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A review of the evidence on smoking bans and incidence of heart disease

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ABSTRACT

We update an earlier review of smoking bans and heart disease, restricting attention to admissions for acute myocardial infarction. Forty-five studies are considered. New features of our update include consideration of non-linear trends in the underlying rate, a modified trend adjustment method where there are multiple time periods post-ban, comparison of estimates based on changes in rates and numbers of cases, and comparison of effect estimates according to post-ban changes in smoking restrictiveness. Using a consistent approach to derive ban effect estimates, taking account of linear time trends and control data, the reduction in risk following a ban was estimated as 4.2% (95% confidence interval 1.8–6.5%). Excluding regional estimates where national estimates are available, and studies where trend adjustment was not possible, the estimate reduced to 2.6% (1.1–4.0%). Estimates were little affected by non-linear trend adjustment, where possible, or by basing estimates on changes in rates. Ban effect estimates tended to be greater in smaller studies, and studies with greater post-ban changes in smoking restrictiveness. Though the findings suggest a true effect of smoking bans, uncertainties remain, due to the weakness of much of the evidence, the small estimated effect, and various possibilities of bias.
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1. Introduction

Sargent et al. (2004) published the first study of the effects of smoking bans on heart disease, reporting a 40% reduction in hospital admissions from acute myocardial infarction (AMI) in Helena, Montana, USA following the introduction of a local law banning smoking in public places and workplaces. In 2011 we reviewed the evidence then available, based on twenty-four studies (Lee and Fry, 2011). We noted "major weaknesses in many studies and meta-analyses, including failure to consider data from control areas or existing trends in AMI rates, incorrect estimation of variability, and use in some meta-analyses of results for population subsets or estimates apparently unrelated to the data reported". Using a consistent approach to derive estimates of the ban effect, and taking account of time trends and control data, our analyses indicated a much smaller reduction in risk of heart disease following a ban than the reductions of 10–19% claimed in some other meta-analyses (Glantz, 2008; Lightwood and Glantz, 2009; Mackay et al., 2010; Meyers et al., 2009), reductions which we demonstrated were implausibly large considering likely changes in smoking habits and passive smoke exposure. Preferring national to regional estimates where available, we estimated a 5% reduction (95% confidence interval [CI] 3–8%), which became 2.7% (2.1–3.4%) when we omitted estimates where trend adjustment was not possible.

Since our review (Lee and Fry, 2011), publications have proliferated, the current review being based on about twice as many publications as considered earlier. Our updated review has some new features. First, we restrict attention to admissions from AMI, or near equivalent endpoints. Evidence relating to mortality will be considered later in a separate publication based on work currently ongoing.

Secondly, as a recent paper (Barr et al., 2012) reported that estimates of the ban effect adjusted for pre-ban non-linear trends in rates may substantially differ from those adjusted only for linear trend, we also derive study-specific estimates adjusted for non-linear trend. This can only be attempted where the run of data pre-ban is sufficiently long.

Third, we modify the method used to adjust for trend where data are available for multiple periods post-ban. Earlier (Lee and Fry, 2011), we derived the ban effect estimate by comparing the total numbers of deaths observed post-ban with that predicted at the midpoint of the post-ban periods based on the underlying trend pre-ban. Here, we fit a model that incorporates information from both the pre-ban and post-ban trend, inference being based

Abbreviations: ACE, acute coronary events; ACS, acute coronary syndrome; AMI, acute myocardial infarction; CHD, coronary heart disease; CI, confidence interval; ETS, environmental tobacco smoke; RR, relative risk; SCA, sudden circulatory arrest.

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on estimates of a dummy variable set to zero pre-ban and to one post-ban. The two approaches produce identical estimates where there is only one post-ban period. The modified approach allows us to fit non-linear forms for the trend, such as the quadratic.

Fourth, we test the validity of an assumption we used earlier (Lee and Fry, 2011). In these analyses, where data for a run of similar periods (usually years) were available pre-ban, we estimated the ban effect based on numbers of cases, assuming that linear trend adjustment would automatically take into account changes in population size. This assumption is not necessarily valid, so we have also carried out analyses based on trends in rates. This often involved obtaining population data from other sources.

Finally, we also include results of meta-analyses comparing ban effect estimates according to measures of the change in smoking restrictiveness following the ban. This better reflects the situation where bans may vary in the extent to which they limit smoking, and may be conducted against a background of various levels of existing restrictiveness.

2. Methods

2.1. Literature searches

Published studies and reviews relating smoking bans to risk of AMI (or heart disease) additional to those considered earlier (Lee and Fry, 2011) were sought from PubMed searches (January 1st 2009 to September 30th 2013) using the terms described by Mackay et al. (2010), and also from papers cited in relevant publications.

2.2. Quantifying levels of restrictiveness

Except for local US studies, and for studies presenting overall results based on multiple bans in different locations, we sought published scores for restrictiveness before and after the ban, using for US studies the method of Chriqui et al. (2002) without pre-emption (as explained below), or a modification of it (American Lung Association, 2009), and for European studies the method of Joossens and Raw (2006), re-expressing the scores as percentages. Although the different ratings are not strictly comparable, this method gives a reasonably detailed assessment of the legislation in a variety of different environments, and of the level of change expressed by the introduction of the ban. Where published scores were unavailable, we conducted internet searches to supplement the descriptions of the ban given in the study publication(s), and estimated the scores using the Chriqui system.

The system of Chriqui et al. (2002) allocated a score of 4 points for each of seven locations (government worksites, private worksites, schools, childcare facilities, restaurants including bar areas of restaurants, retail stores and businesses, recreational and cultural facilities), a bonus point for restrictions on outdoor smoking restrictions in four of the locations (including outdoor seating at bars and taverns under the restaurant category), and a further 5 points each for systems of penalties and enforcement, giving a maximum score of 42 points. Points were deducted if states pre-empted stricter local laws. Chriqui et al. (2002) gave ratings for all states annually for 1993–1999, both with and without adjustment for pre-emption, and the annual reports of the American Lung Association published ratings without pre-emption for 2003–2006 (e.g., American Lung Association, 2008). In a later report (American Lung Association, 2009), a modification to the rating system gave 4 points to each of the original categories, and allocated 4 points each to bars/taverns (in addition to the 4 points for restaurants and their bar areas) and to casinos where relevant, giving a maximum of 40 points in states without casinos, or 44 points in states with casinos. Scores were then adjusted down for pre-emption or up according to the percentage of the population covered by local ordinances. Ratings under the modified system are available up to 2013 (e.g., American Lung Association, 2013).

The Tobacco Control Scale, introduced by Joossens and Raw (2006), included a section on smoke-free work and public places. A score of 10 points was awarded for workplaces (excluding cafes and restaurants), 8 points for cafes and restaurants, and 4 points for other public places (trains, other public places and educational, health, government and cultural places), giving a maximum of 22 points. Ratings were given for 30 European countries in 2005, which have twice been updated (Joossens and Raw, 2007, 2011), although referring to “bars” rather than “cafes”. Ratings using the same scheme were also given by Nguyen et al. (2012) for 11 European countries, annually from at least 1990–2010.

2.3. General approach

In many ways, the approach used is similar to that we used our earlier (Lee and Fry, 2011). Thus:

- We estimate the effect of the ban by comparing the observed number of AMI cases post-ban with that expected in the absence of a ban, referring to the ratio as the “ban effect” or the ban relative risk (RR).
- We consider it essential to account for the tendency for the risk of AMI to vary seasonally by year (Ornato et al., 1996), by comparing numbers pre- and post-ban for whole years or the same periods in a year (e.g., June to November), or by using results which have adjusted for season or factors believed to cause seasonal variation (e.g., temperature, humidity and influenza rates). Studies taking no account of seasonal variation, e.g., comparing five months pre-ban and five months post-ban, are rejected.
- Where possible, we attempt to adjust for any underlying time trend in AMI rates. One method of doing this uses data for a control population where trends are likely to be similar. Another requires data for multiple similar time periods, in order to estimate the trend. Where estimates can be obtained both by use of a control population and by adjusting for trend, we prefer to use the former as the shape of the trend is not always well-defined. However, results are presented based on both approaches.
- Consideration should be given to specific factors that might affect the time trend, such as changes in diagnostic criteria.
- As the great majority of studies consider the post-ban period as starting immediately or just after the ban, we derive estimates on this basis where possible.
- Where a study provides data for multiple control populations, the ban effect is generally estimated from the combined control data. However, control populations with obvious weaknesses may be excluded.
- Some studies report results for subgroups by sex, age, or smoking habit. For consistency, the estimates we use in our meta-analyses are always based on the result for the whole study population, and not on that for subsets. However, we summarize the availability of such data. Exceptionally, where studies present results relating to different ban times in different areas, we report these separately.
- The mathematical methods we use assume that the effect of a ban is to multiply the risk of AMI by a given factor, with the factor invariant of the length of time post-ban. The validity of this assumption is investigated by comparing the estimates of the magnitude of the ban effect in studies with shorter and longer post-ban periods.

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