Meta-Analysis of the Efficacy of Tobacco Counseling by Health Care Providers

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Abstract

Given the proportion of American adults who smoke, even if health professionals only have a small effect on quit rates, the public health impact of this change could potentially be enormous. Yet, health care providers may differ in their cessation efficacy. The purpose of this study was to evaluate recent rigorous trials of smoking cessation counseling among physicians, nurses, dentists, and teams of providers: (1) to compare providers on the efficacy of cessation and (2) to determine which intervention and study characteristics explain variations in intervention effects. Thirty-seven randomized clinical trials or quasi-experiments (with control groups) of health care provider–delivered smoking cessation interventions, out of over 200 articles that were published between 1990 and 2004 were collected through searches of Medline, CINAHL, PSYCINFO, and dissertation abstracts, as well as hand searches. The outcome modeled was the mean difference between intervention and control groups in the cessation rates using Hedges’ g. The univariate results revealed that receiving advice from any health care professional produced increases in quit rates. Multivariate analyses of intervention effects on cessation revealed that physicians were most effective, followed by multiprovider teams, dentists, and nurses. The findings suggest that contact with a health care professional will increase cessation; however, additional training in tobacco control for nurses is warranted. Longer-term studies of smoking cessation, particularly among dentists, are necessary. (Cancer Epidemiol Biomarkers Prev 2004;13(12):2012–22)

Introduction

Tobacco-related disease is one of the leading preventable causes of death in the United States. In 1999, 26% of American men and 22% of women smoked (1). Smoking cessation has led to declines in the rates of cancer deaths and coronary heart disease. As 70% of Americans visit a health care provider every year (2), physicians, nurses, and dentists have much potential for contact with U.S. smokers. Currently, about 70% of physicians report advising their patients to stop smoking (3). Given the proportion of American adults who smoke, even if health professionals only have a small effect on quit rates, the public health impact of this change could potentially be enormous.

Whereas several reviews have evaluated the effectiveness of cessation practices within professional groups, by physicians, nurses, or physician/nurse teams, no meta-analysis has yet systematically and directly compared these groups on cessation outcomes. Few individual studies have compared the effectiveness of health care professionals to one another. Furthermore, none has yet evaluated the relative effectiveness of varied types of clinicians, across diverse disciplines and settings, whereas accounting for the intensity of the intervention. These several questions were left unanswered by the influential clinical practice guideline, Treating Tobacco Use and Dependence (4). Because health care professionals each have critical influences on smoking cessation among their patients, elucidating their relative efficacy would assist in policy and management decisions about how to maximize cessation within a multiprovider facility, as well as how to identify underutilized providers. The purpose of this study was therefore to compare the efficacy of recent rigorous trials of smoking cessation counseling among physicians, nurses, dentists, and teams of providers, using a meta-analysis.

The majority of health care providers report inquiring about patients’ smoking status and recording smoking status on the medical chart, although these practices vary by the patient’s age (5). Anywhere between 34% and 74% (6, 7) of patients have reported receiving cessation advice from their providers. Depending on the setting, 45% to 81% of physicians (8-10), 36% to 71% of nurses (11, 12), and 51% to 61% of dentists (13, 14) report having advised patients to quit, although fewer report assisting patients in cessation or arranging follow-up (15, 16). Factors associated with receiving counseling include white race, male gender, being healthier, younger, and having insurance (17, 18). A recent survey of 1,400 smokers found that over one half welcomed physician advice and stated that it would have a strong influence on their decision to quit (19). Primary care physicians and obstetricians-gynecologists report higher rates of counseling than do pediatricians (20). Research has
found that counseling is done most often during new patient visits, with younger patients, and by physicians who have been in practice for fewer years (4).

Surveys of medical professionals have evaluated their receptivity to providing tobacco cessation counseling. One recent report found that two thirds of nurses believe that it is their obligation to provide cessation advice (21); surveys of dentists have found similar results (22, 23). Some of the common barriers cited by health care professionals were time (24), greater perceived complexity of a smoking cessation protocol (25), and confidence in ability to counsel in this area (26). Other factors related to the provision of counseling include educational level (e.g., year of residency; ref. 10), having received training in cessation counseling (27), whether the provider smoked (26), the provider’s race/ethnicity (26), and the strength of the provider-patient relationship (28). A recent report suggested that providing feedback to physicians improves their motivation to counsel patients in cessation (29).

Training programs have been designed to improve medical professionals’ skills and self-efficacy in this area. In the short term, they have been effective in improving providers’ particularly physicians’ confidence and perceived effectiveness, and increasing rates of asking, advising, and providing self-help materials for cessation (30). Openness to providing counseling can be influenced by the provider’s smoking status, and if the provider does smoke, by his/her desire to quit (31).

Several meta-analyses and literature reviews have assessed the efficacy of smoking cessation advice from varied health care professionals. A 1988 meta-analysis by Kottke et al. (32) of physician-based cessation methods determined that important factors in successful cessation were in-person advice, support from several members of the medical staff including physicians and other personnel, and the number and duration of sessions. A later review by Ockene and Zapka (33) found that interventions by physicians do significantly reduce tobacco use, and that there is a direct correlation between the duration of the intervention and patient quit rates. They also found that using additional modalities, such as nicotine replacement or other support services, was related to greater cessation rates. A review of nursing interventions by Rice (34) found a small but significant effect on cessation rates. Rice found greater quit rates among hospitalized patients than among outpatients, particularly among those with cardiac disease. Although the literature on the effects of cessation counseling among dental care professionals is sparse, review articles on smoking cessation in dental practices (35, 36) and oral cancer (37, 38) have suggested that it may be effective in symptomatic populations (e.g., those with premalignant oral lesions) as well.

The USPHS Guideline summarized the effects of physicians’ counseling on tobacco cessation, finding them more effective than any other professional group alone (4). Two clinicians were the most effective, however (4). One study found that physicians and nurse practitioners reported more asking and advising patients on smoking than did registered nurses (39). Physicians and nurse-midwives have higher reported counseling than dentists and dental hygienists (40-42). One study found that dentists were less likely than other health professionals to be supportive of tobacco interventions, reported weaker knowledge and skills in cessation, and perceived more barriers, although dentists were more effective than other health care providers in assessing patients’ tobacco use (43, 44).

The aims of this study are thus to (1) compare providers on the efficacy of cessation and (2) determine which intervention and study characteristics explain variations in intervention effects. Furthermore, the study is among the first to systematically test the components of the clinical 5 A’s model encouraged by the USPHS Guideline (ask, advise, assess, assist, and arrange; ref. 4) among varied health care providers using meta-analysis.

Materials and Methods

Study Selection. Studies were collected through searches of Medline, CINAHL, PSYCINFO, and dissertation abstracts for articles that were published between 1990 and 2004. We used a sensitive keyword search strategy to uncover randomized clinical trials suggested by ref. (45), in conjunction with the keywords suggested by ref. (32), for tobacco and health care providers. We insured that we uncovered all of the studies identified by the USPHS Guideline (4) and Kottke et al. (32). Additionally, using reference lists, we identified additional authors who had published related articles, and conducted hand searches of selected journals that routinely publish articles on provider counseling in tobacco control (including, Annals of Internal Medicine, Archives of Internal Medicine, Cancer Epidemiology, Biomarkers, and Prevention, Nicotine and Tobacco Research, Preventive Medicine, and Tobacco Control). Experts in the field were contacted for copies of their unpublished manuscripts.

Studies were included in the database if they were randomized clinical trials or quasi-experimental studies of smoking cessation activities by physicians, nurses, dentists and dental assistants, or teams of these professional groups. Consonant with the USPHS Guideline’s definition (4), teams were defined as groups of intervenors that consisted of more than one type of medical professional, regardless of the number of providers of each type. Because cessation interventions tended to differ if the patient was an adolescent or an adult, interventions that targeted youth (i.e., those ages ≤18 years) were excluded from the database. These initial criteria yielded >200 published articles.

After closer scrutiny, 174 studies were eliminated from the database, for the following reasons. Studies were excluded if there was no control group or if the study took place outside of the United States. Some articles were office- or system-focused (e.g., they were designed only to increase provider counseling rates). Some studies were not designed to measure tobacco cessation as an outcome. Seventeen studies were excluded because the intervention did not take place in a medical or dental setting, or because the entire intervention was conducted by other health professionals such as psychologists or smoking cessation counselors. Studies were also excluded if the methodology of the intervention did not include in-person treatment, such as if the purpose of the study was to measure the efficacy of phone calls or letters only. Nine studies addressing broad lifestyle interventions that targeted diet, exercise, and/or...
multiple other behaviors in addition to tobacco use were also excluded. These limitations were instituted in order to compare research that would be most similar in design and setting and to isolate the effects of the provider-patient interaction.

The resulting meta-analysis database consisted of 37 randomized and quasi-experimental interventions that focused specifically on tobacco cessation advice or counseling that occurred during patient visits. For reporting intervention effects, it was required that the proportion of patients who ceased tobacco use be reported separately for intervention and for control groups or be recoverable from the statistics provided. If multiple articles were published that referred to a single study, all were used. If only one part of a study was useful for our purposes, that part was used alone. It was noted if subjects included only patients who had volunteered for the intervention, or if the analysis included all smoking patients in medical practice, regardless of their interest in participating in the intervention. We included one intervention that was aimed at parents who had come with a child to a pediatrician’s visit (46).

Abstraction Methods. Studies were collected by a research assistant, and abstracted in a SAS database designed to capture the variables of interest. The data were entered by a coauthor (J.E.H.) and double checked to ensure reliability (J.E.H. and S.S.G.). Discrepancies were isolated and reconciled by the coauthors.

Study Level Variables. The approach of Kottke et al. (32) guided the capture of the salient study variables and the meta-analysis by Legler et al. (47) informed the reporting of the results. The following variables were taken from written study descriptions and tables: (1) number of subjects; (2) allocation into groups (random and quasi-experimental); (3) type of control group (usual care, brief advice, and placebo); (4) patient type/setting (primary care, hospitalized or those diagnosed with illnesses related to tobacco use, and pregnant or postpartum women); (5) type and number of intervenors (physician, nurse, dentist, and teams); (6) intervention modality; (7) number of sessions and their duration; (8) whether sessions were done individually or in a group; (10) whether subjects had volunteered for the studies or if all smoking patients were included in the analyses; (11) the method of follow up (phone call, mailing, and in-person visit); (12) type of biochemical assay used, if any; (13) how and when cessation (or sustained abstinence) was defined; (14) the duration of follow-up before cessation rates were ascertained; (15) the total number of days the subjects were in contact with the program; (16) whether the intervenor asked the patient about smoking; (17) whether the intervenor advised the patient to stop; (18) whether the patient’s readiness to change, and if the program involved relating the need to change to the patient’s health status. “Assist” was defined as the use of nicotine replacement therapy, self-help print, audio, or video materials. “Arrange” was defined as the use of follow-up calls, mail, or face-to-face (“booster” sessions) for encouragement, and/or if the providers distributed referrals to outside agencies for further assistance. To capture the varied components of these three variables, they were coded as continuous; assess ranged from 0 to 4 (M = 0.88, SD = 0.43), assist ranged from 0 to 3 (M = 1.41, SD = 0.57), and arrange ranged from 0 to 4 (M = 1.33, SD = 0.91).

In 10 of the studies, the patient’s readiness to change was assessed using the transtheoretical model of change (48, 49) or other health behavior theories (50-53). In 14 of the studies, interventions were designed to specifically address patients’ barriers to quitting such as the importance of cessation or withdrawal symptoms; these were generally related to the patient’s health status. Whereas three studies included nicotine replacement therapy as an integral component of the intervention, in an additional 15 studies, providers identified it as a quit strategy or offered patients a small supply of nicotine replacement therapy, usually 1 week, but did not collect nicotine replacement therapy use as a variable for their analyses. In 38 of the studies, intervention strategies included motivational posters or other materials in the waiting room and/or giving patients self-help pamphlets or booklets; 11 programs required patients to watch or listen to motivational audio or videotapes. Two studies used feedback of spirometry and carbon monoxide analysis to encourage cessation. In nine studies, patients were referred to other resources (including outside agencies) for further assistance.

Intervention Group Level. All studies employed in-person cessation advice as a part of the overall methodology. The intensity of advice ranged from brief (3-5 minutes), given once during a health visit, to structured behavioral change interventions, that could last as long as an hour, delivered over multiple visits. If a range of time was given, such as 10 to 15 minutes, the mean was used as the duration of the intervention. Differing types of counseling approaches were used for different segments of the population. As no intervention lasted longer than 1 year, we used the study date to assess the impact of any secular trends, particularly those influenced by the Master Settlement Agreement (S.1415, 1998), on effect sizes over time. All participants were smokers at baseline. As only 55% of the abstracted studies reported the race or ethnicity of participants at baseline, and some only by percent white (70.3% of control subjects and 78.2% of intervention subjects), this variable was not collected for analysis.
including: teaching patients problem solving skills, or relaxation methods, providing social support, or helping patients to obtain intratreatment or extratreatment support.

Comparison Level. Control or comparison groups were defined as those whose members received no intervention, usual care, or only a minimal intervention. Usual care refers to the normal procedure used during provider visits, which may include asking about patient smoking status or providing a brief quit message. Two studies used nicotine replacement therapy, placebo patches or gum as control. Because these definitions of control conditions differed, abstractors deferred to study authors’ definitions.

Ten studies had more than one intervention group, with differing populations or intervention strategies utilized in each one. Typically, one intervention arm used multiple strategies, in comparison with arms testing individual components. When the studies had several arms, the differing groups were stratified in the analysis and analyzed as different studies. This allowed for more specificity of the intervention components.

Outcomes. We used the study-defined outcomes, including point-prevalence cessation or sustained abstinence, to include a wider range of research by follow-up time and type. To increase comparability across studies in the meta-analysis, if two or more cessation outcomes were reported (i.e., refs. 54, 55), we used the point prevalence cessation outcome that authors identified as their primary outcome. Whereas definitions have changed over time, most studies assessed cessation at one point only. If the authors did not identify a primary outcome, we chose (1) the 6-month point prevalence, (2) the 12-month point prevalence, and (3) the 3-month point prevalence. Two papers (56, 57) reported only sustained abstinence measures, whereas eight others reported sustained abstinence in addition to point prevalence (58-65). Thirty-five percent of the studies assessed cessation at one point only. If the authors did not identify a primary outcome, we chose (1) the 6-month point prevalence, (2) the 12-month point prevalence, and (3) the 3-month point prevalence. Two papers (56, 57) reported only sustained abstinence measures, whereas eight others reported sustained abstinence in addition to point prevalence (58-65). Thirty-five percent of the studies assessed point prevalence cessation at 6 months; 18.9% measured it at 12 months after implementation. The remaining studies assessed 5- to 12-week point prevalence (16% of studies), 8-month point prevalence (3% of studies), or sustained abstinence at nine months (3% of studies) postimplementation. In studies of pregnant women, point prevalence cessation was most often assessed at delivery or at the last prenatal visit (16% of studies). Three percent of studies assessed cessation in midpregnancy, and 3% (1 study) measured sustained abstinence throughout the pregnancy. If the time period for prenatal visits was not given, the following were assumed: the first prenatal visit occurred midway through the first trimester (7 weeks). The third trimester visit occurred midway through the third trimester (34 weeks). If the number of prenatal visits was not given, it was assumed to be 13, which is the median number of prenatal visits, nationally (66).

Statistical Analyses. As suggested by refs. (67-69), we described the interventions used for provider-based cessation using a calculated effect size as described forthwith. We then examined intervention effects for individual studies, with the appropriate 95% confidence interval (95% CI).

Descriptive Analyses. As illustrated in Table 1, for each study, we report study authors, publication year, setting patient population or duration of the intervention in minutes, use of assess, assist, or arrange, sample sizes for each group, when quit rates were assessed, type of provider, and estimated intervention effects. Key study level variables were summarized across the literature.

Intervention Effects. The outcome modeled was the difference between intervention and control groups in the cessation rates. Effect size was defined by the mean difference in cessation rates between intervention and control arms divided by the pooled SD (i.e., Hedges’ g; ref. 70). Estimates were corrected for bias using the method of Hedges and Olkin (70, 71). The formulas that we used are listed in Appendix 1.

We provided a summary of the effect estimates of all of the studies by health care provider group and by intervention approach (3 of the 5 A’s). These summaries allowed comparisons across studies. In computing the overall effect size differences, with studies reporting more than one outcome, we compared each arm, point prevalence or sustained abstinence at 3, 6, 9, or 12 months, as specified by the authors, to the control condition. The Cochran Q test (72) was used to test for the homogeneity of intervention effects. Our major interest was the differences in outcomes by health care professionals.

When the Cochran Q test was statistically significant, we took a standard approach to estimating the variability between studies (73). The random effects model was used to account for heterogeneity in estimating the combined intervention effects and the 95% CIs (ref. 73). We used the results of a plot of the residuals from the multiple linear regression analysis of effect sizes and the findings from the residual heterogeneity χ² test to examine the adequacy of the random effects model.

To conduct a metaregression (73), we used the PROC REG command in SAS. Because of our interest in testing a clinical model, we combined individual indicators for 3 of the 5 A’s to create one variable to describe each of the steps. Health care professionals were included in each model a priori. To control for secular trends, the study publication date was included in the model a priori. The other study covariates were included in the model if the corresponding Wald tests were significant at the 0.05 level. We used the formula suggested by ref. (74) to calculate the significance tests for the individual regression coefficients in the random effects model. None of the interaction terms of significant covariates were statistically significant, so they were not included in the final model. Multicollinearity was assessed by examining correlations between the covariates and changes in the coefficient estimates; the model did favorably.

An assessment of publication bias was done by creating a funnel plot for visual inspection (75), as well as computing a (standardized) rank correlation test (76).

Results

Descriptive Analyses. The key characteristics of studies are included on Table 1. Thirty-seven studies (refs. 46, 54-60, 62-65, 77-106) were ascertained in 42
<table>
<thead>
<tr>
<th>Author</th>
<th>Year</th>
<th>Patient population</th>
<th>No. in intervention (control)</th>
<th>Duration in minutes (in person)</th>
<th>Type of provider</th>
<th>Assess</th>
<th>Assist</th>
<th>Arrange</th>
<th>Quit rate assessed*</th>
<th>Difference in proportion that quit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Buchanan</td>
<td>2002</td>
<td>Pregnant women</td>
<td>20 (28)</td>
<td>Not given</td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>At delivery</td>
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</tr>
<tr>
<td>Demers</td>
<td>1990</td>
<td>Primary care</td>
<td>238 (178)</td>
<td>4</td>
<td>Physician</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.036</td>
</tr>
<tr>
<td>Gebauer</td>
<td>1998</td>
<td>Pregnant women</td>
<td>84 (94)</td>
<td>15</td>
<td>Nurse</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>6-12 wk</td>
<td>0.155†</td>
</tr>
<tr>
<td>Gielen</td>
<td>1997</td>
<td>Primary care</td>
<td>193 (198)</td>
<td>30</td>
<td>Nurse</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>3rd trimester</td>
<td>0.007</td>
</tr>
<tr>
<td>Glasgow</td>
<td>2000</td>
<td>Primary care</td>
<td>578 (576)</td>
<td>23</td>
<td>Physican</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.034</td>
</tr>
<tr>
<td>Goldberg</td>
<td>1994</td>
<td>Primary care</td>
<td>69 (118)</td>
<td>Not given</td>
<td>Physician</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>-0.016</td>
</tr>
<tr>
<td>Greene</td>
<td>1994</td>
<td>Primary care</td>
<td>26 (26)</td>
<td>32</td>
<td>Dentist</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>3 mo</td>
<td>0.192†</td>
</tr>
<tr>
<td>Griebel</td>
<td>1998</td>
<td>Oncology</td>
<td>14 (14)</td>
<td>20</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 wk</td>
<td>0.071</td>
</tr>
<tr>
<td>Gritz</td>
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<td>Oncology</td>
<td>50 (46)</td>
<td>Not given</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>12 mo</td>
<td>-0.008</td>
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<tr>
<td>Groner</td>
<td>2000</td>
<td>Primary care</td>
<td>164 (162)</td>
<td>13</td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>-0.001</td>
</tr>
<tr>
<td>Hartmann</td>
<td>1996</td>
<td>Pregnant women</td>
<td>107 (100)</td>
<td>Not given</td>
<td>Physician</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>End of prenatal</td>
<td>0.096</td>
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<td>Hatsukami</td>
<td>1996</td>
<td>Primary care</td>
<td>55 (54)</td>
<td>420</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.158</td>
</tr>
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<td>arm 2</td>
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<td></td>
<td></td>
<td></td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
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</tr>
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<td>arm 3</td>
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<td></td>
<td></td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>-0.119</td>
</tr>
<tr>
<td>Hollis</td>
<td>1993</td>
<td>Primary care</td>
<td>51 (54)</td>
<td>Not given</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.033</td>
</tr>
<tr>
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<td></td>
<td></td>
<td></td>
<td>Team</td>
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<td>Yes</td>
<td>Yes</td>
<td>12 mo</td>
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<td></td>
<td></td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>12 mo</td>
<td>0.148†</td>
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<td>Hurt</td>
<td>1994</td>
<td>Primary care</td>
<td>101 (95)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>12 mo</td>
<td>-0.010</td>
</tr>
<tr>
<td>Katz</td>
<td>2002</td>
<td>Primary care</td>
<td>130 (64)</td>
<td>75</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>6 mo</td>
<td>0.083</td>
</tr>
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<td>Kendrick</td>
<td>1995</td>
<td>Pregnant women</td>
<td>233 (284)</td>
<td>3</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>8th month of pregnancy</td>
<td>-0.010</td>
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<tr>
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<td></td>
<td></td>
<td></td>
<td></td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>8th month of pregnancy</td>
<td>-0.006</td>
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<td></td>
<td></td>
<td></td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>In pregnancy</td>
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<tr>
<td>Kihn</td>
<td>1999</td>
<td>Oncology</td>
<td>14 (11)</td>
<td>Not given</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.100†</td>
</tr>
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<td>Lando</td>
<td>2001</td>
<td>Pregnant women</td>
<td>2035 (1028)</td>
<td>Not given</td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>In pregnancy (sustained)</td>
<td>0.048</td>
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<tr>
<td>Lewis</td>
<td>1998</td>
<td>Hospitalized</td>
<td>62 (61)</td>
<td>2.5</td>
<td>Physician</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.048</td>
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<tr>
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<td></td>
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<td>Masouredis</td>
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<td>Primary care</td>
<td>171 (189)</td>
<td>20</td>
<td>Dentist</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>3 mo</td>
<td>0.081</td>
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<tr>
<td>Morgan</td>
<td>1996</td>
<td>Primary care</td>
<td>279 (380)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.073**</td>
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<tr>
<td>arm 2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.042</td>
</tr>
<tr>
<td>arm 3</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.075†</td>
</tr>
<tr>
<td>Rice</td>
<td>1994</td>
<td>CVD†</td>
<td>63 (48)</td>
<td>300</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>12 mo</td>
<td>-0.190†</td>
</tr>
<tr>
<td>arm 2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>12 mo</td>
<td>-0.187†</td>
</tr>
<tr>
<td>arm 3</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>12 mo</td>
<td>-0.285†</td>
</tr>
<tr>
<td>Schnoll</td>
<td>2003</td>
<td>Oncology</td>
<td>208 (210)</td>
<td>5</td>
<td>Physican</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.025</td>
</tr>
<tr>
<td>Secker-Walker</td>
<td>1994</td>
<td>Pregnant women</td>
<td>188 (226)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>36th week visit</td>
<td>0.039</td>
</tr>
<tr>
<td>Secker-Walker</td>
<td>1998</td>
<td>Pregnant women</td>
<td>44 (48)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>36th week visit</td>
<td>0.002</td>
</tr>
<tr>
<td>Severson</td>
<td>1998</td>
<td>Pregnant women</td>
<td>114 (110)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>1 y Postpartum</td>
<td>0.075</td>
</tr>
<tr>
<td>arm 2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>3 mo</td>
<td>0.009†</td>
</tr>
<tr>
<td>arm 3</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>3 mo</td>
<td>0.090</td>
</tr>
<tr>
<td>Severson</td>
<td>1997</td>
<td>Primary care</td>
<td>1305 (1350)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>3 mo</td>
<td>0.004</td>
</tr>
<tr>
<td>Severson</td>
<td>1997</td>
<td>Primary care</td>
<td>1073 (802)</td>
<td>Not given</td>
<td>Team</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>12 mo</td>
<td>0.008</td>
</tr>
<tr>
<td>Sippel</td>
<td>1999</td>
<td>Primary care</td>
<td>103 (102)</td>
<td>17.5</td>
<td>Physican</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>9 mo (sustained)</td>
<td>-0.050</td>
</tr>
<tr>
<td>Stanislaw</td>
<td>1993</td>
<td>Oncology</td>
<td>12 (14)</td>
<td>75</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>5 wk</td>
<td>0.321</td>
</tr>
<tr>
<td>Stevens</td>
<td>1995</td>
<td>Primary care</td>
<td>541 (632)</td>
<td>10</td>
<td>Dentist</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>12 mo</td>
<td>0.006</td>
</tr>
<tr>
<td>Tashkin</td>
<td>2001</td>
<td>COPD†</td>
<td>204 (200)</td>
<td>Not given</td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>26 wk</td>
<td>0.070</td>
</tr>
<tr>
<td>Taylor†</td>
<td>1990</td>
<td>Post-MI†</td>
<td>72 (58)</td>
<td>Not given</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>12 mo</td>
<td>0.260**</td>
</tr>
<tr>
<td>Taylor</td>
<td>1996</td>
<td>Hospitalized</td>
<td>315 (313)</td>
<td>60</td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 mo</td>
<td>0.141</td>
</tr>
<tr>
<td>Wewers</td>
<td>2000</td>
<td>HIV†</td>
<td>8 (7)</td>
<td>Not given</td>
<td>Team</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>8 mo</td>
<td>0.500†</td>
</tr>
<tr>
<td>Wewers</td>
<td>1994</td>
<td>COPD†</td>
<td>10 (12)</td>
<td>75</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 wk</td>
<td>0.317</td>
</tr>
<tr>
<td>arm 2</td>
<td></td>
<td>General surgery</td>
<td>13 (15)</td>
<td>75</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 wk</td>
<td>-0.056</td>
</tr>
<tr>
<td>arm 3</td>
<td></td>
<td>Oncology</td>
<td>14 (16)</td>
<td>75</td>
<td>Nurse</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>6 wk</td>
<td>0.143</td>
</tr>
</tbody>
</table>

NOTE: CVD = cardiovascular disease; COPD = chronic obstructive pulmonary disease; MI = myocardial infarction; HIV+ = diagnosis of human immunodeficiency virus.

*Some studies assessed quit rates at multiple dates. This table notes the date of quit rates used in the metaregression.

**p < 0.001.

†p < 0.05.

Pediatric setting; one arm focused on the effect of maternal smoking on child health.

Primarily nurses but some medical assistants.

*Colorado and Missouri only. The Maryland site was not used because the type of medical provider was not clear.

STORK program only.

**p < 0.01.

†Pediatric setting.

#Rate includes all subjects; those lost to follow-up were assumed to be smokers.
articles and through personal communication; half of the intervention studies were initiated after 1996; all were completed before 2004. Physicians were the intervenors in 30% of studies, nurses in 30%, dentists in 11%, and teams of providers in 30%. The average number of professionals that patients saw was 1.4 (SD = 0.6); this includes studies where the providers were all of the same type, such as ref. (77), in which one nurse was seen for the in-person part of the intervention and two other nurses followed up with phone contacts. Over two thirds of studies (67.6%) had one provider; 29.7% had two providers, and 2.7% had four providers. As expected, studies with teams had the greatest number of providers (M = 1.77, SD = 0.83); those with physicians had the least (M = 1.23, SD = 0.44). There were no statistically significant differences in effect sizes among the studies by number of providers, however (one provider, Hedges \( g = 1.70; SD = 3.91; >1 \) provider, Hedges \( g = 1.92; SD = 3.63; P = 0.84 \). Studies had a mean sample size of 507 (SD = 714), with 239 (SD = 308) in the intervention arm and 269 (SD = 419) in the control arm. The majority of studies (88%) assigned patients to intervention or control arms at random, although several assigned treatment by patient number, physician’s year of residency, or by using a quasi-experimental design. In the 12 quasi-experimental designs, two studies had minimal intervention as a control, defined as an antismoking message or self-help materials; the remainder had comparisons with usual care. Counter to other secular trends reflecting increased attention to tobacco control (107), there was no significant decrease in cessation rates in the control groups over time (Jonckheere-Terpstra \( Z = -0.30 \), two-sided \( P = 0.76 \)). Nine of the studies had enrolled pregnant or postpartum women; 12 of the studies had hospitalized patients or people who have been diagnosed with smoking-related cancers or cardiovascular disease, and 16 of the studies had primary medical or dental care (see Table 1).

The majority (76%) of studies recruited subjects in an outpatient or hospital visit, with the remainder recruited through community settings. Seventy-six percent of interventions took place in an outpatient setting, 24%, in inpatient settings. Five percent of interventions provided counseling in a group setting. The majority of interventions (97%) included counseling; one study was conducted through the use of written materials (although there was also contact with a health care provider). Nicotine replacement therapy was an integral component of 8% of the studies. Forty-three percent of the subjects had volunteered to participate in the study; the remainder were clinic based. Approximately one half (46%) of the studies included follow-up calls as part of the overall approach. If a call occurred, the average duration was 9.4 minutes (SD = 0.6).

One half of all patients received follow-up by mail and telephone; 49% were followed up during a clinic visit. Biochemical verification (27% saliva, 19% expired air, 8% urinary cotinine, and 3% serum) was used in 57% of the studies. The mean time between the end of the intervention and the ascertainment of smoking status was 30 weeks (SD = 15.3). The average duration of the interventions in days was 76.5 (SD = 82.5). The mean number of sessions was four (SD = 3.8), with a mean duration per session of 22.7 minutes (SD = 13.0).

Sixty-four percent of the studies with physician intervenors, 72% of those with nurses, 100% of those with dentists, and 64% of those with teams provided assessment of smoking. In all studies, patients were assisted with quitting. Finally, in 100% of the physician, 91% of the nurse, 75% of the dentist, and 91% of the team studies, health care providers arranged for cessation follow-up.

**Estimate of Effects.** Estimates of the size of the difference between treatment and control groups, by health care provider group, is shown in Fig. 1, with 95% CIs. If an intervention had multiple arms, the effect size of each relative to the control is shown. Advice from teams and physicians showed significant effects on cessation (teams: overall ES = 0.79; 95% CI, −0.19 to 3.71; \( P = 0.01 \); physicians: overall ES = 6.01; 95% CI, −2.46 to 13.29; \( P = 0.002 \); see Fig. 1). Advice from dentists increased quit rates, but not significantly (overall ES = 0.33; 95% CI, −0.02 to 1.16; \( P = 0.12 \); see Fig. 1), although the power to detect differences was reduced by the small number of studies. Advice from nurses showed a weak and nonsignificant effect on quit rates (overall ES = 0.03; 95% CI, −0.30 to 0.31; \( P = 0.37 \)).

Cochrane’s Q test found heterogeneity within the all four groups (physician: Q = 33.81, \( P < 0.001 \); nurse: Q = 14,652.98, \( P < 0.0001 \); dentist: Q = 1,831.63, \( P < 0.0001 \); team: Q = 660.19, \( P < 0.0001 \)). Thus, we used a random effects model.

In separate analyses (data not displayed), we evaluated the effect sizes for the key components of a provider-based clinical intervention using 3 of the 5 A’s (assess, assist, and arrange) measured as continuous variables. If an intervention had multiple arms, we used the effect size of each relative to the control. Assess was significantly correlated with effect size (\( r = 0.28, P = 0.04 \)). Arrange and assist were not significantly related to effect size (arrange: \( r = 0.23, P = 0.10 \); assist: \( r = 0.05; P = 0.70 \)).

**Metaregression.** When different patient populations were compared, there were no significant differences among primary care, inpatients, or pregnant women in their likelihood to quit, and thus, this factor was left out of the final model. Physicians were significantly more effective in promoting cessation than were multiprovider teams (physicians: \( \beta = 4.13, P = 0.005 \)). Nurses showed a nonsignificant trend toward less effectiveness in cessation than did teams (\( \beta = −0.86, P = 0.45 \)). There were no statistically significant differences between teams and dentists (\( \beta = 0.73, P = 0.69 \)). There was a trend toward more health care providers demonstrating greater cessation than fewer providers (\( \beta = 1.23, P = 0.09 \)); additional multivariate analyses revealed some confounding (\( \beta < 10\% \) by type of provider).

We conducted a series of diagnostic tests on the model to determine its adequacy. A scattergram of the residuals revealed no pattern; the residual-heterogeneity \( Q \) indicated heterogeneity within the groups (\( \chi^2(50) = 175.91, P < 0.0001 \); ref. 73). Using standard criteria (108), we defined outliers as studies in which the effect size differed by greater or less than 2 SDs beyond the mean effect size. By these criteria, two studies of nurses as cessation counselors (ref. 97; arm 3 only) and ref. (65) could be considered outliers. We deleted each study in turn to assess the impact on effect sizes. Neither study
had a statistically significant effect on the results. Overall, the final metaregression of cessation effect sizes fit the data well ($F[14,36] = 4.32, P = 0.0002$; see Table 2).

None of the measured clinical components, assess, assist, or arrange was a statistically significant influence on cessation. Studies published between 1990 and 1992 were significantly more likely to show provider effects on cessation than were later studies ($\beta = 6.18, P = 0.007$), perhaps due to the influence of a small number of studies with multiple arms, and the secular increases in cessation in control groups. Consonant with the findings of other studies, there was a trend toward the more weeks until cessation was assessed, the less the measured quit rate ($12\text{ months or longer}; \beta = -2.32, P = 0.09$).

**Publication Bias.** To assess publication bias, we created a funnel plot (see Fig. 2) that shows the relationship between sample size and effect size (109). Each point represents one study. If studies had more than one intervention arm, the primary outcome is shown. The lopsided shape of the plot suggests that, despite our search for studies “in the drawer (110),” some publications may have occurred. The standardized rank correlation test (ref. 76; Kendall’s $\tau = -0.04, P = 0.64$), however, revealed a weak correlation between the effect size and the variance for the studies. The evidence for publication bias is therefore not strong.

**Sensitivity Analyses.** To assess the effect of large sample sizes on the model, we excluded the study by Lando et al. (56), that with 3,083 subjects was the largest. Using backward elimination, the estimate of effect size remained robust ($F = 4.20, P < 0.0003$), although the $F$ value of the overall model decreased slightly. The statistically significant predictors of cessation remained the same.

To assess the effect of publication date on changes in quit rates across studies, a least squares model was fit to the data. The test for a trend across time was not significant ($\beta = -0.21, t = -1.26, P = 0.21$).

Separate analyses were conducted to compare pairs of providers to one another (physicians versus nurses, nurses versus dentists, physicians versus dentists). Because of the number of analyses conducted, a decision was made a priori to accept a lower $P = 0.02$, as significant. Thus, in the comparison of physicians and nurses, physicians were significantly more effective in providing counseling than were nurses ($\beta = 5.19, P = 0.005$). In comparing physicians and dentists, the differences in cessation outcomes were not statistically significant ($\beta = 4.91, P = 0.73$). Dentists were significantly more effective intervenors than nurses ($\beta = 0.94, P = 0.002$). These results may have been influenced by the relatively small number of dental studies, so should be interpreted with caution.

### Discussion

Our analyses of smoking cessation indicated that receiving advice from any health care provider produces...
a small increase in quit rates. In addition, there was a statistical trend in the multivariate analyses toward increased cessation with more providers rather than less. These findings are generally consonant with the USPHS Guidelines. In both the bivariate and the multivariate findings, physicians were more effective in reducing quit rates than were either multiprovider teams, dentists, or nurses; these findings are also consonant with those of the USPHS Guidelines. Although overall, smoking cessation interventions are cost-effective relative to other medical interventions (4), physicians, as the most highly paid providers, may not be the most cost-effective intervenors relative to other health care professionals. Nurses seemed least effective by comparison with physicians, teams, and dentists, suggesting the importance of additional training for these numerous health care providers (111). The number of studies of dentists was, however, small, suggesting the importance of additional studies of this provider group, as well as the caution with which this finding should be viewed.

Earlier studies (i.e., those during 1990-1992) showed stronger effects on cessation than did later studies, reflecting more general increases in cessation nationally. None of the 3 A’s had consistent effects on cessation, perhaps due to limited interstudy variation in the measures, and the tendency of health care providers to use all 3 A’s ($r^2$, assess, assist, and arrange, range = 0.05-0.23).

Whereas longer-term abstinence is an important outcome in cessation counseling, only a small proportion of the rigorous studies that we examined assessed cessation at 12 months. There was an overall trend toward the association of longer assessment periods and reduced cessation although the small number of relevant studies that were published over the past 14 years limited our ability to conduct subgroup analyses. The findings suggest that more trials be conducted of health provider-based cessation using longer-term cessation as an outcome.

There are some methodologic limitations of the meta-analysis, including heterogeneity among the studies that were combined, multiple interventions from the same study, durability of the effects over time, and publication bias. We combined studies in groups a priori to increase the homogeneity by health care professional, but there were significant differences among the studies. Our criteria for inclusion were rigorous, and we systematically adjusted for differences in sample sizes.

Approaches to the assessment of heterogeneity, whereas central to meta-analyses, are still subjects of debate among researchers. The major criterion for our choice of the random effects model was conceptual, as we sought to draw inferences about a universe of similar diverse, complex intervention studies so as to enrich general scientific understanding of the effects of health provider cessation counseling (112). In addition, the random effects model provides more conservative estimates of intervention effects than does a fixed effects model under conditions of heterogeneity, because the confidence intervals are generally wider (113). Whereas the random effects model gives proportionally greater weight to small studies than does the fixed-effect model, thus increasing the likelihood of publication bias, we have carefully analyzed the effect of this bias and it is relatively small.

Because we were most interested in interprovider comparisons, we conducted sensitivity analyses to sequentially remove the largest study and to examine the effects of publication time on the summary statistics. We conducted subgroup comparisons among the health care providers. These findings suggest that employing physicians may be among the most effective approaches to cessation.

In a few cases, several interventions were reported within a single study, but they were compared with the same control group. As individuals in the control group are counted more than once, assumptions of independence among participants are violated. To examine the possible impact on the summary statistics, we recalculated the statistics twice, by including or excluding the interventions each time. Excluding either one of the interventions did not affect the interpretation of the overall findings.

Extensive efforts were made to obtain unpublished studies; few were uncovered. Although the data are not compelling, the study’s findings may be subject to some publication bias, with unpublished or missed studies finding either stronger or weaker results.

The findings suggest that health care organizations and insurance providers may consider cessation services effective even if provided by health care providers differing in their professional backgrounds and salaries. Physicians, among the most highly paid providers, may show the strongest cessation rates. We did not evaluate the efficacy of smoking cessation counselors, psychologists, or other professional or nonprofessional intervenors (e.g., former smokers with limited tobacco control training). These groups may provide equally effective cessation counseling to the health care professionals assessed in this study.

The trend in reduced cessation over time suggested that, as uncovered in other similar studies, sustaining quit attempts over time is difficult, particularly without additional prompts and reinforcement. Continued provider prompts to smokers to quit, through chart reminders and computerized office-based systems, for example, are important to sustained cessation. Over time, these contacts may be particularly effective means for encouraging longer term abstinence.
These findings support the promise of cessation counseling by all health care professionals, particularly physicians. The results further suggest that additional rigorous longer-term trials of provider-based cessation approaches are warranted.

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Appendix

Effect size was calculated as Hedges $g$ ($M_1 - M_2 / S$ pooled), with the Hedges correction for bias $[\sqrt{\frac{n_2}{n_1 n_2}} + \frac{m_2 - m_1}{S^2}]$. Variance was calculated as $[\frac{n_1 + n_2}{n_1 n_2} + \frac{g^2}{2(n_1 + n_2 - 2)}]$ (70).

References


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Sherri Sheinfeld Gorin and Julia E. Heck


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