Efficacy of smoking-cessation interventions for young adults: a meta-analysis

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CRD summary
This review of 14 studies concluded that smoking cessation treatments should be as effective for young adults as they are for the general adult population. Methodological weaknesses made selection bias likely. The inclusion of extremely variable comparators hindered interpretation and flawed synthesis conflated statistical and clinical significance. Thus, the conclusion is likely to be unreliable.

Authors' objectives
To assess the efficacy of smoking cessation programs (designed for the general adult population) in young adults (18 to 24 years old).

Searching
Studies were identified by searching Clinical Practice Guidelines (2004 to 2008) and via PubMed searches (2008 to 2011). Two search terms were used and studies were restricted to randomised controlled trials (RCTs). Language restrictions were not reported. Individual Patient Data (IPD) were requested from study authors. The authors did not specify that contact with trialists or manufacturers was to request the identification of additional studies and/or unpublished data.

Study selection
Inclusion criteria for participants were that "some participants were likely to be aged 18 to 24 years". Study design (RCT) was included in the search filter but not explicitly identified as study selection criteria. Sample sizes had to be greater than 50 with at least five participants aged 18 to 24 included in each trial arm. Studies had to report a smoking cessation outcome (such as self-reported abstinence) rather than smoking reduction. Smoking cessation interventions geared to the general adult population were specified in the research question (but not selection criteria). Comparators were not defined.

Studies were heterogenous. Duration of follow-up ranged from six to 12 months. The number of young adults ranged from 28 to 544. Interventions included cognitive-behavioural interventions, motivation and problem solving, nicotine replacement therapy, phenyl propanolamine and combinations of drug and psychological intervention. Comparators varied considerably in intensity including (placebo, wait lists, usual care, medication only, counselling, and self help material). Cessation measures were prevalence and prolonged abstinence either self-reported, or confirmed by saliva cotinine or expired carbon monoxide concentration.

Two reviewers assessed study eligibility with reference to a third to resolve discrepancies.

Assessment of study quality
The authors did not report any assessment of study quality. Similarly, no checks on the integrity of randomisation, plausibility of outliers or inconsistency between IPD and published information were reported by the authors, although they did report "cleaning of data". Intention-to-treat analyses were undertaken.

Data extraction
Authors were asked to provide mean outcome measure values, sample sizes and standard deviations for each trial arm, or to provide odds ratios based on IPD for 18 to 24 year olds. Descriptive data detailing type of intervention, comparator and outcome measure were also requested.

It was not clear whether data were requested from all potentially eligible trials. Trials with low mean ages and more than 100 participants were prioritised for contacting authors. The significance of results was also used to prioritise contact, although non-significant results were included. Summary data were not extracted from studies where IPD were unavailable.

The number of reviewers involved in data extraction was unclear.
Methods of synthesis
Pooled odds ratios and 95% confidence intervals (CI) were calculated using an unspecified random-effects method. Analyses were undertaken based on all participants, intention-to-treat, and first (end of study to 12 months) and last (six to 24 months) time points. Heterogeneity was quantified using I² but not explored. Indirect comparisons with results for the younger adult group were made with results stratified by statistical significance of treatment effects in the general adult population. The authors did not report investigation of risk of bias that may have affected the cumulative evidence (such as publication bias).

Results of the review
IPD were obtained from 14 trials.

Different smoking cessation interventions increased the odds of cessation compared to a range of comparators irrespective of time point or whether analysis was intention-to-treat (odds ratios range from 1.48 to 1.76, 95% CIs ranged from 1.11 to 2.46). Heterogeneity was minimal in all analyses. Analyses were also stratified by the statistical significance of the effect in the general adult population. Where this was significant (p<0.05), effects were also significant in the young adult population. Where it was non-significant, effects were also non-significant in the subpopulation (odds ratios for young adults reported for the significant subset, with overlapping confidence intervals consistent with main effects reported above).

Authors’ conclusions
Smoking cessation treatments should be as effective for young adults as they are for the general adult population.

CRD commentary
This review addressed a clearly defined question, but there was ambiguity in the inclusion criteria as this required judgement about the probability of participants occupying the relevant age range. Similarly, study design (RCT) was included in the search filter but not explicitly identified as study selection criteria. The search was heavily reliant on a single source of evidence augmented with database searching to update the review, so there was potential for systematic bias in determining study eligibility. Availability bias in the acquisition of IPD may also be problematic as 36 authors declined or did not respond to requests for data. Furthermore, it was unclear whether data were requested from all potentially eligible trials. The significance of results was used to prioritise contact with authors. Although non-significant results were included, the use of statistical significance to prioritise data acquisition may have fatally biased the analysis.

Clinical heterogeneity in type of intervention and comparator was extremely high, which made interpretation of pooled effects difficult. For example, the control comparators in some trials were equivalent to treatments in other trials. Despite this, statistical heterogeneity was low, which reflected huge imprecision in the effects of individual trials. Use of statistical significance of results of treatment effectiveness in the general adult population to stratify young adult data was a form of vote counting which neglected consideration of effect magnitudes. Therefore, lack of power may plausibly explain the congruence of the statistical significance of general adult and young adult results. The comparison was also indirect, despite the acquisition of IPD which facilitates direct within trial comparisons of age.

The results of the review and the conclusion that smoking cessation treatments are as effective for young adults as they are for the general adult population are therefore likely to be unreliable.

Implications of the review for practice and research
Practice: The authors stated that young adults should be encouraged to participate in smoking cessation interventions.

Research: The authors stated that further IPD analysis may reduce the problems of availability bias and could explain variation in effectiveness associated with patient level covariates other than age.

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