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RESEARCH ARTICLE

Do Smoking Bans Improve Neonatal Health?

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Research Objective. To estimate the effects of smoking bans on neonatal health outcomes and maternal smoking behavior during pregnancy.

Data Sources. Restricted-use 1991–2009 Natality Detail Files, a Clean Air Dates Table Report, and the Tax Burden of Tobacco.

Study Design. A quasi-experimental study using difference-in-differences estimation based on legislative history of smoking restrictions or bans by type/place/county/state level. Dependent variables included average monthly percentage of healthy neonates, of term neonates born with low and very low birth weight, of premature births, of maternal smokers, and average number of cigarettes smoked daily during pregnancy. The analyses were restricted to singleton births and those that occurred in the same county as mother's county of residence.

Data Collection/Extraction Methods. The data from three data sources were combined using Federal Information Processing Standard codes.

Principal Findings. Results of the overall and stratified by maternal smoking status, educational level, and age regression analyses suggested no appreciable effect of smoking bans on neonatal health. Smoking bans had also no effect on maternal smoking behavior.

Conclusion. While there are health benefits to the general population from smoking bans, their effects on neonatal health outcomes and maternal smoking during pregnancy seem to be limited.

Key Words. Smoking bans, birth outcomes, quasi-experimental study, difference-in-differences

Cigarette smoking is widely believed to be damaging to a person's own health as well as to others through second-hand (or passive) smoke (USDHHS 2010). Because of this, a large number of policies have been introduced to curtail smoking or to mitigate its effects on nonsmokers. Among these policies, cigarette taxes and laws regulating indoor smoking in workplaces, restaurants, and bars are the most prominent as well as the most studied (Taskforce on Community Preventive Services 2001; Callinan et al. 2010). There are two possible mechanisms through which smoking bans could affect health. One of the rationales behind their introduction is that they benefit nonsmokers by reducing their exposure to second-hand smoke. Such a reduction could occur by removing smokers from the presence of nonsmokers. The second mechanism is through a reduction in active smoking, either fewer smokers or fewer cigarettes smoked, which may also benefit both smokers and nonsmokers. However, unlike cigarette taxes that are known to affect cigarette consumption (Chaloupka, Yurekli, and Fong 2012), smoking bans can just displace smokers rather than reduce active smoking (Adda and Cornaglia 2010).

The purpose of this study was to examine effects of smoking restrictions or bans on adverse neonatal health outcomes. Both passive and active maternal smoking likely contributes to low birth weight, premature birth, and other adverse health outcomes of newborns (USDHHS 2010). While other research summarized below has examined aspects of smoking bans and neonatal health, our study is unique because we exploit city and county-level smoking bans as well as state-level bans. Also, we use a sufficiently long panel so that we can examine the effect of the first restriction or ban introduced in almost every city or county in the country.

Prior Research

While causal links between maternal active smoking during pregnancy and low birth weight and preterm birth have been established (USDHHS 2004, 2010; Cal/EPA 2005), neonatal health effects of passive maternal exposure need further investigation. The Cal/EPA (2005) recognized that passive smoking was causally related to low birth weight and preterm birth. However, the USDHHS (2006) concluded the evidence was sufficient to infer causality with regard to low birth weight, but not with regard to preterm birth. Effects of smoking on the extent of prematurity also need further investigation. Smoking has been suggested to increase the risk of very preterm birth caused by preterm labor, preterm premature rupture of membranes, and late pregnancy bleedings (Kyrklund-Blomberg, Granath, and Cnattingius 2005). A retrospective population-based case–control study of the Finnish Medical Birth Register data suggests maternal smoking alone explains up to 33 percent of the

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variation in extremely (gestational age <28 weeks), very (born at 28–32 weeks of gestation), and moderately preterm (born at 32–36 weeks of gestation) birth incidence between women of high versus low socioeconomic status (Räisänen et al. 2013).

In 2013, among all U.S. births, 11.39 percent were preterm and 1.92 percent were very preterm. Early preterm (gestational age <34 weeks) rate was 3.40 percent and late preterm (34–36 weeks) rate was 7.99 percent. Hence, about 70 percent of preterm births were late preterm and 17 percent were very preterm. The percentage of neonates born at low birth weight (less than 2,500 g) was 8.02 percent; at very low birth weight (less than 1,500 g), 1.41 percent; and at moderately low birth weight (1,500–2,499 g), 6.61 percent (Martin et al. 2015). The annual societal economic burden associated with preterm birth in the United States is at least $26.2 billion (or $51,600 per preterm infant). Medical care services contribute $16.9 billion to the total cost (or $33,200 per preterm infant), and maternal delivery costs contribute another $1.9 billion (or $3,800 per preterm infant) (IOM 2007). Extremely preterm infants account for more than one-third of the total medical costs associated with preterm birth through 7 years of age. Because of the much higher proportion of very and moderately preterm infants, they account for the large majority of the total societal costs of preterm birth. Based on the 2001 NIS data from the Healthcare Cost and Utilization Project, 8 percent of all 4.6 million infant stays nationwide included a diagnosis of preterm/low birth weight. Costs for their admissions totaled $5.8 billion, representing 47 percent of the costs for all infant hospitalizations and 27 percent for all pediatric stays. Preterm/low birth weight infant stays averaged $15,100, with a mean length of stay of 12.9 days compared to $600 and 1.9 days for uncomplicated newborns. Costs were highest for extremely preterm infants averaging $65,600. However, two-thirds of total hospitalization costs for preterm birth/low birth weight were for the substantial number of infants who were not extremely preterm (Russell et al. 2007).

Because preterm/low birth weight infants in the U.S. account for nearly half of infant hospitalization costs and one quarter of pediatric costs, major infant and pediatric cost savings could be realized by preventing these adverse neonatal health outcomes. The mean average excess direct medical cost per live birth for each pregnant smoker has been estimated as $511 and the total cost as $263 million. Even moderate effects of smoking reduction can have very large cost implications. For example, a 1 percentage point reduction in smoking prevalence each year throughout 7 years (or with 3–4 percent of smokers quitting annually) would result in approximately 57,200 fewer low birth weight infants with a total cumulative savings of $572 million in undiscounted medical costs in the United States (Lightwood, Phibbs, and Glantz 1999).

Prior research has mainly focused on the effects of changes in cigarette prices and smoke-free laws on cigarette consumption. Studies directly related to our study include those on smoking and pregnancy, as well as those on smoking bans and health.

A number of recent papers examine the determinants and outcomes of smoking during pregnancy. Bradford (2003) uses data from the National Maternal and Infant Survey to examine the relationship between cigarette prices and smoking. He finds while higher prices reduce the probability of smoking for both pregnant and nonpregnant women, there is no difference between the size of the effects between the two groups. Evans and Ringel (1999) find smoking participation declines when cigarette taxes are increased; this decline translates into higher birth weights. Lien and Evans (2005) show states with larger increases in cigarette taxes had corresponding decreases in smoking rates among pregnant women. Using these tax increases as an instrument variable, they find smoking during pregnancy doubles the chance an infant is born with a low birth weight. Levy and Meara (2006) exploit the roughly 20 percent increase in cigarette prices from the Master Settlement Agreement in November 1998 to compare the predicted decrease in smoking with the actual change. They find prenatal smoking decreased by less than half of that predicted. Fertig (2010) finds the risks of smoking while pregnant are likely overestimated due to selection; that is, the mothers who choose to smoke while pregnant tend to be younger and have less education and are therefore more likely to have poor outcomes regardless of whether they smoke.

Another stream of the literature focuses on the impact of smoking bans on behavior. Evans, Farelly, and Montgomery (1999) find workplace bans reduce smoking prevalence and smoking intensity. The effect is greatest for those who have longer work weeks. Bitler, Carpenter, and Zavodny (2010) find while bar bans reduced the fraction of bartenders who smoke, other types of smoking bans did not affect other groups. Carpenter (2009) finds the introduction of workplace smoking bans in Canada reduced both active and passive smoking, but this effect was concentrated in "blue collar" workplaces. There was small and insignificant effect in "white collar" workplaces. He notes, "the vast majority of those workers worked in workplaces with privately initiated smoking bans well before local by-laws were adopted." Adda and Cornaglia (2010) use the introduction of state-level smoking bans and

NHANES data to find a "displacement effect" of bans; that is, bans shifted cigarette consumption from public places to the home, where second-hand smoke affected children. Abrevaya and Puzzello (2012) reexamine Adda and Cornaglia (2010) and "find little systematic evidence to support" a displacement effect. Carpenter, Postolek, and Warman (2011) find smoking bans in Canada had no effect on smoking, but they had large effects on second-hand smoke exposure in public places. Additionally, they do not find evidence of displacement of smoking to private homes and cars. Adams and Cotti (2008) find bar smoking bans increase the rate of vehicular deaths; they attribute this effect to drivers responding to smoking bans by crossing county lines to find a bar where smoking is allowed. Using longitudinal data from the German Socio-Economic Panel Study, Anger, Kvasnicka, and Siedler (2011) find no evidence that the introduction of smoke-free legislation in 2007–2008 in Germany (a country with higher smoking rates than in the United States) changed average smoking behavior within the population. However, their estimates point to heterogeneous effects. Individuals who go out more often to bars and restaurants adjusted their smoking behavior, while others were not affected.

In contrast to findings from many observational and small regional quasi-experimental studies, Shetty et al. (2011) find workplace bans are not associated with statistically significant short-term declines in mortality or hospital admissions for acute myocardial infarction or other diseases. Large short-term increases in heart attack incidence following a smoking ban appear to be as common as the large decreases reported in other studies.

To our knowledge, the only papers that have looked at the effects of smoking bans and infant health outcomes are Markowitz (2008), Amaral (2009), and Markowitz et al. (2013). Markowitz (2008) examines the direct effects of cigarette prices, taxes, and state-level smoke-free indoor air laws in explaining changes in the incidence rates of sudden infant death syndrome (SIDS). She finds smoke-free laws (workplace, restaurant, and childcare center) reduced the incidence of SIDS. Amaral (2009) exploits the introduction of local county-level workplace smoking bans followed by the introduction of California's statewide ban in a difference-in-differences framework. She finds no economically meaningful effect of the introduction of workplace smoking bans on neonatal health outcomes in California. Lastly, Markowitz et al. (2013) use 2000–2005 PRAMS data on 29 states and New York City along with state-level data on smoking bans to examine the impact of tobacco control policies on maternal smoking and neonatal health outcomes stratified by mother's age categories. They find smoking policies (both cigarette taxes and workplace smoking bans) have small positive effects on neonatal health outcomes, but these effects are limited to teen mothers and mothers between the ages 25–34.

Our study adds to the literature in a couple of ways. First, we are able to exploit the introduction of city- and county-level smoking bans for almost the whole United States. Previous research has used either state-level smoking bans or a very limited number of local area (selected cities or counties) smoking bans. Second, we investigate workplace, restaurant, and bar smoking restrictions and bans (hereafter referred as smoking bans), whereas much of the previous literature has looked at individual types of bans (primarily workplace bans) in isolation. Third, because we have a long enough panel of data, we can investigate the effect of almost all smoking bans that have been implemented. Additionally, our estimation strategy allows us to plausibly examine a causal relationship between the smoking bans and health outcomes.

Data and Methods

Based on information from a Clean Air Dates Table Report (American Nonsmokers' Rights Foundation 2015), almost all counties had some sort of workplace smoking ban by December 2009; while roughly 25 percent of counties have no restaurant restrictions and 42 percent have no bar restrictions (Table 1). This data source provides information on three types of bans. First, a ban with 100 percent smoke-free implications. All workplaces must be completely smoke-free with some minor exceptions (those with only one employee; family-owned businesses and businesses run by self-employed person, in which all the employees are related to the owner or self-employed person and which are not open to the public; jails or interrogation rooms). All restaurants, including attached bars, and freestanding bars must be completely smoke-free without exceptions. Second, a qualified ban under which workplaces must be smoke-free with two possible general exceptions: (a) workplaces with a specified number of employees or fewer (but more than one employee) are exempt; (b) smoking is permitted in enclosed, separately ventilated smoking rooms. Similarly, under the provisions of the qualified bans in restaurants and bars, the exceptions include permission to smoke in enclosed, separately ventilated rooms. It is also permissible to smoke in a restaurant with a specified number of seats or fewer and attached bars that are separately ventilated. Third, bans requiring some coverage for workplaces, restaurants, and bars, but less than either of the above two categories American Nonsmokers' Rights Foundation 2015).

Table 1: Descriptive Statistics

	All Births (Main Results)		*Nonsmokers*		*Smokers*	
	Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*
Panel A: Outcome variables						
Premature (gestation $\leq$36 weeks)	0.099	0.030	0.098	0.033	0.124	0.062
Very premature (gestation $\leq$32 weeks)	0.020	0.012	0.019	0.012	0.026	0.027
Extremely premature (gestation $\leq$ weeks)	0.007	0.007	0.007	0.006	0.009	0.015
Low birth weight (<2,500 g)	0.060	0.024	0.054	0.023	0.102	0.057
Very low birth weight (<1,500 g)	0.009	0.008	0.009	0.008	0.013	0.017
Smoke (yes/no)	n/a	n/a	n/a	n/a	1	0
Number of cigarettes (daily)	n/a	n/a	n/a	n/a	10.580	2.419
Panel B: Covariates						
Older mother ($\geq$35)	0.115	0.048	0.119	0.050	0.083	0.052
Younger mother ($\leq$20)	0.183	0.069	0.175	0.073	0.233	0.087
White mother	0.767	0.188	0.759	0.192	0.819	0.182
Married mother	0.632	0.128	0.662	0.133	0.420	0.146
College graduate	0.262	0.119	0.292	0.127	0.058	0.046
	Workplace Number of Counties		Restaurant Number of Counties		Bar Number of Counties	
Panel C: Key independent variables						
None	17		759		1,321	
Some coverage	2,697		1,381		715	
Qualified	113		293		163	
100 percent smoke-free	314		708		942	
Total	3,141		3,141		3,141	

Note: The level of observation is a county. Types of smoke-free laws/restrictions: *100% smoke-free*: All workplaces must be completely smoke-free with some minor exceptions (those with only one employee; family-owned businesses and businesses run by self-employed person, in which all the employees are related to the owner or self-employed person and which are not open to the public; jails or interrogation rooms). All restaurants, including attached bars, and freestanding bars must be completely smoke-free without exceptions. *Qualified*: all workplaces must be smoke-free with two possible general exceptions: (1) workplaces with a specified number of employees or fewer (but more than one employee); (2) smoking is permitted in enclosed, separately ventilated smoking rooms. Similarly, under the provisions of the qualified bans in restaurants and bars, the exceptions include permission to smoke in enclosed, separately ventilated rooms. It is also permissible to smoke in a restaurant with a specified number of seats or fewer and attached bars that are separately ventilated. *Some coverage*: there is some coverage for workplaces, restaurants, and bars, but less than either of the above two categories. *None*: there is no workplace, restaurants, or free standing bar coverage at all.

The timing of the introduction of smoking restrictions/bans has varied with a larger number of restrictions/bans being introduced in more recent years. Not only is there wide variation in the year of adoption but also the effective dates are spaced throughout the year. It is this temporal variation combined with the geographic variation that we exploit using a difference-in-differences approach with the unit of analysis at the county-level and quarter of birth.

The data on smoking bans are linked to the Natality Detail Files for the years 1991–2009 from the National Center for Health Statistics, CDC. These data contain information about both the mother and the baby for almost every birth in the United States. Descriptive statistics are shown in Table 1. It is apparent that smokers are different than nonsmokers. They are more likely to be white, unmarried, and not college graduates. Thus, it is not surprising that they are more likely to be "young" (defined hereafter as aged <20 years). Mothers who smoke during pregnancy also are more likely to have a premature baby (defined as a gestational term ≤ 36 weeks) or a baby with low birth weight (defined as weighing $<2{,}500$ g or roughly 5.5 pounds).

The Natality data have information about the smoking behavior of the mother. Because not all states report smoking information for all years, when we segregate mothers into smokers and nonsmokers, we are forced to drop the following states from the analysis: CA, FL, IN, SD, MI, and NY. This also forces us to begin our data analyses in 1991. One potential issue with the information on maternal smoking is it is self-reported. However, Pickett et al. (2005) concludes self-reported smoking may be better than cotinine (a biomarker for exposure to cigarette smoke) measurements.

Identification and Statistical Strategy

We construct three datasets based on the Natality and the smoking ban data. The first dataset contains all singleton births in a county for which the county of birth is the same as the county where the mother resides. This restriction accounts for the majority of births and simplifies the problem of mothers who may live in a county with (without) a smoking ban and work or dine in a county without (with) a smoking ban. Our assumption is that women who give birth in the same county where they reside are likely to work and dine in the same county. This restriction does not change our conclusions. In most of the specifications, we code a county as "treated" if the county, or a city within the county, implemented a smoking ban. When the city encompasses multiple counties, each of them is coded as "treated' if that city-ban was the first one implemented in that county. Additionally, all counties within a state that introduces a smoking ban are considered to be treated. The exclusion of plural births does not change the conclusions; these results are available upon request.

The second and third datasets are composed of the same births as the first but include only smokers or nonsmokers, respectively. We also construct two other subsamples. Because women with low education may respond differently to smoking restrictions/bans, we limit the data to only mothers with less than 12 years of schooling. Additionally, we consider the possibility that teenage mothers may also respond differently and construct a dataset with only mothers 19 years or younger. As some of the variation in smoking bans occurs at the county level, we choose to collapse to the county level. We focus on the quarter (vs. month) of birth for computational reasons.

We estimate the following fixed effects regression model throughout this paper,

$$OUTCOME_{ct} = \alpha_c + \tau_t + \beta' BAN_{ct} + \gamma' X_{ct} + \varepsilon_{ct} \tag{1}$$

where the subscripts c and t denote counties and quarters, respectively, while X is a vector of maternal controls. The dependent variable (*OUTCOME*) is a health outcome believed to be affected by either smoking or second-hand smoke, including *low birth weight* (<2,500 g), *very low birth weight* (<1,500 g), and *prematurity* (gestation ≤36 weeks), as well as *very premature birth* (gestation ≤32 weeks) and *extremely premature* (gestation ≤28 weeks). We use maternal smoking as an additional outcome, both as an indicator of smoking behavior as well as the intensity of smoking (number of cigarettes smoked daily). In all of the regressions, X includes measures of county-level maternal marital status, maternal college attainment, maternal age (≤20 years and ≥35 years), and the fraction of mothers who are white. We also include state-level average cigarette prices (in 2009 dollars) including taxes (Orzechowski and Walker 2009). The terms α_c and τ_t are the county and quarter fixed effects, respectively. The inclusion of these fixed effects accounts for time invariant differences in outcomes across counties and differences in outcomes across time that are common in all counties, respectively. For example, rural counties may have different levels of low birth weight than urban counties, or the probability of a premature birth may have increased in the country as a whole. Robust standard errors are clustered at the county level (Bertrand, Duflo, and Mullainathan 2004). We also estimated the model with standard errors clustered at the state level. The results are qualitatively similar. All models were estimated as ordinary-least square regressions weighted by the number of births in a county/quarter cell.

The variable of interest in all specifications is *BAN*, which is a vector of the three types of smoking restrictions/bans (those providing some coverage, qualified, and 100 percent smoke-free) for the three different locations (workplace, restaurant, and bar). Each type of smoking ban is coded as a zero initially. The quarter after the first smoking ban goes into effect, the variable is switched to one and is thus a difference-in-difference estimator. The coefficients on the three types of restrictions/bans are reported in the tables. Our identification strategy assumes counties/states that adopted a smoking ban would have experienced similar changes in health outcomes as nonadopting counties/states had they not adopted a smoking ban.

We also estimate variations of the Equation 1, including county-specific time trends and lagged smoking restrictions/bans. Because some of the bans are city versus county or state regulations, we also consider a population-weighted regression that accounts for the fraction of the county that lives in the city which passed the ban. This also allows for a county to become "more treated" if additional cities within the county pass smoking bans at different times. To address the possibility that a smoking ban may have an impact increasing in the time it has been in place, we added a measure of the duration that a ban has been in place.

Lastly, the operational definition of the smoke-free ban is based on the date of the first smoking ban, which affected the county (whether implemented on a city, county, or state level). To adjust for more restrictive bans implemented later on and avoid biasing our "treatment" estimates toward a more limited effect, we conducted sensitivity analyses for situations when a moderately restrictive ban was followed by a much more comprehensive ban. Our results remained qualitatively unchanged and are available upon request.

RESULTS

The results of estimating Equation 1 are reported in Tables 2–5. For all of the outcomes we estimate Equation 1 with the specified county-level controls as well as Census Region-by-Quarter fixed effects. There are four Census regions: Northeast, South, Midwest, and West. The inclusion of region-by-quarter effects implicitly compares counties that passed smoking bans to other counties in the same region of the country. As we estimate variations of this model, including county-specific time trends, lagged smoking restrictions/bans, fraction of the county that lives in the city which passed the ban, and a measure of the duration that a ban has been in place, the results are all qualitatively unchanged and are available upon request.

Table 2: Smokers: Smoking Bans, Health Outcomes, and Prevalence of Smoking

	Smoking	*Number of Cigarettes*	*Preterm*	*Very Preterm (≤32 weeks)*	*Extremely (≤28 weeks)*	*LBW*	*VLBW*
Work: some coverage	0.0087	0.1084	0.0002	0.0005	0.0004	0.0014	0.0005
	(0.0030)	(0.0846)	(0.9087)	(0.3613)	(0.1993)	(0.2084)	(0.1220)
Work: qualified	−0.0019	−0.0830	0.0008	0.0000	−0.0003	0.0042	0.0001
	(0.7488)	(0.7515)	(0.8468)	(0.9857)	(0.6327)	(0.0824)	(0.8605)
Work: 100% smoke-free	0.0100	−0.0908	−0.0028	−0.0007	−0.0007	0.0018	−0.0002
	(0.0217)	(0.4232)	(0.2160)	(0.6337)	(0.2344)	(0.3181)	(0.7724)
Restaurant: some coverage	0.0021	0.0661	0.0015	0.0003	−0.0001	0.0006	−0.0008
	(0.6487)	(0.4298)	(0.4213)	(0.6431)	(0.7014)	(0.6585)	(0.0376)
Restaurant: qualified	−0.0037	0.1909	−0.0009	0.0001	0.0003	0.0027	−0.0007
	(0.2729)	(0.0077)	(0.5986)	(0.9008)	(0.5046)	(0.0674)	(0.1760)
Restaurant: 100% smoke-free	0.0086	0.0587	0.0030	0.0016	0.0008	−0.0005	0.0002
	(0.0069)	(0.5327)	(0.0873)	(0.0370)	(0.0224)	(0.6979)	(0.6862)
Bar: some coverage	−0.0044	−0.1094	−0.0005	−0.0002	−0.0001	0.0010	0.0007
	(0.1071)	(0.1528)	(0.7575)	(0.7630)	(0.8154)	(0.4293)	(0.0639)
Bar: qualified	−0.0007	−0.6368	0.0023	0.0020	−0.0004	−0.0021	0.0009
	(0.9096)	(0.0002)	(0.6921)	(0.2119)	(0.6194)	(0.4445)	(0.4465)
Bar: 100% smoke-free	−0.0035	−0.0688	−0.0021	0.0001	0.0001	0.0004	0.0006
	(0.1970)	(0.2981)	(0.1469)	(0.9338)	(0.8257)	(0.7623)	(0.1207)
Number of counties	2,666	2,432	2,449	2,449	2,449	2,449	2,449

Notes: Each column represents a separate regression. The unit of observation is county/quarter. Each cell is weighted by the number of births. Each regression controls for the fraction of "young" and "old" mothers, married, finished college, white, and cigarette prices including taxes. *p*-values are in parentheses.
LBW, low birth weight; VLBW, very low birth weight.

To examine the possibility that smoking bans may have different effects based on whether the mother smokes, we segregate the data by smoking status and estimate Equation 1 for each type of mother (the results are similar when all mothers are included). Because of the multiple comparisons, we use the Bonferroni correction. Because we have nine variables of interest (3 types of bans × 3 different places), we used a p-value of .0056 (.05/9 = .00555...) as the cutoff for statistical significance. We begin with the results for just the smokers. One of the mechanisms through which a smoking ban could improve neonatal health outcomes is by raising the social cost of smoking and therefore either shifting mothers from smokers to nonsmokers or by inducing them to reduce their cigarette consumption. The first possibility is presented in Column 1 of Table 2. In workplaces with "some coverage" (the most limited restriction), the fraction of smoking mothers appears to increase by .9 percent. The remainder of smoking bans are not statistically significant after considering the Bonferroni correction. These results are puzzling as there is no theoretical reason why smoking restrictions with limited coverage should lead to more mothers reporting smoking while pregnant. Looking at the sample means, the increase is relative to only 17 counties with no laws. The coefficient may be reflecting some unobserved characteristics common to the no law areas. As part of the sensitivity analyses, we dichotomized all types of bans as those with 100 percent smoke-free implications versus all others. Our results did not change qualitatively and are available upon request.

Column 2 of Table 2 reports the results when the outcome is the average daily number of cigarettes reported. Again, we find little evidence that smoking bans have an effect on smokers. Indeed, only "qualified" bar restrictions have a statistically significant effect on consumption, but the effects are very small, a decrease in consumption by .6 cigarettes per day. The other bans do not have a statistically significant effect in the preferred specification. The 95 percent confidence intervals suggest any potential reduction in consumption due to these bans would be small.

Given these results, when we turn to examine the health outcomes for smokers in the remaining columns of Table 2, it is not surprising that the estimated effects are essentially zero. In Columns 3–5, the outcome is premature births as defined by differing degrees of prematurity. In none of the specifications do we find a statistically significant effect on premature births with the Bonferroni correction. All of the coefficients are also very small. Taken as a whole, we conclude smoking bans do not meaningfully affect the smoking behavior of pregnant women. Therefore, it is not surprising to find no significant and positive health effects on smokers' newborn babies.

The primary mechanism through which smoking bans could improve neonatal health outcomes is by removing smokers from the presence of nonsmokers, that is, reducing second-hand smoke. The results when we look at the health outcomes for nonsmokers are in Table 3. The only restriction that might possibly matter is "qualified" restaurant restrictions, but the effect on prematurity (≤36 weeks) is small. There are no significant effects on other levels of prematurity. In Columns 4 and 5 of Table 3, we look at the effect of bans on low birth weight and very low birth weight. None of the bans come close to the standard level of statistical significance. Additionally, all of the coefficients are small relative to the underlying means. We unfortunately conclude that smoking bans do not improve birth outcomes for nonsmoking mothers.

In addition to the results presented here, we also limit our sample to counties where the majority of women who work do so in their county of residence. This reduces the possibility that county-level smoking bans are not binding on working women because they work in a different county without a smoking ban. Using data from the 2005–2009 American Community Survey on commuting characteristics (U.S. Census Bureau 2005–2009), we limit our sample to those counties in which ≥70 percent of working women work in the same county where they reside. In the median county, approximately 70 percent of women who work, work in their county of residence. With this restriction, our conclusions do not change: there is no effect of smoking bans on neonatal health outcomes. Unfortunately, there is no similar data on people's restaurant travel.

We next consider mothers with less than a high school education. As mentioned earlier, women with low levels of education may not be aware of the potentially damaging effects of smoking or may just have lower levels of self-control. Smoking bans may affect these women differently than the general population. However, when we look at prematurity, there are mixed results. Restaurants with "qualified" restrictions seem to reduce prematurity by a small amount. The other bans do not affect prematurity, but bars with "qualified" restrictions may increase "very premature" births by a small amount. Again, it is difficult to imagine a process where limiting smoking would lead to an increase in prematurity. Given the lack of a theoretical reason for this result, we consider this potential increase in very premature births as likely a "statistical aberration" and not the true effect of this type of restriction.

Lastly, we consider the effects of smoking bans on teen mothers (Table 5). Given their age, these mothers have likely been smoking for a limited time period and therefore may be less "committed" to smoking than older mothers. Unfortunately, we again see no plausible effects on either

Table 3: Nonsmokers: Smoking Bans and Health Outcomes

	Preterm	*Very Preterm (≤32 weeks)*	*Extremely Preterm (≤28 weeks)*	*LBW*	*VLBW*
Work: some coverage	−0.0008 (0.3853)	−0.0005 (0.0740)	−0.0002 (0.0576)	−0.0004 (0.3584)	−0.0003 (0.0512)
Work: qualified	0.0004 (0.9053)	−0.0004 (0.4792)	−0.0004 (0.0639)	0.0018 (0.1284)	−0.0000 (0.9639)
Work: 100% smoke-free	0.0014 (0.3721)	−0.0001 (0.8598)	0.0003 (0.3293)	0.0009 (0.4083)	0.0003 (0.1778)
Restaurant: some coverage	0.0011 (0.4035)	−0.0001 (0.8441)	0.0001 (0.4332)	0.0007 (0.3152)	0.0002 (0.1963)
Restaurant: qualified	−0.0057 (0.0017)	−0.0005 (0.2557)	−0.0001 (0.5515)	−0.0015 (0.1643)	−0.0004 (0.1618)
Restaurant: 100% smoke-free	0.0007 (0.5023)	0.0005 (0.1343)	0.0000 (0.8142)	0.0001 (0.8418)	0.0000 (0.9560)
Bar: some coverage	0.0016 (0.2110)	0.0004 (0.3104)	0.0000 (0.7702)	−0.0006 (0.4821)	−0.0002 (0.3011)
Bar: qualified	0.0108 (0.0409)	0.0026 (0.0202)	0.0010 (0.0255)	0.0001 (0.9693)	0.0007 (0.3278)
Bar: 100% smoke-free	−0.0002 (0.8342)	−0.0001 (0.7684)	−0.0000 (0.8218)	0.0003 (0.5454)	−0.0001 (0.5834)
Number of counties	2,662	2,662	2,662	2,662	2,662

Notes: Each column represents a separate regression. The unit of observation is county/quarter. Each cell is weighted by the number of births. Each regression controls for the fraction of “young” and “old” mothers, married, finished college, white, and cigarette prices including taxes. p-values are in parentheses.
LBW, low birth weight; VLBW, very low birth weight.

Table 4: Mothers with Low Education: Smoking Bans and Health Outcomes

	Preterm	*Very Preterm (≤32 weeks)*	*Extremely Preterm (≤28 weeks)*	*LBW*	*VLBW*	*Smoking*	*Number of Cigarettes*
Work: some coverage	0.0011	0.0003	−0.0002	0.0016	−0.0000	−0.0010	0.0056
	(0.4357)	(0.4896)	(0.4445)	(0.0769)	(0.8591)	(0.8508)	(0.9463)
Work: qualified	0.0081	0.0015	−0.0000	0.0061	0.0010	−0.0020	−0.2033
	(0.0441)	(0.1558)	(0.9187)	(0.0022)	(0.0462)	(0.8566)	(0.2098)
Work: 100% smoke-free	−0.0010	−0.0006	−0.0004	−0.0016	−0.0006	0.0151	−0.1181
	(0.6551)	(0.5674)	(0.2049)	(0.3052)	(0.1221)	(0.1357)	(0.4609)
Restaurant: some coverage	0.0013	0.0002	0.0002	0.0004	0.0002	0.0023	−0.0932
	(0.5351)	(0.8165)	(0.4363)	(0.7518)	(0.4613)	(0.7558)	(0.3767)
Restaurant: qualified	−0.0061	−0.0001	0.0001	−0.0006	−0.0004	−0.0086	−0.1685
	(0.0027)	(0.9193)	(0.7237)	(0.6883)	(0.3266)	(0.4803)	(0.4199)
Restaurant: 100% smoke-free	0.0030	0.0013	0.0001	−0.0005	−0.0000	−0.0010	−0.0912
	(0.0926)	(0.0417)	(0.6671)	(0.6072)	(0.9063)	(0.8732)	(0.3333)
Bar: some coverage	0.0013	0.0007	0.0003	0.0003	0.0003	−0.0045	−0.0293
	(0.5555)	(0.2667)	(0.1200)	(0.8495)	(0.1176)	(0.5664)	(0.8249)
Bar: qualified	0.0161	0.0035	0.0014	0.0019	0.0009	0.0003	0.0968
	(0.0195)	(0.0000)	(0.0006)	(0.6146)	(0.1653)	(0.9855)	(0.7055)
Bar: 100% smoke-free	−0.0029	−0.0003	0.0001	0.0003	0.0002	0.0011	0.1764
	(0.0420)	(0.5323)	(0.7401)	(0.7182)	(0.2994)	(0.8033)	(0.0224)
Number of counties	2,925	2,925	2,925	2,925	2,925	2,506	2,503

Notes: Each column represents a separate regression. The unit of observation is county/quarter. Each cell is weighted by the number of births. Each regression controls for the fraction of "young" and "old" mothers, married, white, and cigarette prices including taxes. *p*-values are in parentheses. LBW, low birth weight; VLBW, very low birth weight.

Table 5: Young Mothers: Smoking Bans and Health Outcomes

	Preterm	*Very Preterm (≤32 weeks)*	*Extremely Preterm (≤28 weeks)*	*LBW*	*VLBW*	*Smoking*	*Number of Cigarettes*
Work: some coverage	0.0008	0.0002	−0.0003	0.0005	−0.0001	0.0323	0.0033
	(0.5172)	(0.7436)	(0.1732)	(0.4961)	(0.5848)	(0.3917)	(0.3543)
Work: qualified	0.0051	0.0018	0.0006	0.0059	0.0017	−0.0251	0.0037
	(0.2015)	(0.0468)	(0.3127)	(0.0012)	(0.0107)	(0.8276)	(0.7410)
Work: 100% smoke-free	0.0007	−0.0000	0.0001	−0.0005	−0.0002	−0.1153	0.0121
	(0.7329)	(0.9671)	(0.8496)	(0.7607)	(0.6588)	(0.2498)	(0.2137)
Restaurant: some coverage	0.0006	0.0003	0.0007	−0.0001	0.0006	−0.0108	0.0018
	(0.7462)	(0.6857)	(0.0649)	(0.9655)	(0.1713)	(0.8390)	(0.7128)
Restaurant: qualified	−0.0041	0.0010	0.0005	−0.0006	0.0003	−0.0522	−0.0015
	(0.0085)	(0.1905)	(0.2848)	(0.6310)	(0.6292)	(0.5785)	(0.8222)
Restaurant: 100% smoke-free	0.0013	0.0008	0.0002	−0.0005	−0.0002	0.0495	0.0068
	(0.4260)	(0.2571)	(0.5475)	(0.5912)	(0.5549)	(0.3434)	(0.1325)
Bar: some coverage	0.0001	−0.0001	−0.0000	−0.0013	−0.0003	−0.0993	−0.0109
	(0.9489)	(0.8704)	(0.9916)	(0.4297)	(0.4037)	(0.0439)	(0.0105)
Bar: qualified	0.0150	0.0028	0.0006	0.0016	−0.0004	0.0585	0.0006
	(0.0005)	(0.0035)	(0.3154)	(0.4814)	(0.6025)	(0.6164)	(0.9497)
Bar: 100% smoke-free	−0.0035	−0.0007	−0.0001	−0.0007	0.0002	0.0299	−0.0059
	0.0100)	(0.1378)	(0.7190)	(0.1997)	(0.2953)	(0.5085)	(0.0509)
Number of counties	2,760	2,760	2,760	2,760	2,760	2,341	2,345

Notes: Each column represents a separate regression. The unit of observation is county/quarter. Each cell is weighted by the number of births. Each regression controls for the fraction of mothers who are married, finished college, white, and cigarette prices including taxes. *p*-values are in parentheses.

prematurity or low birth weight. Only the “qualified” bar restrictions meet conventional levels of statistical significance; they should have no effect on teenagers, as they are not of legal drinking age. In addition, the estimated coefficients are positive and again likely to be a “statistical aberration.”

Discussion

In the past 20 years, numerous cities, counties and states have passed smoking bans that affect workplaces, restaurants, and bars. The primary rationale for these bans is to reduce second-hand smoke exposure for nonsmokers. In addition, because these laws increase the cost of being a smoker, it is possible that they reduce the number of smokers or the consumption of cigarettes. Given the medical evidence on the dangers of maternal smoking (both active and passive), smoking bans may improve neonatal health outcomes by either reducing maternal smoking or second-hand smoke exposure of nonsmoking pregnant women.

We find limited evidence that these smoking bans have a material impact on neonatal health. They seem to neither induce pregnant women to stop smoking nor reduce the number of cigarettes consumed in a meaningful way. Therefore, it is not surprising that we find no effect on health outcomes of neonates born to mothers who report smoking. From a public health perspective, it is disappointing that we also find no effect on the newborns of nonsmokers.

Our findings are consistent with those of prior methodologically similar studies. For example, in a study examining effects of state-level smoking bans, Markowitz et al. (2013) observed little to no effect on average birth weights and number of gestation weeks in an overall and stratified by maternal characteristics (e.g., age and education) samples. To better evaluate our findings with respect to those by Markowitz et al. (2013), we also ran models with state-level laws. In none of the specifications did our conclusions change: smoking restrictions have no effect on birth outcomes (results are available upon request).

Why do we find little to no effect of these laws on neonatal health outcomes? One possibility given the public health information on smoking and pregnancy, pregnant smokers who continue to smoke are not likely to be deterred by a smoking ban. Indeed, previous research has shown that at least some of the negative effects of smoking are due to selection: women who choose to smoke while pregnant are less likely to have a healthy baby regardless of whether they smoke (Fertig 2010).

In conclusion, while smoke-free laws may have other health or social benefits, we find at the aggregate level, these laws do not improve health outcomes for newborns. At the same time, it is important to note that these laws may have health benefits for older children or the general population. It is also possible that these laws slowly change social norms and in the long run will reduce maternal smoking.

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Supporting Information

Additional supporting information may be found in the online version of this article:

Appendix SA1: Author Matrix.