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Training health professionals in smoking cessation

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Abstract

Rationale

Cigarette smoking is one of the leading causes of preventable death worldwide. There is good evidence that brief interventions by health professionals can increase smoking cessation attempts. However, as new studies become available, the effectiveness of these training programmes needs to be re-assessed to inform public policy, clinical care, and guideline recommendations. This is an update of a Cochrane review first published in 2000, and previously updated in 2012.

Objectives

To assess the effectiveness of training healthcare professionals to deliver smoking cessation interventions to their patients, and to assess the effects of training characteristics (such as content, setting, delivery, and intensity).

Search methods

We searched the following databases from inception to August 2024: Cochrane Central Register of Controlled Trials (CENTRAL); MEDLINE; Embase; PsycINFO; ClinicalTrials.gov (through CENTRAL); and the World Health Organization International Clinical Trials Registry Platform (through CENTRAL). We also searched the references of eligible studies.

Eligibility criteria

We included randomised trials in which the intervention was training of healthcare professionals in smoking cessation. We considered trials for inclusion if they reported outcomes for patient smoking at least six months after the intervention. Process outcomes needed to be reported. However, we excluded trials that reported effects only on process outcomes and not smoking behaviour.

Outcomes

The critical outcome measure was abstinence from smoking six months or more after baseline, using the strictest measure of abstinence available at the longest follow-up. Prolonged or continuous abstinence was preferred over point prevalence. Our important outcome was the number of participants who made a quit attempt.

Risk of bias

Working independently, two review authors evaluated the risk of bias using the Cochrane RoB 1 tool, following guidance from the Cochrane Tobacco Addiction Group.

Synthesis methods

Working independently, two review authors extracted information about the characteristics of each included study (i.e. interventions, participants, outcomes, and methods). We pooled studies using random-effects meta-analysis where possible and otherwise summarised findings using narrative synthesis in text and tables. We used the GRADE framework to assess the certainty of the evidence.

Included studies

We included 29 studies in the review, published between 1989 and 2024. Together, the studies provided training for over 4030 health professionals, and data for 38,178 participants. We assessed 10 studies to have an overall low risk of bias, 17 an unclear risk, and two to have an overall high risk of bias.

Synthesis of results

Sixteen studies compared training of healthcare professionals in smoking cessation to no training, and assessed the effect on the number of participants abstinent at longest follow-up. High-certainty evidence indicates that smoking cessation training for healthcare professionals increases patient smoking cessation compared with no training (risk ratio (RR) 1.34, 95% confidence interval (CI) 1.08 to 1.67; I^2 = 48%; 16 studies, 16,513 participants). We conducted three subgroup analyses to test the effect of specific potential sources of heterogeneity: training intensity, type of healthcare professional trained, and treatment recommended in the training; none found evidence of between-group heterogeneity.

Four studies assessed the effect of high-intensity training for healthcare professionals on the number of participants abstinent at longest follow-up compared with lower-intensity training. The evidence suggests that higher-intensity training may increase smoking cessation compared with lower-intensity training, though confidence intervals were wide and included the potential for no benefit (RR 1.64, 95% CI 0.86 to 3.12; I^2 = 54%; 4 studies, 1151 participants; low-certainty evidence).

Three studies assessed the impact of adjuncts to training on the number of participants abstinent at longest follow-up. We found low-certainty evidence that when the healthcare professionals treating them are trained in smoking cessation, more people may quit when also provided with nicotine replacement therapy (RR 1.64, 95% CI 0.72 to 3.71; I^2 = 69%; 2 studies, 1892 participants), and very low-certainty evidence that providing prompts to healthcare professionals in addition to smoking cessation training may help more people to quit (RR 1.37, 95% CI 0.69 to 2.70; I^2 = 66%; 3 studies, 2429 participants). However, in both cases, confidence intervals were wide and included the potential for no benefit.

Authors' conclusions

High-certainty evidence supports the effectiveness of training health professionals in smoking cessation when compared with no training. Multi-component investigations incorporating new pharmacological interventions for smoking cessation (such as varenicline and bupropion) or other cessation aids alongside physician training should be considered to determine if any additional benefit in long-term abstinence can be obtained.

Funding

Production of this review was supported by PhD scholarship funding from the University of Adelaide and co-funded by Houd Research Group, awarded to KS.

Registration

This review was first published outside of Cochrane in 1994 and subsequently updated as a Cochrane review in 2000 (DOI: 10.1002/14651858.CD000214) and 2012 (DOI: 10.1002/14651858.CD000214.pub2). No protocol was published or registered.

Plain language summary

Can training health professionals in stop-smoking support help more of their patients quit smoking?

Key messages

- Training health professionals in quit-smoking techniques (also known as 'smoking cessation') can help their patients quit smoking.
- Training with multiple components (e.g. counselling of patients plus offers of follow-up appointments, or provision of self-help materials), as well as feedback and ongoing resources for health professionals may be even more helpful.
- More research that combines training of health professionals with stop-smoking medicines (such as nicotine replacement therapy) or other stop-smoking aids should be considered to see if any of this helps more people quit.

What is training for health professionals?

Training health professionals in stop-smoking techniques helps encourage health professionals to ask patients if they are smoking. If a patient is smoking, health professionals are then equipped with the latest evidence to support patients through their attempts to quit smoking.

What did we want to find out?

We wanted to find out if training health professionals in stop-smoking techniques helps their patients to quit, and if more intensive training helps more of their patients to quit.

What did we do?

We searched for studies that trained health professionals in stop-smoking techniques. The health professionals then used these techniques to help support their patients to quit smoking. These studies all compared the trained health professionals to control groups, where health professionals did not receive training, received lower-intensity training, or training on something other than smoking. We compared and summarised the results of the studies, and rated our confidence in the evidence based on factors such as study methods and sizes.

What did we find?

We included 29 studies in this review. Altogether, the studies trained 4,030 health professionals who provided stop-smoking support to 38,178 patients who smoked.

We found that when healthcare professionals were provided with smoking cessation training, more of their patients were likely to quit, compared with patients whose healthcare professionals were not trained.

We found that when healthcare professionals completed higher-intensity training, more patients may have successfully quit than for health professionals who received lower-intensity training.

We also found that more people may quit smoking when the healthcare professionals who treat them receive training in smoking cessation:

- and provide nicotine replacement therapy to their patients; or
- and prompts or reminders.

However, we are uncertain about both of these results, and in each case, they might not help more people to quit.

What are the limitations of this evidence?

We are confident about the finding that when healthcare professionals were provided with stop-smoking training, more of their patients were likely to quit. We have less confidence in the other findings because the methods used by some studies were unclear, and some studies did not conduct biological tests to confirm reports of non-smoking amongst patients.

How current is this evidence?

The evidence is current to August 2024.

Summary of findings

Summary of findings 1

Training health professionals for smoking cessation versus no training or less intensive training

Training health professionals for smoking cessation versus providing no training or less intensive training						
Patient or population: health professionals and their patients who smoke **Setting:** medical settings, e.g. hospitals, clinics, and pharmacies **Intervention:** training healthcare professionals; adjuncts to training **Comparison:** no training, less intensive training, or training without adjuncts						
Outcomes	**Illustrative comparative risks* (95% CI)**		**Relative effect (95% CI)**	**No. of participants (studies)**	**Certainty of the evidence (GRADE)**	**Comments**
	Assumed risk with control	**Corresponding risk with training health professionals**				
Training healthcare processionals versus no training Smoking abstinence at longest follow-up (6 to 14 months)	50 per 1000	68 per 1000 (54 to 84)	RR 1.34 (1.08 to 1.67)	16,513 (16 studies)	⊕⊕⊕⊕ **High**	
More intensive training versus less intensive training Smoking abstinence at longest follow-up (6 months)	82 per 1000	134 per 1000 (70 to 256)	RR 1.64 (0.86 to 3.12)	1151 (4 studies)	⊕⊕⊝⊝ **Low**[a]	

NRT as adjunct to training versus training alone Smoking abstinence at longest follow-up (12 months)	35 per 1000	57 per 1000 (25 to 129)	RR 1.64 (0.72 to 3.71)	1892 (2 studies)	⊕⊕⊝⊝ **Low**[b,c]	
Prompts as adjunct to training versus training alone Smoking abstinence at longest follow-up (6 to 12 months)	47 per 1000	65 per 1000 (33 to 128)	RR 1.37 (0.69 to 2.70)	2429 (3 studies)	⊕⊝⊝⊝ **Very low**[a,c]	

*__The risk in the intervention group__ (and its 95% confidence interval) is based on the assumed risk in the comparison group and the **relative effect** of the intervention (and its 95% CI). The assumed risk in the comparison group is calculated as the median risk in control groups.
CI: confidence interval; **NRT:** nicotine replacement therapy; **RCT:** randomised controlled trial; **RR:** risk ratio

GRADE Working Group grades of evidence
High certainty: we are very confident that the true effect lies close to that of the estimate of the effect.
Moderate certainty: we are moderately confident in the effect estimate; the true effect is likely to be close to the estimate of the effect, but there is a possibility that it is substantially different.
Low certainty: our confidence in the effect estimate is limited; the true effect may be substantially different from the estimate of the effect.
Very low certainty: we have very little confidence in the effect estimate; the true effect is likely to be substantially different from the estimate of effect.

[a]Downgraded two levels for very serious imprecision: CI includes clinically significant benefit and clinically significant harm.
[b]Downgraded one level for serious imprecision: CI includes no clinically significant difference as well as clinically significant benefit.
[c]Downgraded one level for risk of bias: all studies at high risk of bias.

Background

Description of the condition

According to the latest Global Burden of Disease (GBD) study [1], approximately 1100 million people are current smokers. As a result, 5.81 million deaths and 161.3 million disability-adjusted life years were attributable to smoking in 2023 alone [2]. Tobacco use is the leading risk factor for death amongst males [1]. Approximately 80% of tobacco-related deaths occur amongst people living in lower-income countries [3], where there are fewer resources and policies available to support smoking cessation.

Description of the intervention and how it might work

To counter the globalisation of the tobacco epidemic, the World Health Organization (WHO) created the first international treaty adopted by the World Health Assembly in 2003, called the WHO Framework Convention on Tobacco Control [4, 5]. Since its establishment, substantial progress has been made to reduce the tobacco burden across many countries, supported by six actionable measures known by the acronym 'MPOWER' (monitor, protect, offer, warn, enforce, and raise). These measures support the implementation of the WHO Framework Convention recommendations [6], one of which is to offer help for people to quit tobacco use, which is reported as "...an essential component of any tobacco control strategy" [7].

There are several forms of well-evidenced smoking cessation support [8, 9, 10, 11, 12]. Training health professionals in smoking cessation is a critical strategy for maximising quit rates and constitutes an essential element of universal health coverage [7]. Health professionals are at the forefront of tobacco epidemics as they consult millions of people and can encourage them to quit smoking [6, 13]. Across 36 Organisation for Economic Co-operation and Development (OECD) countries, people are reported to visit their family

doctor on average 6.8 times per year [14], with family doctors perceived to be influential sources of information on smoking cessation [15, 16, 17]. However, barriers to the provision of smoking cessation support still exist [18, 19], especially amongst marginalised and vulnerable populations [20, 21, 22]. Similarly, the barriers that impede health professionals in offering quit-smoking support are well established, including lack of time, training, and confidence in their ability to produce a positive result [13, 19, 23, 24, 25].

The 2012 update of this Cochrane review identified several studies showing that training programs for quit-smoking support can be effective in helping smokers quit long-term [26]. These quit-smoking interventions aim to address common barriers to offering smoking cessation support – including lack of time, confidence, and knowledge – by delivering brief training interventions to health professionals so they can deliver brief advice and support to their patients. Examples of these interventions include the very brief advice (VBA) model for smoking cessation, which originated in the United Kingdom but has been translated across several countries [27, 28, 29], and the 5A model (Ask, Assess, Advise, Assist, and Arrange) [30, 31], which has also been further abbreviated to AAR (Ask, Advise, and Refer) [32].

Why it is important to do this review

The economic cost of smoking-attributed diseases worldwide was estimated to be 422,000 million US dollars (USD) in 2012, or 5.7% of global health expenditure [33]. When assessing the burden from health expenditure and productivity losses together, the economic cost of smoking totalled USD 1,436,000 million in 2012, according to purchasing power parity, which is equivalent to 1.8% of the world's annual gross domestic product [33]. Alarmingly, almost 40% of the cost is reported to have been incurred in developing countries [33].

Several versions of this systematic review have been published [34, 35, 36], the most recent being in 2012 [26]. However, several studies have since been published, evaluating new interventions that provide training to health professionals in smoking cessation. These interventions are designed to overcome the frequently reported barriers that impede offers of quit-smoking support from health professionals as well as those reported by patients to accessing these services. We therefore systematically identified and reviewed the evidence from newly published randomised controlled trials that have studied the effects of training and supporting healthcare professionals in providing smoking cessation advice. Furthermore, we assessed the effects of training characteristics, such as the content, setting, and intensity.

Objectives

To assess the effectiveness of training healthcare professionals to deliver smoking cessation interventions to their patients, and to assess the effects of training characteristics (such as content, setting, delivery, and intensity).

Methods

We followed the Methodological Expectations of Cochrane Intervention Reviews (MECIR) when conducting the review, and PRISMA 2020 for the reporting [37, 38]. Following guidance from the Cochrane Tobacco Addiction Group, we have made the following changes to the methods since the 2012 review [26].

- We simplified the main outcome to use the strictest measure of abstinence, in line with the Russell Standard [39]. The previous version included point prevalence and continuous abstinence as separate outcomes.

- Previous versions of the review included additional outcomes measuring provision of support. We removed these because of inconsistent reporting and to prioritise the importance of actual cessation of smoking as the motivator for providing training to healthcare professionals.
- In line with current methodological recommendations [40], we did not consider performance bias during risk of bias assessments because the blinding of study personnel and participants is not feasible when testing behavioural interventions.
- We changed the structure of our analyses from the previous review version to better isolate the effects of training interventions and adjuncts against specific comparators.

Criteria for considering studies for this review

Types of studies

We included only randomised controlled trials and cluster-randomised controlled trials. We did not include quasi-randomised studies, in which the allocation sequence is not truly random (for example, studies where participants' date of birth determines allocation).

Types of participants

We included trials where the unit of randomisation was a healthcare practitioner or practice, and that reported the effects on patients who were cigarette smokers. We excluded studies focused on users of smokeless tobacco, electronic cigarettes, heated tobacco products, or waterpipes, which are covered in separate Cochrane reviews [41, 42, 43, 44].

Types of interventions

We included trials of interventions in which healthcare professionals were trained in methods to promote smoking cessation amongst their patients. There was no minimum intensity for training or pre-specified format for delivery. We included studies that compared training with no training or a different form of training, and studies testing training plus an adjunct designed to improve the efficacy of the training, such as additional prompts or reminders, compared with training alone.

Outcome measures

Critical outcomes

The critical outcome measure was abstinence from smoking six months or more after baseline. In line with the Russell Standard [39], we used the strictest measure of abstinence available, using the longest follow-up and favouring prolonged or continuous abstinence over point prevalence, and biochemically validated abstinence over non-validated self-report. However, we still assessed self-reported abstinence without validation. This is a simplification from the previous review version, which included point prevalence and continuous abstinence as separate outcomes.

In studies where biochemical validation of abstinence from smoking was available, only those participants who met the criteria for biochemically confirmed abstinence were regarded as abstinent. In instances where participants reporting abstinence subsequently failed a validation test, we did not count them as abstinent.

Important outcomes

Our important outcome was the number of participants who made a quit attempt. To be included in the review, studies had to assess our critical outcome. We excluded studies that only assessed our important outcome.

Harms outcomes

The safety and tolerability of pharmacological interventions for smoking cessation are better explored in other reviews [10, 45, 46, 47, 48]. Trials of behavioural interventions are very unlikely to record adverse events, and a 2021 overview of Cochrane reviews of behavioural interventions for smoking cessation found no evidence of harms [8]. However, in the event that any studies reported harms from behavioural interventions, we planned to summarise these narratively.

Search methods for identification of studies

Electronic searches

We searched the following databases from inception to 24 August 2024 for relevant studies:

- Cochrane Tobacco Addiction Group Specialised Register via the Cochrane Register of Studies (crsweb.cochrane.org);
- Cochrane Central Register of Controlled Trials (CENTRAL, Issue 7 2024) via the Cochrane Register of Studies (crsweb.cochrane.org);
- MEDLINE (via Ovid; 1945 to 24 August 2024);
- Embase (via Ovid; 1974 to 24 August 2024); and
- PsycINFO (via Ovid; 1806 to 24 August 2024).

We did not search the Cochrane Tobacco Addiction Group Specialised Register beyond 27 October 2022 as it ceased to be maintained. At the time of the last search, the Register included searches of CENTRAL, 2022, Issue 9; MEDLINE (via OVID) 1945 to update 20221012; Embase (via OVID) 1974 to week 202241; PsycINFO (via OVID) 1806 to update 20221031. See the Cochrane Tobacco Addiction Group's website for full details of how the Register was compiled.

Through our search of CENTRAL, we also covered two online trial registries for the identification of unpublished studies:

- US National Institutes of Health ongoing trials register, ClinicalTrials.gov (www.clinicaltrials.gov); and
- World Health Organization (WHO) International Clinical Trials Registry Platform (apps.who.int/trialsearch).

We did not limit any of our searches by language, year of publication, or publication format. We incorporated the Cochrane search filter for randomised controlled trials [49]. We list search strategies for each database in Supplementary material 1.

Searching other resources

We checked the reference lists of included studies for potentially relevant studies.

We searched for post-publication amendments and examined any relevant retraction statements and errata for included studies (e.g. through PubMed and the Retraction Watch Database (retractionwatch.com/retraction-watch-database-user-guide/)), as errata could reveal important limitations or even serious flaws in the included trials [50]. We are confident that our search strategy will have caught any post-publication amendments currently published, including expressions of concern, errata, corrigenda, and retractions. For future updates of this review, we will check each included study manually for any such additional records.

Data collection and analysis

Selection of studies

Two review authors (from KS, SG, KCC, JLB, TP) independently checked the relevance of the title and abstract of each study retrieved by the search. We resolved any disagreements through discussion, and, if necessary, by consulting a third review author. Two review authors (from KS, SG, KCC, JLB) then independently checked the full text of each potentially relevant study to decide whether the study met our inclusion criteria. We resolved any disagreements through discussion, and, if necessary, by consulting a third review author.

Data extraction and management

Working independently, two review authors (from KS, SG, KCC, JLB, TP) independently extracted data from study reports. We cross-checked extracted data and resolved any disagreements through discussion, and, if necessary, by consulting a third review author. We extracted the following study information:

- country and setting of study, study design, objective/aim, recruitment method, correction for clustering (if any);
- health professional description, number eligible for study, number randomised, number completed, age, gender, any baseline differences;
- patient description, number eligible for study, number randomised, number completed, age, gender, any baseline differences;
- process measures description, biochemical validation, study registration, study funding, reported conflicts of interest;
- description of training of people delivering the intervention to health professionals;
- intervention and control descriptions, duration and intensity of intervention, who delivered the intervention to patients;
- pre-specified outcome data, definitions of point prevalence and continuous abstinence, longest follow-up period reported, other follow-up periods reported;
- critical and important outcomes;
- study funding and study author declarations of interest.

Risk of bias assessment in included studies

Two review authors (from KS, SG, KCC, TP, JLB) independently assessed the risk of bias of each included study using the Cochrane RoB 1 tool [51], following guidance from the Cochrane Tobacco Addiction Group [40]. We cross-checked assessments and resolved any disagreements through discussion, and, if necessary, by consulting a third review author.

We assessed the following domains:

- sequence generation (selection bias);
- allocation concealment (selection bias);
- blinding of outcome assessors (detection bias);
- incomplete outcome data (attrition bias);
- selective reporting (reporting bias);
- other potential sources of bias.

We did not assess performance bias because the blinding of study personnel and participants is not feasible when testing behavioural interventions.

For cluster-randomised trials, we also assessed the following additional domains developed by the Cochrane Effective Practice and Organisation of Care (EPOC) Group [52]:

- recruitment bias resulting from participants being recruited to clusters after cluster allocation;
- bias resulting from unbalanced participant characteristics at baseline;
- whether the analysis appropriately adjusted for the potential correlation of outcomes within clusters.

We judged a study to be at an overall high risk of bias if at least one domain was at high risk, an overall low risk of bias if all domains were at low risk, and of unclear risk otherwise.

Measures of treatment effect

We measured the critical outcome of smoking abstinence using risk ratios (RRs), calculated as the proportion of quitters in the treatment group divided by the proportion of quitters in the control group, with 95% confidence intervals (CIs). A risk ratio greater than 1 indicates a greater chance of abstinence in the intervention group compared with the control group. We applied the strictest definition of abstinence at longest follow-up, with biochemically validated abstinence preferred over self-reported abstinence where available.

Unit of analysis issues

In cluster-randomised trials, outcomes relate to individual patients while allocation to the intervention is by provider or practice, and ignoring this may introduce unit of analysis errors. Using statistical methods that assume, for example, that all participants' chances of quitting are independent ignores the possible similarity between outcomes for participants seen by the same provider. This may underestimate standard errors and give misleadingly narrow confidence intervals, leading to the possibility of a type 1 error [53]. We expected all trials to be cluster-randomised studies. Therefore, we analysed outcomes at the individual level while accounting for clustering using a random-effects model for the pooled meta-analysis, as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions* [51]. A statistician (AE) checked the calculations.

Because included studies did not consistently adjust for clustering, and gaps in reporting left it unclear whether studies analysed on an intention-to-treat basis, counting those lost to follow-up as continuing to smoke, we did not use adjusted effect size (contrast-level data) in our meta-analyses. Instead, we adjusted for clustering by replacing the actual sample size of each study with the effective sample size, calculated using an intracluster correlation coefficient (ICC) from the study report where available, or of 0.02, as recommended by

Campbell and colleagues [54]. Trials may use a variety of statistical methods to investigate or compensate for clustering. We recorded whether studies reported these methods and whether the significance of any effect was altered in Table 2.

In the case of multi-arm trials, we included each pair-wise comparison separately, with shared intervention groups divided approximately evenly amongst the comparators. However, if the intervention groups were deemed similar enough to be pooled, we combined the groups using appropriate formulas, as set out in the *Cochrane Handbook* [51].

Dealing with missing data

We conducted our analyses on an intention-to-treat basis (counting all participants as members of the arms to which they were originally randomised, regardless of whether they received the intervention), and counting participants lost to follow-up as continuing to smoke. We extracted numbers lost to follow-up from study reports as part of our assessment of risk of attrition bias. If information we needed on study methods or outcome results was not available in study reports, we contacted the authors to request the information.

Reporting bias assessment

Where 10 or more studies contributed to any comparison, we assessed the risk of reporting bias using a funnel plot. Regardless of the number of studies included, we considered and narratively discussed the possibility of reporting bias.

Synthesis methods

For each of our outcomes, we conducted meta-analyses of studies measuring that outcome using the random-effects model to estimate pooled treatment effects (as RRs with 95% CIs). We conducted the following comparisons:

- training versus no training;
- more intensive training versus less intensive training;
- training plus adjunct versus training alone.

We expected some study heterogeneity due to factors such as differing characteristics of clinics, practices, and medical surgeries, differences in intervention characteristics, and varying measurement tools used to assess outcomes. We used the I^2 statistic to quantify inconsistency across studies [51]. This describes the percentage of the variability in effect estimates that is due to heterogeneity rather than chance. We interpreted I^2 results using the following rough guide:

- 0% to 40%: might not be important;
- 30% to 60%: may represent moderate heterogeneity;
- 50% to 90%: may represent substantial heterogeneity;
- 75% to 100%: represents considerable heterogeneity.

Investigation of heterogeneity and subgroup analysis

In anticipation of heterogeneity amongst the included studies, we planned three subgroup analyses for each outcome under our main comparison of training for healthcare professionals versus no training. We grouped studies by:

- intensity of training provided (subgroups: up to one hour of training; between one and four hours of training; over four hours of training);
- type of healthcare professional being trained (subgroups: doctors, dentists, pharmacists, mixed healthcare professionals);
- type of treatment recommended by training (subgroups: brief behavioural support; non-brief behavioural support; behavioural support plus pharmacotherapy).

Sensitivity analysis

We conducted sensitivity analyses by removing studies at high risk of bias.

Certainty of the evidence assessment

Following standard Cochrane methods [55], we created a summary of findings table that presents results for our critical outcome of smoking abstinence for four comparisons:

- training versus no training;
- more intensive training versus less intensive training;
- nicotine replacement therapy as adjunct to training verus versus training alone;
- prompts as adjunct to training versus training alone.

We used the five GRADE considerations (risk of bias, inconsistency, imprecision, indirectness, and publication bias) to assess the certainty of the body of evidence for the abstinence outcome for each comparison.

Equity considerations

We have not investigated health inequity in this review, as this is explored in a separate Cochrane review [56].

Consumer involvement

Consumers were not specifically involved in the development of the review protocol, although a consumer was consulted to ensure the accessibility and relevance of the plain language summary.

Results

Description of studies

See Table 1, Supplementary material 2, and Supplementary material 3.

Results of the search

Our most recent searches found 3116 records. From this, 20 studies (34 records) met the inclusion criteria (see Figure 1 for PRISMA diagram). Seven of these studies were ongoing at the time of review update completion, leaving 13 eligible completed studies. Seventeen studies were included in the previous version of this review, of which we excluded two (Swartz 2002 [57, 58, 59, 60]; Twardella 2007 [61, 62, 63]). We also included one study (Pereira 2006 [64]) which was previously excluded, after finding additional data in another Cochrane review [65]. Therefore, we included 29 studies in this review update. We

requested and received more information from two study authors, which has been included in the review (Kastaun 2021 [66, 67, 68, 69, 70]; Klein 2020 [71, 72]). We present detailed information for each included study in Supplementary material 2, and for ongoing studies in Supplementary material 4.

Included studies

Design

All 29 included studies used a randomised controlled trial design, randomising by healthcare professional, and in 15 studies, by practices/hospitals (e.g. general practitioners nested within practice clusters) (Butler 2013 [73]; Cohen (Dent) 1989 [74, 75]; Cohen (Doc) 1989 [76, 77]; Cummings (Priv) 1989 [78]; Gordon 2010 [79, 80]; Hymowitz 2007 [81, 82, 83, 84, 85]; Kastaun 2021; Klein 2020; Kottke 1989 [86]; Lennox 1998 [87]; Olano-Espinosa 2013 [88]; Papadakis 2018 [89, 90]; Strecher 1991 [91, 92, 93]; Unrod 2007 [94]; Wilson 1988 [95, 96]).

Sample sizes

In total, a reported 38,178 participants were assessed at baseline (following randomisation) with 25,025 remaining in the studies at final follow-up. Authors report a total of 5395 individual health professionals recruited at baseline (across a known 565 practices/clinical systems), with data available for 4030 completing. Ten of the included studies did not report the number of individual health professionals receiving training. Sample sizes for individual studies were medium to large, with the smallest number of patients (randomised at baseline) found in Wang 1994 [97] (n = 93) and the largest in Olano-Espinosa 2013 (n = 5970). The smallest sample at follow-up remained with Wang 1994 (n = 82), and the largest remained with Olano-Espinosa 2013 (n = 5910). At the health professional level, Wang 2024 [98] had the largest number of health professionals randomised at baseline (n = 1887) and at follow-up with 1028, while Wang 1994 reported the smallest number of residents at baseline and follow-up (n = 27 for both). Thirteen studies also reported baseline cluster sizes at the practice level: Lennox 1998 (n = 16); Sinclair 1998 [99, 100] (n = 62); Joseph 2004 [101] (n = 20); Hymowitz 2007 (n = 16); Gordon 2010 (n = 14); Butler 2013 (n = 29); Olano-Espinosa 2013 (n = 35); Verbiest 2014 [102, 103] (n = 49); Caponnetto 2017 [104, 105] (n = 42); Papadakis 2018 (n = 15); Kastaun 2021 (n = 58); Klein 2020 (n = 120); and Wee 2020 [106] (n = 20), and clinical system level: Mejia 2016 [107, 108] (n = 6). Pereira 2006 did not report the number of health professionals recruited or trained, nor the number of participants at follow-up. Habfast 2024 [109, 110] did not report the number of health professionals randomised at baseline. Wee 2020 did not report the number of health professionals recruited; outcomes were assessed at the cluster level, with public hospitals as the unit of analysis (n = 20).

Setting

Eleven of the 29 studies were conducted in the USA, three each in the United Kingdom (Butler 2013; Jumbe 2022 [111, 112]; Lennox 1998) and Switzerland (Cornuz 2002 [113, 114, 115, 116, 117, 118, 119]; Grischott 2023 [120, 121, 122]; Habfast 2024), two in Canada (Papadakis 2018; Wilson 1988), and one each in Argentina (Mejia 2016), China (Wang 2024), France (Pereira 2006), Germany (Kastaun 2021), Italy (Caponnetto 2017), Malaysia (Wee 2020), the Netherlands (Verbiest 2014), Scotland (Sinclair 1998), Spain (Olano-Espinosa 2013), and Taiwan (Wang 1994). Three studies were performed in a pharmacy setting (Caponnetto 2017; Jumbe 2022; Sinclair 1998), two studies in a dentistry setting (Cohen (Dent) 1989; Gordon 2010), and one each in an HMO (Health Maintenance

Organisation) medical centre (Cummings 1989 [123]), a Veterans Affairs Medical Centre (VAMC) (Joseph 2004), a secondary care hospital setting (Wee 2020), and one across multiple levels including hospitals, clinics, and pharmacies (Wang 2024). The remaining 20 were conducted within primary care clinics.

Participants

At the health professional level, two studies were performed with dentists (Cohen (Dent) 1989; Gordon 2010), 10 studies included only primary care physicians (Cohen (Doc) 1989; Cummings (Priv) 1989; Grischott 2023; Habfast 2024; Kastaun 2021; Kottke 1989; Pereira 2006; Unrod 2007; Verbiest 2014; Wilson 1988), two studies were conducted with residents (Cornuz 2002 and paediatric residents in Hymowitz 2007), four studies incorporated a combination of primary care physicians and internists (Cummings 1989; Mejia 2016; Strecher 1991; Wang 1994), three studies used pharmacists (Caponnetto 2017; Jumbe 2022; Sinclair 1998), and one study used secondary care physicians (Wee 2020). The remaining seven studies used a combination of health professionals, including physicians, nurse practitioners, physician assistants, psychologists, pharmacists, and other health visitors (Butler 2013; Joseph 2004; Klein 2020; Lennox 1998; Olano-Espinosa 2013; Papadakis 2018; Wang 2024).

The individual patients in 27 of the 29 included studies were those visiting their health professional during the recruitment phase of each study. They were recruited during standard general practitioner (GP), dentist, or outpatient visits, emergency department visits, or from waiting rooms. Hymowitz 2007 was the only study to perform the training in a paediatric setting, targeting the parents/guardians of children visiting 16 primary care clinics. Wang 2024 did not report any information about their participant cohort.

Interventions

Treatment type

Five studies provided patients with counselling and nicotine replacement therapy (NRT) (Cohen (Dent) 1989; Cohen (Doc) 1989; Joseph 2004; Sinclair 1998; Wilson 1988). The two studies by Cohen and colleagues had a second intervention arm of counselling plus a reminder for physicians/dentists to ask about smoking (chart prompt), and a third intervention arm combining the counselling, nicotine replacement therapy, and chart prompt (Cohen (Dent) 1989; Cohen (Doc) 1989). The Wilson 1988 study had two intervention arms in addition to usual care: counselling and nicotine gum (as mentioned above) and a second arm of nicotine gum plus usual care (i.e. physicians were not trained in counselling). Nine studies included multiple intervention methods to curtail smoking, including counselling, nicotine replacement therapy, request for additional follow-up appointments, and/or provision of self-help materials (Cummings (Priv) 1989; Cummings 1989; Gordon 2010; Grischott 2023; Habfast 2024; Jumbe 2022; Papadakis 2018; Pereira 2006; Wang 2024), whilst one study combined three of those four (counselling, nicotine replacement therapy, and self-help materials, Cornuz 2002). Nine studies used counselling alone (Butler 2013; Caponnetto 2017; Klein 2020; Lennox 1998; Olano-Espinosa 2013; Strecher 1991; Unrod 2007; Wang 1994; Wee 2020), four studies used counselling with the addition of self-help materials (Hymowitz 2007; Kastaun 2021; Kottke 1989; Verbiest 2014). One study included monthly e-mails to physicians as reminders with useful tips to help patients stop smoking or manage withdrawals (Mejia 2016).

Treatment intensity

The level of training intensity for health professionals ranged from one 40-minute session in the Unrod 2007 study, to eight weeks of text messages in the Wang 2024 study. Thirteen studies had a training session for one day or less: Wilson 1988 (four hours), Cohen (Dent) 1989 (one hour), Cohen (Doc) 1989 (one hour), Kottke 1989 (six hours), Lennox 1998 (one day), Sinclair 1998 (two hours), Unrod 2007 (40 minutes), Gordon 2010, Papadakis 2018, and Jumbe 2022 (all three hours), Verbiest 2014 (one hour), Kastaun 2021 (3.5 hours), and Habfast 2024 (2.5 hours). Seven studies had two separate sessions: Strecher 1991 (two 1-hour sessions scheduled two weeks apart), Wang 1994 (two sessions of unknown duration), Cornuz 2002 (two 4-hour training sessions scheduled two weeks apart), Butler 2013 (two seminars at each intervention practice attended by one GP and one practice nurse), Mejia 2016 (two 3-hour training sessions with no mention of time between each session), Caponnetto 2017 (two sessions: one 3-hour session that the control group also received, and one 6-hour session for the intervention group only) and Wee 2020 (two days in groups of 20 to 30 practitioners). Seven studies had three or more sessions: Cummings (Priv) 1989 and Cummings 1989 both had three 1-hour sessions over a 4- to 5-week period, Pereira 2006 describes a 3-day training session, Hymowitz 2007 had four 1-hour sessions four times a year, Olano-Espinosa 2013 had four sessions of 90 minutes each, Joseph 2004 had four to five day-long sessions within six months, and Grischott 2023 had five 4-hour sessions. Intensity was not clear in one study (Klein 2020), as clinicians practised screening and counselling delivery with at least three patients and participated in "teach-back" phone calls, completing role playing with study staff until proficient.

Mode of intervention delivery

Three different modes of intervention delivery were used: group sessions, one-on-one, or a combination of the two. Six studies only used one-on-one sessions (Butler 2013; Joseph 2004; Klein 2020; Unrod 2007; Verbiest 2014), with one using digital one-on-one sessions via text message (Wang 2024). Eighteen studies delivered the intervention in a group setting only (Caponnetto 2017; Cummings 1989; Gordon 2010; Grischott 2023; Habfast 2024; Hymowitz 2007; Jumbe 2022; Kastaun 2021; Kottke 1989; Lennox 1998; Mejia 2016; Olano-Espinosa 2013; Papadakis 2018; Sinclair 1998; Strecher 1991; Wang 1994; Wee 2020; Wilson 1988), with another study using group delivery as the primary mode; however, doctors who were unable to attend received a private session in their office (Cummings (Priv) 1989). One study did not describe the mode of intervention delivery (Pereira 2006). Finally, three studies used both modes of intervention delivery (Cohen (Dent) 1989; Cohen (Doc) 1989; Cornuz 2002), with health professionals in the two studies by Cohen and colleagues given the option of a group or individual session.

Theoretical model – behavioural change technique

Eighteen studies used behavioural change theories to underpin the intervention techniques. These included the 'stages of change' (also known as the trans-theoretical) model (Butler 2013; Caponnetto 2017; Cornuz 2002; Kottke 1989; Lennox 1998; Olano-Espinosa 2013; Sinclair 1998; Strecher 1991; Wang 1994) and the '5A' (Ask, Assess, Advise, Assist, and Arrange) approach (Gordon 2010; Kastaun 2021; Klein 2020; Mejia 2016; Papadakis 2018; Unrod 2007; Verbiest 2014, Wang 2024). One study was guided by the UK National Centre for Smoking Cessation and Training (NCSCT) standard treatment program (Wee 2020), another by the Vivre sans tabac ("Living Without Tobacco") curriculum, which is considered usual care in French-speaking Switzerland (Habfast 2024).

Type of professional being trained

Two studies focused only on dentists (Cohen (Dent) 1989; Gordon 2010), three focused on pharmacists (Caponnetto 2017; Jumbe 2022; Sinclair 1998), and the remaining 24 studies all involved doctors. Five of these 24 studies included doctors still undergoing training, either residents (Cornuz 2002; Hymowitz 2007; Strecher 1991; Wang 1994) or a combination of physicians and internists (Cummings 1989). Eight other studies trained other healthcare workers as well as doctors: Lennox 1998 involved nurses and other health visitors; Joseph 2004 included nurses, psychologists, and pharmacists; Butler 2013 included general practitioners and practice nurses; Olano-Espinosa 2013 included general practitioners and nurses; Papadakis 2018 included family health teams involving physicians, nurse practitioners, nurses, pharmacists, and other health professionals; Klein 2020 included paediatricians, nurse practitioners, and physician assistants; Wee 2020 included medical officers, pharmacists, health education officers, nurses, and medical assistants; and Wang 2024 included all healthcare service providers in the recruiting hospitals, clinics, or pharmacies.

Length of follow-up

Nineteen studies reported follow-up periods of between six and nine months after the intervention (Caponnetto 2017; Cohen (Dent) 1989; Cohen (Doc) 1989; Gordon 2010; Habfast 2024; Jumbe 2022; Kastaun 2021; Klein 2020; Lennox 1998; Mejia 2016; Olano-Espinosa 2013; Papadakis 2018; Sinclair 1998; Strecher 1991; Unrod 2007; Verbiest 2014; Wang 1994; Wang 2024; Wee 2020), 15 studies presented 12-month follow-up data (Butler 2013; Cohen (Dent) 1989; Cohen (Doc) 1989; Cornuz 2002; Cummings 1989; Gordon 2010; Grischott 2023; Joseph 2004; Klein 2020; Kottke 1989; Mejia 2016; Olano-Espinosa 2013; Pereira 2006; Wang 1994; Wilson 1988), and two studies assessed extended follow-up periods of 14 months (Lennox 1998) and four years (Hymowitz 2007). However, only 2-year post-intervention data were available for Hymowitz 2007 at the time of writing.

Outcomes

All but one included study assessed smoking abstinence through self-report of either continuous abstinence (no smoking for an extended period of time) or point prevalence (for example, no smoking for seven days prior to the time of outcome collection) (Jumbe 2022). Of the 14 studies that reported continuous abstinence, eight also reported a point prevalence measure of abstinence (Cummings (Priv) 1989; Cummings 1989; Gordon 2010; Lennox 1998; Papadakis 2018; Sinclair 1998; Verbiest 2014; Wilson 1988). Fifteen of the included studies used biochemical validation through either exhaled carbon monoxide (Caponnetto 2017; Cohen (Dent) 1989; Cohen (Doc) 1989; Cornuz 2002; Strecher 1991; Wee 2020), serum cotinine (Kottke 1989), saliva cotinine (Butler 2013; Grischott 2023; Olano-Espinosa 2013; Papadakis 2018; Unrod 2007; Wilson 1988), or a combination of exhaled carbon monoxide and serum cotinine (Cummings (Priv) 1989; Cummings 1989). Seven included studies assessed the review's important outcome – the number of participants who made a quit attempt.

Excluded studies

We excluded 57 studies for the following reasons: 17 included ineligible outcomes, 17 featured an ineligible study design, 15 described an ineligible intervention, four featured an ineligible patient population, three were undertaken in an ineligible healthcare population, and one featured an ineligible comparator. See Supplementary material 3 for more detailed information about the excluded studies.

Risk of bias in included studies

We provide methodological details for the 29 included studies in the risk of bias tables in Supplementary material 2. Key methodological features are also summarised in Figure 2 and Figure 3. We assessed 10 studies as having an overall low risk of bias, 17 with an overall unclear risk, and two with an overall high risk of bias.

Random sequence generation (selection bias)

Fourteen studies reported adequate methods of sequence generation (Butler 2013; Cornuz 2002; Cummings 1989; Grischott 2023; Habfast 2024; Hymowitz 2007; Jumbe 2022; Kastaun 2021; Olano-Espinosa 2013; Papadakis 2018; Unrod 2007; Verbiest 2014; Wang 2024; Wee 2020), and two were at a high risk of bias (Strecher 1991; Wang 1994). The remaining 13 studies provided insufficient information to assess risk of bias for sequence generation and were thus judged to be at unclear risk in this category. Adequate methods included the use of a random number generator or coin toss, whilst unclear methods were described as being 'random' in design, but lacked specific details of methods. The Wang 1994 study assigned physicians to one of three groups by number of years in practice. In Strecher 1991, appropriate randomisation did not occur as residents were assigned by clinic half-day session to one of four groups, which risks introducing bias.

Allocation concealment (selection bias)

Allocation concealment was unclear in 22 included studies as authors did not describe methods of allocation concealment. Four studies had a low risk of bias for allocation concealment (Butler 2013; Grischott 2023; Jumbe 2022; Wang 2024), with methods of allocation concealment clearly reported. Three studies had a high risk of bias for allocation concealment (Unrod 2007; Wang 1994; Wee 2020), as the authors clearly reported an open-label recruitment process.

Blinding of outcome assessors (detection bias)

Fourteen studies reported methods for blinding of outcome assessors that we judged to be at low risk of bias. We considered nine studies to have a high risk of detection bias, as assessors were not blinded to the intervention, no biochemical validation of abstinence was reported, and the control group participants had less contact time compared with the intervention group participants (Cohen (Dent) 1989; Cohen (Doc) 1989; Gordon 2010; Hymowitz 2007; Lennox 1998; Mejia 2016; Sinclair 1998; Verbiest 2014; Wang 1994). The remaining six studies had an unclear risk of bias.

Incomplete outcome data (attrition bias)

We rated 18 studies which adequately accounted for any incomplete outcome data as having a low risk of bias in this domain, and seven studies as having an unclear risk (Cohen (Doc) 1989; Habfast 2024; Mejia 2016; Pereira 2006; Unrod 2007; Verbiest 2014; Wilson 1988). We assessed four studies as having a high risk of attrition bias (Caponnetto 2017; Cohen (Dent) 1989; Jumbe 2022; Wee 2020). Cohen (Dent) 1989 lost more than 50% of randomised participants by the 6-month and 12-month follow-up time points. Caponnetto 2017 lost entire clusters to follow-up, with half of the pharmacies (n = 21/42) lost to follow-up. Wee 2020 reported significantly higher attrition rates in the control group than in the intervention group (52.4% versus 23.3%). In Jumbe 2022, just 11% of smokers consented to provide additional data on quit outcomes at six months, together with saliva samples. Of

these 134 consenting treated smokers, 61 (28 from the intervention arm) were contactable for their 6-month follow-up data collection. Three treated smokers withdrew their consent when contacted at six months, and only five (one from the intervention arm) provided saliva samples. All studies rated as having an unclear risk of bias failed to mention if there were any missing outcome data and, if so, how this was addressed when reporting results.

Selective reporting (reporting bias)

Selective reporting was evident in one study (Gordon 2010): the authors reported that secondary participant outcomes were examined, with no significant differences in any variables, and that therefore, they were not presented in the publication. This study also reported receipt-of-intervention secondary outcomes as percentages in the text, but did not provide comparable information for the control group. We assessed seven studies as having a low risk of bias for selective reporting as they pre-registered or published protocols outlining the work with all outcomes published as stated (Butler 2013; Grischott 2023; Kastaun 2021; Klein 2020; Papadakis 2018; Sinclair 1998; Wang 2024). We assessed the remaining 21 studies as having an unclear risk of bias.

Other bias

We rated three studies as being at high risk of 'other' bias. Strecher 1991 had a high risk of bias due to likely contamination, as clinicians in different study arms worked closely with one another. Gordon 2010 was at high risk because of potential confounding from tobacco tax increases and expanded cessation resources that affected only some clusters. In Wang 2024, the authors provided no information on the patients included in the analysis, such as inclusion criteria, sample demographics, recruitment methods, data collected, or the total number of patients. We judged Caponnetto 2017 as having an unclear risk of bias in this domain because information about products provided to intervention/control participants was reported to have been collected but was not presented in the manuscript. We also judged Pereira 2006 to have an unclear risk of 'other' bias.

Recruitment bias

We classified 13 studies as being at low risk of recruitment bias, 10 studies at unclear risk due to insufficient data reported, and six studies at high risk of bias (Cummings 1989; Cummings (Priv) 1989; Klein 2020; Sinclair 1998; Unrod 2007; Wee 2020). Of these, five studies reported recruitment of participants following randomisation of clusters (Cummings 1989; Klein 2020; Sinclair 1998; Unrod 2007; Wee 2020). Cummings (Priv) 1989 reported that some physicians allocated to the control group were reluctant to permit research staff to survey their waiting rooms. As a result, more smokers in the control group (41%) than in the intervention group (24%) were enroled by staff within the clinic.

Balanced baseline characteristics

We judged 18 studies to be at low risk of bias, reporting no significant between-group differences, and nine studies to be at unclear risk due to insufficient data reported. We judged two studies to be at high risk of bias: Gordon 2010 reported that participants in the intervention group were more likely to be single, to have smoked for more years, to have higher levels of craving, to be thinking about quitting, and to be more ready to quit, while Wee 2020 reported statistically significant differences between groups, especially for "confidence to stop smoking".

Adjustment for clustering in analysis

We considered 20 studies to be at low risk of bias in this domain due to clear reporting of adjustment methods, five studies at unclear risk of bias due to insufficient information reported, and four studies to be at high risk of bias as they made no clustering adjustments (Joseph 2004; Kottke 1989; Strecher 1991; Wang 2024).

Synthesis of results

Training versus no training

Abstinence from smoking

Sixteen studies assessed the effect of training healthcare professionals in smoking cessation on the number of participants abstinent at longest follow-up compared with no training. We found high-certainty evidence that smoking cessation training for healthcare professionals increases patient smoking cessation compared with no training (RR 1.34, 95% CI 1.08 to 1.67; I^2 = 48%; 16 studies, 16,513 participants; Analysis 1.1; Figure 5).

We conducted a sensitivity analysis removing 11 studies at high risk of bias in one or more domains. The five remaining studies at low and unclear risk of bias showed a similar result (RR 1.77, 95% CI 0.99 to 3.17; I^2 = 66%; 5 studies, 7714 participants). We excluded Wang 2024 from the meta-analysis as abstinence outcomes were reported by the healthcare professionals. Overall, more participants in the intervention group (19.63%) reported abstinence at 34 weeks compared with the control group (18.8%) (P = 0.27).

Subgroup analyses

We conducted three subgroup analyses to test the effect of specific potential sources of heterogeneity.

Training intensity

To evaluate the intensity of the training, we divided studies into those providing: up to one hour of training (one study); between one and four hours (six studies); and over four hours of training (seven studies). We excluded Joseph 2004 and Wang 1994 from this subgroup analysis as the duration of training was unclear. Statistical tests found no evidence of between-group differences (P = 0.91; I^2 = 0%).

Type of healthcare professional trained

We divided studies by the type of healthcare professional given training: doctors (10 studies), dentists (one study), pharmacists (one study), and mixed healthcare professionals (four studies). Heterogeneity remained moderate in the subgroups with multiple studies, though statistical tests found no evidence of between-group differences (P = 0.16; I^2 = 42%). When we performed an ad hoc sensitivity analysis removing subgroups to ascertain the source of the heterogeneity, we found that removing the dentist subgroup accounted for all the heterogeneity.

Treatment recommended in the training

We divided studies by the type of support recommended by the training: brief behavioural support (two studies), non-brief behavioural support (three studies), and combination behavioural and pharmacological support (nine studies). We excluded Lennox 1998 and Sinclair 1998 from this subgroup analysis as they did not report the recommended support.

While the brief behavioural support subgroup showed no evidence of heterogeneity and the other groups displayed substantial heterogeneity, statistical tests found no evidence of between-group differences (P = 0.78; I^2 = 0%).

Quit attempts

Six studies assessed the effect of training healthcare professionals in smoking cessation on the number of participants attempting to quit. We found evidence of more quit attempts amongst people receiving care from healthcare professionals who had been trained compared with those whose healthcare professionals were not trained (RR 1.07, 95% CI 0.91 to 1.26; I^2 = 0%; 6 studies, 6059 participants; Analysis 1.2). We conducted a sensitivity analysis removing five studies at high risk of bias. The one remaining study was at unclear risk of bias and did show a benefit, though confidence intervals included the potential for no effect (RR 1.44, 95% CI 0.93 to 2.24; 1024 participants).

Subgroup analyses

We conducted subgroup analyses dividing studies by training intensity and type of healthcare professional trained. Neither analysis showed evidence of a between-group difference (intensity: P = 0.88; I^2 = 0%; type of professional: P = 0.49; I^2 = 0%). We could not conduct subgroup analysis by treatment recommended in the training because all studies combined behavioural and pharmacological support.

More intensive training versus less intensive training

Abstinence from smoking

Four studies assessed the effect of high-intensity training for healthcare professionals on the number of participants abstinent at longest follow-up compared with lower-intensity training. When we pooled these studies, more people successfully quit in the higher-intensity training arm compared with lower-intensity training, though confidence intervals were wide and included the potential for no benefit (RR 1.64, 95% CI 0.86 to 3.12; I^2 = 54%; 4 studies, 1151 participants; low-certainty evidence; Analysis 2.1; Figure 6). This analysis included Habfast 2024, which reported only follow-up numbers via conference abstract. Excluding this study and analysing only based on randomised numbers resulted in an RR of 2.08 (95% CI 1.11 to 3.90; I^2 = 30%; 961 participants). The comparator training in Wee 2020 was significantly less intensive than in the other studies in the analysis, so we conducted a sensitivity analysis removing this study. Without Wee 2020, the RR was 1.24, with 95% CI 0.61 to 2.56 (I^2 = 31%; 3 studies, 818 participants). We conducted a sensitivity analysis removing two studies at high risk of bias, resulting in RR 1.02 (95% CI 0.57 to 1.81; I^2 = 0%; 643 participants). We downgraded the evidence by two levels to low certainty for very serious imprecision.

Quit attempts

Only Papadakis 2018 assessed the effect of high-intensity versus lower-intensity training for healthcare professionals on the number of participants attempting to quit. This study did not find evidence of increased quit rates from higher-intensity training (RR 1.20, 95% CI 0.50 to 2.89; 1 study, 453 participants; Analysis 2.2). We could not conduct our planned sensitivity analysis because there was only one study.

Adjuncts to healthcare professional training

Abstinence from smoking

Three studies assessed the impact of adjuncts to training on the number of participants abstinent at longest follow-up. We found low-certainty evidence that when the healthcare professionals treating them are trained in smoking cessation, more people may quit when also provided with nicotine replacement therapy (NRT), though confidence intervals were wide and included the potential for no benefit (RR 1.64, 95% CI 0.72 to 3.71; I^2 = 69%; 2 studies, 1892 participants; Analysis 3.1, Figure 7). We downgraded the evidence by two levels for imprecision and risk of bias. We also found very low-certainty evidence that providing prompts to healthcare professionals in addition to smoking cessation training may help more people to quit, though confidence intervals were wide and included the potential for no benefit (RR 1.37, 95% CI 0.69 to 2.70; I^2 = 66%; 3 studies, 2429 participants; Analysis 3.1.2). We downgraded the evidence to very low certainty due to clinically significant imprecision (two levels) and risk of bias (one level). Cohen (Dent) 1989 and Cohen (Doc) 1989 each had four study arms, testing the effect of both NRT and prompts in addition to training, so each contributed twice to each analysis. To conduct these analyses, we had to assume study arm denominators for Cohen (Dent) 1989 and Cohen (Doc) 1989, as only overall denominators were reported in the studies themselves. Removing these studies from the analyses left insufficient data to pool, as only Strecher 1991 remained, showing no evidence of benefit from adjunct prompts.

Quit attempts

No studies assessed the impact of adjuncts to training on the number of participants attempting to quit.

Reporting biases

We were able to create funnel plots to test for publication bias in one comparison: training versus no training (Figure 4). Two outlying studies in this plot may indicate the potential for publication bias. However, given the behavioural nature of the intervention, this may not represent true publication bias.

Discussion

Summary of main results

Twenty-nine completed studies (a total of 38,178 smokers and 4030 health professionals) assessed the benefits of interventions to train health professionals to provide smoking cessation initiatives to their patients. Whilst there were methodological variations between studies in relation to intervention, delivery mode, type of health professional, and duration, they were all aimed at training health professionals to help their patients stop smoking. We presented the critical outcome of abstinence from smoking in pooled meta-analyses: high-certainty evidence shows that healthcare professionals who had received training had more abstinent patients at longest follow-up than those who received no training. Low-certainty evidence suggests that more intensive training may be more effective than less intensive training, though confidence intervals did include the potential for no benefit. We found evidence that suggests a benefit from adjuncts to training, such as nicotine replacement therapy (NRT) (low-certainty evidence) or reminders in medical notes (very low-certainty evidence), for smoking abstinence (Summary of findings table 1). However, in both cases,

confidence intervals were wide and included the potential for no benefit. We did not find that healthcare professional training compared with no training or less intensive training increased the number of participants reporting a quit attempt.

Limitations of the evidence included in the review

Study quality represented a moderate limitation of this review. Although many studies had an unclear risk of bias for random sequence generation and allocation concealment, we judged most to be at low risk for attrition bias and other sources of bias. We did not assess performance bias as blinding of healthcare providers in smoking cessation training trials is not feasible due to the nature of the intervention, and blinding of patients is also often not feasible. Therefore, in line with the Cochrane Tobacco Addiction Group guidelines for risk of bias assessment, we based our assessment of detection bias on the presence of biochemical validation and balance of intervention intensity across all arms.

Overall, the body of evidence identified permits a moderately robust conclusion regarding the objectives of this review. We downgraded the evidence presented in the summary of findings table to take into account:

- limitations in design: methods of randomisation, allocation concealment, and/or blinding were not described or inadequate in most studies assessing the particular outcome (downgraded by one level);
- imprecision: there were only a few participants in a few studies available to assess the outcome (downgraded by one or two levels).

In the context of current practice, this review indicates which interventions have a proven effect, and where resources need to be directed for future investigations. Studies which incorporated multiple intervention components – such as provision of nicotine replacement therapy or self-help materials and requests for follow-up appointments – may be more likely to be successful than those with counselling interventions alone, though the evidence is uncertain. Surprisingly, health professionals who were trained using only a single session and in a group setting were just as likely, if not more likely, to have patients quit smoking as those being trained with multiple delivery sessions and one-on-one training (i.e. face-to-face with the trainer). Similarly, shorter training durations (i.e. between 40 minutes and two hours of training overall) were just as effective, and in some cases more so, than training durations exceeding two hours. Studies with multiple follow-up periods and closer monitoring of outcomes by investigators (including the provision of feedback) were more successful than those of lesser intensity. Smoking cessation interventions delivered by a doctor or dentist were more likely to produce successful quit attempts than those delivered by other healthcare workers.

To ensure methodological rigour, future studies should aim to:

- report patient-level outcomes (e.g. abstinence from smoking) as well as health professional outcomes (e.g. physician report of number of smokers counselled), rather than providing details only relating to the consultation process;
- use adequate methods of randomisation and allocation concealment;
- report smoking-related outcome data both pre- and post-intervention;
- incorporate a control group which adequately matches the demographic characteristics of the intervention population.

Limitations of the review processes

We excluded non-randomised and quasi-randomised studies, as well as studies that did not assess our critical outcome of abstinence from smoking six months or more after baseline. Although this approach may have excluded some relevant information, it allowed us to conduct a meta-analysis based on higher-quality evidence on which future research can build. We discuss some of the pertinent information from some of these studies below (see Agreements and disagreements with other studies or reviews), though results should be interpreted with caution.

A key strength of the review process was that two experienced, independent review authors assessed the risk of bias in the included studies, helping to minimise subjective assessment bias. However, this approach cannot fully address potential biases arising from the included studies' methods and reporting, of which we identified three key limitations. First, some studies were insufficiently detailed about their intervention(s). This means that some studies may have included additional intervention components that, had we known they existed, would have led us to classify the study differently within the subgroups. Second, the studies varied widely in terms of how they classified abstinence, which impeded meta-analysis. For example, some more recent studies using biochemical validation reported failed tests amongst some participants that self-reported abstinence. Consequently, study authors excluded these results from analyses. In contrast, for older studies, the absence of biochemical validation may have led to less precise estimates of smoking abstinence. In other studies, self-reported cessation may have been disqualified in the absence of biochemical confirmation. Third, many included studies did not adequately adjust for potential clustering effects. We therefore applied these adjustments – using assumed intracluster correlation coefficients (ICCs) in some cases – to enable meta-analysis.

Agreements and disagreements with other studies or reviews

The results of this review are consistent with findings throughout the literature. For example, in a 2019 systematic review, Hyndman and colleagues found, with low-moderate certainty, that tobacco dependence training for student health professionals resulted in more patients reporting quitting at six months (OR 2.02, 95% CI 1.49 to 2.74; P < 0.001; 3 studies) and at 12 months (OR 1.04, 95% CI 0.54 to 2.01; P = 0.04; 3 studies) [124]. Due to differences in review scopes (the Hyndman review included non-RCTs, and was restricted to student or entry-level health professionals), only two studies were included in both the Hyndman and this review (Cornuz 2002; Strecher 1991). However, results indicate agreement that providing training to health professionals can improve cessation rates in patients. Other reviews have identified an increase in health professional knowledge, attitudes, and self-efficacy following training [125, 126], though these did not report an impact of training on quit support behaviours or patient quit outcomes.

In 2009, the journal *Drug and Alcohol Review* published a special issue dedicated to work addressing education and training for health professionals and students in tobacco, alcohol, and other drugs [127]. While the special issue covered an array of topics, a key theme concerned barriers to the uptake, implementation, and sustainability of smoking cessation training programmes, as well as potential approaches to overcoming these barriers. For example, Richmond and colleagues identified a lack of interest amongst healthcare professionals (who regarded other continuing education topics as a higher priority) and lack of funding for interventions as major barriers [17]. They argued that raising awareness of the importance of smoking abstinence to the health of patients and incorporating education on

smoking cessation into vocational courses for specialities could help to overcome these barriers. In a systematic review of findings from nine studies about postgraduate smoking cessation training for physicians in 28 European countries, Kralikova and colleagues found that such training may not be reaching physicians and was not rigorously evaluated [128]. They and other authors featured in the special issue argued that future research needs to incorporate methods of disseminating effective educational activities with the intention of increasing healthcare professionals' participation [128, 129]. Relatedly, Botelho and colleagues argued that health professional organisations should advocate for the systematic implementation of training programmes to increase the number of patients who receive help to quit [130].

A related theme evident in the wider literature concerns health professionals' resistance toward or reluctance about implementing smoking cessation interventions. For example, in a 2001 study using direct observation of physician-patient encounters, Ellerbeck and colleagues concluded that strategies are needed to assist physicians to incorporate systematic approaches that will standardise smoking cessation care [131]. They reported that discussions about tobacco were more common in practices that utilised standard forms for recording smoking status and during new patient visits. Interestingly, they also found that physicians who had been in practice for more than 10 years discussed tobacco use less often than those with fewer than 10 years of experience, and that older patients were less likely to be engaged in tobacco discussions compared with younger patients [131]. Similarly, Albert and colleagues observed considerable resistance in a cohort of dentists receiving 'academic detailing' (a structured form of educational outreach) to promote tobacco-use cessation counselling in dental offices [132]. Dental staff members were reluctant to participate in the interventions because of increased paperwork, having to deal with recalcitrant patients, and perceptions that few patients use tobacco and counselling does not work [132]. However, Albert and colleagues reported that the initial resistance decreased as follow-up visits progressed and staff became more comfortable with the intervention and the procedures involved.

Finally, the relative costs of smoking cessation training interventions is a theme taken up by some researchers. For example, in a study included in this review, Cornuz and colleagues reported that training residents in smoking cessation counselling is very cost-effective and may be more efficient than most currently accepted tobacco control interventions (Cornuz 2002). This finding has been supported by other systematic reviews and investigations [133, 134, 135]. Ultimately, the provision of counselling, advice, and/or offers of assistance to the patient has the potential to significantly increase the number of quit attempts, which subsequently has the potential to reduce health-related costs, as well as morbidity and mortality associated with ongoing chronic tobacco use.

Authors' conclusions

Implications for practice

There is high-certainty evidence that providing smoking cessation training to healthcare professionals probably increases the likelihood that their patients will quit smoking compared with no training.

It may be the case that providing higher-intensity training, or providing adjuncts to training, such as prompts or nicotine replacement therapy (NRT), results in more people quitting smoking compared with lower-intensity training or training without adjuncts, respectively. However, in all cases, the 95% confidence intervals included the potential for no benefit.

The evidence was limited by unclear methodology and lack of biochemical validation of findings (i.e. biological tests to confirm reports of non-smoking amongst patients).

Implications for research

Multi-component investigations incorporating new pharmacological interventions for smoking cessation (such as varenicline and bupropion) or other cessation aids (such as electronic cigarettes) alongside physician training should be considered to determine if any additional benefit in long-term abstinence can be obtained.

In future studies, investigators should use rigorous methods for sequence generation and allocation concealment, and should report smoking-related outcome data clearly and consistently. Ideally, investigators should collect process data (including intervention delivery, uptake, and context) both before and after the intervention to improve our understanding of how interventions can be implemented to maximise their effectiveness. To enable interventions to be replicated in clinical practice, it is also important that authors of future trial reports describe the content of the training in sufficient detail; for example, detailing the educational methods, strategies, and theories used to train the professionals.

Future reviews could consider including rigorous pragmatic clinical trials in addition to randomised controlled trials, and focus on the effect of training healthcare professionals on patient referral to tobacco treatment specialists and other counselling options. Exploring outcomes such as health professional adherence to brief tobacco treatment models (such as the Ask Advise Refer (AAR) or the 5As model (ask, advise, assess, assist, and arrange)) following training would be beneficial for future reviews. However, most existing studies do not measure or report outcomes in ways that align with these established models. Future research should therefore adopt outcome measures explicitly mapped to recognised tobacco treatment frameworks.

Supplementary materials

[For display in the published PDF only] Supplementary materials are available with the online version of this article: 10.1002/14651858.CD000214.pub3.

[For display on the Cochrane Library only] Supplementary materials are published alongside the article and contain additional data and information that support or enhance the article. Supplementary materials may not be subject to the same editorial scrutiny as the content of the article and Cochrane has not copyedited, typeset or proofread these materials. The material in these sections has been supplied by the author(s) for publication under a Licence for Publication and the author(s) are solely responsible for the material. Cochrane accordingly gives no representations or warranties of any kind in relation to, and accepts no liability for any reliance on or use of, such material.

Supplementary material 1

CD000214-SUP-01-searchStrategy.html

Search strategies

Supplementary material 2

CD000214-SUP-02-characteristicsOfIncludedStudies.html

Characteristics of included studies

Supplementary material 3

CD000214-SUP-03-characteristicsOfExcludedStudies.html

Characteristics of excluded studies

Supplementary material 4

CD000214-SUP-04-characteristicsOfOngoingStudies.html

Characteristics of ongoing studies

Supplementary material 5

CD000214-SUP-05-analyses.html

Analyses

Supplementary material 6

CD000214-SUP-06-dataPackage.zip

Data package

Additional information

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Editorial and peer-reviewer contributions

Cochrane Tobacco Addiction supported the authors in the development of this review update.

The following people conducted the editorial process for this article:

- Sign-off Editor (final editorial decision): Jamie Hartmann-Boyce, University of Massachusetts Amherst;
- Managing Editor (selected peer reviewers, provided editorial guidance to authors, edited the article): Luisa Fernandez Mauleffinch, Cochrane Central Editorial Service;
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- Peer-reviewers (provided comments and recommended an editorial decision): Jamie S Ostroff, Memorial Sloan Kettering Cancer Center (clinical/content review); Emma Axon, Cochrane Methods Support Unit (methods review); Jo Platt, Central Editorial Information Specialist (search review). One additional peer reviewer provided clinical peer review but chose not to be publicly acknowledged.

Contributions of authors

Kelsey Sharrad reviewed the literature, identified studies for inclusion, extracted data, entered and analysed data, updated the text of the manuscript, and updated the review template.

Kristin Carson-Chahhoud updated the protocol, reviewed the literature, identified studies for inclusion, extracted data, entered and analysed data, and updated the text of the manuscript.

Marjolein Verbiest updated the text of the manuscript.

Sarah Greenslade reviewed the literature, identified studies for inclusion, and extracted data.

Tom Parkhouse reviewed the literature, identified studies for inclusion, and extracted data.

Willem Assendelft assisted in updating the protocol and updating the text of the manuscript.

Mathilde Crone updated the protocol, identified studies for inclusion, and updated the text of the manuscript.

Jonathan Livingston-Banks updated the protocol, conducted searches, reviewed the literature, identified studies for inclusion, extracted data, entered and analysed data, and updated the text of the manuscript.

Declarations of interest

KS: University of Adelaide - Grant/Contract

KCC: is an associate editor for the Cochrane Airways Group. She was not involved in the editorial process for this review. Other interests include: Alterity Therapeutics Ltd – Stock; Asthma Australia – Grant/Contract; Channel 7 Children's Research Foundation – Grant/Contract; Houd Research Group – Other Business Ownership; National Health and Medical Research Council – Grants/Contracts; Ogilvy Health – Other Professional Activities; Pfizer Australia – Other Professional Activities; Sax Institute – Grants/Contracts; Thoracic Society of Australia and New Zealand – Other Professional Activities; University of South Australia – Employment; Women's and Children's Hospital – Employment.

MV: has declared involvement in included study Verbiest 2014. Please note that this review author was not involved in the eligibility decisions, data extraction, risk of bias assessment, or GRADE assessment process for this study.

SG: none known

TP: none known

WA: none known

MC: none known

JLB: is an associate editor for Cochrane. He was not involved in the editorial process for this review.

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Registration and protocol

This review was first published outside of Cochrane in 1994 [34] and subsequently updated as a Cochrane review in 2000 [35] (DOI: 10.1002/14651858.CD000214) and 2012 [26] (DOI: 10.1002/14651858.CD000214.pub2). No protocol was published or registered.

Data, code and other materials

As part of the published Cochrane review, the following is made available for users of the Cochrane Library: our search strategies (Supplementary material 1); full citations for all included studies, all ongoing studies, relevant excluded studies, and studies awaiting classification in the reference section of the review; study data, including study information, study arms and risk of bias assessments in our characteristics of studies tables (Supplementary material 2; Supplementary material 3; Supplementary material 4); analysis data, including overall estimates, subgroup estimates, and individual data rows (all the rows in all the analyses) is in the main review and in Supplementary material 5. Data supporting the results of this systematic review are from published information and are available in the review. All analyses have been conducted within RevMan; for details of the computational methods, see https://documentation.cochrane.org/revman-kb/statistical-methods-210600101.html. Data were extracted in Excel and are available from the authors on reasonable request. The data are shared within the published review (Supplementary material 6).

Notes

History

Protocol first published: Issue 2, 1996
Review first published: Issue 2, 1996

Date	Event	Description
12 February 2026	New citation required and conclusions have changed	Review update. Author list updated. Changes to methods, including: changed outcomes and structure of analyses, updated risk of bias assessment, and introduction of GRADE assessment. 14 new studies included.
12 February 2026	New search has been performed	New searches conducted 24 August 2024.

Date	Event	Description
5 December 2013	Amended	Correction to summary of findings table (confidence interval for continuous abstinence)
30 March 2012	New citation required and conclusions have changed	Structure of review changed, body of text updated and re-written; Conclusions changed.
30 March 2012	New search has been performed	Seven new studies added; SOF table, meta-analyses and summary of individual study effectiveness table added.
4 August 2008	Amended	Converted to new review format
31 May 2000	New citation required and conclusions have changed	Substantive amendment

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Tables

Table 1

Overview of included studies table

Study ID	Country	Type of healthcare professional	Study arms	Intervention	Overall risk of bias
Butler 2013	UK	Doctors, nurses	1. Training 2. No training (waiting-list)	Talking Lifestyle Learning program, providing skills to guide talking about behaviour change. Theoretical underpinning: motivational interviewing	Low
Caponetto 2017	Italy	Pharmacists	1. High-intensity training 2. Low-intensity training	Anti-smoking training based on stage-of-change and motivational interviewing theories	Unclear
Cohen (Dent) 1989	USA	Dentists	1. Training + NRT + prompt 2. Training + NRT 3. Training + prompt 4. Training alone	4-step smoking cessation protocol (ask, advise, agree on quit date, check in regularly)	Unclear
Cohen (Doc) 1989	USA	Doctors	1. Training + NRT + prompt 2. Training + NRT 3. Training + prompt 4. Training alone	4-step smoking cessation protocol (ask, advise, agree on quit date, check in regularly)	Unclear
Cornuz 2002	Switzerland	Doctors	1. Training 2. No training (non-tobacco training)	Training program based on 5 principles: 1) recent evidence-based content on tobacco use and cessation, 2) stage-of-change model, 3) pharmacological therapy, 4) educational methods focusing on active skills training, and 5) tobacco control context	Low
Cummings (priv) 1989	USA	Doctors	1. Training 2. No training	Personalised advice, quit date, one follow-up visit, self-help materials and nicotine gum	Unclear
Cummings 1989	USA	Doctors	1. Training 2. No training	Personalised advice, quit date, one follow-up visit, self-help materials and nicotine gum	Unclear
Gordon 2010	USA	Dentists	1. Training 2. No training (waiting-list)	5A approach: Ask, Advise, Assess, Assist, and Arrange	High
Grischott 2023	Switzerland	Doctors	1. Training 2. Other training	Health coaching (including motivational interviewing, shared decision-making, and health promotion activities)	Low
Habfast 2024	Switzerland	Doctors	1. Training 2. Enhanced usual care	Vivre sans tabac curriculum: 1) smoking cessation information and available aids, 2) approach for patients at different stages of change, 3) role-play of learned skills	Unclear
Hymowitz 2007	USA	Doctors	1. High-intensity training 2. Low-intensity training	"Solutions for smoking" program, which included educational and behavioural-change materials	Low
Joseph 2004	USA	Physicians, nurses, psychologists, pharmacists	1. Training 2. No training	Multi-component intervention to increase implementation of 3 recommendations: 1) documentation of tobacco use status in medical record, 2) delivery of intervention to all smokers, and 3) liberal use of smoking cessation medications	Unclear
Jumbe 2022	UK	Community pharmacists	1. Training 2. No training	Guided by COM-B framework for behaviour change, social cognitive theory, and self-determination theory to address skills, attitude, and motivation through practice-based training sessions	Unclear
Kastaun 2019	Germany	Doctors	1. Training 2. Other training	ABC: ask, brief advice, cessation support	Low
Klein 2020	USA	Doctors, nurse practitioners, physician assistants	1. Training 2. No training (non-tobacco training)	5As approach, role-playing skills	Unclear
Kottke 1989	USA	Doctors	1. Training + self-help 2. Self-help alone 3. No training	Tobacco education and intervention skills and techniques	Unclear

Lennox 1998	UK	Doctors, nurses, health visitors	1. Training 2. No training	Workshop based on stages-of-change model	Unclear
Meija 2016	Argentina	Doctors	1. Training 2. No training	"Rx for change: clinician-assisted tobacco cessation" program: based on stages-of-change model and 5As counselling framework	Unclear
Olano-Espinosa 2013	Spain	Doctors, nurses	1. Training 2. No training	Active learning based on stages-of-change model and clinical practice guidelines	Low
Papadakis 2018	Canada	Doctors, nurses	1. High-intensity training 2. Low-intensity training	5As approach + coaching sessions	Low
Pereira 2006	France	Doctors	1. Training 2. No training	Steps in creating and installing routine cessation interventions in general practice	Unclear
Sinclair 1998	Scotland	Pharmacists	1. Training 2. No training	Based on stages-of-change approach	Low
Stretcher 1991	USA	Doctors	1. Training 2. Training + prompts 3. Prompts only 4. No training	Minimal contact counselling	Unclear
Unrod 2007	USA	Doctors	1. Training 2. No training	Based on 5As approach + decision aid	Low
Verbiest 2014	Netherlands	Doctors	1. Training 2. No training	Based on 5As model	Unclear
Wang 1994	Taiwan	Doctors	1. Training 2. Poster 3. No training	Based on stages-of-change + clinical practice guidelines	Unclear
Wang 2024	China	Healthcare service providers	1. Training 2. No training	Based on 5As approach	Low
Wee 2020	Malaysia	Medical officers, pharmacists, health education officers, nurses, medical assistants	1. High-intensity training 2. Low-intensity training	National Centre for Smoking Cessation and Training model: knowledge- and skills-based competencies to deliver behavioural and pharmacological support for smoking cessation based on clinical guidelines	High
Wilson 1988	Canada	Doctors	1. Training + NRT 2. NRT 3. No training	Continuing medical education model on tobacco	Unclear

NRT: nicotine replacement therapy

Table 2

Adjustments for clustering in included studies

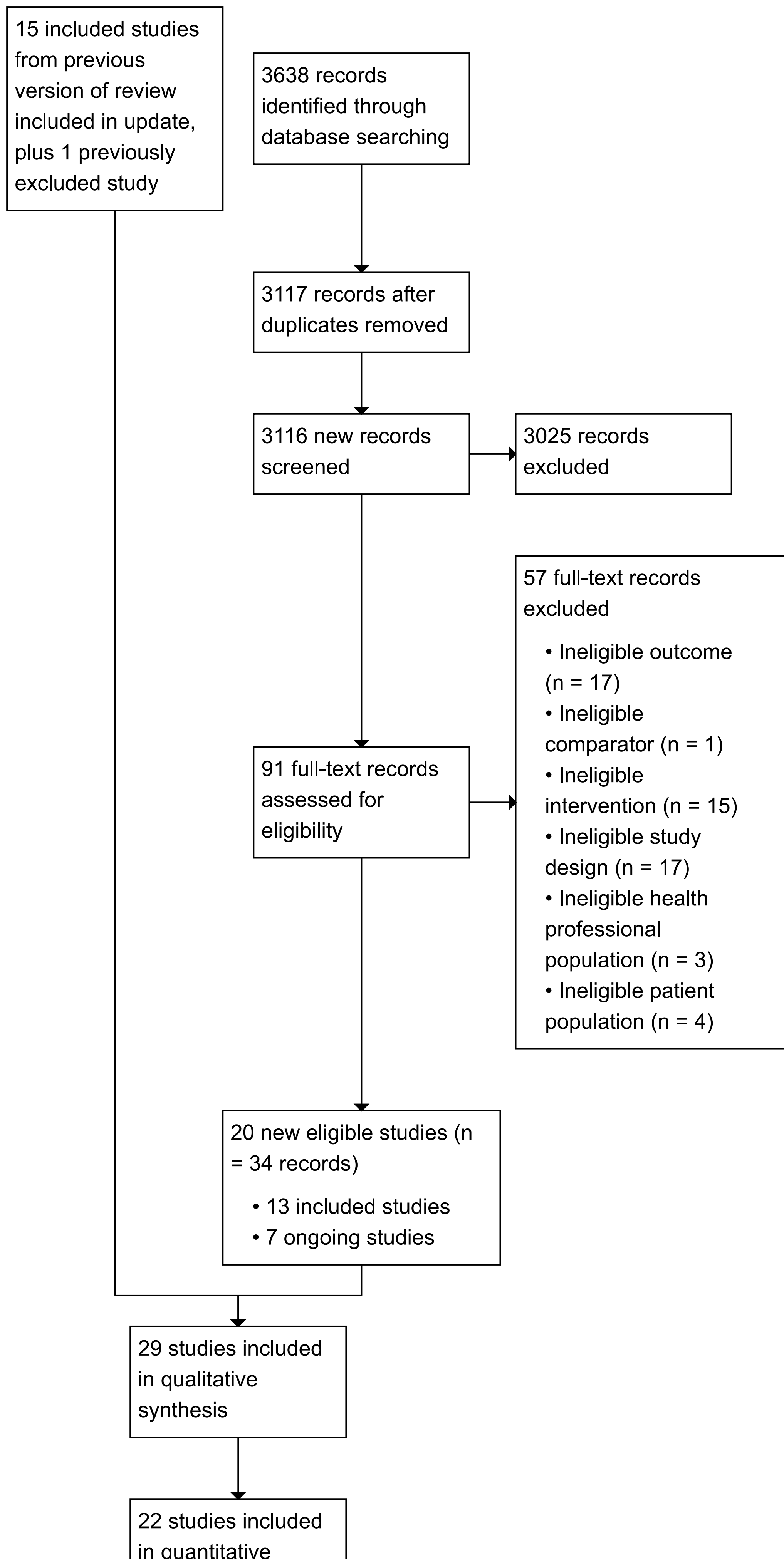
Study ID	Risk of bias from adjustment for clustering	Method of adjustment in study	Adjusted result in study	Method of adjustment in meta-analysis
Butler 2013	Low	ICC 0.05	Smoking cessation: OR 1.14 (95% CI 0.30 to 4.28)	Used ICC reported in study (0.05)
Caponnetto 2017	Unclear	Not reported	Not applicable (unclear if results adjusted for clustering)	Used assumed ICC of 0.02
Cohen (Dent) 1989	Low	Chi^2 statistic used based on changes in the deviance function for a series of nested models	Not reported (P values only)	Used assumed ICC of 0.02
Cohen (Doc) 1989	Low	Chi^2 statistic used based on changes in the deviance function for a series of nested models	Not reported (P values only)	Used assumed ICC of 0.02
Cornuz 2002	Unclear	No adjustment made	Not applicable (results not adjusted)	Used assumed ICC of 0.02
Cummings (Priv) 1989	Low	"We tested the effect of this clustering in other analyses in which the sampling variances were adjusted for cluster sampling"	Smoking cessation: percentage difference +0.7% (95% CI -1.7 to +3.1)	Used assumed ICC of 0.02

Cummings 1989	Low	Estimated logistic regression models with random effects terms representing the groupings by physician	Smoking cessation: percentage difference +1% (95% CI -0.1% to +2.3%)	Used assumed ICC of 0.02
Gordon 2010	Low	Used a generalised linear model to conduct analysis of variance. Reported an ICC of 0.009	Smoking cessation: $F_{1, 12} = 14.62$; P < 0.01	Used ICC reported in study (0.009)
Grischott 2023	Low	Logistic mixed models with adjustments clustering by GPs (random effects)	Smoking cessation: OR 1.21 (95% CI 0.03 to 50.76)	Not applicable (not meta-analysed)
Habfast 2024	Low	ICC 0.03	Smoking cessation: OR 0.78 (95% CI 0.30 to 1.99)	Used ICC reported in study (0.03)
Hymowitz 2007	Low	"Data were analysed based on aggregated data to account for unit of analysis issues". Reported an ICC of 0.005	Not reported	Not applicable (not meta-analysed)
Joseph 2004	High	No adjustment made	Not applicable (results not adjusted)	Used assumed ICC of 0.02
Jumbe 2022	Low	ICC of 0.026 reported for analysis, though abstinence data not analysed because of dropout	Not applicable (results not analysed)	Not applicable (not meta-analysed)
Kastaun 2021	Low	"Data were structured hierarchically in clusters (=practices), with patients within these clusters. Mixed-effects logistic regression models were used to analyse the primary outcome [...] with a fixed effect for time (pre-/post-training) and random effects for the practices and the time effect."	Smoking cessation: OR 1.51 (95% CI 0.62 to 3.67)	Not applicable (not meta-analysed)
Klein 2020	Unclear	Not reported	Not applicable (unclear if results adjusted for clustering)	Not applicable (not meta-analysed)
Kottke 1989	High	No adjustment made	Not applicable (results not adjusted)	Used assumed ICC of 0.02
Lennox 1998	Low	Generalised linear mixed model approach used for regression techniques which added the general practice as a random factor nested within the treatment groups to the other fixed-effect factors	Smoking cessation: percentage difference -1.1% (95% CI -3.03 to +0.83)	Used assumed ICC of 0.02
Mejia 2016	Low	States "adjusted" but method not reported in sufficient detail	Smoking cessation: P = 0.435	Used assumed ICC of 0.02
Olano-Espinosa 2013	Low	Design effect of 1.1	Smoking cessation: OR 6.5 (95% CI 3.3 to 12.7)	Used design effect reported in study (1.1)
Papadakis 2018	Low	ICC 0.02	Smoking cessation: OR 0.82 (95% CI 0.40 to 1.67) P = 0.58; quit attempts: OR 1.36 (95% CI 1.00 to 1.84) P = 0.05	Used ICC reported in study (0.02)
Pereira 2006	Low	Not reported	Not applicable (unclear if results adjusted for clustering)	Not applicable (not meta-analysed)
Sinclair 1998	Low	ICC 0.0001	Smoking cessation: percentage difference +4.6% (95% CI -0.8 to +10.0)	Used ICC reported in study (0.0001)
Strecher 1991	High	Not reported	Not applicable (unclear if results adjusted for clustering)	Used assumed ICC of 0.02
Unrod 2007	Low	"Results were analyzed via generalized and mixed linear modeling controlling for clustering"	Smoking cessation: OR 1.77 (95% CI 0.94 to 3.34) P = 0.078	Used assumed ICC of 0.02
Verbiest 2014	Low	"The impact of the training on GP smoking cessation activities reported by patients was analyzed using generalized estimating equations (GEE) in order to adjust for clustering"	Smoking cessation: OR 1.93 (95% CI 0.77 to 4.89) P = 0.16	Not applicable (not meta-analysed)
Wang 1994	Unclear	Not reported	Not applicable (unclear if results adjusted for clustering)	Used assumed ICC of 0.02
Wang 2024	High	No adjustment made	Not applicable (results not adjusted)	Not applicable (not meta-analysed)
Wee 2020	Low	"Outcomes were compared using multi-level logistic regression analyses (using the melogit command), taking account of cluster"	Smoking cessation: OR 5.04 (95% CI 1.22 to 20.77) P = 0.025	Used assumed ICC of 0.02
Wilson 1988	Unclear	Not reported	Not applicable (unclear if results adjusted for clustering)	Used assumed ICC of 0.02

CI: confidence interval; ICC: intra-cluster coefficient; OR: odds ratio

Figures

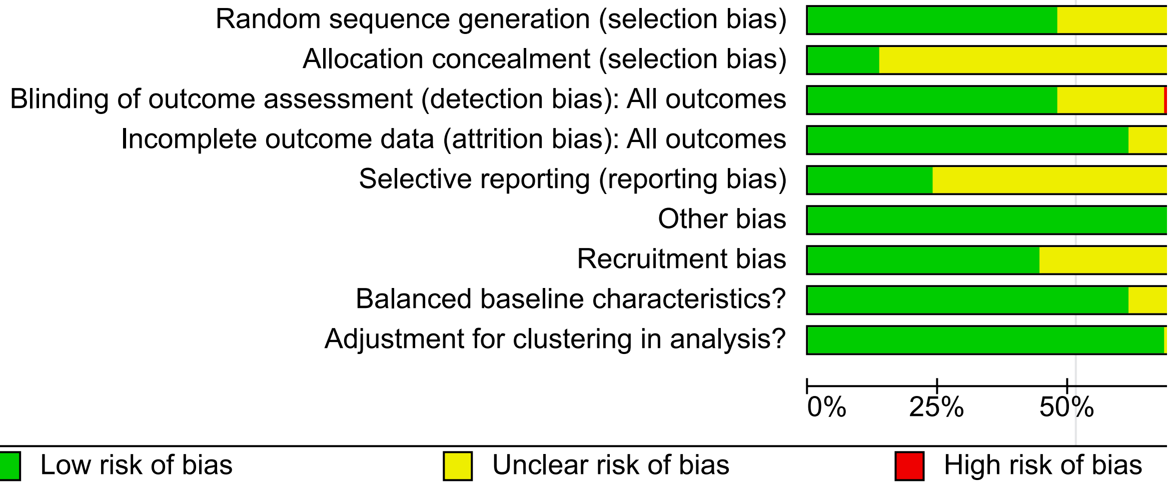
Figure 1


15 included studies from previous version of review included in update, plus 1 previously excluded study
3638 records identified through database searching
3117 records after duplicates removed
3116 new records screened
3025 records excluded
57 full-text records excluded
• Ineligible outcome (n = 17)
• Ineligible comparator (n = 1)
• Ineligible intervention (n = 15)
• Ineligible study design (n = 17)
• Ineligible health professional population (n = 3)
• Ineligible patient population (n = 4)
91 full-text records assessed for eligibility
20 new eligible studies (n = 34 records)
• 13 included studies
• 7 ongoing studies
29 studies included in qualitative synthesis
22 studies included in quantitative

synthesis
(meta-analysis)

Study flow diagram

Figure 2



Risk of bias graph: review authors' judgements about each risk of bias judgement presented as percentages across all included studies

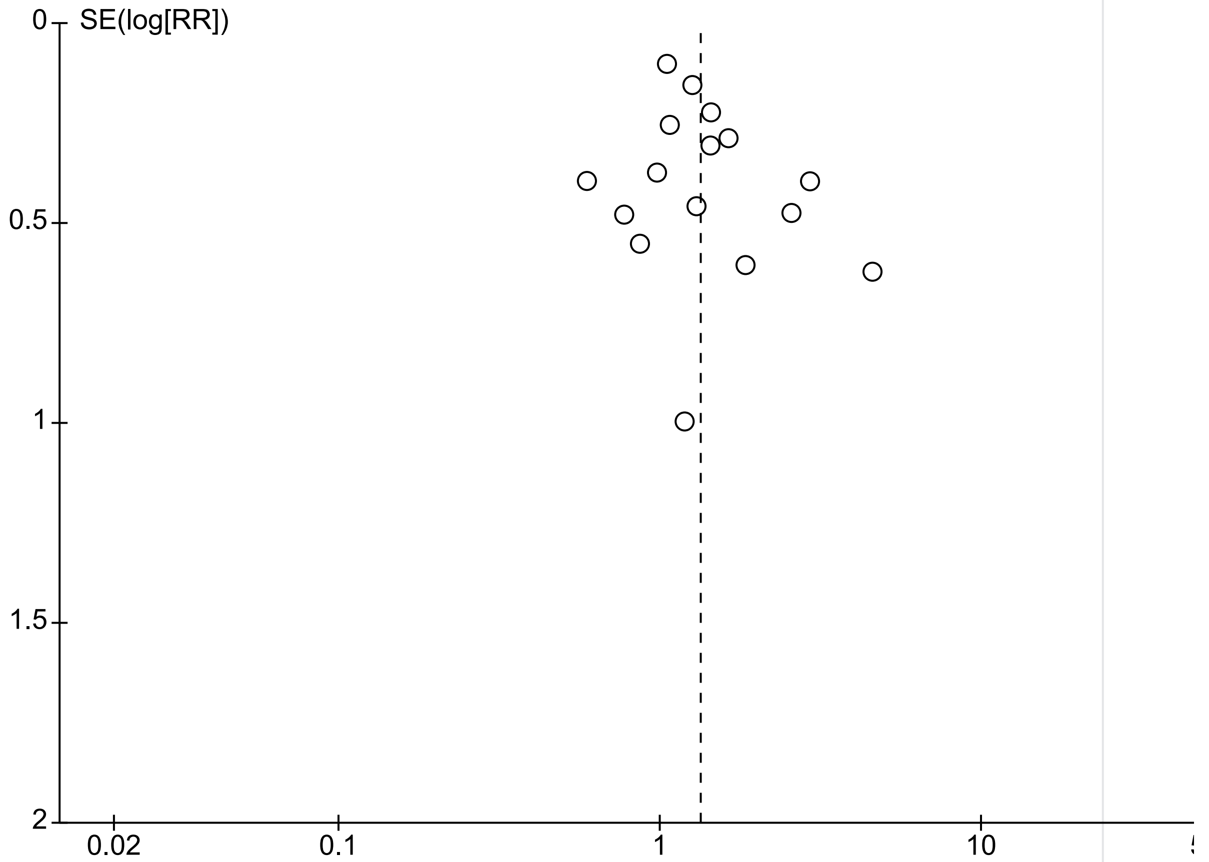
Figure 3

Study	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of outcome assessment (detection bias): All outcomes	Incomplete outcome data (attrition bias): All outcomes	Selective reporting (reporting bias)	Other bias	Recruitment bias	Balanced baseline characteristics?	Adjustment for clustering in analysis?
Butler 2013	+	+	+	+	+	+	+	+	+
Caponnetto 2017	?	?	+	−	?	?	?	?	?
Cohen (Dent) 1989	?	?	−	−	?	+	?	?	+
Cohen (Doc) 1989	?	?	−	?	?	+	+	?	+
Cornuz 2002	+	?	+	+	?	+	+	+	?
Cummings (Priv) 1989	?	?	+	+	?	+	−	?	+
Cummings 1989	+	?	+	+	?	+	−	+	+
Gordon 2010	?	?	−	+	−	−	+	−	+
Grischott 2023	+	+	+	+	+	+	+	+	+
Habfast 2024	+	?	?	?	?	+	?	?	+
Hymowitz 2007	+	?	−	+	?	+	+	+	+
Joseph 2004	?	?	?	+	?	+	?	+	−
Jumbe 2022	+	+	+	−	?	+	+	?	+
Kastaun 2021	+	?	?	+	+	+	?	+	+
Klein 2020	?	?	?	+	+	+	−	+	?
Kottke 1989	?	?	+	+	?	+	+	?	−
Lennox 1998	?	?	−	+	?	+	+	?	+
Mejia 2016	?	?	−	?	?	+	+	?	+
Olano-Espinosa 2013	+	?	+	+	?	+	+	+	+
Papadakis 2018	+	?	+	+	+	+	+	+	+
Pereira 2006	?	?	?	?	?	?	?	+	+
Sinclair 1998	?	?	−	+	+	+	−	+	+
Strecher 1991	−	?	?	+	?	−	?	+	−
Unrod 2007	+	−	+	?	?	+	−	+	+

Verbiest 2014	+	?	−	?	?	+	?	+	+
Wang 1994	−	−	−	+	?	+	?	+	?
Wang 2024	+	+	+	+	+	−	+	+	−
Wee 2020	+	−	+	−	?	+	−	−	+
Wilson 1988	?	?	+	?	?	+	?	+	?

Risk of bias summary: review authors' judgements about each risk of bias item for each included study

Figure 4



Funnel plot of comparison 1: Training versus no training; outcome 1.1: Smoking abstinence at longest follow-up

Figure 5

Study or Subgroup	Training Events	Training Total	No training Events	No training Total	Weight	Risk Ratio M-H, Random, 95% C
Butler 2013	2	256	2	306	1.2%	1.20 [0.17 , 8.4
Cornuz 2002	13	102	6	121	4.1%	2.57 [1.01 , 6.5
Cummings (Priv) 1989	11	337	8	319	4.4%	1.30 [0.53 , 3.1
Cummings 1989	10	688	17	694	5.4%	0.59 [0.27 , 1.2
Gordon 2010	28	534	8	448	5.4%	2.94 [1.35 , 6.3
Joseph 2004	6	404	7	409	3.3%	0.87 [0.29 , 2.5
Kottke 1989	25	446	35	671	8.8%	1.07 [0.65 , 1.7
Lennox 1998	8	328	9	286	4.1%	0.78 [0.30 , 1.9
Mejia 2016	166	689	132	577	14.2%	1.05 [0.86 , 1.2
Olano-Espinosa 2013	28	2471	0	2902	0.6%	66.94 [4.09 , 1095.8
Pereira 2006	113	362	42	170	12.3%	1.26 [0.93 , 1.7
Sinclair 1998	26	224	19	268	7.8%	1.64 [0.93 , 2.8
Strecher 1991[a]	14	246	13	224	5.8%	0.98 [0.47 , 2.0
Strecher 1991[b]	8	239	4	221	2.8%	1.85 [0.56 , 6.0
Unrod 2007	25	239	16	220	7.3%	1.44 [0.79 , 2.6
Wang 1994[c]	10	37	3	51	2.7%	4.59 [1.36 , 15.5
Wilson 1988	41	466	34	558	9.8%	1.44 [0.93 , 2.2
Total (Wald[d])		**8068**		**8445**	**100.0%**	**1.34 [1.08 , 1.6**
Total events:	534		355			

Test for overall effect: Z = 2.62 (P = 0.009)

Heterogeneity: Tau² (DL[e]) = 0.08; Chi² = 30.85, df = 16 (P = 0.01); I² = 48%

Footnotes

[a]Training plus prompts versus prompts alone arms
[b]Training versus no training arms
[c]No intervention arm and poster only arm combined as comparator
[d]CI calculated by Wald-type method.
[e]Tau² calculated by DerSimonian and Laird method.

Risk of bias legend

(A) Random sequence generation (selection bias)
(B) Allocation concealment (selection bias)
(C) Blinding of outcome assessment (detection bias)
(D) Incomplete outcome data (attrition bias)
(E) Selective reporting (reporting bias)
(F) Other bias
(G) Recruitment bias
(H) Balanced baseline characteristics?
(I) Adjustment for clustering in analysis?

Forest plot of comparison 1: Training versus no training; outcome 1.1: Smoking abstinence at longest follow-up

Figure 6

Study or Subgroup	More training Events	More training Total	Less training Events	Less training Total	Weight	Risk Ratio M-H, Random, 95% CI
Caponnetto 2017[a]	11	116	1	59	8.5%	5.59 [0.74 , 42.30]
Habfast 2024[b]	9	73	16	117	29.3%	0.90 [0.42 , 1.93]
Papadakis 2018	10	218	9	235	25.8%	1.20 [0.50 , 2.89]
Wee 2020	62	219	13	114	36.4%	2.48 [1.43 , 4.32]
Total (Wald[c])		**626**		**525**	**100.0%**	**1.64 [0.86 , 3.12]**
Total events:	92		39			

Test for overall effect: Z = 1.49 (P = 0.14)

Fav

Heterogeneity: Tau^2 (DL[d]) = 0.22; Chi^2 = 6.50, df = 3 (P = 0.09); I^2 = 54%

Footnotes

[a]Divided by a design effect figure of 1.2 to correct for clustering

[b]Number randomised unavailable. Denominator at follow-up used.

[c]CI calculated by Wald-type method.

[d]Tau^2 calculated by DerSimonian and Laird method.

Forest plot of comparison 2: More versus less intensive training; outcome 2.1: Smoking abstinence at longest follow-up

Figure 7

Study or Subgroup	Training plus adjunct Events	Training plus adjunct Total	Training alone Events	Training alone Total	Weight	Risk Ratio M-H, Random, 95
3.1.1 Adjunct NRT						
Cohen (Dent) 1989[a]	9	185	5	185	22.6%	1.80 [0.61 ,
Cohen (Dent) 1989[b]	14	185	6	185	24.9%	2.33 [0.92 ,
Cohen (Doc) 1989[a]	15	288	23	288	30.3%	0.65 [0.35 ,
Cohen (Doc) 1989[b]	14	288	4	288	22.2%	3.50 [1.17 ,
Subtotal (Wald[c])		**946**		**946**	**100.0%**	**1.64 [0.72 ,**
Total events:	52		38			
Test for overall effect: Z = 1.18 (P = 0.24)						
Heterogeneity: Tau^2 (DL[d]) = 0.47; Chi^2 = 9.63, df = 3 (P = 0.02); I^2 = 69%						
3.1.2 Adjunct prompts						
Cohen (Dent) 1989[e]	9	185	14	185	21.5%	0.64 [0.29 ,
Cohen (Dent) 1989[f]	5	185	6	185	16.2%	0.83 [0.26 ,
Cohen (Doc) 1989[f]	17	323	15	323	23.7%	1.13 [0.58 ,
Cohen (Doc) 1989[e]	23	288	4	288	17.8%	5.75 [2.01 ,
Strecher 1991	14	246	8	221	20.9%	1.57 [0.67 ,
Subtotal (Wald[c])		**1227**		**1202**	**100.0%**	**1.37 [0.69 ,**
Total events:	68		47			
Test for overall effect: Z = 0.90 (P = 0.37)						
Heterogeneity: Tau^2 (DL[d]) = 0.39; Chi^2 = 11.76, df = 4 (P = 0.02); I^2 = 66%						

Test for subgroup differences: Chi^2 = 0.11, df = 1 (P = 0.74), I^2 = 0%

Footnotes

[a]Training with prompt plus NRT versus training with prompt alone
[b]Training plus NRT versus training alone
[c]CI calculated by Wald-type method.
[d]Tau^2 calculated by DerSimonian and Laird method.
[e]Training with NRT plus prompt versus training with NRT alone
[f]Training plus prompt versus training alone

Risk of bias legend

(A) Random sequence generation (selection bias)
(B) Allocation concealment (selection bias)
(C) Blinding of outcome assessment (detection bias)
(D) Incomplete outcome data (attrition bias)
(E) Selective reporting (reporting bias)
(F) Other bias
(G) Recruitment bias
(H) Balanced baseline characteristics?
(I) Adjustment for clustering in analysis?

Forest plot of comparison 3: Adjuncts to training versus training alone; outcome 3.1: Smoking abstinence at longest follow-up