The Impact of Smoke-Free Laws on Asthma Discharges: A Multistate Analysis

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Objectives. This is the first, to my knowledge, multistate, county-level analysis of Healthcare Cost and Utilization Project state inpatient data to examine the relationship between smoke-free laws and asthma discharges.

Methods. I used treatment and control groups to examine the effects of state and county or city smoke-free laws separately and together (2002–2009). I compared quarterly county-level discharge rates before and after the implementation of 12 state smoke-free laws, accounting for counties with preexisting county or city smoke-free laws and using the data from 5 states without state smoke-free laws as a control group. I used difference-in-differences models, controlling for year and state fixed effects, state cigarette taxes, seasonality, and numerous county-level factors.

Results. I observed statistically significant reductions in asthma discharges after the implementation of county smoke-free laws but no statistically significant effect of state laws besides the effect of county laws or of state laws alone. There was also no statistically significant effect of any smoke-free law on appendicitis discharges.

Conclusions. It may be unwise to pursue state smoke-free laws where they have yet to pass; rather, efforts might be better focused at the local level, where there is evidence of a significant impact. (*Am J Public Health.* 2014;104:e74–e79. doi:10.2105/AJPH.2013.301697)

The implementation of smoke-free laws is one of many interventions available to reduce exposure to secondhand smoke (SHS). Evidence has built over the past 15 years for an association of exposure to SHS with a variety of health conditions. The connection between exposure to SHS and heart attack is perhaps the most studied, with at least 29 articles having been published since 1999.[1–29] Although most of these studies found some relationship between exposure to SHS and heart attack, most suffered from similar limitations: the study of single sites (e.g., cities, counties, states, and countries), the lack of control groups, and the inability to separate the effects of state from county smoke-free laws.

The relationship between exposure to SHS and asthma discharges has been studied less, and the results are more recent. In 2008, Rayens et al. studied Lexington-Fayette County, Kentucky, emergency department visits for asthma in 4 of 5 area hospitals before and after the implementation of a county smoking ban covering all public places except workplaces.[30] Asthma emergency department visits declined 22% from prelaw to postlaw, and the decline was greater for adults than for children. The authors cited possible underestimation of asthma cases because of worker migration into and out of Lexington, differences in coding by physicians and hospitals, and the lack of a matched control group as potential study limitations.

In 2010, Mackay et al. studied the Scotland smoke-free law and analyzed asthma admissions for children younger than aged 15 years using data from the Scottish Morbidity Record and death certificate data.[31] They found there was an 18.2% per year reduction in admissions for asthma relative to the rate before the law's implementation. The authors could not determine if the results were partially attributable to reductions in school-age smoking, reductions of exposure to SHS in the home, or reductions of exposure to SHS in public places. The authors also could not rule out any additional interventions that might have occurred during the same period and affected their results.

Moraros et al.[21] studied the effects of the 2002 Delaware smoking ban on heart attack and asthma in a 2010 study using Delaware hospital discharge data. The risk ratio for asthma in Delaware postban was 0.95 for residents versus 1.62 for nonresidents. The authors did not include law data from surrounding states because of data limitations, so they could not compare their results with neighboring states.

Also in 2010, Naiman et al. studied the effect of the Toronto, Ontario, smoking ban 3 years before the first phase of implementation and 2 years after the ban was fully implemented.[22] There was no significant reduction in asthma after the implementation of the ban's first phase, which affected public spaces and workplaces; however, admissions for respiratory conditions decreased 33% over 3 years after the second phase, affecting restaurants, went into effect. There was no significant reduction in asthma admissions after the third phase, affecting bars and pool halls, was implemented.

In 2011, Herman and Walsh examined hospital discharge data to study the effect of Arizona's statewide smoking ban on acute myocardial infarction, angina, stroke, asthma, and 4 conditions not expected to be affected by the ban.[32] The authors used Hill's guidelines for causality to attempt to establish temporality, strength, dose–response, and biologic plausibility. The authors found a 4% reduction in admissions for asthma in counties that already had a county ban in place and a 22% reduction in admissions in counties without a previous ban after the state ban was implemented. They cited the lack of county data from neighboring states as a potential limitation. Without such comparisons, the authors conceded that the reductions in admissions could have been attributable to some other factor. The authors also cited the lack of generalizability inherent in a single-state study.

Shetty et al.[33] examined the Healthcare Cost and Utilization Project (HCUP) Nationwide

Inpatient Sample, Medicare claims, and the Multiple Cause of Death database to study changes in hospitalization and mortality rates for various causes, including asthma, following smoking bans. They found significant reductions in asthma admissions for working-age adults after the enactment of workplace bans, significant increases for children, and no reductions for adults older than 65 years. They suggested this might indicate an increase in in-home smoking after the enactment of workplace bans, but they highlighted that the statistical significance disappeared after they adjusted for multiple comparisons. The authors also found significant reductions in asthma admissions for working-age adults after any type of ban was implemented, but they did not separate the effect of county versus state bans on asthma admissions.

The most recent countrywide study of smoke-free laws and asthma examined childhood asthma emergency admissions in England after the implementation of a nationwide public indoor smoking ban.[34] The authors reported a significant reduction in child asthma admissions after the law was implemented; however, they also reported limitations similar to previous studies: lack of a control group, the inability to account for external influences other than the law, and the possibility of bias because of diagnostic coding changes over time.

In summary, although the associations of the harmful effects of smoking and exposure to SHS with heart attack have been studied for quite some time, the literature on the effect of exposure to SHS on asthma is still developing. Common study limitations have included the lack of generalizability because of single-site studies, the lack of control groups, and, with the exception of the Herman and Walsh study, the inability to separate the effects of state versus county smoke-free laws.

This study is the first, to my knowledge, multistate, county-level analysis of HCUP state inpatient data (SID) to examine the relationship between smoke-free laws and asthma discharges. On the one hand, the effect of a state smoke-free law in a county that also has a county or city law might be relatively small because the additional reduction in exposure to SHS would likely be small. This would be particularly true in those counties or cities where individuals tend to live, work, and travel within 1 county or city. On the other hand, in areas with a significant level of cross-county or cross-city activity and where counties or cities do not have smoke-free laws, a county or city smoke-free law might have very little independent effect on exposure to SHS, and the effect of state smoke-free laws could be larger. This study builds on the existing body of literature by improving generalizability with 17 states' data (accounting for approximately 35% of the US population over the study period), by including states without state smoke-free laws as a control group, and by examining the effects of state and county or city smoke-free laws on asthma discharges separately and together.

METHODS

I employed a pre–post, nonequivalent control group design and used difference-in-differences models[35] to estimate the change in quarterly county rates of asthma discharges before and after the implementation of state smoke-free laws. I analyzed 12 quarters of data for the years before, during, and after a state law was implemented. I drew data from 2 main sources: the HCUP SID from 2002 to 2009 and the American Nonsmokers' Rights Foundation Smoke-Free Laws database. The HCUP SID contain inpatient discharges from participating states, translated into a uniform format, to facilitate multistate comparisons and analyses.[36] The American Nonsmokers' Rights Foundation is a nonprofit organization that publishes lists of cities, counties, and states that have implemented smoke-free laws. The smoke-free law data examined in this study were current as of April 2011.

The selection of states depended on the date each state implemented its smoke-free law and the availability of each state's HCUP data. Of the 35 states that had some type of smoke-free law as of April 2011, 32 participated in HCUP and 23 participated in HCUP's standardized data program. Nine states' smoke-free laws were implemented too far in the past or too recently, so there were no HCUP data available. Furthermore, Massachusetts and Nevada do not report patient data by county of residence, so I omitted those states' data. The remaining 12 states served as the treatment states. I analyzed 12 quarters of data for each state to account for the years before, during, and after the state smoke-free law was implemented.

Six states participated in the standardized data program but had not enacted state smoke-free laws by April 2011, or data were unavailable to analyze the law's postimplementation period. West Virginia does not report patient data by county of residence, so I omitted it. The remaining 5 states served as control states. The combined population of treatment and control states was more than 103 000 000 individuals, or about 35% of the US population in 2005. Treatment and control states, with dates of law implementation, are listed in Table 1.

The dependent variable was the quarterly county rate of asthma discharges per 10 000 children or working-age adults in county c at time t, where patient residence defined county. It is reasonable to assume patients may be more affected by a clean indoor air law in their county of residence than in the treating hospital's county, especially in counties with little cross-county travel. I also tested models with quarterly county rates of appendicitis discharges per 10 000 to assess plausibility.[32] Appendicitis discharge rates should not be affected by the implementation of smoke-free

TABLE 1—Treatment and Control States: Healthcare Cost and Utilization Project, 2002–2009; American Nonsmokers' Rights Foundation, 2011

State	Treatment or Control	Law Implementation Date
Arizona	T	May 1, 2007
Colorado	T	July 1, 2006
Florida	T	July 1, 2003
Hawaii	T	November 16, 2006
Iowa	T	July 1, 2008
Maryland	T	February 1, 2008
New Jersey	T	April 15, 2006
New York	T	July 24, 2003
Rhode Island	T	May 4, 2005
Utah	T	May 1, 2006
Vermont	T	September 1, 2005
Washington	T	December 8, 2005
Arkansas	C	NA
Kentucky	C	NA
Michigan	C	NA
South Carolina	C	NA
Wisconsin	C	NA

Note. C = control; NA = not available; T = treatment.

laws. A dummy variable indicated the presence or absence of a state smoke-free law in county c at time t. A second dummy variable indicated the presence or absence of a county law in county c at time t. An interaction term tested whether there were reductions in adult or child asthma discharges besides the effect of county laws after the implementation of state laws. A cigarette tax variable adjusted for annual state cigarette taxes, year and state dummy variables controlled for underlying year and state fixed effects, seasonal dummies controlled for seasonality, and a vector of county characteristics (smoking and asthma prevalence, urban residence, percentage living in poverty, percentage uninsured, whether there was a hospital or teaching hospital in the county, primary care physicians per 10 000 residents, and percentage of the population who were non-White) controlled for factors that might also affect asthma discharges.

I classified states and counties as having a smoke-free law only if the law was categorized as 100% smoke-free. Where a city law was present without a corresponding county law, I labeled the county as having a smoke-free law. I did this to not overstate the potential effect of state laws, and it occurred in 31 of 840 counties within the study's time frame. From this point forward, I have referred to these 31 city laws as county laws. Both adult and child models included age-adjusted population weights to account for differences in county population not otherwise captured by the independent variables.[37,38] I used the Stata ROBUST CLUSTER option to adjust for potential serial correlation and heteroskedasticity.[39,40] I assessed overall model fit using the Fisher *F* test. I measured statistical significance using the Student *t* test at the .05 level.

As in the Herman and Walsh study, I used Hill's guidelines as a framework to assess causality.[41] The guidelines are widely used by epidemiologists[42] to indicate causal relationships, including in the Surgeon General's 2006 report on the health consequences of exposure to SHS.

RESULTS

Descriptive statistics for the continuous variables are presented in Table 2 and were in line with national figures. The mean child asthma discharge rate per 10 000 was 9 per quarter, whereas the working-age adult rate was almost 14 per quarter. The county rates ranged as high as 144 per 10 000 for children and 89 per 10 000 for adults. Appendicitis discharge rates were similar for children and adults, but the adult rate ranged up to 182 per 10 000 per quarter in 1 county. The average cigarette tax was 82 cents per pack. On average, 22% of county residents smoked in the study years, and just more than 8% had asthma. The percentage of the county population living in poverty and the percentage uninsured ranged widely. Some counties had no primary care physician, but the mean was just more than 6 per 10 000. Some counties had no racial diversity, whereas 1 county had a non-White percentage of 86%.

TABLE 2—Descriptive Statistics for Continuous Study Variables: Healthcare Cost and Utilization Project, United States, 2002–2009

Variables	Mean (SD)	Minimum	Maximum
Dependent, rate			
Child asthma	9.02 (9.66)	0.00	144.47
Adult asthma	13.95 (7.52)	0.00	88.93
Child appendicitis	2.30 (2.42)	0.00	57.14
Adult appendicitis	2.26 (2.45)	0.00	181.82
Independent			
Cigarette tax	0.82 (0.63)	0.03	2.58
Smoking prevalence	21.84 (4.61)	9.80	32.60
Asthma prevalence	8.24 (1.73)	6.10	40.50
In poverty, %	13.70 (5.73)	0.00	43.80
Uninsured, %	15.73 (4.96)	5.48	39.50
PCP/10 000	6.33 (4.15)	0.00	31.40
Non-White, %	11.00 (14.00)	0.00	86.00

Note. PCP = primary care physicians.

Bivariate results are presented in Table 3. Adult asthma discharge rates were most strongly associated with being non-White (0.26; $P<.001$), living in poverty (0.19; $P<.001$), and the rate of primary care physicians in the county of residence (0.16; $P<.001$). Child asthma discharges were most strongly associated with poverty (0.33; $P<.001$), smoking prevalence (0.24; $P<.001$), and a state's cigarette tax (−0.18; $P<.001$). I included all these variables as control variables in the multivariate models.

The multivariate results are presented in Table 4. There was a statistically significant relationship ($b=-2.44$; $P<.05$) between the implementation of county laws and reductions in working-age adult asthma discharges. However, there was no statistically significant effect of state smoke-free laws on working-age adult asthma discharges besides the effect of county laws. There was also a statistically significant relationship between the implementation of county smoke-free laws and reductions in child asthma discharges ($b=-1.32$; $P<.05$), but there was no statistically significant effect of state laws on child asthma discharges besides the effect of county laws. In both appendicitis models (child and working-age adult), there was no statistically significant relationship between the implementation of state smoke-free laws and appendicitis discharges.

DISCUSSION

This is the first study, to my knowledge, to examine the HCUP SID from multiple states to analyze the impact of smoke-free laws on asthma discharges. It improves on the existing body of literature by improving generalizability with discharge data for 35% of the US population, by including states without state smoke-free laws as a control group, and by examining the effects of state and county smoke-free laws on asthma discharges separately and together.

I expected that working-age adult asthma discharges at the county level would fall even lower after a state law was implemented; however, this was not the case. The effect of state smoke-free laws besides the effect of

TABLE 3—Factors Associated With Asthma Discharge Rates per 10 000: Healthcare Cost and Utilization Project, United States, 2002–2009

Variables	Adult,*** Pearson ρ	Child,*** Pearson ρ
Cigarette tax	0.09	−0.18
Smoking prevalence	0.05	0.24
Asthma prevalence	0.07	0.06
In poverty, %	0.19	0.33
Uninsured, %	−0.08	0.06
PCP/10 000	0.16	0.04
Non-White, %	0.26	0.14

Note. PCP = primary care physicians.
***$P<.001$.

TABLE 4—Selected Results of Multivariate Analyses: Healthcare Cost and Utilization Project, United States, 2002–2009

Variables	Working-Age Adult Asthma, b (RSE)	Working-Age Adult Appendicitis, b (RSE)	Child Asthma, b (RSE)	Child Appendicitis, b (RSE)
Any state law	0.29 (0.42)	0.00 (0.04)	0.12 (0.25)	0.04 (0.05)
Any county law	−2.44* (1.23)	−0.08 (0.13)	−1.32* (0.67)	0.15 (0.12)
State law × county law	1.32 (1.85)	0.16 (0.09)	0.51 (0.79)	−0.04 (0.08)
Cigarette tax	−0.21 (0.40)	0.14* (0.06)	−0.53* (0.24)	−0.06 (0.07)
Smoking prevalence	−0.11 (0.10)	0.03* (0.01)	0.03 (0.08)	0.00 (0.02)
Asthma prevalence	0.07** (0.02)	0.00 (0.00)	0.03 (0.02)	0.00 (0.01)
In poverty, %	0.95** (0.20)	0.02** (0.01)	0.71** (0.11)	0.04** (0.01)
Uninsured, %	−0.31** (0.12)	0.02** (0.01)	−0.07 (0.08)	0.03** (0.01)
Constant	9.78* (4.80)	1.75** (0.36)	0.52 (3.15)	2.26** (0.48)

Note. RSE = robust standard error.
*$P < .05$; **$P < .01$.

county laws was not statistically significant (b = 1.32; $P < .48$). This is not consistent with the findings of Herman and Walsh,[32] the only other study to date that has analyzed a state smoke-free law while accounting for preexisting county laws. They studied the effect of Arizona's state smoke-free law on counties with and without city and county laws and found significant reductions in adult asthma discharges after the law was implemented. In my study, labeling counties as having county smoke-free laws if they contained a city or town with smoke-free laws may have underestimated the effect of state laws besides the effect of county laws; however, this was considered the more conservative approach. In fact, a sensitivity analysis labeling the same counties as not having smoke-free laws (following Herman and Walsh) revealed that the effect of state smoke-free laws besides the effect of county laws was negative and significant (b = −3.78; $P < .03$). This finding reinforced the decision to err on the conservative side of not overestimating the effect of state smoke-free laws.

The fact that the working-age adult state law variable was also not statistically significant (b = 0.29; $P < .50$) disagrees with Shetty et al.[33] However, their study of HCUP inpatient sample data did not differentiate between state and county laws. The county law variable in my study tells an interesting and unexpected story. Its parameter estimate was negative and statistically significant (b = −2.44; $P < .05$), implying that county laws by themselves are, in fact, associated with reductions in working-age adult asthma discharges. My study's ability to separate the effects of state and county laws distinguishes it from previous studies, especially the Shetty study. Shetty et al.'s results may have detected the effect of county smoke-free laws and not state laws.

I expected the result of the interaction of state and county laws in the child model (b = 0.51; $P < .52$). Because children can neither legally work nor enter bars, I did not expect state smoke-free laws to have an effect on child asthma discharges besides the effect of county laws. These results somewhat agreed with Shetty et al.[33] and Rayens et al.,[30] the only 2 recent US studies that specifically analyzed child asthma data after smoke-free law implementation. Shetty et al. found the relationship between the implementation of any smoke-free law and child asthma discharges not to be statistically significant (b = 9.0; $P < .08$), which is similar to my findings for state laws besides the effect of county laws (b = 0.51; $P < .52$) and state laws only (b = 0.12; $P < .65$). The Rayens study found that child emergency department visits were reduced 18% ($P < .01$) after Lexington-Fayette County, Kentucky, implemented a county law. The direction and significance of the Rayens result agrees with the significant and negative relationship between the implementation of county smoke-free laws and child asthma discharges (b = −1.32; $P < .05$) that I found.

Although I could not apply Hill's guidelines for causality to the interaction variables in either model, I could apply them to the effects of county laws alone in both child and adult models. Regarding strength, the reduction in county asthma rates after the implementation of county smoke-free laws was statistically significant. Regarding temporality, the model's difference-in-differences design ensured that the observed reductions in asthma rates occurred after the implementation of county smoke-free laws. Regarding biological gradient, the direction of asthma rates was negative after the implementation of county smoke-free laws. Regarding plausibility, appendicitis discharges were not significantly different after the implementation of county smoke-free laws. Following Hill's guidelines, there is evidence that reductions in county asthma rates were attributable to the implementation of county smoke-free laws.

Limitations

As with most retrospective, nonrandomized studies, there are several important limitations that should be addressed. I believe that I used the best design that could be employed with the available data: a pre–post nonequivalent control group design. With this design, I examined county asthma discharges before and after the implementation of smoke-free laws and included 5 control states that never passed state smoke-free laws during the study period.

The use of difference-in-differences models ensured that I observed effects only after the implementation of smoke-free laws, reducing the possibility of ambiguous temporal precedence. Selection of the treatment states might be a concern, but I compared treatment and control states on numerous socioeconomic and political dimensions, and I controlled for the dimensions in which there were significant differences in the models with 1 exception. States with Democratic majority governments in 2005 were more likely to participate in the HCUP standardized data program. However, once included in the study, there were no significant differences between treatment and control states on whether states had Democratic governors (b = 0.06; $P < .96$), Democratic majority houses (b = 0.48; $P < .64$), or Democratic majority senates (b = 0.56; $P < .58$). I also controlled history and maturation threats to internal validity through the difference-in-differences models. The model captured differences across groups over time and differences across time that were common among groups.

The models may have omitted variables, but the use of state fixed effects partially mitigates this concern. The models controlled for the average difference across states in any observable or unobservable predictors. The fixed effect coefficients absorbed all the across-group effects, leaving the within-group effects. The threat of omitted variable bias was, hence, reduced.[43]

Generalizability of the results is always a concern when the entire population of potential observations is not included in the analysis. As of April 2011, the treatment and control states accounted for approximately 35% of the 2005 US population. To date, this is the most comprehensive smoke-free law study conducted with the HCUP SID. Still, there is the potential one might infer that any individual state not included in the study would have the same results as the group of states included in the study—the ecological fallacy associated with scale. Readers should interpret these results with this in mind.

Policy Implications

My finding of support for county implementation of smoke-free laws does not mean state policy approaches are not effective. There is ample evidence that increasing state tobacco taxes leads to reductions in the numbers of cigarettes smoked and increases in the number of individuals who quit smoking,[44–46] especially among heavy smokers.[47] When fewer people smoke, fewer nonsmokers are exposed to the effects of exposure to SHS. In fact, the Institute of Medicine recommends multifaceted state approaches to reduce the effects of tobacco use and exposure to SHS.[48]

My findings are important for the implications of state law preemption legislation that prevents any local jurisdiction from enacting laws that are more stringent than a state law.[49] As of 2007, 27 states preempted local regulation of tobacco advertising, smoke-free indoor air, or youth access to tobacco.[50] The tobacco industry has found state preemption an effective means for undermining local smoke-free efforts.[51] My findings may indicate it is unwise to pursue state smoke-free laws where they have yet to pass. Rather, efforts might be better focused at the local level, where there is evidence of a significant impact. ■

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