The Impact of Smoking Bans on Birth Weight: Is Less More?

Abstract: I combine data on state and local tobacco control ordinances from Americans for Non-smokers Rights US Tobacco Control Laws Database with a sample of 35 million births from national natality data files to examine the impact of smoking bans on birth weight, the probability of low birth weight, and weeks of gestation. Using difference-in-difference techniques, I identify the effects of state bans net of local bans, as well as the effects of local bans net of state bans. If ban choice is endogenous, then these effects will be biased in opposite directions. Estimated effects may therefore be viewed as lower bounds of central estimates for state ban effects, or upper bounds of central estimates for local ban effects. Applying this logic to the analysis of results suggests that less restrictive bans do more to improve birth outcomes than "100% smokefree" bans do, particularly in urban settings.

Forty-eight states and 2,960 cities and counties in the US currently enforce one or more forms of no-smoking ordinances, usually termed "smoking bans." Smoking bans aim to protect public health from environmental tobacco smoke (ETS) by restricting or eliminating the right to smoking in public or semi-public venues. The direct health benefits of smoking bans, however, remain poorly understood. Contrary to popular belief, smoking bans may not uniformly benefit public health.

A handful of studies analyze the effects of smoking bans on the incidence of acute myocardial infarctions (AMI) in specific counties or municipalities. Overall, these studies find support for the hypothesis that smoking bans decrease the risk of AMI but cannot separate the effects of bans on smokers versus non-smokers, nor track effects over a constant population (Meyer and Neuberger 2008). Markowitz (2008) improves over these studies by using individual-level data to examine the effect of bans on Sudden Infant Death Syndrome (SIDS), but this study finds only mixed support for the hypothesis that smoking bans reduce SIDS cases. Likewise, Adda and Cornaglia (2006) examine individual non-smoker exposure to ETS, as measured by blood sera cotinine, and find that bans have no effect on average cotinine levels in the US. The authors suggest that this zero-net effect occurs because bans shift smoking into private environments where non-smoking family members are still exposed.[1]

The purpose of this paper is to investigate separately the impact of state and local smoking bans on birth weight and related outcomes. According to the US Surgeon General, ETS exposure increases the risk of low birth weight (defined as less than 2500 g or 5.5 pounds) and

[1] Adda and Cornaglia's result raises the question of whether secondhand or "third hand" smoke drives the observed exposure to nicotine because the chemical profile of these two sources of nicotine are different. Secondhand smoke contains hundreds of volatile organic compounds (VOCs) and high concentrations of particulate matter at elevated temperatures. Third hand smoke, which arises from the desorption of cigarette tar from indoor surfaces, contains many of the same VOCs at room temperature and very little of the particulate matter. Both are dangerous to health, but because of the presence of PM in secondhand smoke, secondhand smoke is more likely to cause damage to the lungs in the near term. See Singer *et al.* (2003) for an investigation of the contribution of third hand smoke to exposure profiles.

"represents an avoidable contribution to birth weight reductions" (US Department of Health and Human Services, 2006).[2] Lower birth weight possibly occurs in part because ETS may cause children to be born earlier than they otherwise would, but evidence from the Surgeon General considers evidence on this link only "suggestive" at this point (*ibid*). In turn, birth weight significantly affects the probability of infant death and a variety of individual outcomes later in life.[3] Any increases in mean birth weight due to smoking bans may therefore be viewed as a direct benefit of the ban.

Beyond its first-order implications, birth weight also provides an interesting, continuous measure of the effects of environment on human health, both because of the relatively short period of fetal gestation and because of the likelihood of increased risk aversion during pregnancy. Although mothers are mobile over their lives and may in fact choose where to live or work based on local amenities like smoking bans, nine months is a relatively small interval in their lifetime. Thus, individuals observed in the data as living in a given location are likely to have spent their pregnancy in that location. Research can thus reasonably connect the policies of an individual's location to their infant's birth outcomes.

Further, to the extent that pregnancy increases a woman's aversion to environmental risks, the measured effects of environmental variables on fetuses will be biased downwards. If pregnant women do not spend a lot of time in bars, for instance, bans on smoking in bars may have little effect on their infant's birth weight. Labor supply decisions may have a similar impact on the estimated effects of workplace bans on birth outcomes: if women decrease their

[2] The meta-analysis of Windham *et al.* (1999), which contributes to the Surgeon General's finding, estimates a mean reduction of 28 g (about 1 ounce) in birth weight due to ETS exposure. In an updated meta-analysis over a larger set of studies, Leonardi-Bee *et al.* (2008) estimate a mean decrease of 33 g in birth weight due to ETS exposure, a figure similar in magnitude albeit somewhat larger. Both studies estimate that maternal exposure to ETS increases the risk of low birth weight by about 20%.

[3] See Royer (2009); Almond, Chay, and Lee (2007); or Black, Devereux, and Salvanes (2007) for the most recent work in the area.

labor supply during pregnancy, work-place smoking bans may have a less pronounced effect on their infant's birth outcomes.

On the other hand, it may be the case that bans reduce the health costs of some behaviors, such as working or spending time in bars, which in turn negatively affect birth outcomes. While work does not appear to negatively affect birth outcomes (Baum, 2005), increased time in bars may reduce birth weight if it is positively correlated with alcohol consumption. Smoking bans may also negatively affect birth outcomes by crowding more smoking into private environments. For pregnant women who live or socialize with smokers, a ban on smoking in any kind of public or semi-public space may lead their partner or friends to smoke more in shared private environments. In the reduced-form analysis I present here, I cannot separately identify the contributions of these factors to the estimated effects. Instead, I aim simply to estimate a lower bound for the effect of state-level smoking bans by differencing between infants who were covered by local smoking bans while *in utero* and those who were not.

City and county governments have a longer history of smoking bans than states do, beginning in 1974 in Sacramento County, California. Before state-level bans, local bans covered highly populated areas such as Los Angeles County and New York City, which began restricting smoking in 1985 and 1988 respectively. In contrast, statewide bans begin in 1979 (Nebraska) with the bulk occurring after the EPA declared ETS a Class A carcinogen in 1993. Thus, if state smoking bans have a true positive effect on a given outcome, then estimations of their effect derived from analyses that do not account for the local bans may be biased downward. In essence, the impact of local bans may dilute the measured effect of state bans.

At the same time, the presence of a ban at either the state or local level indicates that the median voter in that jurisdiction prefers a ban. If stronger preferences for smoking bans correlate with relatively stronger preferences for health goods in general, the measured effect of smoking bans on health outcomes will be biased upward. A simple story for this endogeneity problem is that the people who vote for a smoking ban may be the same health conscious people who consider the impact of their activities and environments on their unborn child's health. Thus, observed birth weights in jurisdictions with smoking bans may be higher because people in those areas do more in general to promote healthy birth weight.

The ideal instrument for this problem would identify individuals with a taste for health goods independently of people who live in an area subject to a smoking ban. In this paper, I settle for a second-best: I eschew precise estimation of local smoking bans for a conservative estimate of state-level bans by using the presence of a local ban to identify observations in my data who may be more likely to have a relatively stronger preferences for health goods. If local bans correlate better with individual preferences than state bans, then the effects of statewide bans on jurisdictions that did not put a local ban in place will be underestimated.

This paper thus contributes to the research on the effects of state-level smoking bans by providing estimates of their impact that accounts for both the dilution of state ban effects and the endogeneity of ban choice. To obtain these estimates, I connect birth weight data from the National Vital Statistics System (NCHS, 1989-2004) to state and local policy data compiled by Americans for Non-smokers Rights (ANR, 2008), controlling for differences in cigarette prices across states and years using standard data from Orzechowski and Walker (2007). I then use difference-in-difference techniques within local-level fixed effects models to measure the impact of state-level bans on populations that did not previously have a local ban in place. If people choose where to live based on preferences for health-related goods, and smoking bans reveal local preferences for those goods, then estimates of the effects of state-level smoking on locations that did not previously ban smoking will be biased downwards. At the same time, estimates of the effects of local bans in jurisdictions not covered by state bans will be biased upwards. Thus, the estimates reported here may be viewed as lower bounds for the effects of state bans and upper bounds for the effects of local bans. To the extent that local and state bans are comparable, both estimates taken together may inform policymakers about the consequences of smoking bans in general.

Using this approach, I estimate the impact of bans on birth weight and the probability of low birth weight. I also analyze their effect on weeks of gestation. Exposure to ETS has known negative effects on the first two outcomes, but the link between ETS and weeks of gestation is less well established. I therefore consider the analysis of the impact of smoking bans on gestation as a contribution to the exploration of this link. For each of these outcomes, I estimate the effects of smoking bans using two samples: one with 34.8 million births linked to county and state policy, and a sub-sample of 9.8 million births linked to the municipal level as well.

I estimate two sets of models for each sample, using one of two distinct sets of policy controls to account for smoking bans. The data on bans identify the type of venues the ban covers (workplaces, restaurants, or bars), the level of government responsible for the ban, and ANR's rating of the ban's strength. Strength ratings fall into three categories: "100% smokefree", "qualified", or "some coverage". Bans that are "100% smokefree" essentially prohibit smoking with almost no exceptions, "qualified" bans allow for smoking in separately ventilated spaces, and bans that provide "some coverage" restrict smoking in a way that does not meet the standard of "qualified. In the first set of models, I simply use indicator variables to control for the presence of the various types of smoking bans at the time of birth.

In the second model, each policy control counts the number of months that a fetus was covered by various types of smoking bans while *in utero.* This continuous set of control variables provides a more precise measure of policy coverage than the indicators used in the first set of models and in prior studies and therefore a better way to test ban effectiveness. While I prefer this set of controls, it has at least one limitation: its semi-functional dependency on weeks of gestation makes it ill suited to analyze the impact of bans on gestation itself. Thus, for weeks of gestation, I report results only for models that use indicator variables to control for smoking bans.

Estimates of the impact of smoking bans using the county-within-state policy sample show that strong state level bans covering restaurants have positive and significant effects on infant birth weight across both models, although "100% smokefree" bans may not outperform slightly weaker bans, which appear to increase birth weight by at least 4.4 g *for every month covered while in utero* in this sample. Only the weakest workplace bans show positive and significant impacts on birth weight and reduced chances of low birth weight, and these effects are somewhat small: an increase of approximately 0.8 g in birth weight and approximately an 0.1% point reduction in the probability of low birth weight for every month covered. None of the estimates of the effects of county level bans on birth weight or the probability of low birth weight contravene these results. Bar bans have little significant impact relevant to this analysis, and gestation does appear to be significantly and positively related to smoking bans in this sample.

The municipality-within-county-within-state sample affords a better-specified model, but uses a smaller number of observations drawn from more urban environments. In this sample, weaker workplace and restaurant bans imposed by states appear to significantly improve birth outcomes in the policy month control models, increasing birth weight by at least 3.5 g and 14.5 g for every month of coverage, respectively. On the other hand, the estimated effects at the local level in this model suggest that "100% smokefree" bans may worsen birth outcomes. In both the indicator and policy month models, "100% smokefree" bans are associated with significant *decreases* in birth weight and increases in the probability of low birth weight. Under the assumption of endogeneity, these are upper bounds for the effects of 100% bans, which would suggest that their true impact is more negative. Bar bans again have little relevant impact, and gestation does not appear to be as tightly linked to policy in this sample.

Overall, these results suggest that bans that are less than "100% smokefree" may do a better job at improving birth outcomes. This finding has at least two implications. First, the impact of bans likely differs between urban and non-urban settings. Significant problems with "100% smokefree" bans only appear in the more urban sub-sample. State-level policy may therefore do more to improve public health by imposing less restrictive smoking bans and allowing communities to self-determine stricter "100% smokefree" coverage. Second, less may be more for smoking bans because prohibiting smoking in public places entirely shifts more smoking into private spaces where non-smokers are exposed. Given that people are going to smoke, public health might be improved by providing designated space for smokers where no non-smokers will be exposed to their emissions rather than prohibiting smoking entirely.

I proceed to show these results in five sections. In the first section, I describe the research design of the paper in more detail and provide the econometric framework for my analysis. Information on the natality and smoking policy data that I use appear in Section 2. I report descriptive statistics for the sample I analyze in Section 3. In Section 4, I present and discuss the results of the regression analyses, focusing on the estimates of the policy control variables by model and outcome. I conclude the paper by summarizing my results, considering their limitations, and offering questions for further research.

1. Research Design and Econometric Methods

In this paper, I use a reduced-form, difference-in-difference approach to identify the treatment effect of state level smoking bans on birth outcomes. Riechman *et al.* (2006) investigate the use of reduced-form analyses of infant birth weight and consider how estimations of effects might be affected by typically-unobserved-but-theoretically-important variables (TUV), other non-standard covariates (NSC), and input reporting. While the authors find that self-reporting of some inputs—like tobacco use—can lead to overestimates of their effects and that both TUV and NSC have significant effects on birth outcomes, they conclude that neither the use of self-reported variables nor the exclusion of TUVs or NSCs appreciably affects other input estimates. In the context of this paper, if the presence of a smoking ban results in increased under-reporting of tobacco use, the estimated effect of the smoking ban would be biased downwards. The reduced-form approach is thus in keeping with the aim to provide a conservative estimate of the effects of bans.

To investigate the effect of smoking bans on birth outcomes, I estimate a set of regressions based on the following fixed-effects model:

$$(1)\quad y_{icst} = \gamma_1 b_{st} + \gamma_2 b_{ct} + \gamma_3 b_{st} b_{ct} + \gamma_4 P_{st} + \beta_1 X_i + \alpha_c + \varepsilon_{ist} \; .$$

In equation (1), y_{ilst} denotes the birth outcome of individual i whose mother resides in location l within state s at the time of birth t, b_{st} denotes a vector of state- and time-varying policy controls, b_{ct} is a vector of county- and time-varying controls, P_{st} is the average real price of cigarettes in state s and time t, X_i is a vector of maternal, infant, and birth characteristics, α_c

is a time-invariant fixed effect for county c, and ε_{ist} is a mean zero random error. The vector $b_{st}b_{ct}$ captures the values of state and county policy controls when both state and local policy are controls non-zero, taking on the value (b_{st}, b_{lt}). The vectors of parameters γ_1-γ_4 and β_1 are to be estimated. I use ordinary least squares (OLS) to estimate the model.

Since the vector $b_{st}b_{ct}$ takes on the values of both state and county bans when both are present, the vector of parameters γ_1 represents the effects of state bans on individuals in counties where no county ban is in place. Likewise, γ_2 represents the effects of county level bans on individuals living in counties that have ban within a state that does not. Thus, the presence of a county level ban does not dilute the estimate of the effect of state bans given by γ_1. Further, if the presence of county bans indicates relatively stronger individual preferences for health related goods, γ_2 is biased upwards and γ_1 is biased downwards. In this case, γ_1 represents a set of lower bound estimates for the effects of state bans.

I extend this approach to the municipal level where data is available by using the following model:

$$(2)\quad y_{iscmt} = \gamma_1 b_{st} + \gamma_2 b_{ct} + \gamma_3 b_{mt} + \gamma_4 b_{st} b_{ct} + \gamma_5 b_{ct} b_{mt} + \gamma_6 b_{st} b_{mt} + \gamma_7 b_{st} b_{ct} b_{mt} + \gamma_8 P_{st} + \beta_1 X_i + \alpha_m + \varepsilon_{isct}\,.$$

The definitions of the data and parameter variables in this model are similar to model (1) with some additions. I use the subscript m to denote variables that depend on municipal characteristics. In particular, the vector b_{mt} represents bans present in municipality m at time t and the fixed effects α_m are set at the municipal level. As in (1), this model uses interaction terms to capture when an observation in the data is simultaneously covered by policy at any combination of the state, county, or municipal levels. Specifically, the vector $b_{st}b_{ct}$ gets the values (b_{st}, b_{ct}) when both state and county policy are present $b_{ct}b_{mt}$ takes the value (b_{ct}, b_{mt})

when both county and municipal policy are present, $b_{st} b_{mt}$ takes the value (b_{st}, b_{mt}) when both state and municipal policy are present, and $b_{st} b_{ct} b_{mt}$ takes the value (b_{st}, b_{ct}, b_{mt}) when an observation is covered by policy at all three levels. I again use OLS to estimate the model parameters.

Model (2) has both advantages and disadvantages relative to model (1). As in the first model, the interaction terms imply that the vector of parameters γ_1 represents the effects of state bans on birth outcomes for infants not covered by county or local policy. Thus, the estimate γ_1 represents the undiluted effect of state bans. Likewise, γ_1 is again biased downwards if the presence of county or municipal bans indicates relatively stronger individual preferences for health related goods. The second model does a better job than the first, however, in controlling for dilution of state policy by local policy and in controlling for idiosyncratic local effects that may be correlated with outcomes. On the other hand, because the data requirements for the second model constrain the sample, the analysis may be less broadly applicable. I turn to this and other issues in the next section.

2. Data

For this paper, I integrate data on smoking bans, cigarette prices, and births. In this section, I report on the granularity of each data set, the length of time that it covers, the time intervals of observations, and any limiting factors that lead to notable lacunae in the samples I use in my analyses. After describing each data set in turn, I discuss the samples of interest and how I construct the policy control variables for analysis.

2.1 Smoking Bans

State and Local tobacco control ordinance data were provided by the American Nonsmoker's Rights (ANR) Foundation U.S. Tobacco Control Laws Database© (2008).[4] This database provides information on smoking bans put into effect at the state, county, and municipal levels of government. At the county level, the data set provides information on whether the ban covers incorporated areas as well as unincorporated areas in the county. For each ban, the database reports what type of venue it covers (workplaces, restaurants, or bars), the effective date for the ban by venue; the state, county, or place FIPS code that the ban covers; and the "strength" of the ban coverage. The three characterizations of ban strength are defined by ANR (2008) as follows:

Workplaces

<u>100% Smokefree</u>: All workplaces must be completely smokefree, with some minor exceptions: A) Workplaces with only one employee are exempt. B) Family-owned businesses and businesses run by self-employed persons, in which all the employees are related to the owner or the self-employed person and which are not open to the public are exempt. C) With respect to public workplaces, jails or interrogation rooms are exempt.

<u>Qualified</u>: Workplaces must be smokefree with two possible general exceptions: A) Workplaces with a specified number of employees or fewer (but more than one employee) are exempt. If the exemption in a law is for one employee only (whether or not the employer), this field will be marked "Yes." B) Smoking is permitted in enclosed, separately ventilated smoking rooms.

<u>Some Coverage</u>: There is some coverage for workplaces, but less than either of the above two categories.

Restaurants

<u>100% Smokefree</u>: All restaurants, including attached bars, must be completely smokefree, *without exception*. If, *by law*, there are no bars in the community, this field will be marked "Yes" even though the law does not specifically address attached bars.

<u>Qualified</u>: Restaurants must be smokefree with three possible exceptions: A) Smoking is permitted in enclosed, separately ventilated dining rooms. B) Restaurants with a specified number of seats or fewer. C) Smoking is permitted in attached bars that are separately ventilated.

<u>Some Coverage</u>: There is some coverage for restaurants, but less than either of the above two categories.

Bars

<u>100% Smokefree</u>: All freestanding bars must be completely smokefree, *without exception.*

<u>Qualified</u>: Freestanding bars must be smokefree with one possible exception: Smoking is permitted in enclosed, separately ventilated rooms.

<u>Some Coverage</u>: There is some coverage for bars, but less than either of the above two categories.

[4] Abstracts based on this data are available online at ANR's website, www.no-smoke.org.

The data also provide information on when a ban was weakened, partially repealed, or repealed. In total, the ANR U.S. Tobacco Control Laws Database represents a historical record of tobacco ordinances in the US from 1974 to present. To my knowledge, no key observations are missing from this data. With respect to data on smoking bans, this data set is ideal for addressing the research question in this paper.

2.2 Cigarette Prices

To account for variation in the after-tax price of cigarette, I use data from the annual *Tax Burden on Tobacco* (Orzechowski and Walker, 2007). This data set gives average after-tax prices for a pack of 20 cigarettes by state and year, from 1970 to present. I deflate these prices using the US Bureau of Labor Statistics National Consumer Price Index (1982–1984 = 100). While these data provide some control for variation in cigarette prices, they fall short of ideal data in that they do not account for variation within states or within years. To my knowledge, however, this is the best available national data series on cigarette prices. I link these data to births by year and state.

2.3 Birth data

The US National Center for Health Statistics provides data on all births in the US in each month from 1968 to present. Variables that connect mothers to their place of residence by FIPS county codes first appear in the national birth data in 1982. To protect privacy, these data are only available for births to mothers living in counties with 100,000 or more people. Data that connect births to municipal place of residence by FIPS codes are available from 1994 forward. As with counties, this information is also restricted to births to mothers living in cities with greater than 100,000 people as of the most recent census. Beginning in 2005, the natality files no longer contain location in publicly available data above the state level due to privacy concerns.

The birth data include a wide variety of maternal and infant controls. I use a small set of these in this study. The controls I use include mother's age, race/ethnicity, education, marital status, total number of prior live births, self-reported smoking during pregnancy, infant's sex, weeks of gestation, and plurality of birth (singleton, twin, etc). Self-reported risk factors, including maternal tobacco use, are first reported in the data beginning in 1989. These factors directly affect birth weight and are likely affected by tobacco policy.[5] Maternal tobacco use is not reported for births occurring in California. Before 1999, maternal tobacco use was also not reported for the states of Indiana; the state of New York, excluding of New York City; and South Dakota.[6]

2.4 Samples of interest and ban controls

These data constraints suggest two samples of interest. The larger, primary sample uses data from 1989 to 2004, when county location and the full set of appropriate controls are available. Analyses of this sample can provide estimates of the effects of statewide smoking bans on counties that do not have bans in place. Because of a lack of data, births to mothers living in counties of less than 100,000 cannot be connected to policy and therefore are not included in the sample. Interpretation of the results thus may not extend to more rural counties. Indeed, it is plausible that smoking bans may have less impact on more rural counties because rural counties tend to be less densely populated. Estimated effects based on this sample can therefore be interpreted as the effect of a statewide smoking ban on birth outcomes for people living in a county of population 100,000 or more that did not previously have a ban in place.

Estimated effects based on the primary sample do not account for dilution of effects due to municipal smoking bans. The ideal data set would thus control for municipal policy as well. I

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[5] See for example Evans *et al.* (1999).
[6] New York City, however, reports maternal tobacco use from 1989 forward.

construct a sub-sample of observations from the birth data that meet this requirement for a supplementary analysis. This sub-sample runs from 1994 to 2004 and contains data on all births in the US in municipalities with populations greater than 100,000. The greater population density of these areas suggests that results from these analyses apply only to larger cities. The results from analyses on this sample can be interpreted as the effects of state-level bans on births to people living in cities of greater than 100,000 people that did not previously have a ban in place.

For observations in each of these two samples, I construct two sets of controls for smoking bans. Each set of controls identifies smoking bans that cover the mother's area of residence by the level of government responsible for the ban, the type of place it covers, and the strength of the bans. The first set of controls are simple indicator variables which take a value of one if the area of residence was covered by the ban it represents during the year and month of the infant's birth. Because similar controls appear in other smoking ban studies, the results I offer based on can be compared to findings in other research.

The data, however, allow for a more precise measurement of policy coverage. I construct a second set of controls that count the number of months that the infant was covered by a ban during its gestation. I calculate the number of months of ban coverage based on the ban's effective date, the infant's total number of weeks of gestation at the time of birth, and the month and year of birth.[7] For example, if a smoking ban were passed in January and the infant were born in March, then the infant would have been covered by the ban for two months while *in utero*. Because this measure relies in part on weeks of gestation, however, it cannot be used to analyze the impacts of ban coverage on weeks of gestation itself.

[7] Because only the month and year of birth are reported, I convert weeks of gestation to months by using an average of $365.25/12*7 \approx 4.35$ weeks per month.

3. Descriptive Statistics

In this section, I present descriptive statistics for the samples of interest to provide adequate background for the interpretation of regression analyses in the next section. I focus on the basic means and standard deviations of the maternal and infant birth controls for the two samples of interest. I also provide some basic tabulations of the policy control variables. For the price data, I only report overall mean and standard deviations for observations within the sample—see Orzechowski and Walker (2007) for further information on these data.

Table 1 presents sample means and standard deviations of outcomes and select controls for the county-within-state sample. This sample contains 34.8 million of the 64.3 million births that occurred from January 1989 through December 2004. Observations within the sample come from 530 different counties spread across every state in the US except Wyoming. Because California does not report maternal tobacco use, only a scant 6,000 births in this data are to California residents. The summary statistics reflect the fact that the births within this sample are drawn from more urban settings, where the percentages of minority populations are relatively higher. The percentage of self-reported smokers in this sample appears to underestimate the true percentage of smokers: 12% of mothers report smoking at some time during their pregnancy, while national estimates suggest that the true figure is closer to 19% (SAMSHA, 2000). Twenty-six percent of births in this sample were to mothers who lived in an area covered by at least one smoking ban, and 23% were covered by at least one state ban.

Similar sample characteristics appear in the summary statistics of the municipality-within-county-within-state sub-sample shown in Table 2. This sub-sample contains data on 9.8 million

Table 1
Summary Statistics: County-within-State Sample

N=34817843

	Mean	*Std. Dev.*
Birth Weight (g)	3311.14	608.32
Percent with low birth weight	0.08	0.27
Infant controls		
Weeks of Gestation	38.84	2.63
Plural birth	0.03	0.17
Female	0.49	0.50
Maternal controls		
Smoked during pregnancy	0.12	0.33
Age	27.25	6.08
Number of prior living births	2.03	1.22
Years of education	12.94	2.72
Married	0.68	0.47
Hispanic	0.16	0.37
White, non-Hispanic	0.60	0.49
Black, non-Hispanic	0.19	0.39
Other	0.05	0.22
Price and policy controls		
Real price of cigarettes (pack of 20), 1982-84 = 100	1.56	0.45
Covered by at least one county smoking ban	0.26	0.44
Covered by at least one state smoking ban	0.23	0.42

Table 2
Summary Statistics: County-within-State Sample

N=9781176

	Mean	*Std. Dev.*
Birth Weight (g)	3254.2741	615.4404
Percent with low birth weight	0.09	0.28
Infant controls		
Weeks of Gestation	-1.32	2.74
Plural birth	0.03	0.17
Female	0.49	0.50
Maternal controls		
Smoked during pregnancy	0.10	0.30
Age	26.55	6.23
Number of prior living births	2.08	1.31
Years of education	12.46	2.86
Married	0.56	0.50
Hispanic	0.27	0.44
White, non-Hispanic	0.39	0.49
Black, non-Hispanic	0.28	0.45
Other	0.06	0.24
Price and policy controls		
Real price of cigarettes (pack of 20), 1982-84 = 100	1.74	0.50
Covered by at least one municipal ban	0.69	0.46
Covered by at least one county smoking ban	0.33	0.47
Covered by at least one state smoking ban	0.18	0.39

births that occurred from 1994-2004. Observations come from 235 different municipalities within 201 different counties across 44 states.[8] Relative to the larger sample, mothers in the sub-sample tend to be slightly younger, slightly less educated, and much more likely to be minority. A large share of the births in this sample—69%—are to mothers who live in areas covered by a municipal smoking ban. Thirty-three percent are covered by a county ban, and only 18% are covered by state bans.

Tables 3 and 4 contain descriptive statistics for the policy control variables I analyze. For each ban type, I report the mean and standard deviations for the policy indicator and months of coverage variables. For the county-within-state sample, these statistics appear in Table 3. Table 4 reports this summary for the municipality-within-county-within state sub-sample. These means reveal that bans offering "some coverage" are the most common for observations in these samples. Bans rated "qualified" or "100% smokefree" cover relatively few infants born in these samples from a proportional perspective, but the raw numbers covered are still large. For example, in the main sample over 200,000 births are covered by "qualified" workplace bans and more than 150,00 are covered by "100% smokefree" bans. Similar calculations show that bar bans, while relatively uncommon, nonetheless covered hundreds of thousands of births. "Qualified" bans covering bars are completely absent from both samples, and "qualified" restaurant bans at the state level are absent in the sub-sample. Last, policy month variables are not simply equal to nine times indicator variables, due to variation in weeks of gestation and bans taking effect in the middle of some pregnancies.

[8] Delaware, Maine, Montana, North Dakota, Vermont, West Virginia, and Wyoming are not represented.

Table 3
Summary Statistics for Policy Controls: County-within-State Sample

	Ban type	Indicators Mean	Indicators Std. Dev.	Policy Months Mean	Policy Months Std. Dev.
State	**Workplaces**				
	Some Coverage	0.189	0.391	1.646	3.453
	Qualified	0.004	0.066	0.033	0.522
	100% Smokefree	0.009	0.096	0.051	0.593
	Restaurants				
	Some Coverage	0.128	0.334	1.122	2.957
	Qualified	0.0004	0.021	0.004	0.186
	100% Smokefree	0.021	0.144	0.149	1.089
	Bars				
	Some Coverage	0.008	0.090	0.070	0.786
	Qualified	*NA*			
	100% Smokefree	0.007	0.086	0.045	0.580
County	**Workplaces**				
	Some Coverage	0.233	0.423	2.039	3.738
	Qualified	0.006	0.079	0.053	0.682
	100% Smokefree	0.004	0.066	0.031	0.499
	Restaurants				
	Some Coverage	0.089	0.285	0.789	2.533
	Qualified	0.012	0.107	0.098	0.925
	100% Smokefree	0.005	0.072	0.038	0.556
	Bars				
	Some Coverage	0.008	0.090	0.072	0.797
	Qualified	*NA*			
	100% Smokefree	0.005	0.068	0.035	0.532

Table 4
Summary Statistics for Policy Controls:
Municipality-within-County-within-State Sub-sample

	Ban type	Indicators Mean	Indicators Std. Dev.	Policy Months Mean	Policy Months Std. Dev.
State	**Workplaces**				
	Some Coverage	0.153	0.360	1.342	3.184
	Qualified	0.005	0.071	0.039	0.569
	100% Smokefree	0.006	0.080	0.031	0.443
	Restaurants				
	Some Coverage	0.093	0.290	0.819	2.577
	Qualified	*NA*			
	100% Smokefree	0.015	0.120	0.098	0.878
	Bars				
	Some Coverage	0.014	0.119	0.125	1.045
	Qualified	*NA*			
	100% Smokefree	0.003	0.059	0.020	0.373
Municipal	**Workplaces**				
	Some Coverage	0.601	0.490	5.335	4.381
	Qualified	0.014	0.118	0.117	1.005
	100% Smokefree	0.017	0.131	0.128	1.022
	Restaurants				
	Some Coverage	0.533	0.499	4.729	4.460
	Qualified	0.030	0.170	0.242	1.424
	100% Smokefree	0.013	0.115	0.095	0.874
	Bars				
	Some Coverage	0.041	0.199	0.346	1.707
	Qualified	0.001	0.024	0.003	0.142
	100% Smokefree	0.008	0.090	0.057	0.677

4. Regression Analyses

In this section, I report the parameter estimates and standard errors for policy control variables using the county-within-state sample. I also report the effects of price on outcomes in each model, but the effects of price are likely attenuated because of the lack of variation in tobacco prices within years and within states. I briefly discuss the precise implementations of my models and the general interpretation of estimates for this analysis before reporting results.

For both the county-within-state sample and the municipality-within-county-within-state sub-sample, I separately regress birth weight, probability of low birth weight, and weeks of gestation against the policy control variables at both the state and county level as well as a set of interactions as described in Section 1. I report these results in Tables 5 and 6, respectively. The first three columns in each table represent estimates of ban impacts based on indicator control variables. The last two columns represent estimates based on policy month controls.

I do not report estimates of the impact of policy month controls on weeks of gestation. Other control variables not reported include the maternal and infant characteristics tabulated in Section 3, as well as sets of dummy variables for each year (2004 omitted), a separate set of dummies for each month (January omitted), and a set of county-level fixed effects. I do not include weeks of gestation as an explanatory variable in models where it is a dependent variable. County policy effects are estimated in the models for the municipality-within-county-within-state sample, but I do not report them because they lack clear interpretation in this context. Each model is estimated via ordinary least squares, with linear probability models used for the probability of low birth weight.

The estimates in the upper-half of Tables 5 and 6 represent the effects of state-level smoking bans on birth outcomes to mothers living in areas with no local ban coverage in place. Under the assumption of endogeneity, estimates in this half of the table are biased downwards and estimates in the lower half are biased upwards. Estimated negative effects of state policy controls on birth weight or weeks of gestation, as well as positive marginal effects on the probability of low birth weight may therefore be explained by endogeneity: poor outcomes may be due to location selection based on preferences for health related goods. For the same reason, however, positive impacts of state policy on birth weight or weeks of gestation, or reductions in the probability of low birth weight, are likely underestimated. Therefore, significant results in this analysis that show state bans improve outcomes are credible even under the assumption of endogeneity.

The lower half of the tables may be read in the opposite fashion. Estimates in this half of the table represent the impact of local bans in areas without state bans. Under the assumption of endogeneity, estimates in this half of the table are biased upwards. Estimated positive effects of local policy on birth weight or weeks of gestation, or reductions in probability of low birth weight, may be due to stronger local preferences for health related goods. On the other hand, if estimates suggest that local bans worsen outcomes despite an upward bias, unbiased estimates would still suggest bans have a negative impact. Thus, in this half of the table, significant results that show local bans worsen outcomes are credible even under the assumption of endogeneity.

Results in the top half of Table 5 suggest that weaker state-level bans on smoking in restaurants and workplaces do more to improve birth weight than 100% smokefree bans, and that bar bans have small but significant impacts on the probability of low birth weight. This disparity in the effects, as well as the general lack of positive and significant results for 100% smokefree bans, may be due to a true difference in relative ban impacts or due to endogeneity bias. At a minimum, however, the results show that state bans on smoking in restaurants rated “qualified”

Table 5
Effects of Smoking Bans on Birth Outcomes: County Sample

	Indicator Controls			*Policy Month Controls*	
Policy Controls	**Birth Weight (g)**	**Low Birth Weight (*P*)**	**Gestation (weeks)**	**Birth Weight (g)**	**Low Birth Weight (*P*)**
State Bans					
Workplaces					
Some Coverage	-1.105	-0.001 **	-0.018 **	0.802 ***	-0.001 ***
	(0.761)	(0.0004)	(0.004)	(0.085)	(0.0000)
Qualified	-9.375 **	0.001	0.044 **	-0.542	0.000
	(2.875)	(0.0013)	(0.015)	(0.389)	(0.0002)
100% Smokefree	-13.093 ***	0.000	0.032 **	-1.182 **	0.000
	(2.056)	(0.0010)	(0.011)	(0.367)	(0.0002)
Restaurants					
Some Coverage	-23.027 ***	0.003 ***	-0.020 **	-1.578 ***	0.000 ***
	(1.262)	(0.0006)	(0.007)	(0.130)	(0.0001)
Qualified	35.192 ***	0.003	0.079 **	4.435 ***	0.000
	(5.770)	(0.0027)	(0.031)	(0.646)	(0.0003)
100% Smokefree	5.613 ***	0.001	-0.011	0.473 **	0.000 *
	(1.513)	(0.0007)	(0.008)	(0.174)	(0.0001)
Bars					
Some Coverage	3.275	-0.002	-0.095 ***	0.187	-0.001 ***
	(3.565)	(0.0017)	(0.019)	(0.457)	(0.0002)
100% Smokefree	-1.822	0.000	-0.035 ***	-0.076	0.000 **
	(2.005)	(0.0009)	(0.011)	(0.389)	(0.0002)
County Bans					
Workplaces					
Some Coverage	-0.039	0.000	0.013 ***	0.223 ***	-0.001 ***
	(0.520)	(0.0002)	(0.003)	(0.058)	(0.0000)
Qualified	3.209	-0.002	-0.049	4.007 ***	-0.003 ***
	(4.775)	(0.0023)	(0.025)	(0.533)	(0.0002)
100% Smokefree	3.058	-0.002	-0.027 *	0.368	-0.001 ***
	(2.582)	(0.0012)	(0.014)	(0.345)	(0.0002)
Restaurants					
Some Coverage	5.076 ***	-0.002 ***	-0.006	1.320 ***	-0.001 ***
	(1.056)	(0.0005)	(0.006)	(0.118)	(0.0001)
Qualified	7.759 ***	0.001	0.039 ***	1.574 ***	-0.001 ***
	(1.920)	(0.0012)	(0.010)	(0.226)	(0.0001)
100% Smokefree	-8.106	0.002	0.105 ***	-2.361 ***	0.001
	(4.433)	(0.0022)	(0.023)	(0.718)	(0.0003)
Bars					
Some Coverage	3.922	-0.002	-0.315 ***	-2.723	-0.010 *
	(15.598)	(0.007)	(0.083)	(2.099)	(0.001)
100% Smokefree	-4.744	-0.001	-0.160 ***	0.893	0.000
	(5.177)	(0.002)	(0.027)	(0.794)	(0.000)
Average Price	-0.021	0.003 ***	0.110 ***	-0.826	0.003 ***
	(0.633)	(0.000)	(0.003)	(0.633)	(0.000)

*** Significant at $\alpha = 0.001$
** Significant at $\alpha = 0.01$
* Significant at $\alpha = 0.05$

Table 6
Effects of Smoking Bans on Birth Outcomes: Municipality-within-County-within-State Sub-sample

	Indicator Controls			*Policy Months Controls*	
Policy Controls	**Birth Weight (g)**	**Low Birth Weight (*P*)**	**Gestation (weeks)**	**Birth Weight (g)**	**Low Birth Weight (*P*)**
State Bans					
Workplaces					
Some Coverage	-20.825 ***	-0.001	-0.099 *	2.911 ***	-0.006 ***
	(5.556)	(0.0027)	(0.031)	(0.676)	(0.0003)
Qualified	-27.196 *	0.001	-0.035	3.528 *	-0.006 ***
	(10.748)	(0.0053)	(0.059)	(1.555)	(0.0008)
100% Smokefree	-38.256 ***	0.008	0.046	-1.443	-0.004 ***
	(8.489)	(0.0042)	(0.047)	(1.206)	(0.0006)
Restaurants					
Some Coverage	-46.624	-0.033	-1.601	14.472 ***	-0.025 ***
	(164.362)	(0.0810)	(0.903)	(1.349)	(0.0007)
100% Smokefree	19.938 *	-0.002	0.046	-2.273 *	0.004 ***
	(7.957)	(0.0039)	(0.044)	(0.952)	(0.0005)
Bars					
Some Coverage	13.640	-0.003	-0.023	-5.302 **	0.005 ***
	(12.649)	(0.0062)	(0.069)	(1.746)	(0.0009)
100% Smokefree	2.711	0.004	0.094	-1.962	0.002
	(8.977)	(0.0044)	(0.049)	(1.956)	(0.0010)
Municipal Bans					
Workplaces					
Some Coverage	-5.255	-0.005	-0.038	2.421 ***	-0.018 ***
	(6.166)	(0.0030)	(0.034)	(0.433)	(0.0002)
Qualified	4.633	0.001	0.009	0.953	-0.002 ***
	(6.094)	(0.0030)	(0.033)	(0.846)	(0.0004)
100% Smokefree	-11.164 ***	0.002 *	-0.098 ***	-1.491 ***	0.000
	(2.403)	(0.0012)	(0.013)	(0.330)	(0.0002)
Restaurants					
Some Coverage	-5.145	0.005 ***	0.040 *	-1.793 ***	-0.002 ***
	(2.893)	(0.0014)	(0.016)	(0.297)	(0.0001)
Qualified	1.750	0.000	0.009	0.510	0.000 **
	(2.280)	(0.0011)	(0.013)	(0.268)	(0.0001)
100% Smokefree	-15.647 **	0.004	0.111 *	-1.916 *	0.000
	(5.722)	(0.0028)	(0.031)	(0.774)	(0.0004)
Bars					
Some Coverage	-7.944	0.001	-0.053 *	-1.151	0.000
	(4.433)	(0.0022)	(0.024)	(0.642)	(0.0003)
Qualified	-11.286	0.004	-0.046	-4.431	-0.004 *
	(14.378)	(0.0071)	(0.079)	(3.850)	(0.0019)
100% Smokefree	15.941 *	-0.003	-0.110 **	1.999 *	0.000
	(6.309)	(0.0031)	(0.035)	(0.834)	(0.0004)
Average Price	2.568 *	0.0028 ***	0.138 *	3.321 **	0.0015 *
	(1.246)	(0.001)	(0.007)	(1.249)	(0.001)

*** Significant at $\alpha = 0.001$
** Significant at $\alpha = 0.01$
* Significant at $\alpha = 0.05$

are associated with birth weight increases of 35.2 g, or 1.2 oz, in the indicator model and 4.4 g per month of coverage in the policy month model. One hundred percent smokefree restaurant bans at the state-level are significantly associated with an increase of 5.6 g in birth weight or 0.47 g increase per month of coverage. State-enforced workplace bans that provide “some coverage” per ANR’s ratings are also associated with small but significant reductions in the probability of low birth weight in both types of models, and birth weight increases of 0.8 g per month of coverage in the policy month models.

The bottom half of Table 5 offers no contrary evidence on the impacts of bans on birth weight in this sample. They do suggest, however, that county-level workplace bans that are “100% smokefree” decrease weeks of gestation. The estimated effect is small (equivalent to less than a fraction of a day), but may be biased upwards. Local bar bans also significantly reduce weeks of gestation by at least one to two days.

Average cigarette prices have little beneficial impact on birth outcomes in this sample. While an increase of $1 in the real price of cigarettes increases gestation by approximately 0.8 days, it paradoxically increases the probability of low birth weight by 0.3 percentage points. The available data do not permit a thorough investigation of this result.

Analysis of the municipality-within-county-within-state sub-sample shown in Table 6 yields some similar results, but offers more caveats. Weaker state bans on smoking in workplaces again appear to significantly improve birth outcomes: bans that offer “some coverage” increase birth weight by at least 2.9 g per month and reduce the probability of low birth weight by more than ½ a percentage point for each month they are in place. Estimates of the effects of “qualified” state-level workplace bans are similar. Restaurant bans that offer “some coverage” have dramatic effects on birth weight: at a minimum, the results suggest that for every month of coverage, these bans increase birth weight by 14.5 g and decrease the probability of low birth weight by 2.46 percentage points for births in this sample. Unfortunately, no states in this sample offer "qualified" restaurant bans, so their impact in this sample cannot be analyzed.

State bans that are "100% smokefree" are associated with some positive impacts in this sample, but estimates of their effects at the local level give cause for some concern. In the indicator control model, "100% smokefree" restaurant bans increase birth weight by 19.9 g, but this result is only weakly significant ($\alpha = 0.05$). No similar results for "100% smokefree" restaurant bans appear in other models. Likewise, "100% smokefree" state bans on smoking in workplaces significantly reduce the probability of low birth weight in the policy indicator model, but similar effects do not appear in other models or for other outcomes.

At the same time, however, municipal workplace and restaurant bans that are "100% smokefree" are associated with significantly worse birth outcomes in several models. In particular, workplace bans of this type appear to reduce birth weight by at least 11.2 g or 1.5 g per month of coverage while increasing the probability of low birth weight by 0.25 percentage points. They also have a significant and negative marginal effect on weeks of gestation. Similarly, municipal "100%-smokefree" restaurant bans reduce birth weight by at least 15.6 g or 1.9 g per month of coverage in this sample. It is worth noting that municipal restaurant bans that meet the "some coverage" and "qualified" criteria also appear to significantly worsen birth outcomes. While the estimated marginal effects for these policies are not as large in magnitude, the small estimates may again be due to a true difference in relative policy impacts or due to endogeneity bias. Municipal bans rated "qualified," however, appear to cause no problems in workplaces.

Moving beyond birth weight, bans do not appear to have a significant positive effect on weeks of gestation in this sample. In fact, municipal "100% smokefree" workplace bans and bar bans rated "some coverage" and "100% smokefree" have significant and negative impacts on weeks of gestation. These negative impacts are consistent across both samples.

Last, increases in average real cigarette prices significantly improve birth weight outcomes in this sample. They are also again associated with an increase in weeks of gestation. However, the paradoxical result that higher cigarette prices lead to greater chances of low birth weight persist in this sample as well.

5. Conclusions and Questions for Further Research

The results of this analysis suggest that less-restrictive bans may do more to improve birth outcomes than "100% smokefree" bans. In both samples, workplace and restaurant bans with "some coverage" or "qualified" ratings appear to outperform "100% smokefree bans", although the difference in these effects cannot conclusively be attributed to superior effectiveness for less restrictive bans. At the municipal level, however, "100% smokefree" bans in restaurants and workplaces appear to significantly worsen birth weight outcomes.

Overall, state bans that meet ANR's "qualified" rating seem to perform best: looking across the results from both samples, bans at this level of coverage show the strongest positive results when implemented at the state level and the fewest significant problems at the municipal level. While summary statistics reveal that only a small proportion of births were covered by these kinds of bans, the total number of births that this proportion represents are nonetheless large and estimated effects tend to be highly significant.

In general, the results in this paper suggest some small but significant impacts of smoking bans on weeks of gestation. These results, however, are not consistent across both samples. In the main sample, the effects of bans work in the expected direction: bans appear to increase weeks of gestation. In the sub-sample, however, no significant positive effects of state bans on weeks of gestation appear. On the contrary, municipal bans that offer "100% smokefree" coverage of workplace or coverage of bars significantly reduce weeks of gestation.

While the data used in this paper are not well suited to analyze the effectiveness of cigarette prices as policy instruments to protect health, the findings here show that increases in cigarette prices significantly *increase* the probability of low birth weight. This unexpected finding may be due to any number of factors that may be illuminated by an analysis of cigarette prices. Greater expenditures on healthcare or access to better neo-natal care in states with higher cigarette taxes may explain this finding. Investigation of this relationship may provide insight into determinants of maternal smoking, maternal ETS exposure, or the effectiveness of public health spending on birth outcomes.

This analysis cannot determine why less restrictive bans appear to perform better or why bans may cause harm. One explanation is that more restrictive bans crowd smoking into private environments where non-smokers are exposed. Another explanation may be that bans reduce the costs to pregnant women of spending time in workplaces, restaurants, or bars. In turn, time spent in these places may correlate with activities or consumption that reduces infant health. I do not find this explanation plausible for the effects of workplace bans (Baum, 2005), but it may well explain the effect of bar bans on weeks of gestation. A careful analysis over a smaller sample with more information on maternal choices may be able to provide more insight into this issue.

Finally, while birth outcomes do have long-run implications, they provide information only on the short-run effects of smoking bans. More strict smoking bans may reduce the probability that someone begins smoking, increase the probability that they will quit smoking, or reduce their smoking habit. Evans *et al.* (1999) shows support for the idea that workplace bans will reduce cigarettes smoked (by 10%) and decrease smoking prevalence (by 5%), but I know of no research that investigates whether these effects vary by ban strength and if so, by how much. In any case, however, bans do not target these outcomes well. Supplementing less restrictive bans with continued support for cessation and education programs may be the best approach.

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