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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.

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.

Some differences should be noted, however, as mentioned in the introduction. Thus:

- We do not consider mortality and restrict attention to admission rates for AMI or near equivalents including AMI discharges, Medicare claims, and incidence, as well as admissions from acute coronary syndrome (ACS), admissions from acute coronary events (ACE), and sudden circulatory arrest (SCA).
- We test the effect of adjusting for non-linear as well as linear trend, where the data are sufficient to attempt this.
- We use a modified method to adjust for time trend, where there are multiple periods post-ban.
- We calculate trend-adjusted RRs, not only based on numbers of cases, but also on rates. Where necessary, we sought relevant population data to estimate rates from numbers (or *vice versa*). If not given in the study publication(s), the population data were obtained from the WHO mortality database, or from a relevant government website.

2.4. Estimating the ban effect

2.4.1. No control data and no trend information present

The RR associated with a ban, and the variance of its logarithm, are estimated by:

$$\mathrm{RR}_1 = \mathrm{M}_{\mathrm{TA}}/\mathrm{M}_{\mathrm{TB}} \tag{1a}$$

and

$$\mathrm{V}_1 = (1/\mathrm{N}_{\mathrm{TA}}) + (1/\mathrm{N}_{\mathrm{TB}}) \tag{1b}$$

where M refers to the mean number of cases per year (or period of interest), N refers to the total number of cases, the subscripts A and B refer to the period after and before the ban, and the subscript T refers to the test (ban) area.

The 95% CIs are estimated by

$$\mathrm{RR}_{\mathrm{L1}}, \mathrm{RR}_{\mathrm{U1}} = \exp(\log \mathrm{RR}_1 \pm \mathrm{Z}\sqrt{V_1}) \tag{1c}$$

where Z is the standard normal deviate corresponding to 0.025. Though seasonal effects are taken into account, provided the periods considered cover the same months of the year, no account is taken of any underlying trend pre-ban, so estimates using formula 1 are considered less reliable than those taking trend into account.

2.4.2. Control data present

The RR and the variance of its logarithm are estimated by

$$\mathrm{RR}_2 = (\mathrm{N}_{\mathrm{TA}}/\mathrm{N}_{\mathrm{CA}})/(\mathrm{N}_{\mathrm{CB}}/\mathrm{N}_{\mathrm{TB}}) \tag{2a}$$

where the subscript C refers to the control (no ban) area, and

$$V_2 = (1/\mathrm{N}_{\mathrm{TA}}) + (1/\mathrm{N}_{\mathrm{TB}}) + (1/\mathrm{N}_{\mathrm{CA}}) + (1/\mathrm{N}_{\mathrm{CB}}) \tag{2b}$$

with the lower and upper 95% CI of RR_1 estimated by

$$\mathrm{RR}_{\mathrm{L2}}, \mathrm{RR}_{\mathrm{U2}} = \exp\ (\log \mathrm{RR}_2 \pm \mathrm{Z}\sqrt{V_2}) \tag{2c}$$

These formulae assume that the lengths of the pre- and post-ban periods for the test area are the same as for the control area, so seasonal effects automatically cancel out. Any underlying trend is accounted for by assuming that the trend in the control area would also have been observed in the test area in the absence of a ban.

2.4.3. No control data, and adjustment for linear trend possible

Where data are available on the number of cases occurring in successive periods pre-ban and in one or more periods post-ban, Poisson log-linear regression analysis (Draper and Smith, 1998) was performed using SAS Version 9.2 (SAS Institute Inc., 2009). The log of the number of deaths seen pre- and post-ban was modeled as a linear effect over year, with a dummy variable included, set to zero pre-ban and one post-ban. With no effect of the ban, the estimate for this dummy variable should be zero. However if there was an offset to the linear trend caused by the ban, this estimate will give a value for the effect. As the Poisson model in SAS models the deaths in terms of log number of deaths, the ban effect, RR_3, is given by the exponential of the estimate, with the 95% CI derived from its standard error (SE_3):

$$\mathrm{RR}_{\mathrm{L3}}, \mathrm{RR}_{\mathrm{U3}} = \exp\ (\log \mathrm{RR}_3 \pm \mathrm{ZSE}_3) \tag{3}$$

The methodology assumes that each period covers the same months of the year, so seasonal effects are not an issue.

Note that the methods described above (loosely referred to below as formula 3) can also be applied where control data are available, providing that pre-ban data are available for successive periods, simply by ignoring the control area data.

The methodology described above is based on the numbers of cases occurring in each period, ignoring changes in population size. Where population data are available for each period, the method is adapted by adding the log of the population as an offset to the model. The relative risk and CIs are estimated from the dummy variable as above.

2.4.4. No control data, and adjustment for non-linear trend possible

Where data are available on the number of cases for at least three periods pre-ban and in one or more periods post-ban, the same methods are used, except that the prediction equation includes years squared as a quadratic term.

2.5. Meta-analyses

Independent RR estimates from multiple studies are combined using random-effects meta-analysis (Littell et al., 2006) weighted on the inverse of the variance of the RRs. Results of fixed-effect meta-analyses (SAS Institute Inc., 2009), conducted using weighted linear regression with the SEs adjusted as recommended by Berlin et al. (1993) are also shown.

Meta-analyses of AMI admission data (or near equivalent) are conducted separately by type of estimate (i.e., based on formulae 1, 2 or 3) and overall. They also investigate variation by region, study weight, the lengths of the pre-ban and post-ban periods, change in restrictiveness following the ban, and the age range of the population studied. Meta-analyses are also conducted excluding regional estimates where national estimates are available and omitting estimates where trend adjustment was not possible.

The meta-analyses carried out were defined in advance.

3. Results

3.1. Literature searching

Initially, 57 studies were identified, published between 2004 and 2013. Two were rejected as no useful ban effect estimate could be made (Marlow, 2012; Naiman et al., 2010), one as the endpoints (ever AMI, ever coronary heart disease (CHD) or angina) were inappropriate (Lippert and Gustat, 2012) and five as only mortality data were available (Dove et al., 2010; McAlister et al., 2010; Rodu et al., 2012; Stallings-Smith et al., 2013; Villalbí et al., 2011). Two studies were rejected as they compared pre-ban and post-ban periods of only a few months covering different seasons of the year, with no seasonal adjustment possible (Gudnason et al., 2009; Johnson and Beal, 2013). The final two studies rejected (Sargent et al., 2012; Vander Weg et al., 2012) were longer term, involving bans introduced in different regions at various times, with no seasonal adjustment made or possible from the data presented. Of the 45

Table 1
Summary of location and timing for the 45 studies.

Study	Reference(s)	Location[b]	Timing of ban	Data available relative to a ban[a]	
				Before	After
S1	Sargent et al. (2004)	Helena, Montana, USA (vs Outside city)	5 Jun 02[c]	Jun to Nov 98,99,00,01	Jun to Nov 02[d]
S2	Barone-Adesi et al. (2006, 2009a,b)	Piedmont, Italy	10 Jan 05	Feb to Jun 02,03,04[e]	Feb to Jun 05[f]
S3	Alsever et al. (2009) and Bartecchi et al. (2006)	Pueblo, Colorado, USA (vs El Paso County[g])	1 Jul 03	Jan 02 to Jun 03	Jul 03 to Jun 06
S4	Bullen et al. (2006)	New Zealand	10 Dec 04	Dec 96 to Nov 04	Dec 04 to Nov 05
S5	Cronin et al. (2007, 2012)	Southwest Ireland	29 Mar 04	Year ending 28 Mar 04	Years ending 28 Mar 05, 06, 07
S6	Heinz et al. (2007)	Boise, Idaho, USA	1 Jul 04	2 years pre-ban	1 year post-ban
S7	Juster et al. (2007) and Loomis and Juster (2012)	New York State, USA	24 Jul 03	95,96...02	04[h,i]
S8	Khuder et al. (2007)	Bowling Green, Ohio, USA (vs Kent)	Mar 02	Mar 99 to Feb 02	Mar 02 to Feb 05[j]
S9	Seo and Torabi (2007)	Monroe County, Indiana, USA (vs Delaware County)	1 Aug 03	Aug 01 to May 03	Aug 03 to May 05
S10	Barnett et al. (2009)	Christchurch, New Zealand	Dec 04	03,04	05,06
S11	Cesaroni et al. (2008)	Rome, Italy	10 Jan 05	02,03,04[e]	05
S12	Lemstra et al. (2008)	Saskatoon, Saskatchewan, Canada	1 Jul 04	Years ending June 01,02,03,04[e]	Year ending June 05
S13	Pell et al. (2008)	Scotland	End Mar 06	Jun 05 to Mar 06	Jun 06 to Mar 07
S14	Vasselli et al. (2008)	Four areas, Italy	10 Jan 05	10 Jan to 10 Mar 02,03,04[e]	10 Jan to 10 Mar 05
S15	Gasparrini et al. (2009)	Tuscany, Italy	10 Jan 05	02,03,04[e]	05
S16	Shetty et al. (2009, 2011)	By region, USA	Varies from 90 to 04	90,91...04[k]	
S17	Villalbí et al. (2009)	Barcelona, Spain	1 Jan 06	04,05	06
S18	Di Valentino et al. (2010, 2011a,b,c)	Ticino, Switzerland	13 Apr 06	Years ending 12 Apr 04,05	Years ending 12 Apr 06,07
S19	Mathews, 2010	74 cities, USA	Varies from 03 to 06	Year before each ban	Year after each ban
S20	McMillen et al. (2010)	Starkville, Mississippi, USA (vs Outside city)	20 May 06	29 Jul 04 to 19 May 06	20 May 06 to 7 Apr 09
		Hattiesburg, Mississippi, USA (vs Outside city)	1 Jan 07	21 Apr 05 to 31 Dec 06	1 Jan 07 to 30 Jan 09
S21	Moraros et al. (2010)	Delaware, USA (vs non-state residents)	Nov 02	Q1 99 to Q3 02	Q1 03 to Q4 04[l]
S22	Sims et al. (2010)	England	1 Jul 07	Jul 02 to Jun 07	Jul 07 to Sep 08
S23	Dautzenberg (2008), Séguret et al. (2013) and Thomas et al. (2010)	France	Feb 07,Jan 08	03,04,05,06	08,09[m]
S24	Bonetti et al. (2011) and Trachsel et al. (2010)	Graubuenden, Switzerland (vs Lucerne[n])	1 Mar 08	Years ending Feb 07,08	Years ending Feb 09,10
S25	Barone-Adesi et al. (2011)	Italy	10 Jan 05	02,03,04	05[o]
S26	Bruckman and Bennerr (2011)	Ohio, USA	3 May 07	05,06	08,09[i]
S27	Bruintjes et al. (2011)	Greeley, Colorado, USA (vs Outside city)	Dec 03	Jul 02 to Nov 03	Dec 03 to Jun 06
S28	Ferrante et al. (2012)	Santa Fe province, Argentina	Aug 06	Aug 05 to Jul 06	Aug 06 to Jul 07
		Buenos Aires city, Argentina	Oct 06	Oct 05 to Sep 06	Oct 06 to Sep 07
S29	Gupta et al. (2011)	Kanawha County, West Virginia, USA	95,00,03	Jan 00 to Sep 08[p]	
S30	Hahn et al. (2011)	Lexington, Kentucky, USA	27 Apr 04	01,02,03	05,06[i]
S31	Herman and Walsh (2011)	Arizona, USA	May 07	Years ending Apr 05,06,07	Year ending Apr 08
S32	Hurt et al., 2011, 2012	Olmsted County, Minnesota, USA	1 Jan 02,1 Oct 07	18 months before each ban	18 months after each ban
S33	North Carolina Tobacco Prevention and Control Branch (2011)	North Carolina, USA	1 Jan 10	08,09	10
S34	Xuereb et al. (2011)	Malta	Apr 04	99 to 03	05 to 09[i]
S35	Barr et al. (2012)	387 counties in 9 states, USA[q]	Varies from 00 to 07	Jan 99 to Dec 08[p]	
S36	Christensen et al. (2012)	Denmark	15 Aug 07	Years ending Aug 03,04,05,06,07	Years ending Aug 08,09
S37	de Korte-de-Boer et al. (2012)	South Limburg, Netherlands	1 Jan 04	Jan 02 to Dec 03	Jan 04 to Jun 08[r]
S38	Head et al. (2012)	Beaumont, Texas, USA (vs Tyler[s])	Jul 06	Years ending Jun 05,06	Years ending Jun 07,08
S39	Huesch et al. (2012)	New Jersey, USA	15 Apr 06	May-Mar 04,05	May-Mar 06[t]

S40	Kent et al. (2012)	Ireland	29 Mar 04	02 to 03	05 to 06
S41	Loomis and Juster (2012)	Florida, USA[u]	Jul 03	Jan 90 to Dec 06[k]	
S42	Roberts et al. (2012)	Rhode Island, USA	Mar 05	03,04	06,07,08,09[i]
S43	Agüero et al. (2013)	Girona, Spain	1 Jan 06	02,03,04,05	06,07,08
S44	Gaudreau et al. (2013)	Prince Edward Island, Canada[v]	1 Jun 03[w]	98,99,00,01,02[x]	04,05,06,07,08[i]
S45	Sebrié et al. (2013)	Uruguay	1 Mar 06	Mar 04 to Feb 06	Mar 06 to Feb 08

[a] Refers to the data used in our meta-analysis, other available data being mentioned in footnotes.
[b] Control location shown in brackets.
[c] Ban suspended 3 Dec 02.
[d] Data also available for year after ban suspended but not used.
[e] Data for earlier years ignored as diagnostic criteria changed.
[f] Data from later paper to Jun 07 ignored, as only available analysis includes 2001.
[g] Data for outside Pueblo city limits ignored as may have been affected by ban.
[h] Data from later paper to 2006 ignored as relevant detail not available.
[i] Data for year of ban ignored.
[j] Data for Jan and Feb 99, and Mar to Jun 05 not used to avoid seasonal effects.
[k] Analysis based on model-fitting, with data by period not given.
[l] Q = quarter.
[m] Data for year between two stages of the ban ignored.
[n] Control data for Lucerne not used – see Appendix A.
[o] Incomplete data for 2006 ignored.
[p] Multiple bans so cannot define pre- and post-.
[q] Arizona, Delaware, Illinois, Massachusetts, Minnesota, New Jersey, New York, Ohio, Washington.
[r] Data relevant to a second ban (1 Jul 2008) not used – see Appendix A.
[s] Data for all Texas hospitals also available but not used as mixture of bans.
[t] Data for April each year not used. Also data for Jan-Mar 04 and May-Dec 07 not used to avoid seasonal effects.
[u] Data relevant to bans in two localities in Oregon (Corvallis 1998 and Eugene 2000) not used – see Appendix A.
[v] Data for control area, New Brunswick, ignored as not presented in suitable form.
[w] There was a minor extension to the ban 1 Jul 2006, which was found by the original author to have had no significant effect.
[x] Data for 1995–1997 not used, as trend very different.

Table 2
Age of population, endpoint used, availability of data for subgroups, and restrictiveness score details.

Study	Reference(s)[a]	Age	Endpoint used	Data also available by	Rating system[b]	Restrictiveness score %		
						Before	After	Difference
S1	Sargent et al. (2004)	18+	AMI admissions					
S2	Barone-Adesi et al. (2006)	All	AMI admissions[c]	Age[d]	J	27	77	50
S3	Alsever et al. (2009)	All	AMI admissions					
S4	Bullen et al. (2006)	15+	AMI admissions[e]		C1[f]	45	88	43
S5	Cronin et al. (2012)	18+	ACS admissions		J	14	95	82
S6	Heinz et al. (2007)	All	AMI admissions					
S7	Juster et al. (2007)	35+	AMI admissions		C1	45	88	43
S8	Khuder et al. (2007)	18+	CHD admissions					
S9	Seo and Torabi (2007)	All	AMI admissions	Smoking habit				
S10	Barnett et al. (2009)	30+	AMI admissions	Sex and age	C1[f]	45	88	43
S11	Cesaroni et al. (2008)	35–84	ACE episodes	Sex and age	J	27	77	50
S12	Lemstra et al. (2008)	All	AMI discharges		C1[f]	62	79	17
S13	Pell et al. (2008)	All	ACS admissions	Sex and age	J	5	95	91
S14	Vasselli et al. (2008)	40–64	AMI admissions	Region, sex and age	J	27	77	50
S15	Gasparrini et al. (2009)	30–64	AMI deaths or admissions		J	27	77	50
S16	Shetty et al. (2011)	All	AMI admissions					
S17	Villalbí et al. (2009)	25+	AMI discharges	Sex and age	J	23	68	45
S18	Di Valentino et al. (2010)	All	ACS admissions[g]	Sex and age	J[h]	27	50	23
S19	Mathews (2010)	65+	AMI claims[i]					
S20	McMillen et al. (2010)	All	AMI admissions					
S21	Moraros et al. (2010)	18+	AMI discharges		C1	40	93	52
S22	Sims et al. (2010)	18+	AMI admissions	Sex and age	J	5	95	91
S23	Séguret et al. (2013)	18+	ACS admissions	Sex and age	J	27	77	50
S24	Bonetti et al. (2011)	All	AMI admission and angiography		J[h]	27	50	23
S25	Barone-Adesi et al. (2011)	All	ACE admissions	Sex and age	J	27	77	50
S26	Bruckman and Bennerr (2011)	All	AMI discharges[j]		C1	29	93	64
S27	Bruintjes et al. (2011)	All	AMI admissions					
S28	Ferrante et al. (2012)	18+	ACS admissions		C1[f,k]	7	90	83
					C1[f,l]	7	48	40
S29	Gupta et al. (2011)	18+	ACS admissions					
S30	Hahn et al. (2011)	35+	AMI admissions	Sex				
S31	Herman and Walsh (2011)	All	AMI admissions[e]	Areas with and without prior local ban	C1	31	93	62
S32	Hurt et al. (2012)	All	AMI incidence	Two bans separately				
S33	North Carolina Tobacco Prevention and Control Branch (2011)	18+	AMI admissions	Sex and age	C2	15	53	38
S34	Xuereb et al. (2011)	All	ACS admissions		J[m]	0	77	77
S35	Barr et al. (2012)	65+	AMI admissions	State and age				
S36	Christensen et al. (2012)	30+	AMI admissions		J	14	50	36
S37	de Korte-de-Boer et al. (2012)	20–75	SCA events		J	5	41	36
S38	Head et al. (2012)	All	AMI discharges	Race				
S39	Huesch et al. (2012)	All	AMI admissions		C1	48	86	38

S40	Kent et al. (2012)	20–69	AMI admissions[n]	J	14	95	82
S41	Loomis and Juster (2012)	35+	AMI admissions	C1	48	81	33
S42	Roberts et al. (2012)	18+	AMI admissions	C1	45	88	43
S43	Agüero et al. (2013)	35–74	AMI incidence[o]	J	23	68	45
S44	Gaudreau et al. (2013)	35+	AMI admissions[e]	C1[f]	24	83	60
S45	Sebrié et al. (2013)	20+	AMI admissions	C1[f]	2	93	90

[a] Reference for main endpoint.
[b] C1 – rating system from Chriqui et al. (2002) without adjustment for pre-emption, maximum 42 points. Data sources American Lung Association, (2004, 2005, 2006) and Chriqui et al. (2002). C2 – Chriqui system as modified in American Lung Association (2009), maximum 40 (states without casinos) or 44 points (states with casinos). Data sources American Lung Association (2009, 2010). J – smoke-free section of the Tobacco Control Scale from Joossens and Raw (2006), maximum 22 points. Data sources (Joossens and Raw, 2006, 2007, 2011; Nguyen et al., 2012). Scores have been re-expressed as percentages, see methods. No rating attempted for local US studies or where bans in multiple locations were considered.
[c] Results also available for ACE admissions.
[d] Only for ACE admissions.
[e] Results also available for angina.
[f] No published rating available, scores "before" and "after" estimated from all available information.
[g] Results also available for ST-elevations myocardial infarction.
[h] National scores, although available information suggests that cantonal scores would be similar.
[i] Based on Medicare claims for beneficiaries diagnosed with AMI.
[j] Results also available for heart attack and AMI as percent of emergency department admissions.
[k] Scores for Santa Fe.
[l] Scores for Buenos Aires.
[m] No published rating available for "before", score estimated as zero.
[n] Results also available for ACS admissions.
[o] Results also available for AMI admissions and AMI case fatality.

studies accepted, 21 had been considered in our earlier review (Lee and Fry, 2011). Additional data were available from later publications in some studies considered earlier.

3.2. Study characteristics

Table 1 gives, for each study, the relevant references, the location of the study area (and the control area if applicable), the timing of the ban, and the periods pre- and post-ban for which data are available. The studies are identified by codes S1 to S45. Studies were conducted in 15 countries. National estimates are available for seven European countries – Denmark, England, France, Ireland, Italy, Malta and Scotland – and also for New Zealand, Uruguay and the USA. Only regional estimates are available for Argentina, Canada, Netherlands and Switzerland. There are regional, as well as national, studies for Ireland, Italy and particularly the USA. Twenty-two studies were conducted in the USA, and five in Italy, with other countries having only one or two studies. The studies in the USA varied widely in their coverage, with one study (S16) conducted nationally, one (S19) in 74 cities, one (S35) in multiple states, seven (S7, S26, S31, S33, S39, S41, S42) in single states, and a further 12 in specific locations within a state. Of the 37 studies which considered a single ban, all the bans occurred in 2002–2010, with the number of studies for each of those years being, respectively, 3, 6, 9, 6, 7, 4, 1, 0 and 1. There were eight studies which considered effects of multiple ordinances (either in multiple locations or successive ordinances in individual locations).

Table 2 presents further study details on the age of the populations studied, the endpoint used in our analyses, the availability of estimates for study subsets, and the restrictiveness scores. Eighteen of the studies considered the whole age range, and a further 19 studies only excluded children or younger adults where the risk of heart disease would have been quite small. Six studies considered a defined age range limited below and above, such as 30–64 years. Two studies, both based on Medicare enrollees, restricted attention to ages 65+ years.

Although only the combined results from each study are considered in our analyses, 17 of the studies presented results for study subsets. Ten presented results by age and sex (one also by location), one by age only, one by sex only, one by race only, one by location only, and one by location and age. One presented results separately for smokers and non-smokers, and one separately for multiple bans.

Of the 45 studies, 34 had AMI as the main endpoint, 24 referring to AMI admissions, five to AMI discharges, two to AMI incidence, and one each to deaths or admissions, claims, or admissions and angiography. ACS admissions was the main endpoint used in seven studies, with four other endpoints each used in one study: CHD admissions; ACE; ACE admissions; and SCA events. Eight studies provided data for alternative heart disease endpoints.

Table 2 presents pre-ban and post-ban restrictiveness scores and their difference, scores being unavailable for 14 studies. The 32 sets of scores for the remaining 31 studies (S28 having two sets) have been derived by three methods: the Chriqui system (Chriqui et al., 2002) (14 studies), the modified Chriqui system (American Lung Association, 2009) (1 study) and the Joossens and Raw system (Joossens and Raw, 2006) (17 studies), and then expressed as a percentage of the maximum score possible. Pre-ban scores range from 0% to 62% (median 27%), post-ban scores from 41% to 95% (median 79%) and differences from 17% to 91% (median 50%). The largest differences, of 91%, were for the UK (studies S13 and S22).

Further details of each study are given in Appendix A. Apart from giving details of the nature of the ban and the results reported by the authors, reference is made to weaknesses in the original estimates and to why (where relevant) some subsets of the study results were rejected, and a clear description is given of how the main RR estimate we used was derived. Where relevant, comments are made on the RRs used in some earlier reviews and meta-analyses (Glantz, 2008; Institute of Medicine, 2010; Lightwood and Glantz, 2009; Mackay et al., 2010; Meyers et al., 2009).

3.3. Studies where adjustment for trend was not possible

In six studies, conducted in five countries, adjustment for time trend was not possible, as there was no control area, or data for only one time period pre-ban. Table 3 gives, for each study, the mean number of cases pre- and post-ban, and the ban effect RR derived using formula 1 (or in study S19 as provided by the author). Of the seven RR estimates (study S28 presenting results for two areas), six were below 1, five significantly so (at $p < 0.05$), with the random-effects meta-analysis estimate 0.91 (95% CI 0.84–0.99).

3.4. Studies using control data

Eight studies, all in the USA, provided results for a control population where no ban was in force. Table 4 gives, for each study, the number of cases pre- and post-ban in each area and the ban effect RR derived using formula 2 (or in studies S27 and S38 the ratio of the authors' separate estimates of the post-ban decline in the ban and the control areas). Of the nine RR estimates (study S20 presenting results for two areas), all were below 1, four significantly so (at $p < 0.05$), with the random-effects meta-analysis estimate 0.80 (95% CI 0.68–0.95).

3.5. Studies adjusting for linear trend

There were 31 studies without control data for which results adjusted for linear trend were available, being either provided by the authors themselves or derived by us. Thirteen studies were conducted in the USA, with five in Italy, two each in Canada, New Zealand, Spain and Switzerland, and one in each of five countries. Table 5 gives, for each study, the numbers of cases in each time period pre- and post-ban, and the ban effect RR derived as described in Section 2.4.3, except where indicated. Of the 31 estimates, 26 were below 1, nine significantly so (at $p < 0.05$), and five were above 1, three significantly. The random-effects meta-analysis estimate is 0.97 (95% CI 0.95–1.00).

Of the studies with controls considered in Table 4, three allowed an alternative estimate adjusted for linear trend using formula 3. Table 6 compares the alternative and original ban effect RRs. The alternative estimate was higher in two studies and lower in one.

The estimates in Table 5 were based on the numbers of cases by period pre- and post-ban. If the population were changing over time, these estimates might be somewhat biased. Where rates are available, an alternative estimate can be derived, based on rates rather than numbers. Table 7 compares the estimates calculated both ways for the 24 studies where both numbers of cases and rates are available. The pairs of estimates are generally remarkably similar, with 13 the same to two decimal places, and none differing by more than ±0.02. This indicates that the approach based on numbers we used earlier (Lee and Fry, 2011) is adequate. Since populations or rates are not always available, we continue to use estimates based on numbers for our main meta-analyses.

3.6. Studies adjusting for quadratic trend

For those 17 studies considered in Table 5 with at least three time periods pre-ban, Table 8 presents alternative estimates adjusted for quadratic trend, with those adjusted only for linear trend also presented for comparison. There is no strong evidence of any systematic effect on the estimate, with five of the quadratic estimates higher, 11 lower and one the same (to two decimal places), compared to the linear estimates. The quadratic estimates have wider 95% CIs, particularly in three studies (S2, S14, S15). Because of this, the weight (inverse-variance) of the quadratic estimates is lower than those of the linear estimates. The meta-analysis estimates are similar, 0.96 (95% CI 0.93–0.99) using quadratic adjustment, and 0.97 (0.95–0.99) using linear adjustment.

3.7. Further meta-analyses

Table 9 presents results of fixed-effect and random-effects meta-analysis for all 47 estimates included in Tables 3–5, and also results of meta-analyses either replacing some of the main analysis estimated by alternatives included in Tables 6–8, or excluding various estimates from the analysis. It can be seen that in all eight analyses, both fixed and random estimates showed a modest ban effect, ranging from a 2.5% reduction (RR = 0.975) to a 5.8% reduction (RR = 0.942). All these reductions were statistically significant at $p < 0.05$, though sometimes marginally so. The individual estimates included in each analysis showed highly significant ($p < 0.001$) heterogeneity.

Based on the random-effects estimate, the main analysis, A1, gave an RR of 0.958 (95% CI 0.935–0.982). Using the alternative estimates from Table 6 (using formula 3 rather than 2) in analysis A2 had very little effect on the estimates. Using rates rather than numbers in analysis A3 slightly reduced the estimated decrease in risk from bans, while using quadratic rather than linear estimates slightly increased it (analyses A4 and A5). However, estimates

Table 3
Effect of smoking bans on AMI admissions (or near equivalent) for studies where adjustment for trend was not possible.

Study	Reference(s)	Location	Pre-ban period		Post-ban period		RR (95% CI)[a]
			Mean cases	(yrs)	Mean cases	(yrs)	
S5	Cronin et al. (2012)	South West Ireland	1216	1	1020.33	3	0.84 (0.77–0.91)
S13	Pell et al. (2008)	Scotland	3235	10 months[b]	2684	10 months	0.83 (0.79–0.87)
S19	Mathews (2010)	74 cities, USA	–	Varies	–	Varies	0.97 (0.95–0.99)[c]
S28	Ferrante et al. (2012)	Santa Fe, Argentina	1612	1	1277	1	0.79 (0.74–0.85)
		Buenos Aires, Argentina	1699	1	1608	1	0.95 (0.88–1.01)
S34	Xuereb et al. (2011)	Malta	850	5	890	5	1.05 (0.95–1.15)
S40	Kent et al. (2012)	Ireland	4072	2	3876	2	0.95 (0.91–0.99)
Meta-analysis – random effects ($n = 7$ estimates)							0.91 (0.84–0.99)[d]

[a] All estimates were calculated using formula 1, except where indicated.
[b] Same 10 months in year as for pre-ban data.
[c] Estimate by the author based on data for 74 cities, with the mean rate for 2000–2008 in the post-ban period being divided by that for the pre-ban period.
[d] Heterogeneity chisquared = 65.20 on 6 d.f., $p < 0.001$.

Table 4
Effect of smoking bans on AMI admissions (or near equivalent) for studies using control data.

Study	Reference(s)	Location	Cases in ban area		Cases in control area		RR (95% CI)[a]
			Pre-ban	Post-ban	Pre-ban	Post-ban	
S1	Sargent et al. (2004)	Helena, Montana, USA (vs Outside City)	162	24	46	18	0.38 (0.19–0.76)
S3	Alsever et al. (2009)	Pueblo, Colorado, USA (vs El Paso County)	399	528	1299	2471	0.70 (0.60–0.81)
S8	Khuder et al. (2007)	Bowling Green, Ohio, USA (vs Kent)	211	218	300	315	0.98 (0.77–1.26)
S9	Seo and Torabi (2007)	Monroe County, Indiana, USA (vs Delaware County)	25	12	26	22	0.57 (0.23–1.38)
S20	McMillen et al. (2010)	Starkville, Mississippi, USA (vs Outside County)	83.87[b]	38	35.58[b]	19	0.85 (0.43–1.67)
		Hattiesburg Mississippi, USA (vs Outside County)	506.93[b]	299	1541.35[b]	1090	0.83 (0.71–0.98)
S21	Moraros et al. (2010)	Delaware, USA (vs non-residents)	6769	3441	1300	679	0.97 (0.88–1.08)
S27	Bruintjes et al. (2011)	Greeley, Colorado, USA (vs Outside City)	0.73 (0.59–0.90)[c]		0.83 (0.61–1.14)[c]		0.88 (0.60–1.28)[d]
S38	Head et al. (2012)	Beaumont, Texas, USA (vs Tyler)	514	376	313	346	0.66 (0.54–0.81)[d]
Meta-analysis – random effects (n = 9 estimates)							0.80 (0.68–0.95)[e]

[a] All estimates were calculated using formula 2, except where indicated.
[b] Numbers of cases pre-ban standardized so that they relate to the same number of days as the post-ban period.
[c] RR for post-ban vs pre-ban within area.
[d] Ratio of RRs for ban area and control area.
[e] Heterogeneity chisquared = 27.25 on 8 d.f., $p < 0.001$.

varied only by about ±0.01 or 0.02 from the main analysis, which itself had a 95% CI of almost ±0.025.

Excluding results from the main analysis had rather more effect, whether excluding (to avoid double-counting) original estimates where national estimates were available (analysis A6) or excluding Table 6 results which did not adjust for trend (analysis A7). Both exclusions seem scientifically appropriate and the net effect is to reduce the main analysis reduction of 4.2% (RR = 0.958) to 2.6% (RR = 0.974) (analysis A8).

Table 10 studies how the ban effect estimates vary by eight different factors. There was no evidence of variation significant at $p < 0.05$ by any factor studied, though there was some indication of an effect for some.

Ban effects were greater (i.e., smaller RR) for the 10 estimates with the least weight (<100), six of which were from the studies summarized in Table 4 using control data. There was also some evidence that the effect related to change in the restrictiveness score, with a 4–5% reduction seen where the change in score was by 40% or more, but no reduction seen where it was less than 40%. There was also some indication that estimated reductions were greater in studies with a short pre-ban period, and in studies conducted outside USA or Western Europe. No clear relationship was seen with the length of the post-ban period, or with either the lower or upper age limit of the populations studied.

4. Discussion

4.1. Rapid increase in the number of publications

In our earlier review (Lee and Fry, 2011), we considered data from 24 studies published between 2004 and 2011, three rejected in our current analysis. Subsequently, in little over two years, the number of studies has risen substantially, from 24 to 57 (with 12 rejected), illustrating the increasing number of bans and level of interest in their effect.

4.2. Weaknesses of the studies and published estimates

As highlighted earlier (Lee and Fry, 2011), the estimates derived by the authors of the source papers are based on a variety of methods, many suffering from weaknesses. These are discussed quite fully in Appendix A, so here we merely summarize some of the problems, including failure to take seasonal variation into account, failure to use control data, failure to account for the underlying trend pre-ban, basing estimates on selected subsets rather than on the whole available population, failure to consider changes in diagnostic criteria, and incorrect estimation of results. Also, there is the possibility of publication bias, with studies finding no effect of a ban possibly never being published, and bias due to "regression to the mean" if bans tend to be introduced, or studies conducted, in areas with a high AMI rate, though this is more relevant to local rather than national studies. Though publication bias and regression to the mean are hardly relevant to the national studies, they are quite plausible sources of bias in the small area studies using control data (Table 4), and may help to explain their greater apparent ban effect.

4.3. Need for a consistent approach

A consistent approach is clearly needed, and to achieve this we derived our own estimates based on the whole available data, and, where possible, adjusting for the underlying time trend, using control data if available or, if not, based on data for multiple pre-ban periods. We rejected studies where seasonal variation could not be taken into account, and studies seriously biased for other

Table 5
Effect of smoking bans on AMI admissions (or near equivalent) for studies without control data for which adjustment for linear time trend is possible.

Study	Reference(s)	Location	Periods[a]	Cases by time period		RR (95% CI)[b]
				Pre-ban	Post-ban	
S2	Barone-Adesi et al. (2006)	Piedmont, Italy	2002–2004, 2005	3230 3473 3581	3655	0.96 (0.91–1.02)
S4	Bullen et al. (2006)	New Zealand	Dec 1996–Nov 2004, Dec 2004–Nov 2005	6480 6690 7235 7565 9055 9895 11,315 11,125	12,045	0.95 (0.92–0.97)
S6	Heinz et al. (2007)	Boise, Idaho, USA	Jul 2002-Jun 2004, Jul 2004-Jun 2005	NA	NA	0.82 (0.63–1.06)[c]
S7	Juster et al. (2007)	New York State, USA	1995–2002, 2004	44,683 45,449 44,961 44,651 45,889 48,010 48,015 47,943	45,412	0.92 (0.91–0.93)
S10	Barnett et al. (2009)	Christchurch, New Zealand	2003–2004, 2005–2006	809 768	734 763	0.96 (0.82–1.13)
S11	Cesaroni et al. (2008)	Rome, Italy	2002–2004, 2005	6935 7025 6890	6739	0.98 (0.93–1.02)
S12	Lemstra et al. (2008)	Saskatoon, Saskatchewan, Canada	Jul 2000–Jun 2004, Jul 2004–Jun 2005	351 323 363 341	312	0.90 (0.76–1.07)
S14	Vasselli et al. (2008)	Three areas, Italy[d]	2002–2004, 2005	1056 1151 1182	1148	0.91 (0.82–1.01)
S15	Gasparrini et al. (2009)	Tuscany, Italy	2002–2004, 2005	2319 2269 2254	2190	0.99 (0.92–1.07)
S16	Shetty et al. (2011)	USA	1990–2004[e]	NA	NA	0.98 (0.93–1.03)[f]
S17	Villalbí et al. (2009)	Barcelona, Spain	2004–2005, 2006	4686 4503	4127	0.95 (0.89–1.02)
S18	Di Valentino et al. (2010)	Ticino, Switzerland	Apr 13 2004–Apr 12 2006, Apr 13 2006–Apr 12 2008	659 673	564 569	0.82 (0.69–0.98)
S22	Sims et al. (2010)	England	Jul 2002–Jun 2007, Jul 2007–Sep 2008[g]	NA	NA	0.98 (0.96–0.99)[h]
S23	Séguret et al. (2013)	France	2003–2006, 2008–2009	127,151 126,185 125,125 124,446	120,812 121,811	0.99 (0.98–1.00)
S24	Bonetti et al. (2011)	Graubünden, Switzerland	Mar 2006–Feb 2008, Mar 2008–Feb 2010	168 177[i]	133 132[i]	0.73 (0.51–1.04)
S25	Barone-Adesi et al. (2011)	Italy	2002–2004, 2005	182,856 188,079 193,897	193,354	0.97 (0.96–0.98)
S26	Bruckman and Bennerr (2011)	Ohio, USA	2005–2006, 2008–2009	22,632 21,608	20,623 19,351	1.06 (1.02–1.11)
S29	Gupta et al. (2011)	Kanawha County, West Virginia, USA	2000–2002, 2004–2008	1637 1571 1530	1286 1256 1199 1062	0.99 (0.90–1.08)

					1000	
S30	Hahn et al. (2011)	Lexington, Kentucky, USA	2001–2003, 2005–2006	473 490 450	426 425	0.97 (0.78–1.21)
S31	Herman and Walsh (2011)	Arizona, USA	May 2004–Apr 2007, May 2007–Apr 2008	9942 9725 9731	9943	1.04 (1.00–1.07)
S32	Hurt et al. (2012)	Olmsted County, Minnesota, USA	Ban 1 1995–2001, 2002–2003	175 191 140 169 145 163 186	162 173	[1.01 (0.85–1.21)]
			Ban 2 2004–2006, 2008–2009	188 185 184	136 122	[0.76 (0.53–1.09)]
			Combined			0.77 (0.52–1.15)[j]
S33	North Carolina Tobacco Prevention and Control Branch (2011)	North Carolina, USA	2006–2009, 2010	9428 8317	8000	1.09 (1.03–1.15)
S35	Barr et al. (2012)	387 counties in 9 states, USA	1999–2008	NA	NA	0.95 (0.94–0.96)[k]
S36	Christensen et al. (2012)	Denmark	Sep 2002-Aug 2007, Sep 2007-Aug 2009	16,536 16,509 15,830 15,445 15,224	14,819 14,731	1.00 (0.98–1.03)
S37	de Korte-de-Boer et al. (2012)	South Limburg, Netherlands	2002–2003, 2004	NA	NA	0.93 (0.80–1.09)[l]
S39	Huesch et al. (2012)	New Jersey, USA	May 2004-Mar 2006, May 2006-Mar 2007[m]	20,065 18,207	17,958	1.09 (1.05–1.13)
S41	Loomis and Juster (2012)	Florida, USA	Jan 1990-Jun 2003, Jul 2003-Dec 2006[n]	NA	NA	0.82 (0.72–0.91)[o]
S42	Roberts et al. (2012)	Rhode Island, USA	2003–2004, 2006–2009	3041 2721	2427 2177 2194 1995	0.99 (0.92–1.06)
S43	Agüero et al. (2013)	Girona, Spain	2002–2005, 2006–2008	613 653 647 631	601 605 686	0.91 (0.81–1.02)
S44	Gaudreau et al. (2013)	Prince Edward Island, Canada	1998–2002, 2004–2007	388 403 424 413 388	406 419 416	0.94 (0.82–1.09)
S45	Sebrié et al. (2013)	Uruguay	Mar 2004-Feb 2006, Mar 2006-Feb 2008	2152 2195	1793 1806	0.80 (0.73–0.89)
Meta-analysis – random effects (n = 31 estimates)						0.97 (0.95–1.00)[p]

[a] The comma separates the pre-ban and post-ban periods. 12-month periods unless indicated.
[b] All estimates were calculated using formula 3, except where indicated.
[c] Author estimate adjusted additionally for weather and outdoor air quality, and based on daily data.
[d] Excluding Piedmont as already considered in study S2.
[e] Timing of bans varied.
[f] Author estimate for any smoking restrictions.
[g] Weekly data.
[h] Author estimate adjusted additionally for season, population size, temperature, flu rates and Christmas holidays.
[i] Cases are in residents. Control data for Lucerne not used – see Appendix A.
[j] Combined effect of two bans.
[k] Author estimate adjusted additionally for age, sex and season.
[l] Estimate derived from comparison of 300 cases observed post-ban with an expected incidence of 322 based on an analysis adjusting for linear time trend, population size, ambient temperature, air pollution and influenza.
[m] 11-month periods ending in March.
[n] Quarterly data.
[o] Author estimate adjusted for linear time trend.
[p] Heterogeneity chisquared = 223.94 on 30 d.f., $p < 0.001$.

Table 6
Effect of smoking bans on AMI admissions (or near equivalent) for studies with control data and multiple periods pre-ban; comparison of alternative estimates based on formula 3, with main estimates based on formula 2.

Study	Reference(s)	Periods[a]	Cases by time period		RR (95% CI)	
			Pre-ban	Post-ban	Alternative[b]	Original[c]
S1	Sargent et al. (2004)	Jun 1998-Nov 2001, Jun 2002-Nov 2002[d]	33	24	0.42 (0.25–0.72)	0.38 (0.19–0.76)
			37			
			42			
			50			
S8	Khuder et al. (2007)	Mar 1999–Feb 2002, Mar 2002-Feb 2005	81	87	1.03 (0.86–1.25)	0.98 (0.77–1.26)
			69	66		
			61	65		
S21	Moraros et al. (2010)	1999–2001, 2003–2004	1733	1815	0.87 (0.78–0.97)	0.97 (0.88–1.08)
			1761	1626		
			1945			
Meta-analysis – random effects ($n = 3$ estimates)					0.88 (0.56–1.40)[e]	0.96 (0.65–1.41)[f]

[a] The comma separates the pre-ban and post-ban periods. 12-month periods unless indicated.
[b] Estimate derived from data in ban area only using formula 3.
[c] Estimate derived using control data using formula 2, as given in Table 4.
[d] Only June to November considered in each year. 2003 ignored as ban rescinded in Dec 2002.
[e] Heterogeneity chisquared = 9.95 on 2 d.f., $p < 0.01$.
[f] Heterogeneity chisquared = 7.03 on 2 d.f., $p < 0.05$.

Table 7
Effect of smoking bans on AMI admissions (or near equivalent); comparison of alternative estimates based on rates with original estimates based only on numbers of cases.[a]

Study	Reference(s)	RR (95% CI) Estimates based on rates	RR (95% CI)[b] Estimates based on numbers
S2	Barone-Adesi et al. (2006)	0.98 (0.93–1.04)	0.96 (0.91–1.02)
S4	Bullen et al. (2006)	0.94 (0.91–0.96)	0.95 (0.92–0.97)
S7	Juster et al. (2007)	0.94 (0.93–0.95)	0.92 (0.91–1.03)
S10	Barnett et al. (2009)	0.96 (0.82–1.13)	0.96 (0.82–1.13)
S11	Cesaroni et al. (2008)	0.97 (0.93–1.02)	0.98 (0.93–1.02)
S12	Lemstra et al. (2008)	0.89 (0.75–1.06)	0.90 (0.76–1.07)
S14	Vasselli et al. (2008)	0.90 (0.81–1.00)	0.91 (0.82–1.01)
S15	Gasparrini et al. (2009)	1.00 (0.93–1.08)	0.99 (0.92–1.07)
S17	Villalbí et al. (2009)	0.96 (0.89–1.03)	0.95 (0.89–1.02)
S18	Di Valentino et al. (2010)	0.83 (0.69–0.99)	0.82 (0.69–0.98)
S23	Séguret et al. (2013)	0.99 (0.98–1.00)	0.99 (0.98–1.00)
S24	Bonetti et al. (2011)	0.73 (0.51–1.04)	0.73 (0.51–1.04)
S25	Barone-Adesi et al. (2011)	0.97 (0.96–0.98)	0.97 (0.96–0.98)
S26	Bruckman and Bennerr (2011)	1.06 (1.02–1.11)	1.06 (1.02–1.11)
S29	Gupta et al. (2011)	0.99 (0.90–1.08)	0.99 (0.90–1.08)
S30	Hahn et al. (2011)	0.97 (0.78–1.21)	0.97 (0.78–1.21)
S31	Herman and Walsh (2011)	1.04 (1.00–1.08)	1.04 (1.00–1.07)
S32	Hurt et al. (2012)	0.77 (0.52–1.15)	0.77 (0.52–1.15)
S33	North Carolina Tobacco Prevention and Control Branch (2011)	1.09 (1.03–1.15)	1.09 (1.03–1.15)
S36	Christensen et al. (2012)	1.01 (0.99–1.03)	1.00 (0.98–1.03)
S39	Huesch et al. (2012)	1.09 (1.05–1.13)	1.09 (1.05–1.13)
S42	Roberts et al. (2012)	0.99 (0.92–1.06)	0.99 (0.92–1.06)
S43	Agüero et al. (2013)	0.91 (0.81–1.02)	0.91 (0.81–1.02)
S44	Gaudreau et al. (2013)	0.95 (0.83–1.10)	0.94 (0.82–1.09)
Meta-analysis – random effects ($n = 24$ estimates)		0.99 (0.96–1.01)[c]	0.98 (0.96–1.01)[d]

[a] Based on studies where estimates adjusted for linear trend using formula 3 was possible, and where rate data were available.
[b] Adjusted for linear trend using formula 3.
[c] Heterogeneity chisquared = 160.62 on 23 d.f., $p < 0.001$.
[d] Heterogeneity chisquared = 191.21 on 23 d.f., $p < 0.001$.

reasons. We recalculated our own estimate except where the data were not available to allow us to do this.

4.4. Our findings compared to those of others

Based on data for all 45 studies which provided estimates for AMI admissions (or a near equivalent definition) our overall estimate (random-effects) of the ban effect was 0.958 (95% CI 0.935–0.952), which became 0.974 (0.960–0.989) when we excluded regional estimates where national estimates were available (to avoid overlap) and also excluded estimates where we could not adjust for time trend. This is equivalent to a reduction in risk following the ban of 2.6% (1.1–4.0%) and can be compared with our quite similar earlier estimate of 2.7% (2.1–3.4%). The estimated reductions are less than reported in other meta-analyses. Thus, Glantz (2008) estimated 19%, Lightwood and Glantz (2009) 17%, Meyers et al. (2009) 17%, Mackay et al. (2010) 10%, Lin et al. (2013) 13%, while Tan and Glantz (2012) estimated 8% for workplace bans, 5% for workplace and restaurant bans and 15% for the most comprehensive bans.

Differences between our results and those reported in the meta-analyses reported in 2008–2010 have been discussed in our earlier review (Lee and Fry, 2011). The review by Lin et al. (2013) only considers 15 of the 45 studies we consider, and includes only ban effect estimates reported by the authors, making no attempt to revise them to take control data into account or adjust for trend,

Table 8
Effect of smoking bans on AMI admissions (or near equivalent); comparison of estimates adjusted for quadratic and for linear trend.

Study	Reference(s)	Time points pre-ban	Time points post-ban	RR (95% CI) Quadratic adjustment	RR (95% CI) Linear adjustment
S2	Barone-Adesi et al. (2006)	3	1	1.03 (0.89–1.20)	0.96 (0.91–1.02)
S4	Bullen et al. (2006)	8	1	0.94 (0.91–0.97)	0.95 (0.92–0.97)
S7	Juster et al. (2007)	8	1	0.89 (0.87–0.91)	0.92 (0.91–0.93)
S11	Cesaroni et al. (2008)	3	1	1.03 (0.93–1.14)	0.98 (0.93–1.02)
S12	Lemstra et al. (2008)	4	1	0.88 (0.64–1.20)	0.90 (0.76–1.07)
S14	Vasselli et al. (2008)	3	1	1.00 (0.78–1.30)	0.91 (0.82–1.01)
S15	Gasparrini et al. (2009)	3	1	0.96 (0.80–1.16)	0.99 (0.92–1.07)
S23	Séguret et al. (2013)	4	2	0.97 (0.95–0.98)	0.99 (0.98–1.00)
S25	Barone-Adesi et al. (2011)	3	1	0.97 (0.95–0.98)	0.97 (0.96–0.98)
S29	Gupta et al. (2011)	3	5	0.94 (0.85–1.04)	0.99 (0.90–1.08)
S30	Hahn et al. (2011)	3	2	0.96 (0.74–1.26)	0.97 (0.78–1.21)
S31	Herman and Walsh (2011)	3	1	1.00 (0.91–1.09)	1.04 (1.00–1.07)
S32	Hurt et al. (2012)	5, 3[a]	2, 2	0.67 (0.39–1.15)[b]	0.77 (0.52–1.15)
S35	Barr et al. (2012)	NA	NA	1.01 (0.98–1.03)	0.95 (0.94–0.96)
S36	Christensen et al. (2012)	5	2	0.99 (0.96–1.03)	1.00 (0.98–1.03)
S43	Agüero et al. (2013)	4	3	0.88 (0.77–1.00)	0.91 (0.81–1.02)
S44	Gaudreau et al. (2013)	5	3	0.95 (0.82–1.09)	0.94 (0.82–1.09)
Meta-analysis – random effects (*n* = 17 estimates)				0.96 (0.93–0.99)[c]	0.97 (0.95–0.99)[d]

[a] Two separate bans.
[b] Based on estimates of 0.79 (0.60–1.05) for ban 1 and 0.84 (0.53–1.35) for ban 2.
[c] Heterogeneity chisquared = 75.50 on 16 d.f., $p < 0.001$.
[d] Heterogeneity chisquared = 111.94 on 16 d.f., $p < 0.001$.

Table 9
Effect of smoking bans on AMI admissions (or near equivalent). Further meta-analyses of data considered in Tables 5–8.

Analysis number	Estimates included	No. of estimates	RR (95% CI)		Heterogeneity chisquared[a]
			Fixed effect	Random effects	
Main analysis					
A1	All in Tables 3–5	47	0.966 (0.962–0.970)	0.958 (0.935–0.982)	347.1***
Variants of main analysis					
A2	Use alternative not original estimates from Table 6	47	0.966 (0.962–0.970)	0.956 (0.932–0.981)	353.4***
A3	Use estimates based on rates not on numbers from Table 7	47	0.969 (0.965–0.974)	0.967 (0.948–0.987)	324.8***
A4	Use quadratic rather than linear estimates from Table 8	47	0.963 (0.957–0.970)	0.942 (0.913–0.971)	314.1***
A5	Use quadratic rather than linear estimates from Table 8 only if at least 4 pre-ban points	47	0.963 (0.958–0.968)	0.948 (0.923–0.975)	322.6***
Exclusions from main analysis					
A6	Exclude regional estimates where national estimates available[b]	19	0.972 (0.967–0.977)	0.972 (0.957–0.986)	117.7***
A7	Exclude Table 3 estimates	40	0.968 (0.964–0.973)	0.972 (0.950–0.995)	269.3***
A8	Exclusions as in analyses A6 and A7	14	0.975 (0.969–0.980)	0.974 (0.960–0.989)	46.2***

[a] Coded as $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $(^{*})\ p < 0.1$, NS $p > 0.1$.
[b] Based on studies S4, S12, S13, S16, S17, S18, S22, S23, S24, S25, S28 (two estimates), S34, S36, S37, S40, S43, S44, and S45. The national estimate for the USA from study S16 is preferred to that from study S19 as it was adjusted for trend.

though the discussion refers to the need for adjustment for non-linear trend, citing Barr et al. (2012). The review by Tan and Glantz (2012) is more comprehensive, including a large number of the studies we consider. However, there are some differences in approach. Thus, for studies that give results by age, they use results for <65 years of age, as the risk of CHD from smoking is known to decrease with age, whereas we use all age results, for consistency with the other studies. They also give separate sex results, where available, whereas we prefer combined sex results, and they never use estimates based on control data, strangely preferring simple after/before comparisons within the ban area. We did attempt to compare Tan and Glantz's estimates with ours (detailed results now shown), where this was possible, and found numerous differences. The reason for this was not always clear, as they did not explain in detail how their estimates were derived. Of 31 cases where direct comparison was possible, the ban effect was the same in only six, and in as many as 20 of the remaining 25 they calculated a stronger ban effect than we did. The most notable difference was for study S33 where their estimate, ignoring trend, was 0.79, and ours, adjusting for trend, was 1.09. Other notable differences were seen for study S31 (0.84 vs 1.04) and study S27 (0.73 vs 0.88).

4.5. New features

Our review extends and modifies our methods in various ways.

4.5.1. Quadratic vs linear adjustment for time trend

Following the report by Barr et al. (2012) that estimates derived assuming the underlying trend is linear may be substantially biased if a non-linear trend exists pre-ban, we attempted to derive estimates adjusted for a quadratic trend. We were limited by only 16 other studies providing data for at least three time periods pre-ban, the minimum number of periods to fit a quadratic trend. Moreover, over half of these provided data for only three periods. Noting that there seemed to be no consistent directional difference between quadratic and linear estimates, with random-effects overall estimates for the 17 studies quite similar (see Table 8), that the quadratic estimates have a wider 95% CI than do the linear estimates, sometimes quite substantially, and that quadratic estimates are not available for many studies, we decided to keep to our original approach (Lee and Fry, 2011) and not include quadratic estimates in our main analyses. We do, however, accept the premise of Barr et al. (2012) that this is not totally desirable if trends actually are non-linear, and that, especially where the

Table 10
Effect of smoking bans on AMI admissions (or near equivalent). Variation in relative risk by different factors.[a]

Factor	Level	No. of estimates	RR (95% CI)		Heterogeneity between levels
			Fixed effects	Random effects	
Weight of estimate	>10,000	5	0.963 (0.959–0.968)	0.961 (0.911–1.014)	$p = 0.085$
	1001–10,000	12	0.988 (0.979–0.998)	0.990 (0.954–1.028)	
	501–1000	6	0.919 (0.892–0.947)	0.916 (0.861–0.976)	
	101–500	14	0.908 (0.879–0.938)	0.902 (0.854–0.952)	
	<100	10	0.808 (0.732–0.891)	0.807 (0.702–0.928)	
Source of estimate[b]	Table 3	7	0.940 (0.925–0.955)	0.911 (0.865–0.959)	$p = 0.130$
	Table 4	9	0.841 (0.788–0.897)	0.831 (0.743–0.929)	
	Table 5	31	0.969 (0.965–0.973)	0.974 (0.949–1.000)	
Region	USA	23	0.957 (0.950–0.964)	0.978 (0.944–1.014)	$p = 0.126$
	Western Europe	17	0.974 (0.967–0.980)	0.956 (0.923–0.989)	
	Other	7	0.926 (0.908–0.945)	0.901 (0.844–0.963)	
Pre-ban period[c]	Up to 18 months	9	0.932 (0.917–0.948)	0.885 (0.836–0.938)	$p = 0.131$
	19 to 30 months	17	1.012 (0.994–1.031)	0.975 (0.927–1.025)	
	31 to 42 months	7	0.972 (0.964–0.979)	0.983 (0.925–1.044)	
	>42 months	11	0.965 (0.958–0.971)	0.960 (0.918–1.004)	
Post-ban period[c]	Up to 18 months	20	0.961 (0.956–0.966)	0.958 (0.924–0.993)	$p = 0.065$
	19 to 30 months	12	0.989 (0.980–0.998)	0.951 (0.899–1.005)	
	31 to 42 months	9	0.853 (0.812–0.895)	0.856 (0.782–0.937)	
	>42 months	3	0.998 (0.947–1.052)	0.997 (0.890–1.116)	
Change in restrictions[d]	<40%	8	1.023 (1.006–1.040)	1.022 (0.967–1.081)	$p = 0.093$
	40–60%	15	0.963 (0.958–0.968)	0.959 (0.929–0.990)	
	>60%	9	0.968 (0.955–0.980)	0.954 (0.914–0.996)	
Lower age limit	<21	34	0.975 (0.970–0.980)	0.952 (0.921–0.983)	$p = 0.100$
	21–64	11	0.944 (0.934–0.953)	0.949 (0.899–1.003)	
	65+	2	0.957 (0.946–0.968)	0.960 (0.877–1.051)	
Upper age limit	None	41	0.966 (0.962–0.971)	0.957 (0.932–0.983)	$p = 0.857$
	64–84	6	0.960 (0.935–0.985)	0.955 (0.890–1.025)	

[a] Based on all estimates in Tables 6–8, i.e. main analysis in Table 9.
[b] Random effects estimates differ slightly from those given in Tables 3–5 due to differing residual error.
[c] Excludes studies S16, S29 and S35 where this varied according to the ban.
[d] Excludes 15 studies where data were unavailable (see Table 2).

data cover a long time period, assuming linearity may lead to some bias.

4.5.2. Modifying the method of adjusting for trend

The approach used in our earlier paper (Lee and Fry, 2011) only used the pre-ban data to determine the shape of the time trend. Here we estimate the shape of the underlying trend using pre- and post-ban data. While the estimates are unchanged where there is only one post-ban period, they differ when there are multiple post-ban periods. In preliminary work (not reported here), we found that the revised method produced more stable estimates when adjusting for quadratic trend, as it incorporated more information.

Our method assumes the ban effect is simply to multiply subsequent risk by a factor, without affecting the underlying slope of the trend. Lack of clear evidence of a relationship between the length of post-ban period and the estimated ban effect to some extent supports this assumption, though it must be admitted that the effect may be less simple than we have posited. We have not attempted to fit an alternative model in which the ban also affected the underlying trend, partly as this could only be fitted to studies with multiple post-ban periods, and partly as it seems likely that the estimates could be unstable and difficult to interpret where there are limited post-ban periods.

4.5.3. Numbers or rates

We also investigated the effect of using population data and thus rates, where available, to estimate ban effects, rather than assuming there was no meaningful change in the at risk population. As shown in Table 7, and in the alternative analysis in Table 9, using estimates based on rates rather than numbers had little effect, so justifying the assumption we used earlier (Lee and Fry, 2011). In any case, a marked change in population in a short period is likely to be associated with immigration or emigration, and the changing make-up of the population may have introduced other factors affecting the outcome apart from the ban. Furthermore, if the change in population is linear, the numbers of cases predicted post-ban by formulae 3 will still be correct.

4.5.4. Subgroup analyses

We included analyses comparing estimates of the ban effect by various factors (see Table 10). There was a greater reduction in smaller studies, possibly related to small studies that fail to find an effect being less likely to report their results. This bias may also explain the greater reductions in the studies in Table 4 (using control populations) which were typically conducted at US county level.

We also observed that the ban effect was only seen in studies with larger changes in restrictiveness score following the ban, and not seen at all where the change was smallest. Although the method used to rate the restrictiveness of the bans was not fully consistent (being based on published ratings using different schemes in the US and Europe, and on our own estimates elsewhere), this finding seems to align with what one might expect if there were a true small reduction in risk associated with the ban. We found no evidence that the estimated ban effect varied with the overall age of the population studied.

4.6. Plausibility of a ban effect

As discussed in more detail earlier (Lee and Fry, 2011), there are various reasons why one might expect a true effect of a smoking ban on AMI rates. These include:

- Increased risk of heart disease in smokers (US Surgeon General, 2004; Yusuf et al., 2004) that declines quite rapidly on quitting (Lee et al., 2012; US Surgeon General, 1990),
- Increased risk in nonsmokers exposed to environmental tobacco smoke (ETS) (Glantz and Parmley, 1991, 1995; He et al., 1999; Law et al., 1997; Lee et al., 2013),
- Evidence that smoking bans lead to a reduction in the prevalence of smoking and in consumption per smoker (Bauer et al., 2005; Fichtenberg and Glantz, 2002; Gallus et al., 2006; Heloma and Jaakkola, 2003; Lemstra et al., 2008), and
- Evidence that smoking bans lead to a marked reduction in cotinine levels in nonsmokers (Haw and Gruer, 2007; Pechacek et al., 2007).

This picture is reinforced by the new evidence that ban effects seem greater if the change in restrictiveness following a ban is greater. One study (S31) reported similar results in their detailed analyses.

It is also clear from calculations carried out earlier (Lee and Fry, 2011) that any expected drop in heart disease rates following a ban would be quite modest, and not of the order of almost 20% claimed in some early reviews (Glantz, 2008; Lightwood and Glantz, 2009; Myers et al., 2009).

However, various uncertainties remain, due to the weakness of much of the published evidence on bans, the small magnitude of the estimated effect, and the possibilities of bias.

4.7. Limitations

One limitation of this assessment clearly arises from the nature of the results available in the published literature, presented in various ways, some making precise analysis difficult, and consistency difficult to achieve.

Another limitation is that the extent to which bans have been complied with is not taken into account. This is rarely reported in these studies. We have not sought independent sources for such data. Evidence that ban effects are greater where compliance is better would strengthen the argument that the effect is a real one and not due to bias.

A further concern relates to changes in diagnosis. As the national study in Italy (S25) restricted attention to data for years from 2002, diagnostic changes having been introduced in 2000, and as the annual data for one of the regional studies in Italy (S11) was consistent with a different trend before 2002, we decided to ignore pre-2002 data for all the regional studies in Italy (S2, S11, S14, S15). However, we have not attempted any similar exclusion of data for other countries, nor investigated whether any other such diagnostic or classification changes are relevant. Pre-2002 data are used in relatively few studies (S4, S7, S12, S16, S29, S30, S32, S35, S41, S44), and in some of those (S16, S35, S41) the annual data are not available to correct our estimates.

Variation in endpoints used is also another issue that could, perhaps, be given more detailed attention.

5. Conclusions

Our updated review confirms the existence of important weaknesses in many published studies and meta-analyses. In contrast to various meta-analyses that claim large effects of introducing bans on incidence of AMI, we demonstrate that estimated effects are much smaller, if a valid and consistent approach, as far as possible taking account of time trends and control data, is used. Based on all 45 studies considered, the reduction is estimated to be by 4.2% (95% CI 1.8–6.5%). Excluding regional estimates where national estimates are available, and excluding studies where adjustment for the underlying trend was not possible, this reduces further, to 2.6% (1.1–4.0%). This reduction is consistent with a true effect on heart disease resulting from the ban modifying cigarette consumption and ETS exposure, an effect which would be important on a public health level. However, various uncertainties remain, due to the weakness of much of the published evidence on bans, the small magnitude of the estimated effect, and the possibilities of bias.

Conflict of interest

Peter Lee is a long-term consultant to the tobacco industry, while John Fry and Barbara Forey are employees of Peter Lee's Company, P.N. Lee Statistics and Computing Ltd. However, this is an independent scientific assessment, the views expressed being those of the authors alone.

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Appendix A. Supplementary data

Appendix A "The individual studies" describes the main features of the 45 studies, giving the study location, timing of the ban, comparisons made and the main ban effect estimates reported by the authors. Where relevant, it also describes weaknesses in these estimates and provides, and explains the derivation of, the alternative ban effect estimate used in Tables 3–5. Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.yrtph.2014.06.014.

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