Abstract

Our objective was to review the research on the effects of public clean air laws on smoking rates, compare these effects to those found in studies on the impact of private worksite restrictions and derive estimates of the potential reductions in smoking rates that might be expected from the implementation of the two types of policies. Data sources were computerized databases, references identified from pertinent peer-reviewed journal articles and books, and suggestions by experts on tobacco control policy. Comprehensive public clean air laws have the potential to reduce prevalence and consumption rates of the entire population (including non-working and non-indoor working smokers) by about 10%. Studies on private worksite regulations also suggest that strong worksite restrictions have the potential to reduce the prevalence rate of the entire population by about 6% over the long-term and the quantity smoked by continuing smokers by 2–8%, depending on the length of time after the ban. Further research is needed on the effects of the different types of public clean air policies on the entire smoking population and on different sociodemographic groups, how the effects of public clean indoor air laws depend on private restrictions already in place, and how the effect of private restrictions depend on whether or not they are supported by public clean air laws.

Introduction

Clean air laws have been enacted to reduce the harmful effects of environmental tobacco smoke (ETS) on non-smokers by restricting or banning smoking in designated public areas (Centers for Disease Control and Prevention, Office on Smoking and Health, 1986; Environmental Protection Agency, 1992; National Cancer Institute, 1999). While clean air laws are often justified in terms of reducing exposure to second-hand smoke, they may also reduce smoking rates. Restrictions on smoking may limit smoking by reducing opportunities to smoke and by changing attitudes towards smoking (Department of Health and Human Services, 2000; Levy and Friend, 2001). By reducing opportunities to smoke, smoking restrictions directly reduce the quantity of cigarettes smoked in the restricted areas, which may improve the chances of quitting. Smoking restrictions may also change norms regarding the social acceptability of smoking. As social attitudes change, smokers may be induced to attempt to quit or not initiate, thereby reducing the number of smokers.

Recent attention has centered on getting clean indoor laws passed as part of a comprehensive tobacco control strategy (Centers for Disease Control and Prevention, 1999; Department of Health and Human Services, 2000). Smoking may be restricted at worksites, restaurants and other public places, such as grocery stores.
shopping malls and public transit. Clean air laws in the US are mostly implemented at the state or local level. In 2001, two states did not allow any smoking in restaurants, one state required that work sites and restaurants allow smoking only in designated smoking areas with separate ventilation, 28 states required or allowed smoking in restaurants in designated areas, and 20 states required or allowed smoking in worksites in designated areas (www.cdc.gov/nccdphp/osh/state/rpt_map). The numbers have not changed since 1996. By 1998, there were about 850 communities with major smoke-free worksite and/or restaurant ordinances. Most of the growth occurred in several states in the years 1993 and 1994 (National Cancer Institute, 2000).

Many private businesses in the US have adopted smoking restrictions in states that have not enacted clean air laws (National Cancer Institute, 2000). The percent of indoor workers who reported that their worksites were smoke-free was 47% in 1993 (Gerlach et al., 1997), 64% in 1996 (Burns et al., 2001) and 69% in 1999 (Shopland et al., 2001). While the percentage of indoor air workers who work in places that have smoking bans is correlated (Shopland et al., 2001), many of the private worksite bans were implemented in states and cities that did not have public laws requiring such extensive restrictions. Further, many workers in states with worksite laws have reported that there were no smoking restrictions at their worksites.

Prior reviews have mostly examined studies of the effects of smoking restrictions implemented by individual private worksites [e.g. (Brownson, 1997; Eriksen and Gottlieb, 1998; Chapman et al., 1999; Burns et al., 2001; Hopkins et al., 2001; Fichtenburg and Glantz, 2002)]. Moreover, these studies have generally been limited to firms in particular industries, which may not be representative of firms in the rest of the country. These reviews are sometimes used to justify the passage of public clean air laws, without explicitly considering differences between public laws and private restrictions, and the issues involved in translating laws into practice.

In this paper, we consider a broader literature than previous reviews. In addition to considering studies of individual firms or firms in particular industries, we extensively consider population studies of the effect of private work restrictions on smoking behaviors. In addition, we review an extensive literature on the effects of public clean air laws. Within and across the different literatures, we compare the results found for effects of smoking restrictions on smoking behaviors. The results of these different types of investigations are also compared in terms of their respective strengths and limitations. Interestingly, although the literatures generally employ different methodologies, they provide complementary results. Limitations of the studies are then considered to guide future research.

**Methods**

Studies were collected using the Centers for Disease Control and Prevention’s Office of Smoking and Health’s website, the Medline website, Science Citation Index, Social Sciences Citation Index and the Tobacco Control website. In addition, we examined references identified from pertinent articles and books, including recent reviews (Brownson et al., 1997; Eriksen and Gottlieb, 1998; Chapman et al., 1999; Centers for Disease Control and Prevention, 2000; Burns et al., 2001; Hopkins et al., 2001). We also consulted with a panel of experts on tobacco control policy regarding available studies. The panel helped ensure that we included all relevant studies and helped us integrate the research to derive best estimates of the effects of clean air policies and smoking restrictions.

The review is limited to studies conducted in the US since 1985. We limit the review to one country and a limited time period so that the results from the different studies can be more reliably compared, but we address studies from other nations in the Discussion section. We only include studies that examined the effect of smoking restrictions on at least one type of smoking behavior, such as
changes in quantity smoked per smoker, prevalence and cessation rates. In attempting to provide a broad overview of all studies, we include any study that either made comparisons over time (i.e. before and after smoking restrictions were implemented) or between populations exposed and those not exposed to smoking restrictions. We include published articles and books, and refereed reports (e.g. by the National Bureau of Economic Research).

For purposes of comparing the different studies, differences in smoking behaviors between restricted and unrestricted areas were measured relative to their levels in the absence of restrictions (i.e. the absolute change relative to the base rate without restrictions) wherever possible. Comparing results across a divergent set of studies is a challenging task. We considered conducting meta-analysis to address this issue. However, because investigations varied widely in their designs, methodologies, samples and outcomes, we chose not to employ this technique (Hedges and Olkin, 1985). Instead, we decided to integrate study results by comparing the range of results and characteristics of the sample (time period and population), and by considering study quality. We gave greater emphasis to the better-conducted studies, such as those that included comparison groups and followed cohorts over time, in developing overall estimates. We also enlisted the assistance of tobacco control experts to ensure that the derived estimates seemed reasonable in deriving a reasonable range of the possible impact of laws and private smoking restrictions.

The effects of private worksite restrictions

Studies of individual worksites

Numerous studies have examined the effects of smoking restrictions in an individual firm or selected worksites within a particular industry. This research has generally compared smoking-related behaviors at a point or points in time after workplace restrictions were implemented to baseline rates, using both prospective and retrospective approaches (see Table I). The more well-designed studies included control sites and followed a cohort of workers over time.

Because comprehensive reviews of this literature have already been conducted [e.g. (Brownson et al., 1997; Eriksen and Gottlieb, 1998; Chapman et al., 1999; Hopkins et al., 2001; Fichtenburg and Glantz 2002)], we summarized the overall findings (see Table III). In general, retrospective studies of the impact of individual worksite restrictions reported that quantity smoked among continuing smokers was reduced in the range of 10–20% after 6–13 months. Prospective cohort studies also reported similar reductions in prevalence rates, ranging from 7 to 20% reductions, after the restrictions had been in place 1 year or more. The effects on quit rates were less consistent.

As important as the magnitude of the effects on quantity and prevalence is how these effects appear to unfold over time. Reductions in quantity smoked appeared to show their greatest decline within the first 6 months and then decreased over time [e.g. (Centers for Disease Control and Prevention, 1990; Hudzinski and Frohlich, 1990; Stave and Jackson, 1991; Jeffery et al., 1994; Olive and Ballard, 1996)]. By contrast, prevalence and quit rates generally showed no or little immediate effect [e.g. (Rosenstock et al., 1986; Becker et al., 1989; Mullooly et al., 1990)] and subsequent greater increases over time [e.g. (Centers for Disease Control and Prevention, 1990; Stillman et al., 1990)] although the evidence for this trend is less consistent than for quantity smoked (Jeffrey et al., 1994). Longo et al. (Longo et al., 1996, 1999) found that quit rates of workers in hospitals with bans increase over the 6 years following a ban, and that relapse rates appear to be similar to those who quit and are not in firms with bans.

Thus, evidence suggests that the effects of restrictions on quantity erode, while the effects on quit rates increase and are maintained. Smokers who reduce their consumption may have a greater likelihood of quitting, but successful cessation may require more than one attempt (Hughes, 2000). The return to higher levels of quantity smoked found in
<table>
<thead>
<tr>
<th>Study and year</th>
<th>Sample and type of restriction</th>
<th>Study methods</th>
<th>Percent change in cigarettes smoked per smoker</th>
<th>Percent change in smoking prevalence unless otherwise indicated</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rosenstock et al. (1986)</td>
<td>HMO; ban except in one room</td>
<td>retrospective; no control</td>
<td>−11.4% at 4 months</td>
<td>NS at 4 months</td>
</tr>
<tr>
<td>Peterson et al. (1988)</td>
<td>insurance company; ban in work areas only</td>
<td>retrospective; no control</td>
<td>−29.5% at 3 months</td>
<td>−6.3% at 3 months; +0.5% quit rate</td>
</tr>
<tr>
<td>Biener et al. (1989)</td>
<td>hospital; designated areas</td>
<td>prospective with controls</td>
<td>N/A</td>
<td>NS (incl. quit attempts)</td>
</tr>
<tr>
<td>Becker et al. (1989)</td>
<td>children’s hospital; total ban</td>
<td>prospective; no control</td>
<td>NS at 6 months</td>
<td>−6.6% at 6 months</td>
</tr>
<tr>
<td>Scott et al. (1989)</td>
<td>insurance company; designated areas</td>
<td>Retrospective; one group, post-ban only; no control</td>
<td>heavy smokers: −47%; light smokers: −22%; OR = 3.08</td>
<td>+11% QR at 7 months</td>
</tr>
<tr>
<td>CDC (1990)</td>
<td>psychiatric hospital; designated areas (pre-ban) to total ban</td>
<td>retrospective cohort; no control</td>
<td>−11% at 12 months</td>
<td>−13.8% at 13 months; −17.2% at 17 months</td>
</tr>
<tr>
<td>Gottlieb et al. (1990)</td>
<td>state government agency; designated areas</td>
<td>prospective cohort; no control</td>
<td>% smoking 15 or more cigarettes: −5.7% at 1 month; +2% at 6 months</td>
<td>+13.8% at 1 month; −14.8% at 6 months; +1.6% QA at 1 month; +2.2% QA at 6 months</td>
</tr>
<tr>
<td>Hudzinski and Frohlich (1990)</td>
<td>hospital; total ban, except psychiatric inpatients</td>
<td>prospective cohort; no control</td>
<td>−20.1% at 12 months</td>
<td>−9% at 6 months; −36% at 1 year</td>
</tr>
<tr>
<td>Mullooly et al. (1990)</td>
<td>non-physician employees at large HMO; 11 sites; total ban</td>
<td>prospective cross-sectional, no control</td>
<td>NS at 12 or 24 months</td>
<td>NS (including quit attempts) at 12 or 24 months</td>
</tr>
<tr>
<td>Stillman et al. (1990)</td>
<td>hospital; total ban</td>
<td>prospective cohort; no control</td>
<td>−20% at 6 months</td>
<td>−25% at 6 months; +25.6% quit rate at ≥8 months</td>
</tr>
<tr>
<td>Baile et al. (1991)</td>
<td>cancer center; total ban</td>
<td>retrospective; no control</td>
<td>−54.2% at 4 months</td>
<td>too small (N = 5) to analyze</td>
</tr>
<tr>
<td>Stave and Jackson (1991)</td>
<td>two medical centers; total ban</td>
<td>retrospective with controls</td>
<td>−24.1% at intervention site at 1 year; −1.1% at control site</td>
<td>self-reported: +45% QR at 3 months; +69% at 9 months; CO validated: +85% QR at 3 months; +73% at 9 months</td>
</tr>
<tr>
<td>Daughton et al. (1992)</td>
<td>hospital; total ban</td>
<td>prospective cohort; no control</td>
<td>−18.6% at 5 months</td>
<td>+14% QR rate at 12 months; +82% at 24 months</td>
</tr>
<tr>
<td>Offord et al. (1992)</td>
<td>hospital; total ban</td>
<td>retrospective; no control</td>
<td>30.2% decreased, 7.4% increased and 62.4% no change at 2 years</td>
<td>−17.4% at 30 months; +22.5% QR at 30 months</td>
</tr>
<tr>
<td>Brigham et al. (1994)</td>
<td>hospital; total ban</td>
<td>prospective cohort with control</td>
<td>N/A</td>
<td>NS QR at 4 weeks</td>
</tr>
<tr>
<td>Jeffery et al. (1994)</td>
<td>32 worksites; ban in most areas versus no ban or designated area</td>
<td>retrospective and prospective cohort with control</td>
<td>−11.2% at 2 years; NS in sites without ban</td>
<td>NS prevalence, QA or QR</td>
</tr>
<tr>
<td>Olive and Ballard (1996)</td>
<td>two federal hospitals; designated area versus total ban</td>
<td>prospective cross-sectional; no control</td>
<td>Hospital 1: −13.7% at 6 months; −14.3% at 12 months; Hospital 2: −7.4% at 6 months</td>
<td>Hospital 1: −20.4% at 12 months; +43% QR at 6 months, +70.6% at 1 year; Hospital 2: −18.5% at 6 months; +30.1% QR at 6 months</td>
</tr>
</tbody>
</table>
more long-term studies may be due to smokers with lower levels of consumption quitting, resulting in heavier smokers assuming a disproportionate number in the sample of remaining smokers.

Like previous reviews (Brownson et al., 1997; Chapman et al., 1999; Eriksen and Gottlieb, 1998; Hopkins et al., 2001), we found that worksite policies are generally associated with reductions in the quantity of cigarettes smoked per smoker, but less clear patterns emerge for cessation and prevalence rates. Overall, there is considerable variation in the amount of reduction in smoking behaviors observed for each of the different measures. Some of the variations can be attributed to differences in sample size, time of follow-up, type of industry, the way in which smoking is measured (e.g. whether a smoker who quits is included in follow-up consumption measures), the extent of smoking restrictions and interventions implemented in conjunction with the ban (e.g. cessation treatment programs). In particular, the type of workplace in which a ban is implemented may influence the impact of the policy. For example, many of the studies examined health care facilities. If those sites were more likely to attract non-smokers as employees, restrictions may be more enforceable. Because the investigations of individual worksites only examined specific firms, their results may not generalize to other types of businesses. Moreover, because surveys were often conducted at the workplace, there is the potential for bias in reporting due to the social unacceptability of smoking.

**Population-based studies of worksites**

Nine different population-based studies are shown in Table II. Population-based studies typically employ a random selection of workers in a large area, such as a state or an entire country. Population-based worksite studies may be more representative of the effects of workplace bans than the studies of individual firms discussed above. While businesses choosing to implement workplace bans may still be different in some way from those that do not, specific firms are not singled out. Consequently, these studies are likely to be more

**Table I. Continued**

<table>
<thead>
<tr>
<th>Study and year</th>
<th>Sample and type of restriction</th>
<th>Study methods</th>
<th>Percent change in smoking prevalence unless otherwise indicated</th>
<th>Percent change in cigarettes smoked per smoker</th>
</tr>
</thead>
<tbody>
<tr>
<td>Longo et al. (1996)</td>
<td>hospital; total ban</td>
<td>retrospective; logistic analysis</td>
<td>QR = 0.35, OR = 2.1 at 6 months; QR = 0.066, OR = 1.8 at 1 year; QR = 0.115, OR = 1.9 at 1.5 years; QR = 0.27, OR = 2.6 at 3 years; QR = 0.506, OR = 2.3 at 5 years</td>
<td>N/A</td>
</tr>
<tr>
<td>Longo et al. (1999)</td>
<td>hospital; total ban versus other non-smoke-free workplaces</td>
<td>prospective cohort, with hazard rate models</td>
<td>post-ban QR range of 44% higher at 5 years (OR = 1.6), 98% higher at 6 years (OR = 2.4)</td>
<td>N/A</td>
</tr>
</tbody>
</table>

Percent changes measured relative to initial rates, QA = quit attempts, QR = quit rate, NS = not significant (other results are significant), OR = odds ratio.
<table>
<thead>
<tr>
<th>Study and year</th>
<th>Methods</th>
<th>Survey, year and (sample size)$^{a}$</th>
<th>Smoking restrictions</th>
<th>Percent change in quantity smoked</th>
<th>Percent change in prevalence unless otherwise indicated</th>
</tr>
</thead>
<tbody>
<tr>
<td>Woodruff et al. (1993)</td>
<td>means tests and logistic regression</td>
<td>California Tobacco Survey, 1990 (11 704)</td>
<td>smoke-free (SF), work area (WA), lesser restrictions (LR) versus no restrictions (NR) $^{13%}$</td>
<td>everyday: $-33%$; OR: SF $= 1.0$, WA $= 1.15$, LR $1.36$, NR NS; someday: NS $-34%$</td>
<td>$-48.8%$</td>
</tr>
<tr>
<td>Kinne et al. (1993)</td>
<td>means tests</td>
<td>Washington, 1989–1990 (1228)</td>
<td>compared smoke-free versus no restrictions</td>
<td>NS</td>
<td>NS</td>
</tr>
<tr>
<td>Patten et al. (1995)</td>
<td>means tests and logistic regression</td>
<td>CA Tobacco Survey, 1990 versus 1992 (1844)</td>
<td>compared smoke-free versus not smoke-free</td>
<td>SF: $-2.78$ cig/smoker, DA: $-1.17$ cig/smoker, QA: SF: OR $= 1.27$, DA: OR $= 1.16$, NSF: OR $= 1$; OR (6+ months): SF: OR $= 1.27$, DA: OR $= 1.0$ (NS), NSF $= 1$</td>
<td></td>
</tr>
<tr>
<td>Glasgow et al. (1997)</td>
<td>means tests and logistic regression</td>
<td>COMMIT, 1988 baseline and 1993 follow-up (8271)</td>
<td>compared smoke-free (SF), designated areas (DA) versus not smoke-free (NSF)</td>
<td>NA</td>
<td></td>
</tr>
<tr>
<td>Farkas et al. (1999)</td>
<td>logistic regressions (weighted) with home/workplace restrictions</td>
<td>Current Population Survey, 1993 (48 584)</td>
<td>compared smoke-free (SF), work area ban (WAB) versus not smoke-free (NSF)</td>
<td>(more likely to smoke &gt;15 cigarettes per day) SF: OR $= 1.53$ WAB: OR $= 1.10$ (NS)</td>
<td>QA in last year: SF: OR $= 1.04$ (NS), WAB: OR $= 1.14$; QR 6+ month: SF: OR $= 13.4%$, OR $= 1.21$; WAB: OR $= 9.7%$, OR $= 0.93$ NS</td>
</tr>
<tr>
<td>Biener and Nyman (1999)</td>
<td>logistic and multiple regression</td>
<td>Massachusetts Tobacco Survey, 1993 baseline and 1996 follow-up (369)</td>
<td>compared continuously smoke-free, became smoke-free and not smoke-free</td>
<td>NA</td>
<td>QA at 3 years: NSF: OR $= 1$; NS, except in workplaces with low ETS exposure.</td>
</tr>
<tr>
<td>Evans et al. (2000)</td>
<td>normalized probit or OLS estimate</td>
<td>Current Population Survey, 1993 (97 882)</td>
<td>compared work area bans (WAB) versus not smoke-free (NSF)</td>
<td>WAB: $-14%$ mean difference $-2.0$ cigarettes</td>
<td>WAB: $-15%$, mean difference $-0.048$</td>
</tr>
<tr>
<td>Evans et al. (2000)</td>
<td>normalized probit and OLS and two-stage least squares</td>
<td>National Health Interview Survey, 1991 (9704) and 1993 (8386)</td>
<td>compared work area ban (WAB) versus not smoke-free (NSF)</td>
<td>WAB: $-12%$ mean difference $-2.5$ cigarettes</td>
<td>WAB: $-19%$ mean difference $-0.057$</td>
</tr>
<tr>
<td>Farrelly et al. (1999)</td>
<td>normalized probit and OLS</td>
<td>Current Population Survey, 1993 (97 882)</td>
<td>compared smoke-free (SF), work area ban and restricted common area (WARCA), partial work area and common area (PWACA) versus no ban</td>
<td>absolute change: SF: $-0.057$; WARCA: $-0.026$; PWACA: 0.005 cigarettes</td>
<td></td>
</tr>
</tbody>
</table>

$^{a}$Sample sizes are approximate.
representative of the population impact of smoking bans than studies that select one or a limited number of firms. In addition, the surveys were not conducted at the workplace, thereby reducing the likelihood of workers only providing socially desirable responses.

Apart from having a narrower sample, most of the better firm level case studies discussed in the previous section examined smoking behaviors over a particular period of time, such as a year after a ban was implemented. By contrast, most population studies compared smoking behavior at a point in time in those firms with smoking restrictions to those without restrictions, without distinguishing when the restrictions were adopted. The latter investigations did not control for how long the smoking restrictions has been in effect, but tended to show more clearly the long-term impact of smoking restrictions, since the bans have been in effect in most firms for more than one year.

Population-based worksite studies usually employed multivariate regression analysis to control for other factors affecting the relationship between the bans and changes in smoking behaviors. Nevertheless, because they did not directly examine changes in behavior over time, their reliability in isolating the role of smoking restrictions depends on the ability to control for confounding factors that may explain differences across firms.

We chose to present general results of population-based worksite studies, with greater detail provided in Table II. Consistent with the findings reported for individual worksite restrictions, population-based bans were also associated with lower daily quantity smoked among smokers who continued to smoke. Various authors (Woodruff et al., 1993; Glasgow et al., 1997; Evans et al., 1999; Farkas et al., 1999) reported between 7 and 15% fewer cigarettes smoked per smoker. Patten et al. (Patten et al., 1995) obtained decreases of similar magnitudes in quantity smoked, but the results were not statistically significant. Larger effects (34%) were indicated by Kinne et al. (Kinne et al., 1993), but they only examined mean differences in smoking rates and did not control for other confounding factors.
### Table III. Studies of effects of clean air laws on smoking behavior, combining all age groups

<table>
<thead>
<tr>
<th>Study and year</th>
<th>Methods and outcome variable</th>
<th>Sample&lt;sup&gt;a&lt;/sup&gt;</th>
<th>Measure of clean air laws</th>
<th>Results&lt;sup&gt;b&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Emont et al. (1992)</td>
<td>means and ordered alternatives, regression analysis; consumption, prevalence and quits (ratio of former to ever smokers)</td>
<td>1989 Current Population Survey for prevalence and quits; 1989 Tobacco Institute for consumption by state</td>
<td>state-level restrictiveness index</td>
<td>means (extensive laws versus others): −14% prevalence rates; −12% per capita consumption; +12% quits; significant effects for moderate and extensive laws on quits.</td>
</tr>
<tr>
<td>Chaloupka and Saffer (1992)</td>
<td>linear, single and simultaneous equation; consumption</td>
<td>1975–85, Tobacco Institute, by state and year</td>
<td>two state-level indicator variables: public use (PU: 4 or more places), worksite (WS)</td>
<td>single equation: −4 to −8% PU and −8% WS; simultaneous equation: −20% PU; simultaneity bias found for WS laws significant effects at basic and moderate levels but no additional effect at extensive level; greater effect on men than women</td>
</tr>
<tr>
<td>Chaloupka (1992)</td>
<td>rational addiction regression model; consumption</td>
<td>1976–1980 NHANES II, individual</td>
<td>four 0–1 state-level indicators; variables based on USDHHS</td>
<td>significant effects at basic and moderate levels but no additional effect at extensive level; greater effect on men than women</td>
</tr>
<tr>
<td>Keeler et al. (1993)</td>
<td>time series regression; consumption</td>
<td>1980–90 California Tobacco Survey, monthly, by community</td>
<td>restrictiveness index for local anti-smoking ordinances only</td>
<td>significant effect of smoking restrictions, except when time trend included −4.7% in 1995 due to existing laws, similar effects to a 10% price increase</td>
</tr>
<tr>
<td>Yurekli and Zhang (2000)</td>
<td>multiple regression; consumption</td>
<td>1970–95 Tobacco Institute annual consumption data by state</td>
<td>state-level restrictiveness index, weighted by time in effect and type</td>
<td>odds ratios: weak laws: 0.99; NS moderate laws: 1.38; NS strong laws: 1.61; similar results over various socio-demographic groups</td>
</tr>
<tr>
<td>Moskowitz et al. (2000)</td>
<td>logistic regression; quit in the last 6 months</td>
<td>1990 California Tobacco Survey, by community</td>
<td>local ordinance: compared no law, weak, moderate and strong</td>
<td></td>
</tr>
</tbody>
</table>

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<sup>a</sup>Sample is national unless otherwise indicated.

<sup>b</sup>Results are in terms of percent change where able to determine.

NS = not significant (other results are significant). Restrictiveness index: 1.0 if private workplaces, 0.75 if restaurant restrictions (75%), 0.5 if four or more areas, 0.25 if one to three areas. WP = workplace. PU = public use laws. Note: consumption refers to cigarettes per capita, quantity refers to cigarettes per smoker and prevalence refers to smoker status.
Workers in firms with worksite bans were also found to have lower prevalence rates than workers in firms without bans. Evans et al. (Evans et al., 1999) and Farrelly et al. (Farrelly et al., 1999) found 15–20% lower prevalence rates, with absolute differences of 5 percentage points. Greater differences were indicated by Woodruff et al. (Woodruff et al., 1993) and Kinne et al. (Kinne et al., 1993), but these studies did not control for confounding factors that may have also contributed to smoking reductions.

Workers in firms with worksite bans have also generally found higher rates of quit attempts and successful quits lasting at least 3 months. Burns et al. (Burns et al., 2001), Farkas et al. (Farkas et al., 1999) and Glasgow et al. (Glasgow et al., 1997) reported that cessation rates were about 10–15% higher in firms with bans. Some studies, however, reported little or no significant effects (Burns et al., 2001; Patten et al., 1995). Biener and Nyman (Biener and Nyman, 1999) found relatively large, but insignificant, effects after 3 years.

Population-based studies also considered the impact of different levels of smoking restrictions. In general, partial restrictions appeared to have little or no effect on smoking behaviors, whereas larger effects were observed for more extensive restrictions (Glasgow et al., 1997; Farrelly et al., 1999). These findings suggest that the ability to circumvent a smoking ban will be an important consideration in determining a ban’s impact.

In summary, population-based studies have generally found that firms with strong smoking restrictions reported a 10–15% reduction in quantity consumed and 15–20% decrease in smoking prevalence relative to businesses with minimal or no restrictions. Most studies also reported at least 10–15% higher cessation rates in firms that implemented total smoking bans. In addition, larger effects were found for those who worked longer hours, suggesting a dose–response relationship (Evans et al., 1999).

Discrepancies among study results may be accounted for in part by the fact that some studies did not control for confounding factors, such as smoker sociodemographic characteristics [e.g. (Kinne et al., 1993; Woodruff et al., 1993)]. In addition, studies may need to consider the health habits of the workers being examined to account for the possibility that those firms with smoking bans may be those where the workers were generally more health conscious (Evans et al., 1999). Nevertheless, the results of the population-based studies are generally consistent with and complement the studies of individual firms. Moreover, by providing estimates of population impacts over longer periods of time, they increase our confidence in our overall estimates of effects. Interestingly, the effects on prevalence rates are somewhat higher and for quantity consumed per continuing smoker are lower for the population-based studies than the studies of individual firms.

The effects of public laws on smoking behaviors

Eighteen studies were found that directly examined the effects of public clean air laws on population smoking rates. Table III includes studies that combine all ages, whereas Table IV includes studies for particular age groups. All of the studies in Tables III and IV employed some form of multivariate analysis, in which they controlled for the effects of other tobacco control policies, such as taxes, and for other factors, such as smokers’ sociodemographic characteristics.

Studies of public clean air laws either individually categorized different clean air policies or combined the laws into a single index. In the latter case, an index of restrictiveness was used, based on a classification scheme proposed by the Surgeon General (Centers for Disease Control and Prevention, Office on Smoking and Health, 1986, 1989). Typically, the index was calibrated on a 0–1 scale, in which 0 indicated no restrictions, 0.25 represented basic restrictions (e.g. one to three designated smoking areas locations, excluding private worksites and restaurants), 0.5 represented nominal restrictions (e.g. four or more designated areas, excluding private worksites and restaurants), 0.75 represented a moderate policy (e.g. restaurant but no private worksite restrictions) and 1.0
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aSample is by individual and national unless otherwise indicated.
bResults in terms of percent change where able to determine.

NS = not significant (other results are significant), RI = restrictiveness index: 1.0 if private workplaces, 0.75 if restaurant restrictions, 0.5 if four or more other public areas, 0.25 if one to three other public areas. WP = workplace. PU = public use laws. Note: consumption refers to cigarettes per capita, quantity refers to cigarettes per smoker and prevalence refers to smoker status.
indicated extensive clean air laws (e.g. laws that included private worksite and other restrictions).

Most studies examined the effect of clean air laws measured in terms of cigarettes smoked per capita (including non-smokers). Results from Emont et al. (Emont et al., 1992) indicated that states with extensive clean air laws had 12% lower per capita consumption rates than other states. After accounting for smoking sentiment, Chaloupka and Saffer (Chaloupka and Saffer, 1992) found about a 20% reduction in per capita consumption from public clean air laws, compared to 4–8% effects without such controls. They found, however, that the effect of worksite laws fell from an 8% reduction to statistically non-significant effects when social attitudes regarding smoking were taken into account. They also reported that basic and moderate levels of restrictions were sufficient to yield significant effects, whereas Emont et al. (Emont et al., 1992) found that moderate or even extensive restrictions were necessary. Wasserman et al. (Wasserman et al., 1991) estimated that expanding smoking restrictions from minimal to extensive policies would reduce overall per capita smoking by almost 6%. Yurekli and Zhang (Yurekli and Zhang, 2000) used a modified restrictiveness index, in which worksite laws and laws that had been in effect longer were more heavily weighted. They estimated a 5% reduction in smoking due to the clean air laws that were in effect in 1995. Upon extrapolating to all states implementing extensive laws, their results implied that consumption would be reduced by about 10% in changing from no laws to extensive laws. [These calculations were derived by multiplying the maximum value of the restrictiveness index (3) by the coefficient on the restrictiveness index.]

Few investigations, except for studies of youth smoking, examined the effects of clean air laws on smoking prevalence. Results from Emont et al. (Emont et al., 1992) indicated that states with extensive clean air laws had mean prevalence rates that were 14% lower than other states. Consistent with this result, Ohsfeldt et al. (Ohsfeldt et al., 1998) found that extensive laws, relative to minimal laws, were associated with a 13% lower smoking rate for all males, with a smaller effect for females. Among different age groups, the largest effects were found for those aged 25–44. Like the Chaloupka and Saffer (Chaloupka and Saffer, 1992) investigation described above, Ohsfeldt et al. (Ohsfeldt et al., 1998) also considered smoking sentiment, but generally found greater reductions in smoking when attitudes toward smoking sentiment were taken into account.

Two studies explored the effects of clean air laws on quit rates. Results from Emont et al. (Emont et al., 1992) indicated that states with extensive clean air laws had 12% higher mean quit (former current) rates than other states. Moskowitz et al. (Moskowitz et al., 2000) reported a 38% higher 6-month cessation rate in areas with a strong local ordinance. Similar results were obtained for different sociodemographic groups and for 5-year quit rates.

**Impact on youth smoking**

While restrictions by private firms affect the workers who are generally above age 18, clean air laws may also affect youth. Recent studies of youth and young adult cigarette demand have also shown that clean air laws reduced smoking among these groups. Wasserman et al. (Wasserman et al., 1991) found that increasing state restrictions to the most comprehensive level reduced youth per consumption per smoker by over 40%. However, their measure of clean air laws was highly correlated with price, rendering this result questionable. Chaloupka and Grossman (Chaloupka and Grossman, 1996) found that restrictions on smoking in schools resulted in decreased quantity smoked by young smokers, but the effects of other clean air laws were often insignificant. Using a broader range of smoking restrictions (e.g. self-reported home restrictions) and enforcement variables, Wakefield et al. (Wakefield et al., 2000) found more consistent effects of clean air policies on youth quantity smoked, including relatively consistent effects for reducing progression to established smoking. Tauras and Chaloupka (Tauras and Chaloupka, 1999) also found rela-
tively consistent effects of clean air laws for older youth and young adults.

Several studies have found that the effects of clean air laws on youth varied by demographic subgroup. Lewitt et al. (Lewitt et al., 1997) found that only males reduced their smoking rates in response to clean air policies, and Chaloupka and Pacula (Chaloupka and Pacula, 1999) reported that only males and whites were influenced. Chaloupka and Wechsler (Chaloupka and Wechsler, 1997) found almost significant effects for college aged youth.

Discrepant results have been reported for school restrictions. Chaloupka and Grossman (Chaloupka and Grossman, 1996) did not find effects on use, suggesting that school clean air policies alone might be insufficient to combat youth smoking. In contrast, Wakefield et al. (Wakefield et al., 2000), using refined statistical techniques and more recent data, obtained significant effects of school restrictions on smoking rates.

**Prospective case studies**

Unlike the studies cited above, which used multivariate regression analyses, Pentz et al. (Pentz et al., 1989) and Paulozzi et al. (Paulozzi et al., 1992) used a prospective case study approach. Pentz et al. compared smoking rates in schools with strict bans to schools with only smoking education programs. After 17 months, they found that those schools with comprehensive restrictions and smoking education programs were more effective in reducing smoking behavior, especially quantity smoked among seventh graders than schools with education programs only. Paulozzi et al. considered a Vermont worksite smoking law. Sixteen months after the ban, the number of cigarettes and the percent of adults smoking declined about 30% at work and about 20% at home.

**Summary of the research on clean air laws**

In summary, relatively extensive clean air laws were generally found to be associated with lower smoking consumption and prevalence rates, and higher cessation rates. Studies found that overall consumption was reduced in the 4–20% range. The results also suggested that workplace clean air laws may have their greatest impact on men, especially those between the ages of 25 and 44, who are more heavily represented in the workforce than women. In general, studies found that clean air laws yield reduced smoking behaviors among youth, but provided little guidance on the magnitude of effects.

In evaluating the studies of clean air laws, several methodological issues merit attention. First, these studies often did not control for the length of time that a law was in place. In addition, most studies used a single index for all laws, due to the difficulty in untangling the effects of different clean air laws. Related to the previous point, many studies did not consider the restrictiveness of the law (e.g. whether it applied to only designated areas). These methodological problems may limit the validity of our estimates of the laws’ impact. In addition, all of the studies except Yurekli and Zhang (Yurekli and Zhang, 2000) examined state laws in effect before 1994, when strict laws were unlikely to have yet been implemented (National Cancer Institute, 2000). The impact of the more extensive laws currently in place and the role of additional private restrictions remain to be determined.

A potential problem in determining the specific effect of clean air laws is that the states with strongest anti-tobacco attitudes may be more likely to enact and enforce clean air policies. Thus, it is difficult to distinguish what part of the reduction in smoking rates is attributable to the laws and what is attributable to attitudinal changes. Anti-smoking sentiment may result in increased compliance with the law [(Levy and Friend, 2001; Rigotti et al., 1992, 1993, 1994; Pierce, 1994), but see (Moskowitz et al., 1999)], suggesting that one could not exist without the other and that attempting to determine their individual unique role may be difficult.

If the government is held responsible for enforcement, the extent of compliance may depend on the specific government agency required to
carry out this task. Local laws may be accompanied by greater community support, which may increase compliance with the laws and support norms against smoking. Enforcement efforts may, however, be more efficient at the state level. In particular, states laws reduce the ability of workers to avoid more stringent laws in neighboring communities by changing jobs. Almost all of the studies examined only state laws. Studies are needed to determine the effects of local versus state laws on compliance. In the absence of government enforcement, voluntary compliance may be high if there is sufficient public support for the law, as indicated by recent studies of compliance with non-worksite laws (Jacobson and Wasserman, 1997; Goldstein and Sobel, 1998; Hyland et al., 1999).

Discussion

Overall summary

A multitude of studies using different methodologies have found that smoking restrictions, whether imposed by public laws or private firms, reduced the quantity smoked per smoker and smoking prevalence. Studies of clean air laws indicated reductions of between 5 and 20% in rates of smoking per capita over the entire population. A rough estimate of the impact of changing from minimal clean air laws (three or fewer areas, excluding restaurants or worksites) to extensive laws (including worksite and restaurant bans) is a 10% reduction in prevalence rates. Thus, this decrease implies that the current US prevalence estimate of 22.8% (Centers for Disease Control and Prevention 2002) would drop to 20.5% once the laws had been in place for several years. Similar effects were found for studies of total cigarettes consumed. Ohsfeldt et al. (Ohsfeldt et al., 1998) explicitly distinguished worksite laws from other clean air laws and found that they explained about half of the effect of clean air laws.

The research on individual firms found that private workplace restrictions reduced smoking rates, with the initial effects focusing mostly on quantity smoked per smoker and later effects on use rates. These studies obtained stronger evidence on quantity smoked than on prevalence rates or quitting, but cross-sectional studies yielded quite consistent effects on smoking prevalence. Population-based studies indicated effects closer to the 20% estimate for prevalence rates and 5–20% for quantity smoked per smoker.

The results from studies of private work restrictions are consistent with those of clean air laws. In comparing results, we take into account that that only 67% of workers worked indoors (Evans et al., 2000) and that 63% of the population worked (Department of Commerce, 1997). Thus, the effects are spread over 42% of the population. The 10–20% reductions in smoker prevalence by workers would imply between 4 and 8% reduction for the overall adult smoking population, which is consistent with the effects found in studies of clean air worksite laws. Reductions in quantity smoked per smoker would be between 2 and 8%, depending on the length of time after the ban was implemented. These effects appear to erode over time, as those who most reduce their quantity may quit and are no longer represented as smokers with reduced quantities smoked.

We have not included studies from other countries in our estimates of effects of smoking restrictions and clean air laws because other countries may differ in important ways from the US in their smoking policies and practices. Nonetheless, these studies generally provide similar results as those reported above. Three Australian studies, a prospective cohort study by Borland et al. (Borland et al., 1990), a prospective cross-sectional study by Borland et al. (Borland et al., 1991) and a population study by Wakefield et al. (Wakefield et al., 1992), all found a 20–25% reduction in the number of cigarettes smoked per smoker. The two studies by Borland et al. (Borland et al., 1990, 1991) reported between 5 and 10% lower prevalence rate in worksites with a ban. Etter et al. (Etter et al., 1999) found increased numbers of quit attempts, but no difference in the number of cigarettes smoked or prevalence rates, after 4 months in a retrospective cohort study for a Swiss
university. By contrast, a population study conducted by Brenner and Fleischle (Brenner and Fleischle, 1994) for Germany found 36% fewer cigarettes smoked per smoker and a 7% lower prevalence rate in firms with smoking bans. A Canadian cohort prospective study by Broder et al. (Broder et al., 1993) found an 11% reduction in the number of cigarettes smoked, but no change in smoking rates. Two studies of clean air laws were found for countries other than the US. In Canada, Stephens et al. (Stephens et al., 1997) found that the odds ratio for being a smoker was 1.21 where clean air laws covered a smaller percent of the population, compared to an odds ratio of 1.26 where cigarettes were relatively inexpensive, suggesting that stricter clean air laws are nearly as effective as large price differences in reducing smoking rates. Stephens et al. (Stephens et al., 2001) found that the restrictiveness of municipal bylaws limiting public smoking was positively associated with the odds of being a non-smoker and negatively with amount smoked for women but not men. Following implementation of a national smoke-free law in Finland, Heloma et al. (Heloma et al., 2001) found declines in smoking prevalence of 17% and a reduction in the number of cigarettes smoked per smoker of 16% in firms previously without bans.

**Suggestions for future research**

While there is a strong basis for claiming that smoking restrictions yielded reductions in smoking behaviors, our ability to determine which specific aspects of clean air laws were responsible for changes in smoking rates is subject to the limitations indicated above. In particular, the influence of social norms merits additional study. Further attention is also needed to determine how the effects of clean air laws unfold over time. While smoking restrictions implemented by private firms appear to first affect the quantity of cigarettes smoked per smoker and later lead to cessation, knowledge about the interaction between reductions in quantity smoked and future quits is limited (Hughes, 2000). Attention also needs to be directed at developing rigorous measures of quit rates and determining the effects of clean air laws on future cessation.

Another limitation of the literature is that studies with null findings may not be published (‘the file drawer problem’). The possibility of publication bias suggests that the actual effects are lower than that which would be indicated by the empirical literature. We have, however, found similar relatively similar results across a diverse set of studies and methodologies, and, in particular, for population studies where observations are randomly chosen over the population.

The effect of newly implemented clean air laws may depend on which firms already have strong workplace restrictions (Levy and Friend, 2001). Sorensen et al. (Sorensen et al., 1996) reported that non-manufacturing firms and firms with more female workers were more likely to adopt worksite restrictions. In general, those workplaces with more smoking restrictions may be those that have the fewest smokers, the most health conscious workers or those that originally had the greatest ETS problem (Woodruff et al., 1993; Evans et al., 1999). Over time, smokers may be drawn to work in firms with the fewest restrictions.

The issue of compliance with worksite restrictions and clean air laws also merits further study. Compliance with new clean air laws may be more difficult in those firms that have not already voluntarily enacted bans, while the implementation of clean air laws may increase compliance rates in firms that had previously implemented worksite restrictions.

At the same time, studies of private workplace restrictions typically have not considered how the impact of those restrictions may depend on the state local laws governing their areas. Biener and Nyman (Biener and Nyman, 1999) found that many firms do not enforce their own policies. Smokers in firms that already have restrictions may reduce their smoking rates if new laws increase compliance with individual firm restrictions or the difficulty of workers switching jobs in order to work at a firm that allows smoking.

Thus, previous studies of clean air laws have not considered the effect of private restrictions already
in place and population studies of private restrictions have not considered the effect of clean air laws. The rate of firms with smoking restrictions and the extent of clean air laws varies both in the US and across the globe. Additional investigations are needed that examine the inter-relationship between clean air laws and private smoking restrictions already in place. In those countries without norms against smoking, voluntary firm restrictions may be less likely and clean air laws may be more difficult to enforce.

The effect of both private restrictions and clean air laws may be enhanced by other tobacco control policies already in place. While some studies of clean air laws control for taxes, none specifically consider interactive effects of other policies with clean air laws. For example, Farkas et al. (Farkas et al., 1999) found large effects of voluntary home smoking bans on smoking rates. These bans may reinforce the effect of clean air laws and workplace restrictions, although the ability to distinguish the effect of worksite bans from home smoking bans may be difficult if the imposition of home bans and firms bans are inter-related. Other policies may intensify the effects of clean air laws if they reinforce anti-smoking norms, such as media publicity to generate support for and continued compliance with the law.

Additional research is also needed on how smoking restrictions influence different sociodemographic and smoker groups. Clean air laws are likely to be particularly effective at reducing smoking by those in the workplace. With substantial interest directed at those below the age of 18, the effects of public policy on those aged 18–24 becomes more important as these individuals become both legal purchasers of cigarettes and full-time workers. The effect of college bans also merits consideration. In addition, future work is warranted that explores the effects of laws on heavy versus light smokers.

In conclusion, clean air policies have a direct effect on smokers. While studies of private firms and of public laws both indicate that smoking restrictions affect smoking behavior, further information is needed on the interactions between private and publicly imposed restrictions and the time pattern of effects. Clean air laws have a well-defined place in comprehensive tobacco control programs (Centers for Disease Control and Prevention, 1999; Department of Health and Human Services, 2000), but better information is needed on how their effects compare with other policies. Knowledge of the effects of clean air laws, alone and in combination with other tobacco control policies, and how they affect different sociodemographic groups, will be important in developing comprehensive strategies to reduce smoking rates.

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Effects of clean indoor air laws


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