and perceptions of coping resources. We have previously demonstrated that SE judgments after a quit attempt decrease in response to negative affect, craving, and smoking cues, even over the course of hours (Gwaltney, Shiffman, & Sayette, 2005).

The inclusion of multiple items in an SE assessment was expected to increase the reliability of the measure, which should have resulted in an increased ability to predict outcome. Additionally, because multiple-item measures explicitly cue the respondent to think about specific high-risk situations, they were expected to increase the accuracy of self-efficacy judgments, which would have strengthened the relationship with outcome. However, the number of items included in the SE assessment did not moderate the relationship between SE and cessation outcome. Indeed, the trend was for multi-item scales to demonstrate poorer predictive utility. By implicitly equally weighting smokers’ confidence in abstaining across a large range of situations, the aggregate multi-item SE measures may fail to give extra weight to situational contexts that are particularly important. Additionally, context-specific self-efficacy judgments load on a single higher-order general factor (Gwaltney et al., 2001; Velicer, DiClemente, Rossi, & Prochaska, 1990) suggesting that judgments can be aggregated into a single ‘global’ index. In this case, a single self-efficacy response would be just as accurate an estimate of SE as would responses to multiple items. Further, although single-item measures do not ask about specific high-risk contexts, they do typically ask about confidence in ability to abstain for a period of time (e.g., 24 hours, 6 months, 1 year; e.g., Drobles, Meier, & Tiffany, 1994). Asking about a period of time may capture relevant aspects of self-confidence that can not be captured in assessments that ask about high-risk situations. Thus, the added benefit of asking multiple items may be negligible in predicting relapse. (However, multi-dimensional measures can be used to predict the contexts that will be associated with lapses and, therefore, retain clinical value; Gwaltney et al., 2002.)

The meta-analysis had several limitations. First, the analysis was based on studies using correlational designs. Therefore, even observing a significant association, we can not

conclude that SE exerts a causal effect on cessation outcome. Until studies are completed where SE is experimentally manipulated, causality can not be assumed. Next, substantial heterogeneity was evident among the ESs. Although the moderators accounted for some of this variance, much of it remains unexplained. This additional variability may be because of unidentified moderators or could simply be because of the large number of studies included in analysis (the power to detect significant heterogeneity is increased with more studies). In any event, there is no reason to believe that the unexplained heterogeneity should temper the overall conclusions drawn from the meta-analysis. Even when outlier ESs were dropped from analysis, the pooled ES was still significant and of a similar magnitude to the full complement of analyses. Finally, in several studies, SE measures were completed after the administration of some form of smoking cessation treatment. SE ratings may have been influenced by the type of treatment received. For example, SE ratings may have generally been higher among participants receiving some sort of active treatment than those receiving a control treatment. However, SE should be related to outcome, regardless of what type of treatment was received. In fact, SE would be expected to mediate any treatment effects (Bandura, 1997). Therefore, it is unlikely that this would substantially influence the observed results.

Conclusions

In cognitive-behavioral models, self-efficacy is assumed to play a central role in smoking cessation. This meta-analysis of over 50 studies challenges this assumption. Although the relationship between SE and outcome is significantly different from 0, it is surprisingly small. When stronger associations are observed, they tend to be found in studies that confound SE with concurrent smoking behavior, such that SE may reflect, rather than predict, relapse. These findings contradict previous qualitative reviews of the literature that suggested a consistent, robust association between SE and cessation outcome (Ockene et al., 2000).

These results may have implications for smoking cessation treatments. In particular, the results raise questions about the common practice of targeting SE in cognitive-behavioral treatments (e.g., Abrams et al., 2003). If SE and cessation outcome are only modestly related, it is unlikely that increasing SE in treatment will have a substantial impact on the odds of successfully quitting. In other words, if there is only a small relationship between SE and outcome, changing SE can not have a robust association with cessation. In addition, it is unlikely that individual differences in SE will be useful as a treatment-matching tool, i.e., in triaging patients into interventions of different types or intensities. (However, situation-based SE measures may provide insight into individuals' relapse risk in particular high-risk situations, which can then be addressed in treatment; Gwaltney et al., 2002; Velicer et al., 1990).

The literature reviewed here provides only limited support for the role of stable, almost trait-like individual differences in SE judgments to predict outcomes over long periods. However, some of the most important models of the role of SE in abstinence and relapse (e.g., Marlatt & Donovan, 2005; Niaura, 2000) actually focus on the more acute and immediate role of SE in particular situations, and thus on the role of within-person variation in SE (Gwaltney, Shiffman, Balabanis, & Paty, 2005). Indeed, research has shown that SE varies over time, in response to relevant events, such as smoking lapses or increases in smoking (Shiffman et al., 1997), and also in response to situational circumstances, such as present mood and craving intensity (Gwaltney, Shiffman, & Say-ette, 2005). It seems important to study how these within-person changes in SE may moderate the immediate risk of smoking and how they are, in turn, affected by episodes of smoking during the progression from lapse to relapse.

Given the extant literature on SE, it is unlikely that additional research assessing the linear relationship between individual differences in SE and smoking outcomes, assessed weeks or months later, will add much to the existing knowledge base. However, further exploration of possible curvilinear associations may be informative (“overconfidence”; Haaga & Stewart, 1992; Staring & Breteler, 2004). It would also be useful to explore novel aspects of the SE-smoking relationship, such as the determinants of SE judgments (Gwaltney, Shiffman, & Sayette, 2005; Shadel & Cervone, 2006). Alternative measures, such as the articulated thoughts method (Haaga, Davison, & McDermut, 1993; Dijkstra & Wolde, 2005), may also shed new light on SE. These types of novel study designs and measures may further illuminate the relationship between SE and outcome, which could potentially reveal new opportunities to enhance smoking cessation interventions.

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Figure 1.

Effect Size (d) estimates for individual studies. Effect sizes below 0 indicate that those who were not smoking at follow-up had higher SE at baseline than those who were smoking at follow-up. Asterisks indicate studies with outlier effect sizes.

A

B

Figure 2.

Funnel plot of effect sizes for (A) prequit analyses and (B) postquit analyses. Empty circles indicate observed studies, dark circles indicate imputed effect sizes, correcting for publication bias.

Table 1

Table of Studies Included in Meta-Analysis

Authors	No. of analyses	Sample size ^a	Timing of assessment			Smoking status at time of assessment			No. of items		% Not smoking at follow-up ^b	Latency from assessment to outcome (days) ^c
			Prequit	Postquit	No con	Stat con	Abs	1	> 1			
Abrams et al., 2000	4	1527	4	0	2	2	0	4	0	NI	548	
Baer et al., 1986	26	110	0	26	14	8	4	0	26	43	88	
Bennett et al., 1999	2	37	2	0	1	1	0	2	0	NI	90	
Borland & Balmford, 2005	1	195	0	1	1	0	0	0	1	70	21	
Borrelli & Mermelstein, 1994	2	122	0	2	1	1	0	2	0	52	11	
Borrelli et al., 2002	1	281	1	0	0	1	0	0	1	26	84	
Brandon et al., 2003	1	96	0	1	0	0	0	1	0	28	365	
Carey & Carey, 1993	1	146	1	0	1	0	0	0	1	30	365	
Carey et al., 1993	1	269	1	0	1	0	0	0	1	17	365	
Christie & Etter, 2005	2	44	0	2	0	0	2	0	2	25	37	
Cinciripini et al., 2003	6	121	1	5	6	0	0	0	6	NI	166	
Coelho, 1984	6	62	3	3	5	0	1	0	6	40	76	
Condiotte & Lichtenstein, 1981	1	78	0	1	1	0	0	0	1	44	90	
DiClemente et al., 1985	1	99	0	1	0	0	1	0	1	64	120	
DiClemente, 1981	1	63	0	1	1	0	0	0	1	67	122	
Digiusto & Bird, 1995	4	137	4	0	2	2	0	0	4	56	NI	
Dijkstra & Borland, 2003	2	324	0	2	0	0	2	0	2	76	210	
Dijkstra & Brosschot, 2003	2	337	1	1	0	1	1	0	2	NI	240	
Dijkstra & Devries, 2000	2	605	2	0	2	0	0	0	2	11	NI	
Dijkstra & Wolde, 2005	1	308	0	1	0	0	1	0	1	6	210	
Domelas et al., 2006	1	105	1	0	1	0	0	0	1	19	NI	
Drobes et al., 1994	6	49	0	6	0	0	6	6	0	35	358	
Gritz et al., 1992	4	1251	4	0	0	4	0	4	0	14	501	
Gulliver et al., 1995	10	247	6	4	0	6	4	0	10	72	62	
Gwaltney et al., 2001	6	222	4	2	3	3	0	0	6	NI	NI	
Gwaltney et al., 2005	3	305	3	0	2	1	0	0	3	41	44	

Authors	No. of analyses	Sample size ^a	Timing of assessment			Smoking status at time of assessment			No. of items		% Not smoking at follow-up ^b	Latency from assessment to outcome (days) ^c
			Prequit	Postquit		No con	Stat con	Abs	1	> 1		
Haaga et al., 1993	1	93	0	1		0	0	1	0	1	NI	365
Haaga, 1989	4	88	0	4		0	0	4	0	4	25	228
Haaga, 1990	2	96	0	2		0	0	2	0	2	25	228
Johnson et al., 2000	3	251	3	0		3	0	0	0	3	NI	180
Kavanagh et al., 1993	2	56	0	2		2	0	0	0	2	34	195
Lipkus et al., 1999	2	160	2	0		2	0	0	2	0	22	NI
McBride et al., 1992	3	89	0	3		1	0	2	1	2	74	161
McIntyre et al., 1983	18	55	6	12		15	0	3	0	18	38	166
Mothersill et al., 1988	4	333	4	0		4	0	0	4	0	21	NI
Mudde et al., 1995	10	95	4	6		6	0	4	5	5	49	107
Mullen et al., 1999	6	277	0	6		0	0	6	0	6	78	163
Nevid & Javier, 1995	3	93	3	0		3	0	0	0	3	10	NI
Niaura et al., 2002	1	76	1	0		0	1	0	1	0	21	150
Nicki et al., 1984	10	46	8	2		10	0	0	0	10	NI	91
Ockene et al., 1992	2	206	2	0		0	2	0	0	2	NI	273
Schmitz et al., 1993	1	26	1	0		1	0	0	1	0	50	132
Schnoll et al., 2003	2	26	1	1		1	0	1	0	2	51	90
Scholte & Breteler, 1997	2	133	1	1		1	0	1	0	2	44	383
Sperry & Nicki, 1991	1	48	0	1		1	0	0	0	1	15	150
Staring & Breteler, 2004	4	369	2	2		4	0	0	0	4	20	386
Strecher et al., 1985	1	119	1	0		0	1	0	0	1	NI	NI
Stuart et al., 1994	3	118	2	1		2	0	1	0	3	60	139
Tucker et al., 2002	3	381	3	0		2	1	0	0	3	15	1825
Valois et al., 1996	2	58	2	0		2	0	0	2	0	17	NI
Walker & Franzini, 1985	1	64	1	0		1	0	0	0	1	23	NI
Wiggers et al., 2005	2	115	0	2		2	0	0	0	2	40	49
Wojcik, 1988	2	75	NI	NI		2	0	0	0	2	39	90
Woodby et al., 1999	2	435	2	0		2	0	0	2	0	25	NI

Note. No con = Includes smokers, no control for smoking rate; Stat con = Include smokers, statistically control for smoking rate; Abs = Abstinent only; NI = Not indicated in study.

- ^a Average sample size when multiple analyses are included.
- ^b Average percent when multiple analyses are included.
- ^c Average latency when multiple analyses are included.