HOSPITAL ADMISSIONS FOR ACUTE MYOCARDIAL INFARCTION FOLLOWING OREGON'S SMOKING BAN

By

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A THESIS

Presented to the Department of Public Health & Preventive Medicine
and the Oregon Health & Science University
School of Medicine
in partial fulfillment of
the requirements for the degree of

Master of Public Health

June 2015

Department of Public Health and Preventive Medicine

School of Medicine

Oregon Health & Science University

CERTIFICATE OF APPROVAL

This is to certify that the Master's thesis of

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Table of Contents

Acknowledgements

First and foremost I would like to thank the members of my thesis committee. From our first meeting Donald Austin exuded a patience and excitement about the project that was essential to its completion. Rochelle Fu was central to the development of the modeling, providing answers, support, and guidance even on the minutest questions. Finally, Karen Girard’s expertise in Oregon tobacco policy and data, as well as her fresh perspective were greatly appreciated. I am so thankful she was willing to take time out of her day to assist in this project. The road was not always smooth, but I could always rely on my committee for detailed feedback and support. I have learned a great deal from all three of them in this process.

I would also like to thank the professors in the department for preparing me for this project, and for my future in public health and biostatistics. In every course it was plain to see the excitement of the teaching staff in the subject matter.

Finally I would like to thank Andrew DuBuisson for his work not only as an editor, but as a sounding board for ideas, worries, and oddball thoughts. This project would not exist without his tireless support over this process.

Abstract

On January 1, 2009 the Oregon Indoor Clean Air Act took effect, banning smoking in all workplaces including restaurants and bars except certified smoke shops and cigar bars, and banning smoking in all outdoor areas within 10 feet of an entrance to a workplace throughout Oregon. While it is well established that exposure to secondhand smoke can lead to acute myocardial infarction (AMI), and that smoking ban legislation is associated with a reduction in the incidence of AMI in affected communities, the effect of Oregon's legislation on AMI has not been fully studied. This thesis research project used a generalized linear mixed model to assess the impact of the 2009 legislation on hospital admissions for AMI. The study used hospital discharge data for 2007 to 2010, comparing counties without a pre-existing comprehensive smoking ban ("case" counties) with counties with pre-existing comprehensive smoking bans ("control" counties). The study found that while there was an annual 5.5% decrease in AMI over the study period (RR 0.95; 95% CI 0.92 to 0.97; p<0.01), there was not a greater decrease around January 1, 2009 (p=0.99). For case counties, there was no significant difference in AMI admissions between the pre- and post-legislative period after adjusting for seasonality and temporal trend (RR 1.00; 95% CI 0.93 to 1.08; p=0.99). For the control counties the rate ratio for the pre- and post-legislative period after adjusting for seasonality and temporal trend was also not significant (RR = 1.08; 95% CI 0.93 to 1.24; p=0.31). These results indicate that while it is reasonable to assume Oregon's comprehensive smoking ban had some impact on secondhand smoke exposure, and that any reduction is likely to impact AMI admissions, the no impact of the legislation on AMI admissions was detected in this study.

Introduction

Exposure to second-hand tobacco smoke (SHS) has been associated with a many adverse health outcomes in the long and short term (Surgeon General 2006). Passive exposure to SHS is therefore a major concern for policymakers and public health officials. In the last few decades, banning tobacco smoking in public places and workplaces, including public buildings, restaurants and bars has gained popularity as an approach to reducing SHS exposure. The evidence supports this approach—smoking bans are associated with reduced exposure to SHS, reduced smoking prevalence, and lower cigarette consumption (Fichtenberg *et al* 2002).

One of the major health concerns related to SHS is acute myocardial infarction (AMI). Of the nearly 50,000 deaths caused by SHS in the United States each year, 46,000 are from heart disease (Surgeon General 2006). In addition, because SHS is not only a long-term, but also a short-term cause of AMI (Surgeon General 2006), AMI represents an opportunity to study the immediate impact that smoking ban legislation can have on the health of a community.

Since 2004 more than 20 studies examining the association between AMI and smoking bans in communities across the United States, and around the world have been published (Aguero *et al* 2013; Barone-Adesi *et al* 2006; Barr *et al* 2012; Bartecchi *et al* 2006; Bonetti *et al* 2011; Bruintjes *et al* 2011; CDC 2009; Cesaroni *et al* 2008; Christensen *et al* 2014; Cronin *et al* 2012; Dove *et al* 2010; Herman *et al* 2011; Hurt *et al* 2012; Juster *et al* 2007; Khuder *et al* 2007; Lemstra *et al* 2008; Pell *et al* 2008; Roberts *et al* 2012; Rodu *et al* 2012; Sargent *et al* 2004; Schmucker *et al* 2013; Seo & Torabi 2007; Shetty *et al* 2011; Stallings-Smith *et al* 2013). Taken together, these studies provide strong evidence that smoking bans can lead to a reduction in AMI incidence in affected communities.

On January 1, 2009 the Oregon Indoor Clean Air Act took effect, banning smoking in all workplaces including restaurants and bars, except certified smoke shops and cigar bars. The act also banned smoking in all outdoor areas within 10 feet of an entrance to a workplace throughout Oregon. While it has been well established that exposure to SHS can lead to AMI, and that smoking ban legislation is associated with a reduction in the incidence of AMI in affected communities, the effect of Oregon's legislation on AMI has not been fully studied.

This thesis research project is driven by this question: What was the effect of the Oregon Indoor Clean Air Act on hospital admissions for AMI in affected Oregon counties?

From a general scientific and public health perspective the body of evidence that smoking bans cause reductions in AMI incidence is robust. On a local level, however, such evidence remains important. In 2013 18% of Oregon adults reported exposure to SHS indoors, meaning well over half a million Oregon adults were exposed that year (BRFSS 2013). Furthermore, 11% of adults reported exposure to SHS at work, around 1 in 5 of whom are exposed indoors while at work (BRFSS 2013). Nationwide, in 2011-2012 around one quarter of nonsmokers in the United States had measurable levels of cotinine, a measure of exposure to SHS (CDC 2015). While the level of exposure is lower than before, the risk to these Oregonians is still a significant public health concern. Additional evidence on the tangible health impact that efforts to reduce SHS exposure in Oregon have achieved would support continued efforts to reduce rates of smoking and SHS exposure in the state.

Acute Myocardial Infarction and Second-Hand Smoke

The focus of this thesis research project is on immediate reductions in AMI events related to a reduction in exposure to SHS due to regulatory changes in Oregon. Exposure to SHS can have an almost immediate effect on the risk of AMI (Surgeon General 2006). There are two major mechanisms to this process. Exposure to SHS can lead to increased platelet activation, increasing the risk of thrombosis (Otsuka *et al* 2001; Surgeon General 2006). Exposure to SHS can also interfere with endothelial-dependent vasodilation, reducing the ability of coronary arteries to dilate and increasing the risk of AMI (Otsuka *et al* 2001; Surgeon General 2006). These effects can be seen after as little as 30 minutes of exposure to SHS and evidence suggests the magnitude of the effect may be similar for SHS exposure and direct smoking (Otsuka *et al* 2001).

Acute Myocardial Infarction and Smoking Ban Legislation

In 2004 a study demonstrating an association between reduced AMI admissions and a municipal smoking ban was published (Sargent *et al* 2004). The study centered on a smoking ban in effect in Helena, Montana and showed a 40% decrease in admissions for AMI in Helena when the law was in effect followed by an increase when the law was suspended. Taken alone, the Helena study was limited by its small sample size, short time-frame, and observational design (Kabat 2004). Since then, however, there have been more than 20 studies examining the association between AMI and smoking bans in communities across the United States, and around the world (Aguero *et al* 2013; Barone-Adesi *et al* 2006; Barr *et al* 2012; Bartecchi *et al* 2006; Bonetti *et al* 2011; Bruintjes *et al* 2011; CDC 2009; Cesaroni *et al* 2008; Christensen *et al* 2014; Cronin *et al* 2012; Dove *et al* 2010; Herman *et al* 2011; Hurt *et al* 2012; Juster *et al* 2007; Khuder *et al* 2007; Lemstra *et al* 2008; Pell *et al* 2008; Roberts *et al* 2012; Rodu *et al* 2012; Schmucker *et al* 2013; Seo & Torabi 2007; Shetty *et al* 2011; Stallings-Smith *et al* 2013). Of these, only one found no association (Shetty *et al* 2011), one found that the association only in certain States (Rodu *et al* 2012), and another found that while an association was found when assuming a linear trend, relaxing that assumption reduced the measured effect nearly to zero (Barr *et al* 2012). All other studies found significant decreases in AMI admissions following smoking ban legislation.

Though the methods varied to some extent, the majority of the studies followed a similar design. The studies compared observed AMI admissions and/or deaths before and after the implementation of the smoking ban in question, usually using Poisson regression or some other form of generalized linear mixed model. Within that framework there was some variation. Several studies excluded the time period just after the legislation; some included a control community not affected by the ban; one study included control diagnoses; several examined confounding variables such as age, gender, air pollution, influenza, hypertension, obesity, diabetes, or air temperature; and studies used AMI data from different source types.

The enactment of a smoking ban in a community is not done under ideal experimental conditions. No control community is specifically identified and excluded from the law, residents are not confined to specific ban versus non-ban areas, and study subjects are not specifically followed to document behavior or AMI. Instead, the researchers used the circumstances presented to them and whatever applicable data was available to build their analyses. Due to the nature of how the interventions occurred, researchers consistently used the same observational, before versus after, study design. The body of research showing measurable results from so many of these observational studies further supports the potential to use an observational study design in evaluating smoking bans and AMI.

Three studies used different methodology than the other studies (Juster *et al* 2007; Shetty *et al* 2011; Rodu *et al* 2012). While the majority of studies to date used some form of generalized linear mixed model, Juster *et al* (2007) took another approach. The study, which examined legislation in New York State, used county-level trends in admissions for AMI and stroke over a 10 year period to study the effect of several different local and state-wide laws. Laws were differentiated into those that were comprehensive (banning smoking in all workplaces including restaurants, bars, and other hospitality venues) and those that were moderate (banning smoking in most places, but not most hospitality venues). The results showed decreases in AMI associated with both types of legislation, with larger decreases resulting from stricter comprehensive bans. The model used in the study adjusted for age, pre-existing smoking legislation, seasonal trends, county differences, and secular trends. The modeling used in the study allowed the researchers to examine several smoking ban events of various types occurring at different times in New York.

The study by Shetty *et al* (2011) performed a nation-wide analysis of whether smoking ban legislation in the United States as a whole was associated with a reduction in admissions or deaths for AMI. The study differed from other studies in several key ways. First, the study by Shetty *et al* (2011) used national All-Cause Mortality Data, Medicare claims, and the Nationwide Inpatient Survey to get admissions and deaths on a zip code or county level. In linking the data to the county and zip code level, only 60% of deaths and 4% of all hospital admissions were included in the sample. The analysis also combined communities from all over the United States based on the type of smoking ban legislation. The study found no significant association between AMI and smoking-ban legislation.

The study by Rodu *et al* (2012) focused on States that enacted state-wide smoking bans between 1995 and 2003. The study differed from other studies in several ways. The study examined AMI mortality rather than hospital admissions. The model was also different. The authors calculated the average annual change in AMI mortality for each state for the three years prior to the smoking ban. The average annual change was used to predict the "expected" mortality rate the next year. The observed change in mortality was then compared to the "expected" as well as to the change seen in other states with no smoking ban legislation during that period. The study found that of the 6 states analyzed, one state (North Dakota) had an increase in AMI mortality, three showed decreases that were not statistically significant from the expected, and two showed decreases greater than the expected.

In addition to the numerous studies just discussed, at least five meta-analyses have also been completed (Dinno & Glantz 2007; Glantz 2008; Lightwood & Glantz 2009; Meyers *et al* 2009; Mackay *et al* 2010). The meta-analyses yielded different pooled estimates depending on when the analysis was completed and which studies were included, ranging from a 10% reduction (Mackay *et al* 2010) to 27% (Dinno & Glantz 2007). Several of the meta-analyses examined whether sample size or other factors contributed to the variable size of results and found that it did not. The major factor associated with the size of the effect was length of the period of study. Notably, none of the meta-analyses included the data from the recent study by Shetty *et al* (2011), though two discuss it as a study pending publication.

Several concerns have been raised regarding the body of evidence on AMI and smoking ban legislation that should be addressed. Some authors questioned whether chance might explain some of the observed associations (NC 2011; Rodu *et al* 2012). The two nation-wide studies from the United States did both find wide variation in AMI data month to month and year to year. While these results indicate that it is possible that some associations could be explained by chance, given the variation observed by these authors one would expect many more studies showing no association, or even increases in AMI admissions. Considering this, the volume of studies consistently showing an association negates the possibility that all the evidence resulted from chance alone.

It is also important to consider whether un-related downward trends in AMI admissions due to other factors might be responsible for the measured decreases, rather than the legislation. A study in North Carolina (NC 2011) found that when three "dummy" dates were used instead of the date of the smoking ban, one date was significantly associated with a reduction in AMI admissions similar to the association for the true date. It is feasible that month to month variation, combined with an overall downward trend in AMI incidence over a study period could lead to a significant result, or exaggerate a result, in a few studies. As the North Carolina results show, however, even with a downward trend only one of three “dummy” dates were associated with a significant reduction in AMI admissions (NC 2011). As such it is unreasonable that natural variation and overall downward trends are responsible for the results in over 20 studies.

Another concern raised is that additional studies which found no association may exist that have not been published due to publication bias (Shetty *et al* 2011). It is plausible that some local public health researchers evaluating smoking bans may have completed analyses showing no significant association and did not seek publication or could not be published. The observational design makes such studies feasible for many more communities. For these unpublished studies to call the results of so many published studies into question, however, there would need to be at least as many studies showing no results to begin to raise concerns regarding the more than 20 studies showing an association. Furthermore, for publication bias to discredit these studies one must assume that the published studies come from a much broader body of work in which, by chance, many communities showed significant decreases associated with the legislation. Therefore to truly suggest that the associations found in other studies are by chance these unpublished works would need to often be showing an increase in AMI associated with smoking bans. Given the volume of studies showing an association in the expected direction, publication bias is insufficient to explain away the evidence for an association.

Another potential concern is that few studies control for many confounding variables. Quite a few factors can affect the rate of AMI in a population. The fact that so many studies from disparate locations and time periods found an association, however, makes it unlikely that an unknown confounder is actually causing the reduction in AMIs. The before versus after method also allows a community to serve, to some extent, as its own control. Furthermore, the studies focused on changes in AMI at one point in time. A confounding variable would therefore need to consistently change at the same time as the legislation in all these communities to explain away the consistent findings of reductions in AMI.

The body of evidence as a whole provides significant evidence in support of a causal association between smoking ban legislation and reductions in admissions for AMI. Using Hill's criteria the association is strong, with point estimates ranging from an 8% reduction to 40%. The association is biologically plausible, with established evidence that exposure to second-hand smoke causes physiological changes that can lead to AMI. The association followed patterns consistent with the biological mechanism, with greater effects among non-smokers consistent with the idea that the reductions are largely due to SHS exposure (Seo & Torabi 2007). Some studies have shown a form of dose-response, with more restrictive legislation associated with greater decreases (Juster *et al* 2007). In these studies the cause (a smoking ban) is temporally linked to the effect (reduced AMI admissions). Finally, the studies are consistent, with all but one (Shetty *et al* 2011) finding some association between smoking ban legislation and AMI. The body of evidence, in fact, was deemed by the Institute of Medicine (IOM) to be sufficient to conclude that smoking bans cause a reduction in the risk of coronary heart disease (IOM 2009). On this point I agree that the evidence shows a causal relationship between smoking bans and a reduction in the risk of AMI.

Smoking Ban Legislation in Oregon

The first comprehensive smoking ban in Oregon was passed in Corvallis in 1997, banning smoking in all workplaces, including restaurants and bars. Subsequently, Eugene and Philomath passed similar comprehensive smoking bans, and several other areas including Multnomah County passed moderate bans. On January 1, 2002 the Oregon Indoor Clean Air Act was amended to include a moderate ban on smoking in most workplaces and public buildings including restaurants, but not bars, bar areas of restaurants, bingo halls, or bowling centers. In 2004 Multnomah County passed restrictions regarding smoking on hospital grounds. It was not until January 1, 2009 that the Oregon Indoor Clean Air Act banned smoking in all workplaces including restaurants and bars except certified smoke shops and cigar bars and in all outdoor areas within 10 feet of an entrance to a workplace throughout Oregon.

Thesis Research Project

The following thesis research project assesses whether Oregon's 2009 move from a moderate smoking ban to a comprehensive smoking ban is associated with a reduction in AMI admissions in Oregon counties. The study uses a generalized linear mixed model (GLMM) to analyze monthly admissions for AMI in the two years before and after enactment of the comprehensive ban.

Given the strength of the evidence on the association between smoking ban legislation and reductions in AMI, why is it important to perform such a study in Oregon? While it is true that this thesis research project is not necessary from a general scientific and public health perspective to prove a causal association, on a local level further research remains important. The results of this study may be useful to inform policy makers and public health officials on the magnitude of the impact of Oregon's comprehensive smoking ban and help shape future efforts in Oregon and potentially in other communities as well. Further research can also serve to draw additional attention to the risks of SHS exposure. This is important because although exposure to SHS is far lower than it once was, in 2013 18% of Oregon adults reported exposure to SHS indoors, indicating well over half a million adults are exposed (BRFSS 2013). Furthermore, 11% of adults reported exposure to SHS at work, around 1 in 5 of whom were exposed indoors while working (BRFSS 2013). As long as Oregonians continue to be exposed to SHS, particularly in their work environment, it will remain important to continue to study the impacts SHS has on the community.

Aims

To determine whether the Oregon Indoor Clean Air Act enacted on January 1, 2009 is associated with a reduction in hospitalization for acute myocardial infarction (AMI) in Oregon counties where the act was newly implemented

Hypothesis

There will be a reduction in the admission rate for acute myocardial infarction (AMI) in Oregon counties directly after the implementation of the Oregon Indoor Clean Air Act on January 1, 2009 in counties without pre-existing comprehensive smoking bans.

Methods

The study was a longitudinal analysis of hospital admissions for acute myocardial infarction (AMI) in Oregon counties before and after the implementation of the Oregon Indoor Clean Air Act in 2009. The study methods and data sources were submitted to the Oregon Health & Science University Institutional Review Board (IRB) and found to be exempt from IRB approval.

Selection of Patients

Hospital admissions data were taken from the Healthcare Cost and Utilization Project (HCUP) State Inpatient Dataset (SID) for Oregon for January 1, 2007 to December 31, 2010. Analysis included records with primary diagnoses of the following International Classification of Diseases, Ninth Revision (ICD-9) codes: acute myocardial infarction (AMI; 410.x0 and 410.x1).

Admission counts were aggregated by month based on the admission date on the record. Admission counts were also aggregated by county of residence. County of residence was based on the zip code on the record. Each Oregon zip code was assigned to one county based on the most current data available. Zip codes crossing county lines were assigned to the county containing the largest share of the zip code by land area. Patients with no zip code or with zip codes outside Oregon were excluded from the analysis. Analysis for AMI was limited to patients age 35 and older at admission based on the age reported in the discharge abstracts.

Study Period

Among studies examining changes associated with a single legislative change a four year period was the longest time period studied. The study was also focused on changes specific to January 1, 2009, making a longer time-period unnecessary to examine the impact. Given resource constraints and the availability of data at the inception of the project, a study period of four years was selected. The study period was January 1, 2007 through December 31, 2010. The 24-month period from January 1, 2007 through December 31, 2008 constituted the pre-legislation period and the 24-month period from January 1, 2009 through December 31, 2010 constituted the post-legislation period. Hospital admissions were assigned to a period based on hospital inpatient admission date.

The SID provided data on all discharges for calendar years 2007-2010. Because data were available based on year of discharge rather than admission, not all admissions in December 2010 are available in the data set. Examining prior years, the percent of December AMI admissions who were also discharged in December was similar over the study period (86.3% in 2007, 86.1% in 2008 and 86.3% in 2009, average 86.22% for all years). To adjust for the missing data in December

2010 the population offset for that month was set to 86.22% of the population estimate for that month.

Air Pollution

The study investigated controlling for monthly average particulate matter (sized less than 2.5 micrometers; $PM_{2.5}$) air pollution, as measured by nephelometer. $PM_{2.5}$ data came from the Oregon Department of Environmental Quality (DEQ) and the Lane Regional Air Protection Agency (LRAPA). Data were assigned to the county where the station was located. Data were expressed as monthly means of all measurements taken at a given station. Where multiple stations existed in one county the combined mean of those stations' monthly means were used in the model. Not all Oregon Counties were monitored for $PM_{2.5}$ air pollution for all of the study period, including several counties without any monitoring. Overall 20 of Oregon's 36 counties had $PM_{2.5}$ data for the study period. In the end $PM_{2.5}$ was not a significant variable and was not included in the final model.

County Population

The natural logarithm of monthly county population was used as an offset variable in the model. Monthly county population was calculated from yearly county population estimates for 2006-2011 from the Population Research Center at Portland State University. Yearly estimates were for July 1 of each year. Assuming linear population growth from July 1 of one year to July 1 of the next, monthly estimates were calculated for each county in the study.

Counties Included in Study

Multnomah County was excluded from the final model due to concerns about additional regulations regarding smoking in workplaces prior to the Oregon Indoor Clean Air Act of 2009.

This left 35 of 36 counties in the final analysis. Combined these counties were home to 92.4% of the Oregon population at the start of the study, and 92.1% of the population at the end of the study.

County Control Group Model

The model compared "case" counties and "control" counties. The two groups were defined based on whether the Oregon Indoor Clean Air Act changed the smoking policy in the entire county (case), or there was already a comprehensive smoking ban in all or part of the county prior to 2009 (control). A comprehensive ban was defined as one which included restaurants and bars. Based on this criterion Benton County and Lane County constituted the "control" counties. Benton was considered a control due to pre-existing comprehensive bans in Corvallis and Philomath, home to 69% of the county's population. Lane was considered a control due to a pre-existing comprehensive ban in Eugene, home to 44% of the county population. These two counties contained 11.4% of the population of Oregon in 2010. The remaining 33 counties in the final study sample constituted the "case" counties. Based on my hypothesis a larger decrease in AMI admissions would be found in the "case" counties where the law changed existing smoking policy for the entire county. There may be some reduction in the "control" counties associated with the legislation since not all county residents lived in the aforementioned cities, and residents may also cross county lines, but any decrease in the "control" counties was expected to be smaller than in the "case" counties.

Model Selection

The analysis was conducted using SAS University Edition. The initial steps of the model selection process involved determining the core specification to use in the model. The analysis was conducted using a Generalized Linear Mixed Model (GLMM). A Mixed Model was appropriate for the study because the study was longitudinal, involving a series of correlated measurements with the same county in the study period. The data in the model involved both fixed effects (legislative time period, county, month) and random effects (identification variable for the counties). The fixed effects relate the outcomes to the independent variables in the model, and the random effects take the correlation of multiple measurements in the same county into account.

There are several types of GLMM. The outcome of interest in the model was a discrete count of AMI events so both a Poisson and negative binomial distribution model were possible choices. A negative binomial model was used over the Poisson model to account for over-dispersion often present in count data. Several covariance structures were also tried within the model including an unstructured covariance matrix, compound symmetry structure, and heterogeneous compound symmetry structure. Based on AIC the compound symmetry structure was the best fit for the data. To determine the best control for seasonality, models were run with month as a categorical variable as well as with sine and cosine functions of month to determine the best method to control for seasonal variation. Based on AIC categorical month was the best fit for the data.

Once the core model specifications on data distribution and covariance structure were determined several different models were run to determine which model best fit the data. In every model the count of AMI admissions in a county, offset by the natural logarithm of the county population, was modeled based on county control group (0 for control, 1 for case counties), legislative period (0 for prior to the ban, 1 for after the ban), an interaction term between county group and legislative period, year, county, and a categorical month variable to adjust for seasonality. In some models monthly average $PM_{2.5}$ air pollution was also included. The interaction term was used to assess whether the impact of the legislation was different for our case and control counties. The interaction term was key in the study as the hypothesis was not only that the legislation would have a measurable impact, but that the impact would be larger in the case counties.

Particulate matter air pollution ($PM_{2.5}$) was investigated as a potential factor in the model. To determine whether $PM_{2.5}$ should be included in the model it was included in the first model ("Model #1"; See Appendix 1). Because $PM_{2.5}$ was included, the model tested only the 20 counties where data were available for the study period. In this model only the seasonality control variable for month was a statistically significant factor in modelling AMI hospital admission rates. $PM_{2.5}$ air pollution was not a statistically significant variable ($p=0.29$). Given that $PM_{2.5}$ was not a significant variable in the model, and that 16 counties were excluded due to lack of $PM_{2.5}$ data, additional models were then run without $PM_{2.5}$ data.

To assess the impact of removing $PM_{2.5}$ the initial model was run again, with the same 20 counties and same variables, but without $PM_{2.5}$ (Model #2; See Appendix 2). Based on the AIC Model #2 without $PM_{2.5}$ was a better fit for the data. Removal of $PM_{2.5}$ also had little to no impact on the rate ratio estimates or statistical significance of the other variables in the model. Since $PM_{2.5}$ air pollution was not statistically significant in Model #1, the model fit improved when it was removed, and the impact on the other variables was minor, $PM_{2.5}$ was removed from the model.

Without $PM_{2.5}$ 16 additional counties could now be included in the model, so a third model was tested (Model #3; See Appendix 3). In this model only the seasonality control variable for month and the variable for calendar year were statistically significant in modelling AMI hospital admission rates ($p<0.01$ and $p<0.01$).

Multnomah County was of note in the study for having passed some additional regulations regarding smoking in workplaces prior to the Oregon Indoor Clean Air Act. To assess the effect of including Multnomah County in the case counties, a model excluding Multnomah County was run (Model #4; See Appendix 4). Removal of Multnomah County had only minor effects on the model results. Removal of Multnomah County did not have much effect on the significance of any of the variables in the model. Removing Multnomah County did change the estimated effect of the legislation slightly from a rate ratio of 1.006 in Model #3 (with Multnomah County; p=0.85) to 0.9997 in Model #4 (without Multnomah County; p=0.99), but that change was minimal. Though the impact of removing Multnomah County was small, the direction of the impact was consistent with concerns that additional smoking regulations in that county could obscure the impact of the 2009 smoking ban. As such Model #4 was selected as the best fit model for the data (See Appendix 4).

Results

Data Description

The final model included 35 of 36 Oregon counties. Of these, two counties (Benton and Lane) were classified as control counties due to pre-existing comprehensive smoking bans in those counties. The remaining 33 were classified as case counties. Combined these counties were home to 92.4% of the Oregon population at the start of the study, and 92.1% of the population at the end of the study.

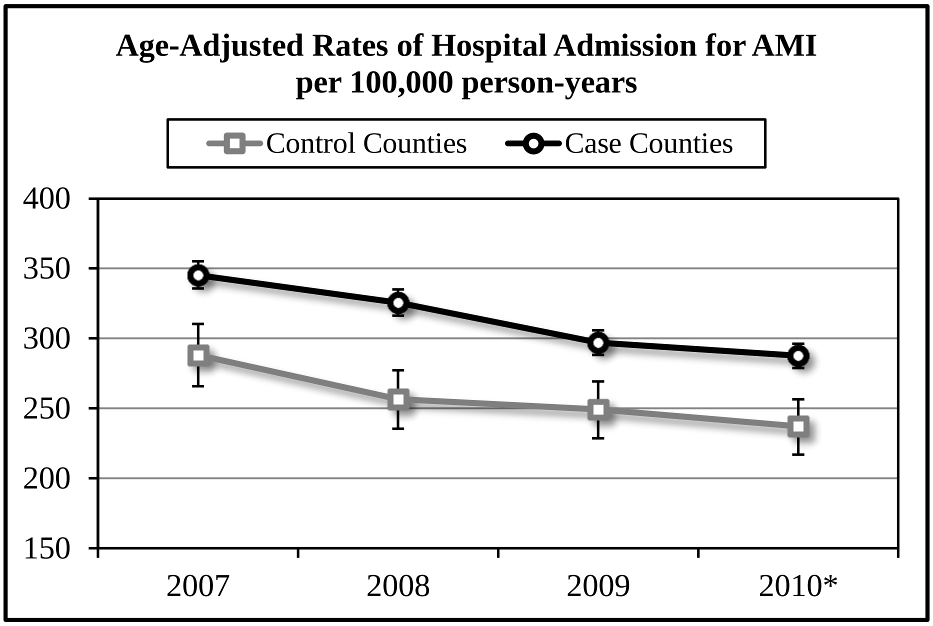
Table 1

Age Adjusted Rates of Hospital Admission for AMI per 100,000 person-years
For Control and Case Counties by Year

	Control Counties	Case Counties	All Counties
2007	288	345	337
2008	257	326	316
2009	249	297	290
2010*	237	288	281

* 2010 AMI count adjusted to estimate missed December admissions

Chart 1



Age-adjusted rates of hospital admission for AMI per 100,000 person years were calculated for each year of the study period (see Table 1 and Chart 1). For both case and control counties the rates decreased each year from the prior year. From 2007 to 2010 the rates fell 16.7% in the case counties and 17.7% in the control counties. Examining the rates year to year the largest decrease in AMI rates for case counties was an 8.7% decrease between 2008 and 2009, compared with a 5.7% drop from 2007 to 2008 and 3.2% from 2009 to 2010. For control counties the greatest decrease was an 11.0% between 2007 and 2008, compared with a 2.8% drop from 2008 to 2009 and 4.9% from 2009 to 2010. The largest decrease in the case counties was from 2008 to 2009, which coincides with the legislation change, and the decrease in the control counties was only 2.8% between those years. These observations are consistent with the hypothesis that there would be a decrease in AMI hospital admissions in the case counties from 2008 to 2009, and that the decrease would be smaller or lacking for the control counties. The age-adjusted rates, however, are not sufficient to determine if there was a statistically significant impact on January 1, 2009 in our case counties.

Model Results

Table 2

Association between hospital admission for AMI and Oregon smoking ban
Based on the generalized linear mixed effects model

Effect	Estimate (Rate Ratio)	95% Confidence (Rate Ratio)		Pr > |t|
		Lower	Upper	
County Group (Case vs Control; for Post-Legis)	1.21	0.65	2.24	0.54
County Group (Case vs Control; for Pre-Legis)	1.30	0.70	2.41	0.40
Legislation Period (Post vs Pre; for Case Counties)	1.00	0.93	1.08	0.99
Legislation Period (Post vs Pre; for Control Counties)	1.08	0.93	1.24	0.31
Year of Admission	0.95	0.92	0.97	<0.01

The model with the best fit for the data was a generalized linear mixed model with a negative binomial distribution, a compound symmetry structure of covariance, and a categorical variable for month to control for seasonality (see Appendix 4 and Table 2). In this best fit model only the variables for month and calendar year were statistically significant factors in modelling AMI hospital admission rates (see Table 2).

While there was an annual 5.5% decrease in AMI (RR 0.95, 95% CI 0.92 to 0.97; p<0.01), there was no significant difference in AMI admissions between the pre- and post-legislative period after adjusting for seasonality and temporal trend among case counties (RR 1.00; 95% CI 0.93 to 1.08; p=0.99). For the control counties the rate ratio for the pre- and post-legislative period after adjusting for seasonality and temporal trend was also not significant (RR 1.08; 95% CI 0.93 to 1.24; p=0.31). While these results are consistent with my hypothesis that there would be a decrease in the case counties associated with the legislation, and that the decrease would be absent or smaller for the control counties, the estimated decrease in the case counties was nearly zero and was not statistically significant after adjusting for overall time trend.

Comparing the case and control counties, the model estimated that after controlling for seasonality and yearly changes the case counties had AMI rates higher than the control counties both prior to the legislation taking effect (RR 1.30; $p=0.40$), and after the legislation took effect (RR 1.21; $p=0.55$), though the differences were also not statistically significant. Pre-existing differences between case and control counties may relate to the pre-existing comprehensive smoking ban in the control counties. Without controlling for other differences between the county sets such as age, gender, smoking prevalence, and other potential contributors to AMI rates, however, it is not possible to know why the rates were lower for the control counties.

There results also indicated a statistically significant annual decreases in AMI for the years in the study period (RR 0.95; 95% CI 0.92 to 0.97; $p<0.01$). These results indicate an overall trend of decreasing rates of AMI admission over the study period.

Discussion

The best fit model of the impact of the Oregon Indoor Clean Air Act on hospital admission rates for AMI found no statistically significant relationship between the two. While there was an annual 5.5% decrease in AMI for all counties in the study (RR 0.95; 95% CI 0.92 to 0.97; $p<0.01$), there was not a greater decrease around January 1, 2009, the date separating the pre- and post-legislation periods among the case counties ($p=0.99$). What do these results mean for the true impact of the legislation on AMI in Oregon? The inability of the model to detect a decrease in hospital admission rates for AMI in Oregon means that if there was any change, it could not be detected statistically in this study.

If there had been a large decrease along the lines of the 8 to 40% measured in prior studies it is likely the model would have detected it. In the case of Oregon, however, there are several factors that make it unlikely that changes of that magnitude would have occurred in this case.

First, unlike in some of the other studies, Oregon did not move from having no smoking bans in place to a comprehensive ban. On January 1, 2002 the Oregon Indoor Clean Air Act was amended to include a moderate ban on smoking in most workplaces and public buildings including restaurants, but not bars, bar areas of restaurants, bingo halls, or bowling centers. As such the 2009 amendments which created a comprehensive ban which is the focus of this study would not be expected to have as large of an effect as the bans studied in other areas of the country. A small effect might not be detectable in this study.

It is also possible that in anticipation of the legislation some bars and other affected establishments began changing their policy in advance of January 1, 2009. Some businesses may also have changed their smoking policy voluntarily prior to 2009 as nationwide smoking bans were quite common by that time, meaning greater pressure on businesses to ban or restrict smoking. In this study the focus was on a point change on that date. If there was incremental adoption of the stricter smoking ban prior to January 1, 2009 this would lessen the point impact on that date, potentially obscuring the true impact of the legislation. In considering this, one must consider that the model found a statistically significant annual decrease in AMI for the counties over the study period (RR = 0.95; 95% CI 0.92 to 0.97; $p<0.01$). These results indicate an overall trend of decreasing admissions for AMI over the study period. It is possible that part of that trend relates to gradual adoption of the stricter smoking standards by businesses in 2007 and 2008.

The study was also affected by the size of the population of the State of Oregon and the concentration of that population in a subset of all counties in the State. In smaller counties there is greater percent variation month to month in the admissions for AMI than for counties with large populations as the number of admissions were low. With greater variation in the outcome measures small changes in the admission rates for AMI may not be detectable in a statistical model.

It is also possible that a change in AMI rates was obscured by an additional confounder which was not controlled for in this study. The study controlled for seasonality and found that air pollution was not a confounder in this case, but did not control for any other variable that might have affected the outcome of interest. As there are numerous factors that can affect AMI rates it is possible that additional confounders may have obscured the impact of the legislation in this study. Again, however, if the impact were along the lines of the 8-40% seen in other studies it is unlikely that an unknown confounder could have completely obscured such an impact.

Use of control counties attempted to isolate the impact of the legislation on counties without similarly strict prior legislation. Comparing the case and control counties, the model estimated that after controlling for seasonality and yearly changes the case counties had AMI rates higher than the control counties both prior to the legislation taking effect (RR 1.30; p=0.40), and after the legislation took effect (RR 1.21; p=0.55). Pre-existing differences between case and control counties may relate to the pre-existing comprehensive smoking ban in the control counties. Without controlling for other differences between the county sets such as age, gender, smoking prevalence, education, and other potential contributors to AMI rates, however, it is not possible to know why the rates were lower for the control counties. The differences were also not statistically significant (p=0.40 and p=0.55). Given these results it is possible that Benton and Lane Counties were not ideal controls for the other counties in Oregon given their lower general rates of hospitalization for AMI. The role of the control in this study, however, was to reinforce any observed impact of the smoking ban by testing whether the impact was strongest in the counties without a pre-existing comprehensive ban (the case counties). For case counties there was no significant difference in AMI admissions between the pre- and post-legislative period after adjusting for seasonality and temporal trend (RR 1.00; 95% CI 0.93 to 1.08; p=0.99). Since this result indicates no impact of in the case counties it makes little difference whether the control counties were suitable or not.

Finally, while this study found no impact of the smoking ban legislation on AMI admissions for Oregon as a whole, it is possible that the impact was significant locally in specific communities around Oregon. As discussed, the smaller size of many counties meant large month to month variations. There were also 33 case counties in the model. Significant impacts in specific communities may exist, but have been obscured in this study by other communities where the impact was smaller.

Summary & Conclusions

Given the limitations discussed, as well as this study's finding of no significant association, it may not be possible to quantify the impact of the Oregon Indoor Clean Air Act on AMI hospitalization in the State. However, looking at the core question which drove this study—did the comprehensive smoking ban in Oregon lead to fewer AMIs in the State—one must consider the evidence that smoking bans are causally associated with reductions in hospitalizations from AMI. The Institute of Medicine (IOM) concluded there was sufficient evidence to conclude that smoking bans cause a reduction in the risk of coronary heart disease (IOM 2009). Furthermore, the evidence is consistent with Hill's criteria for causal association. The association is strong, with point estimates ranging from and 8% reduction to 40%. The association is biologically plausible, with established evidence that exposure to second-hand smoke causes physiological changes that can lead to AMI. The association follows patterns consistent with the biological mechanism, with greater effects among non-smokers consistent with the idea that the reductions are largely due to SHS exposure (Seo & Torabi 2007). Some studies have shown a form of dose-response, with more restrictive legislation associated with greater decreases (Juster *et al* 2007). In these studies the cause (a smoking ban) is temporally linked to the effect (reduced AMI admissions). Finally, the studies are consistent, with all but three showing reductions after implementation of the bans.

Examining the logistics of the ban in Oregon it is reasonable to assume the ban reduced second hand smoke (SHS) exposure to at least some portion of the Oregon population. Given the strong evidence that reduced SHS exposure can cause reduced AMI admissions, even though this study did not find a statistically significant association between the 2009 smoking ban and hospitalization rates for AMI, it is reasonable to assume the legislation has positively affected the heart health of at least a few Oregonians. In 2010 the data show nearly 5,800 hospital admissions for AMI for adults 35 and over who were residents of Oregon. Even a 1% reduction would mean around 58 fewer admissions for AMI that year.

Examining the impact of Oregon's efforts to create smoke-free workplaces for its residents it is also important to consider all that Oregon has done, not just the portion which took effect on January 1, 2009. While this study indicates that if there was any impact on January 1, 2009 on AMI admission rates it was likely much smaller than the 8% to 40% seen in other places around the United States it is possible that Oregon's admission rates for AMI might have been much higher if no level of smoking ban had ever been enacted in the State. This study took advantage of the "natural experiment" represented by the legislation change in 2009. Considering the 7 year time lapse between 2002 (when the moderate smoking ban took effect) and 2009 (when the comprehensive ban took effect) the modeling used in this study could not be used to assess how the combined impact of these laws. Inability to study and measure the full impact, however, does not mean the impact was not significant from a public health perspective.

Looking forward, Oregon still faces challenges in controlling smoking and smoking related morbidity and mortality. In 2013 18% of Oregon adults reported exposure to SHS indoors, indicating well over half a million adults are exposed (BRFSS 2013). Furthermore, 11% of adults reported exposure to SHS at work, around 1 in 5 of whom are exposed indoors (BRFSS 2013). These Oregonians deserve protections in terms of SHS exposure, and as Oregon moves toward that goal additional opportunities may arise to evaluate the impact of policy changes.

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Appendices

Appendix 1

Model #1—20 Counties included, with $PM_{2.5}$ air pollution included in the model

Model AIC: 5145.73

Effect	Estimate (Rate Ratio)	95% Confidence (Rate Ratio)		p-Value
		Lower	Upper	
County Group (Case vs Control; for Post-Legis)	1.12	0.68	1.83	0.65
County Group (Case vs Control; for Pre-Legis)	1.22	0.74	1.99	0.43
Legislation Period (Post vs Pre; for Case Counties)	0.96	0.88	1.06	0.40
Legislation Period (Post vs Pre; for Control Counties)	1.05	0.90	1.22	0.57
Year of Admission	0.97	0.94	1.00	0.05
PM2.5 Air Pollution (µg/m3)	1.00	1.00	1.01	0.27

Appendix 2

Model #2—20 Counties as in Model #1, without $PM_{2.5}$ air pollution in the model

Model AIC: 5144.49

Effect	Estimate (Rate Ratio)	95% Confidence (Rate Ratio)		Pr > |t|
		Lower	Upper	
County Group (Case vs Control; for Post-Legis)	1.12	0.68	1.83	0.65
County Group (Case vs Control; for Pre-Legis)	1.22	0.74	1.99	0.44
Legislation Period (Post vs Pre; for Case Counties)	0.97	0.89	1.06	0.49
Legislation Period (Post vs Pre; for Control Counties)	1.05	0.91	1.23	0.50
Year of Admission	0.96	0.93	0.99	0.02

Appendix 3

Model #3—All 36 Counties included

Effect	Estimate (Rate Ratio)	95% Confidence (Rate Ratio)		Pr > |t|
		Lower	Upper	
County Group (Case vs Control; for Post-Legis)	1.20	0.65	2.20	0.56
County Group (Case vs Control; for Pre-Legis)	1.28	0.70	2.34	0.43
Legislation Period (Post vs Pre; for Case Counties)	1.01	0.94	1.08	0.85
Legislation Period (Post vs Pre; for Control Counties)	1.07	0.94	1.23	0.30
Year of Admission	0.94	0.92	0.97	<0.01

Appendix 4

Model #4—35 Counties included (Multnomah County excluded)

This is the final, "best fit" model used in results and discussion.

Effect	Estimate (Rate Ratio)	95% Confidence (Rate Ratio)		Pr > |t|
		Lower	Upper	
County Group (Case vs Control; for Post-Legis)	1.21	0.65	2.24	0.54
County Group (Case vs Control; for Pre-Legis)	1.30	0.70	2.41	0.40
Legislation Period (Post vs Pre; for Case Counties)	1.00	0.93	1.08	0.99
Legislation Period (Post vs Pre; for Control Counties)	1.08	0.93	1.24	0.31
Year of Admission	0.95	0.92	0.97	<0.01