# The Intergenerational Consequences of Tobacco Policy

## A Review of Policy's Influence on Maternal Smoking and Child Health.

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Tobacco policy has long been used to incentivize healthy behavior. Pregnant women are a group of particular interest due to their unique position to pass health capital down to the next generation. We create a resource for economists, policy analysts, and public health professionals by critically reviewing and synthesizing the findings from relevant papers on this topic. We first evaluate the use of cigarette taxes as a natural experiment and discuss the econometric models typically employed. By carefully comparing estimates across papers we highlight several interesting findings; most notably, while pregnant women are responsive to taxes and taxes improve child health, their responsiveness has declined over time. We show that these trends are consistent with a change in the composition of smoking mothers; specifically, the least addicted smokers quit in the 1990s, leaving the pool of smoking mothers to be dominated by less price elastic smokers in the 2000s. We then turn to reviewing other tobacco policies with a focus on clean indoor air laws. Among other things, we show that the effect of a state-level U.S. ban is roughly three times the effect of a 10% increase in prices using elasticity estimates from more recent periods. Throughout this review, we identify areas for improvement in the literature and offer a number of ideas for promising future research projects.

JEL Codes: H2, I1, J1

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## I. Introduction

In the wake of increasing expenditures on health care, policymakers continue to look for cost-effective ways to improve health. One of the most modifiable behaviors for health risks is tobacco use, making it a natural target for policies seeking to defray costs by improving health. At the same time, a growing literature suggests that investments in the early life environment of a child lead to disproportionately large returns to health (for a recent review of this literature, see Almond and Currie (2011a)). This paper reviews a literature at the intersection between these two issues: the use of tobacco policies to reduce smoking during pregnancy and to improve child health outcomes. We focus on the smoking behavior of pregnant women because they are in a unique position to pass health capital down to the next generation. Moreover, public health professionals have identified smoking during pregnancy as the greatest risk factor for low birth weight that can be modified through maternal behavior (Kramer, 1987; Shiono, 1995). Thus the literature on smoking during pregnancy is of particular interest for a review as it directly associates health behavior to the intergenerational transmission of health capital in a way that can (potentially) be influenced by policy.

Smoking during pregnancy poses a large range of health risks to both the mother and child. The 2014 Surgeon General's Report concludes that nicotine negatively affects maternal and fetal health in a variety of ways. Prenatal smoking contributes to adverse pregnancy and birth outcomes such as preterm delivery, fetal growth restriction, placenta previa, placental abruption, sudden infant death syndrome (SIDS), some congenital anomalies, ectopic pregnancy, and reduced preeclampsia risk and is associated with stillbirth and spontaneous abortion. Furthermore, maternal smoking has been causally linked to worsened post-birth outcomes for children such as reduced lung function in infants and has been associated with disruptive behavioral disorders in children such as attention deficit hyperactivity disorder.

While smoking during pregnancy has declined over time, it remains a substantial concern to maternal and child health. Even when women are exposed to the best practices for prenatal smoking cessation, many continue to smoke during their pregnancies. Evidence suggests behavioral counseling leads to as little as an additional 1 in 20 women quitting smoking during pregnancy (Lumley et al., 2009); universally applied best-practice counseling is projected to reduce maternal smoking by no more than 1% in the U.S. (Kim et al., 2009). It is perhaps unsurprising that, despite these efforts, over 400,000 live-born infants in the United States are

exposed in utero to tobacco from maternal smoking annually (Hamilton et al., 2012; Tong et al., 2013).

On the other hand, policies such as increases in cigarette taxes and clean indoor air laws (CIALs) that restrict or ban smoking in various venues have made some progress in reducing childhood exposure to smoke, both directly through impacts on the incidence and intensity of smoking and indirectly through second hand exposure (Surgeon General's Report, 2014). In the U.S., the federal government, all 50 states and the District of Columbia, and many local governments tax tobacco products. CIALs began to be implemented sporadically in the 1970s, but CIAL coverage has expanded dramatically in recent years. As of 2013, smoking restrictions are enforced in 36 states and 22,576 municipalities, covering about 82% of the U.S. population (Hyland, Barnoya and Corral, 2011; American Nonsmokers' Rights Foundation, 2016a and 2016b).

In Figure 1, we show trends in smoking during pregnancy alongside changes in the state cigarette excise tax over time. Maternal smoking rates have plummeted in recent years, however, the rate of the decline has not been constant; while maternal smoking rates decreased steeply until around 2003, the rate of decline slowed in subsequent years. Conversely, we see almost the opposite pattern in state excise taxes, whose growth rate increased after the year 2000. This graph illustrates a paradox that we investigate in much more careful detail in this paper: if cigarette taxes truly decrease maternal smoking, why does the slackening decline in maternal smoking correspond to a period in which tax rates were rapidly increasing? Is this due to a changing relationship between taxes and maternal smoking, or are both taxes and smoking during pregnancy less related than a large earlier literature would lead us to believe?

Figure 2 displays the trends in smoking ban policy coverage over time, as measured by the proportion of the national population (excluding territories) that is covered by at least one 100% smoke-free indoor air law.[1] The earliest restrictions were imposed in the early 1990s at the municipality- and county-level in California. Growth in ban coverage was relatively slow until the late 1990s, with a small spike in 1998 representing California's statewide ban on smoking in workplaces, bars and restaurants (the first state to do so) and a large spike and break in trend around

[1] Authors' own calculations based on smoking restrictions in workplaces, restaurants and bars at the municipality-, county- and state-level reported by the American Non-smokers' Rights Foundation.

2002, when restrictions became increasingly widely implemented. By 2009, national coverage reached over 70%.

Figures 3A and 3B show correlations over time between smoking and child health, as measured by the proportion of births classified as low birth weight (under 2500 grams). The naïve correlation between low birth weight status and smoking for the full sample (Figure 3A) is negative until recent years, which may be surprising but is almost certainly affected by omitted variables and secular trends in both variables[2]. Figure 3B focuses on mothers with a high school education or less.[3] For this group, the relationship between low birth weight and prenatal smoking is positive over the entire period. Figure 3B also indicates that less educated mothers are more likely to smoke and more likely to have low birth weight children overall, implying that if there are health gains from reductions in smoking, these health gains are likely to be focused on mothers with low human capital. Much of the research we review attempts to shed light on these national time series trends by looking at plausibly exogenous policy changes at the state, county, or municipal level.

We believe that there are several key elements that distinguish this review from other surveys that focus separately on either smoking or the importance of a child's early life environment. First, we look specifically at how policy affects prenatal smoking and the subsequent smoke exposure of children. This decision is supported by the medical literature which suggests that the long term damage from early life smoke exposure is extreme and comprehensive. Smoking during pregnancy is a relatively common adverse health behavior that has considerable impacts on child health – and thus in turn on the intergenerational transmission of human capital. Secondly, smoking is unique in that it has been identified as one of the most modifiable risk factors for birth outcomes (Shiono, 1995). Therefore it is particularly relevant for policy seeking to improve early life health. By examining the intersection of tobacco policy and the health impacts of early life environment, we believe this study is unique in providing an understanding for the scope for tobacco policy to affect maternal behavior and whether this can be used to realize high returns at a relatively low cost. To this end, we include a discussion of the estimated price elasticity of smoking during pregnancy and describe how this elasticity has differed over time and across

[2] For example, Buckles and Guldi (2016) show that the rise in the proportion of low birth weight births over the period from 1989 to the mid-2000s is in part explained by trends towards (usually elective) early term inductions, which are causally related to low birth weight. Starting in the mid-2000s (when early term inductions begin to fall), the correlation between smoking and low birth weight becomes positive.

[3] This group of mothers are the least likely to deliver by cesarean section and undergo non-medically indicated inductions (i.e. are the least like to elect to deliver early); they are also the group that experienced the smallest rise in cesarean sections over the period 1996-2003 (Darney et al., 2013; Menacker, Declercq and Macdorman, 2006; Moscariello, 2011).

subgroups. In the course of doing so, we provide a common empirical and theoretical framework against which to benchmark other work. We then apply this framework to study the impacts of other smoking-related policies – namely, smoking restrictions and bans – on maternal smoking. We additionally offer a critical guide to the policy research that has been done on how smoke exposure influences the health outcomes of a vulnerable portion of the population: children.

We apply the following criteria for including papers in this review: we include only papers that estimate the impact of policy on tobacco consumption by mothers and pregnant women and/or health outcomes for children of primary school-going age or younger. In this way we distinguish this review from studies of teen health behaviors and instead look at a potential intergenerational externality that adult smoking has on younger generations. By focusing on a discussion of tobacco policy, we hope to appeal to the interest of economists, policymakers, and public health researchers who seek knowledge about the degree to which tobacco policy can affect the intergenerational transmission of human capital. The remainder of the review is organized as follows. The next section reviews cigarette tax variation and how it has been used to study maternal smoking behavior and child health outcomes. We then examine smoke-free air laws and their effects on parental smoking, second hand smoke exposure and child outcomes. Throughout the paper we critically analyze the findings and approaches of the previous literature while providing ideas for future work. We identify both the major empirical issues and common fixes used. We conclude with a brief overview that compares the tax-based and smoke-free policies and indicate the areas for future research that we believe have the highest returns. Finally, in the online appendices to this paper we describe the relevant datasets for research in this area, including those that contain information on maternal smoking and child health and those that contain information on cigarette taxes and smoking-related policies.

## II. Cigarette taxes as a quasi-experiment

### A. Identification

Several factors make state excise taxes the most popular identification strategy used to study the effects of smoke exposure on child outcomes. Cigarette tax levels have increased steeply from 1980 to 2010 by roughly 80 cents (333%), providing the needed policy variation for estimating precise effects. Studies investigating tobacco industry responses to taxation have found pass-

through rates close to 100%, making it typical and reasonable to interpret a change in a tax as a direct change in price (Chaloupka and Warner, 2000; Gruber, 2001). Since roughly 80% of the within-state variation in prices is explained by within-state variation in taxes, this makes cigarette taxes the natural policy variation to use in models that include state fixed effects (Gruber and Koszegi, 2001).

The state cigarette excise tax identification strategy typically uses a two-way fixed-effect model to estimate tax elasticities. Exact models vary across studies but typically follow the same general form:

$$1)\ S_{i,s,t} = f(\alpha_1 \tau_{s,t} + \alpha_2 X_{i,s,t} + \delta_t + \theta_s + v_{i,s,t})$$

Here, $S_{i,s,t}$ is an indicator variable for any smoking during pregnancy by mother $i$ in state $s$ during time $t$. $\tau_{s,t}$ is the level of the cigarette excise tax in state $s$ during time $t$. $\alpha_1$ is the coefficient of interest, the percentage point change in the probability of smoking during pregnancy per unit change in taxes. From here most studies directly use $\alpha_1$ to estimate a tax elasticity.[4] Time and state fixed effects are captured by $\delta_t$ and $\theta_s$, respectively, and $X_{i,s,t}$ is a vector of maternal demographic controls. Here, state fixed effects ensure that $\alpha_1$ is identified off of within-state changes in the tax rate. The time fixed effects absorb aggregate changes over time that are not specific to any state, including any general differences in smoking before relative to after a tax increase. In this way, equation 1 above and equations 2-3 below can be seen as a generalized form of difference-in-differences.[5]

Typically, studies focus on the decision to engage in any smoking during pregnancy (i.e., smoking participation) instead of the number of cigarettes smoked.[6] The justification for this is that pregnant women are unlikely to remember the exact number of cigarettes they have smoked

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[4] Consider the equation for the elasticity of participation of smoking (Evans and Ringel, 1999): $e_d = \frac{\alpha_1}{\frac{\partial p}{\partial \tau}} \frac{\bar{p}}{\bar{s}}$. Here, $e_d$ is the elasticity of participation. If we assume full pass through, then $\frac{\partial p}{\partial T} = 1$, and the price participation elasticity can be calculated by multiplying $\beta_1$ by the ratio of mean prices to the smoking rate. Some studies directly instrument for prices using the change in the tax, others use different datasets for calculating the mean price, and still others just present estimates of $\beta_1$. In this review, we standardize the literature by reporting participation elasticities whenever possible.

[5] Often equation 1 is estimated as a linear model but is sometimes estimated with a logit, probit, or ordered probit, particularly when the outcome of interest is dichotomous or categorical.

[6] In the literature on adult smoking more generally it is common to look at transitions between categories of smokers (for example, the move from a never smoker, to a sometimes smoker, to a daily smoker, etc.). This is less frequently used when studying smoking during pregnancy, likely because these categories are not reported in the Vital Statistics, one of the commonly used datasets in this literature (described in greater detail in Appendix B).

at the time of delivery, making smoking intensity a more noisy measure than participation. However, the two outcome variables can yield very different estimated effects, in a way largely unexplored in the literature. We discuss measurement error and the identification of different treatment effects in more detail in Section II C. ii.

Following the estimation of equation 1 above, it is natural to attempt to understand how smoke exposure affects child health. One way the literature has done this is to simply estimate the reduced form policy impact of taxes on measures of child health, $H_{i,s,t}$:

$$2)\ H_{i,s,t} = g(\beta_1 \tau_{s,t} + \beta_2 X_{i,s,t} + \delta_t + \theta_s + \mu_{i,s,t})$$

Some studies combine the above equations to estimate a two stage or instrumental variables impact of smoking during pregnancy on child health, using cigarette taxes as an instrument.

$$3a)\ H_{i,s,t} = h(\gamma_1 S^*_{i,s,t} + \gamma_2 X_{i,s,t} + \delta_t + \theta_s + \varepsilon_{i,s,t})$$
$$3b)\ S^*_{i,s,t} = f(\alpha_1 \tau_{s,t} + \alpha_2 X_{i,s,t} + \delta_t + \theta_s + v_{i,s,t})$$

As these models can be thought of as a generalization of difference-in-differences estimation, the two-way fixed effects approach is subject to the same pitfalls and concerns of DID methods. We consider these issues and other methodological concerns in detail in Section II C, along with the problems they present when making comparisons across studies and potential corrections and improvements for future work.

## B. Datasets and related issues

Implementing a study on the impact of taxes on maternal smoking and child outcomes requires detailed and sometimes difficult to obtain data. The major elements to look for in a dataset include information on: pregnancy and child health outcomes, smoking behavior and smoking dynamics, a large sample size to identify potentially moderate or small policy effects, and geographical and temporal identifiers. Appendix A, Table 1 lists the major datasets available and what they offer on the dimensions discussed above. We also discuss these different elements in

more careful detail in Appendix B. The datasets described are also the most commonly used datasets to study the impacts of smoking-related policies generally.[7]

C. Methodological issues

*C.i. Local average treatment effects (LATE), non-constant price elasticities and intensive versus extensive margin effects.* The coefficient on the excise tax in equations 1-3 above should be interpreted as the effect of the tax on women who react to a price change, not of the average smoker.[8] According to the theory of rational addiction, price responsive mothers are typically younger, more budget constrained and less addicted than other smokers. Likewise, older smokers and those that smoke the heaviest are likely to quit smoking only when faced with large tax increases.[9] This implies decreasing marginal impacts of a cigarette excise tax on smoking behavior; at higher tax levels the most price sensitive mothers will have already quit, so tax elasticities should fall as the tax increases in absolute value.

Thus understanding the LATE has implications for optimal policy design as it relates to excise taxes:

1. Health benefits (in terms of induced quits and reductions in smoking intensity) of tax increases will accrue disproportionately to the most budget constrained, which may help offset critiques of the regressive nature of these taxes.

2. There are large geographic differences in smoking rates; for example, the rate of cigarette use in adults is highest in tobacco-producing states such as Kentucky (26.2%) and lowest in western and northern states, such as California (12.8%) (CDC, 2016). If the price-elastic smokers are the ones who have not yet quit in areas where prices are low: regionally targeted taxes might be more effective in locations which have a low pre-period price. Thus, a tax increase in the South – where tax rates are low and preexisting smoking rates are high – is likely to have a larger impact than in the North. Likewise, policymakers may want to target groups that traditionally have larger smoking populations, as there will be more leverage for policy to affect behavior in these areas.

3. Finally, complementarities between tobacco policies and increasing marginal health returns implies that a multi-dimensional approach to cessation policy will improve

[7] All of the studies that use variation in state excise taxes use the Tax Burden on Tobacco dataset, which is available through CDC's STATE system. However. A recent study (Pesko et al., 2016) highlights the availability of two sources of data on local prices: the Tobacco Use Supplement of the Current Population Survey and Nielsen retail data. Data on tobacco policies are described in Appendix A, Table 1B and Appendix C.

[8] This is relevant when comparing tax studies to other tobacco policies. For example, taxes are likely to affect price sensitive mothers whereas smoking bans in restaurants will most directly affect women that frequent these venues.

[9] Not all empirical findings are consistent with this prediction; for example Evans and Ringel (1999) and Ringel and Evans (2001) show that effects of tax hikes are centered on married, older, and more educated women.

> child health. The decision to smoke likely comes from weighing costs and benefits along multiple dimensions. Such dimensions include the availability of smoking venues, the price of cigarettes and information about the costs of smoking. Hypothetically, if the monetary price of cigarettes has increased to the point where the effect on cessation of a marginal price increase is low, then other policies may be relatively more efficient at influencing the "heavy smokers" who have not yet quit. For example, the psychological resources provided by behavioral counseling may be relatively more effective (than further prices increases) at getting price inelastic mothers to quit, and the impacts on the health of the children of these mothers are likely to be high. As an empirical test, one could investigate how the *interaction* between different tobacco policies effects child health in a regression framework. Little empirical work in the literature focuses on the interaction between different tobacco policies and future research could rigorously test this theory.

Different samples, methodologies and outcome and independent variables identify different effects of taxes on smoking. For example, the LATE has likely evolved over the past 15 years as taxes have (in real terms) raised cigarette prices. The most price sensitive mothers quit smoking in response to tax increases at lower price levels. Therefore, a study on the tax effects of smoking during pregnancy in the 1990s will be identifying the behavior of a different population than a study in the mid-2000s. Indeed, after 2000 taxes seem to have much smaller estimated effects on mothers than had been found in previous periods, though existing work has not yet found a consistent explanation for this shift.[10] We investigate competing explanations for decreasing price elasticities in Section II D.ii. Similarly, the dependent variable used to measure smoking can affect the LATE. In the case of smoking participation, the LATE is identified only for those mothers who would be induced to quit smoking altogether in response to a change in price (i.e. mothers who are less addicted and/or more budget constrained). On the other hand, when changes in taxes are used to identify the effects of price on smoking intensity (such as average cigarettes per day or transitions between smoking categories) the LATE will be relevant for a potentially wider set of smokers, as heavier smokers who do not quit may still try to smoke less.[11] It is also possible to combine these approaches by treating all categories together in ordered regression (Ross, Chaloupka, and Wakefield, 2006) where stages of quitting (heavy to moderate, moderate to quit,

[10] A similar pattern of declining price responsiveness to smoking has been shown for all adult smokers (Callison and Kaestner, 2014; Hansen, Sabia, and Rees; 2015).

[11] Even in the cases when smoking is measured on the intensive margin, estimated effects will not capture the impact of price changes on the intensity with which each cigarette is smoked (Adda and Cornaglia, 2006). Therefore it is possible that though a price increase reduces observed cigarette *consumption*, the net effect of a price increase on cigarette *exposure* is ambiguous.

or heavy to cold turkey) are separately considered. This could reveal impacts of taxes on margins of behavior that have not yet been studied in the maternal smoking literature.[12]

*C.ii. Measuring child health.* Economists are ultimately interested in studying the initial health stock of children, a concept theoretically postulated in Grossman (1972) as an initial endowment of health capital that depreciates with age. However overall endowments of health are not typically observed and thus researchers focus on proxies such as (continuous) birth weight and birth weight classifications, most commonly low birth weight status (LBW; defined as less than 2500 grams). An advantage of using LBW as an outcome (over continuous birth weight) is that doing so will capture the effect of a policy on children who were on the margin of being born around the LBW cutoff, i.e. children who are on average less healthy, more likely to be born to smokers and have more to gain from anti-smoking policies. Therefore, using LBW as an outcome could pick up stronger effects. As a result, many authors in this literature consider both continuous measures of birth weight and birth weight categories. Indeed, Markowitz et al. (2013) find stronger health effects of tax increases on a range of different birth weight categorical indicators (very low birth weight, low birth weight and normal birth weight) than on average birth weight.

However, there are some concerns with using multiple cutoffs in the birth weight distribution. First, using thresholds may impose artificial differences in health for children at the margin of such thresholds (e.g. a baby born at 2499 grams will be classified differently from one born at 2501 grams, despite being arguably similar in birth weight and underlying health at birth). Second, small measurement error around cutoffs can magnify measurement issues by leading to misclassification; for example, a baby weighing 2501 grams may be mistakenly recorded as 2499 grams, which has large consequences for low birth weight status but not for continuous birth weight. Third, focusing on any specific cutoff identifies primarily changes around that margin and utilizes less information in the estimates from the rest of the distribution. Finally, looking at many different thresholds means running many different models which could increase the likelihood of getting a spurious finding due to type one error.

[12] Another way in which LATEs can vary across studies is due to differences in methodologies. For example, the LATE identified from a two-stage instrumental variables strategy (e.g. using state taxes as an instrument for smoking) is likely to be different than that identified from an RD strategy (e.g. using date of conception relative to the date of a tax hike as a running variable). Relatedly, the independent variables used may change the LATE; for example, studies using state-level prices taxes may yield a different LATE than those using local or national level prices changes.

One alternative to using different birth weight thresholds such as LBW is to use quantile estimation techniques to directly estimate the effects of taxes at different points in the birth weight distribution. Quantile estimation has been used in the welfare reform literature (investigating earnings, labor supply, transfers and income) and has shown that just focusing on average treatment effects of income often misses important heterogeneity in effects across the distribution (Bitler, Gelbach and Hoynes, 2006). Regarding child outcomes, Abrevaya and Dahl (2008) use quantile estimation techniques to look at the effect of several birth inputs (including smoking during pregnancy, without using taxes as an instrument) across the birth weight distribution. Thus one avenue for future work in this literature would be to apply quantile estimation to better understand how cigarette taxes and other policies affect child health across different parts of the birth weight distribution.

Naturally, different proxies of initial health stock introduce varying degrees and types of measurement error. There are a number of problems with relying on birth weight as a proxy, as has been standard in the literature so far. The use of birth weight ignores other important dimensions of infant health such as symmetric birth status (wherein there is the same level of growth across all organs); in fact, the long term cognitive effects of low birth weight have been shown to be concentrated on symmetric births (Robinson, 2013). Only a handful of papers have considered effects of taxes (or maternal smoking) on alternative measures of infant health like gestational age and/or an indicator for a pre-term birth (gestation under 37 weeks) (Abrevaya, 2006; Markowitz et al., 2013; Yan, 2013; Yan, 2014).[13] However, one issue with these indicators of health (including birth weight) is that they are all highly interrelated; for example, babies that are born pre-term are also born at much lower weights. Regressions that do not control for alternate measure of health at birth (e.g. those that do not control for gestational age when examining effects on birth weight) will estimate an overall effect of taxes on that single health measure, including any effects that operate through other channels. To isolate impacts of policy on a single health outcome, some studies control for these alternate measures (see, for example, Bharadwaj, Johnsen and Løken (2014)). Regardless of the approach used, it is important to recognize that the

[13] One relevant biological mechanism through which prenatal smoking potentially affects health at birth is vasoconstriction in the placenta, which may initiate premature delivery and be separate from the effect of smoking on birth weight (Surgeon General's Report, 2001).

underlying parameter being estimated is different depending on whether alternate health measures are included, especially when making comparisons across studies.[14]

The APGAR score is also available on birth certificate data and is a composite rating for the health of a newborn on five scales: appearance, pulse, grimace, activity, respiration. This is arguably a more comprehensive proxy for health at birth, though it equally weights the five categories whereas some individual categories may be more relevant to a given study than others. Infant and neonatal mortality, stillbirths and spontaneous abortions are also relevant birth and neonatal outcome but they occur with low incidence in the US, making it difficult to detect effects on these dimensions.[15] A final relatively unexplored outcome in this literature is ventilator assisted births, which is directly related to medical expenses associated with delivery. Gestational age, APGAR scores and an indicator for ventilator assisted births are available in the PRAMS and on birth certificates. A relatively straightforward extension of the current literature would be to replicate earlier studies on the infant health effects of taxes and expand them to include these alternative measures of infant health.

*C.iii. Mismeasurement of smoking.* Smoking during pregnancy is chronically mis-measured in survey data. Nicotine is highly addictive, and at the same time it is considered highly taboo for a mother to smoke while pregnant. This stigma can lead mothers to hide smoking during pregnancy, contributing to high underreporting of smoking on birth certificates. Using serum cotinine levels to verify smoking status; small, non-nationally representative studies find that pregnant women under-report their smoking during pregnancy by rates of up to 35%.[16]

While economists typically do not worry about classical measurement error in a dependent variable, when the dependent variable is dichotomous then the measurement error is necessarily (and mechanically) correlated with the true value (Aigner, 1973; Brachet, 2008; Cascio, 2005; Hausman, 2001). The bias that this type of measurement error introduces is two-fold. First, false

[14] A related issue is whether or not studies control for other moderating channels, such as pregnancy complications and maternal weight gain during pregnancy, as taxes and maternal smoking are likely to affect these factors as well. The set of controls used is not always consistent across studies, so it is important to note these differences when making cross-study comparisons.

[15] A few papers focus on a specific type of infant mortality: sudden infant death syndrome (SIDS). See King, Markowitz and Ross (2015) and Markowitz (2008).

[16] Estimates of the level of under reporting come comparing survey responses to results of tests for cotinine levels in blood. Cotinine levels above a critical cutoff (typically around 5 ng/ml) indicate a regular smoker. For more details, see Boyd et al. (1998), Brachet (2008), Dietz et al (2011), England et al. (2001), Ford et al. (2008), Nafstad et al. (1996), Parazzini et al. (1996) and Shipton et al. (2009).

negatives attenuate the impact of taxes on smoking.[17] Second, for papers that implement an instrumental variables strategy to account for the endogeneity of maternal smoking, the estimated effects in the first stage are likely to be lower bounds and thus the second stage coefficients will be inflated (Brachet, 2008).[18]

Brachet (2008) offers the most extensive econometric treatment of this issue in the literature. He derives a likelihood estimator to directly determine the degree of misclassification in the data[19]:

$$4)\ L(\alpha_0, \alpha_1, P) = N^{-1} \sum_i [S_i \ln\big(\alpha_0 + (1 - \alpha_0 - \alpha_1) F_v(Z_i' P)\big) + (1 - S_i) \ln(1 - \alpha_0 - (1 - \alpha_0 - \alpha_1) F_v(Z_i' P)))]$$

where $S_i$ is the observed smoking rate, $\alpha_0$ is the probability a mother reports a false negative, $\alpha_1$is the probability a mother reports a false positive and $F_v(Z_i' P)$ is the distribution of the error term determining smoking (conditional on the relevant covariates and instruments). In order to estimate equation 4, one must make an assumption about the distribution of the error term in the equation of the demand for smoking of pregnant women, $F(v)$. Brachet assumes this is a logistic distribution allowing him to back out values of $\alpha_0$ and $\alpha_1$ in the Vital Statistics data. After calculating $\alpha_0$ and $\alpha_1$, it is relatively simple to construct the true (instrumented) probability that a mother is a smoker. This method can be used to correct for the bias in the first stage and to obtain estimates of the causal effects of smoking on an outcome of interest. The two major drawbacks of Brachet's approach are (i) that it relies upon computationally intensive maximum likelihood techniques and (ii) that to give valid estimates, the assumed distribution of $F(v)$ must be correct.[20]

In the just identified case of a single instrument (such as taxes) for a single endogenous variable (such as smoking) the downward bias on the first stage tax coefficient is equal to

[17] It is worth noting that this issue might also affect regressions in which the dependent variable is a dichotomous birth outcomes (such as LBW). Given that LBW status is determined by being above or below a cutoff, classical measurement error in birth weight may also lead to misclassification of LBW status. Moreover, the possibility that birth weight is recorded strategically with regard to the care an infant may receive (which depends heavily on birth weight status) suggests that the measurement error in birth weight status is potentially non-random (Bharadwaj, Løken and Nielsen, 2013).

[18] Intuitively, consider a Wald estimator in a model instrumenting smoking participation with cigarette taxes to estimate the impact of maternal smoking on a dichotomous indicator for child outcomes (such as low birth weight status). In this case the instrumental variables estimator is the reduced form estimate divided by the first stage correlation between endogenous smoking behavior and taxes. An attenuated relationship between smoking and taxes due to measurement error will lead to a smaller denominator for the Wald estimator and thus to a *larger* second stage effect.

[19] His approach follows the earlier literature (Hausman, Abrevaya and Scott-Morton, 1998).

[20] One can test the sensitivity of the results to other distributions, though it is never possible to be sure that the correct distribution is being used.

$(1 - \alpha_0 - \alpha_1)$. Therefore the bias in the first and second stages can directly be corrected for, given good estimates of $\alpha_0$ and $\alpha_1$. Importantly the majority of the literature falls into this just identified case; with accurate calculations of $\alpha_0$ and $\alpha_1$, the Brachet correction can be applied to both past and future work. However, obtaining estimates of $\alpha_0$ and $\alpha_1$ without making assumptions about the distribution of $F(v)$ is not straightforward. Brachet (2008) discusses the possibility of using surveys such as NHANES III (1988-1994) and the Health Survey of England that provide information on both self-reported smoking and cotinine to back out the rates of false positives and negatives. However, the very limited samples of pregnant women with valid cotinine levels in these surveys preclude reliable calculations of $\alpha_0$ and $\alpha_1$. One potential way to overcome the small sample of pregnant women in individual surveys would be to pool many small surveys that specifically test pregnant women for cotinine and conduct a meta-analysis across these datasets to estimate values for $\alpha_0$ and $\alpha_1$.[21] Alternatively, the NHANES survey is ongoing and there are rounds now available from 1999-2016, suggesting that the sample size of pregnant women with cotinine levels may have more than doubled since NHANES III. Future work could also combine multiple waves of the NHANES to directly study the effect of taxes on smoking during pregnancy using both self-reported smoking status and cotinine levels. This in turn could be used to understand how under-reporting influences traditional estimates of smoking elasticities.[22]

Finally, measurement error could potentially be better addressed in future work using recent birth certificate data. Since 2003, the standard birth certificate was revised to include a great deal of additional information, and this revised certificate has gradually been adopted at the state level. Of particular relevance to this issue is the information on average number of cigarettes smoked during pregnancy by trimester. The explicit reference to the timing of smoking as well as the information on the intensity of smoking likely reduces the conventional measurement error in smoking status. It also allows researchers to distinguish between mothers who only smoked early

[21] There are numerous smaller epidemiological studies that test for cotinine levels in pregnant women (see Brachet (2008) for a complete review).

[22] An additional problem with the use of survey smoking measures is that even if smoking is reported without error, it is not clear that these measures appropriately capture maternal and fetal exposure to nicotine and other harmful chemicals. A number of studies find that the (self-reported) number of cigarettes consumed is only loosely correlated with cotinine levels (Adda and Cornaglia, 2006 and 2010; Boyd et al., 1998; England et al., 2001) and others find that smokers substitute into cigarettes that are longer and/or contain more nicotine and tar content when faced with higher taxes, indicating that fewer cigarettes smoked does not necessarily translate to lower nicotine consumption (Adda and Cornaglia, 2006; Evans and Farelly, 1998).

in the pregnancy and subsequently quit versus those who smoked throughout the pregnancy. Surprisingly, this revised birth certificate data has not yet been widely used in the economics literature. One way to use this new information would be to compare reported smoking in a given year across states that have and have not adopted the revised birth certificate format. By matching observationally similar mothers across the two types of birth certificates, one could estimate how likely a woman is to report she is a smoker in a "pre-2003" birth certificate state relative to a woman in a given trimester in a "revised" birth certificate state. Additionally, as discussed in Section C.iv, a paper that estimates elasticities by trimester would be a useful addition to this literature.

*C.iv. The timing and measurement of child health and maternal responses.* Is there a critical time in a woman's life during which exposure to a tax hike matters for smoking behavior? From a policy standpoint, it is important to fully understand whether women are the most price-sensitive during adolescence (the most common age of initiation), pregnancy (when many women consider quitting or reducing cigarette consumption) or other periods of life. The theory of rational addiction implies that current smoking is dependent on starting and quitting decisions over many past periods and that both past taxes and expectations of future taxes matter for current smoking behavior (Becker and Murphy, 1988; Becker, Grossman and Murphy, 1994). Indeed, Gruber and Zinman (2001) find that taxes experienced during teen years (ages 14-17, when most smokers begin smoking) are highly predictive of smoking behavior during pregnancy, even conditional on current tax rates. Gruber and Koszegi (2001) find that the enactment of tax increases – even before they become effective (and conditional on current tax rates) – have anticipatory effects on both cigarette sales and smoking behavior. Given these results and the intertemporal nature of smoking decisions, when estimating equation 1 above researchers should ideally control for the tax level faced as a teen and in the future, though this is not typically done.[23] It is also worth noting that Gruber and Zinman's study has not been replicated using more recent cohorts or been expanded to look at responsiveness to taxes by mothers at significant points in life other than in teen years.

A related question is whether tax rates from early or late during the pregnancy should be used. Since the revised birth certificates now record maternal smoking by trimester, this allows

[23] Colman, Grossman and Joyce (2003) find a strong relationship between taxes faced at age 14 and maternal smoking behavior three months prior to pregnancy, though they do not examine the impact of taxes at age 14 on smoking during pregnancy.

for more precise measurement of estimates of price elasticities by trimester. In addition, smoking by trimester allows a construction of a pseudo panel with multiple observations on smoking for women at different times.[24] The advantage of this approach, which has not yet been implemented in the tax literature, is that using such a short panel compares sudden within-mother changes in smoking, therefore avoiding time-invariant confounders specific to the mother. Moreover, though existing research has estimated tax elasticities of smoking during pregnancy at different trimesters (using non-panel methods), this analysis has never been done with other types of tobacco policy. Comparing the responsiveness of smoking by trimester by different policies could shed light on what types of interventions matter most in terms of the timing of maternal smoking.

Timing is also important for estimating the effects of tobacco taxes on child health. Not only do taxes from previous periods impact smoking during pregnancy, but the influence of maternal smoking on child health can vary over the course of a pregnancy.[25] Simon (forthcoming) addresses this issue by including the taxes faced in all three trimesters of pregnancy within the same regression framework and finds that across adult health outcomes, the effects of in utero exposure to smoking consistently loads onto exposure in the third trimester. More precisely pinning down when smoking matters for child health is also easier with data on smoking by trimester now available in the 2003 version of the revised birth certificate. Though (as far as we know) this has not yet been explored through comparative regressions of the effect of taxes on infant health across different trimesters in the Vital Statistics.

*C.v. Differential state trends and policy endogeneity.* Causal interpretation of difference-in-difference estimates is based on the assumption of no differential preexisting trends between treatment (tax-increasing or large tax hike) and control (non-tax increasing or relatively small tax hike) groups. Critics of the excise tax identification strategy point out that enacting tax changes depends on state legislation, which is influenced by state political and demographic trends; this issue is sometimes referred to as policy endogeneity. Such trends are problematic if they are related to determinants of maternal smoking and/or child health. For example, if long term trends in health are improving more for states that increase taxes, this will result in a spurious relationship between taxes and our outcomes of interest. Similarly, if smoking is becoming less prevalent and

[24] One working paper that takes this approach is Pesko, Seirup and Currie (2016), which estimates the effect of minimum e-cigarette purchase age on smoking and child health.
[25] The available data to do this is discussed in detail in Appendix B.

declining in states that end up passing legislation then this reverse causality could cause the coefficient on the excise tax to be over stated. Spurious differential trends could also be driven by demographic changes or the enactment of other policies that happen to occur around the time that taxes are being implemented. We now review the major tests for differential trends used in this literature, discuss their shortcomings and offer alternative methodologies.

*C.v.a. Controlling for observables.* The most basic test for policy endogeneity is to control for other policies or demographic trends occurring at the same time as cigarette taxes. Sensitivity of the tax coefficients to the addition of these trends can be taken as a sign that the natural experiment of taxes in this context is compromised.[26] However, this only works if the correct controls are chosen, and so far the literature has not landed on a consistent set of policies that need to be controlled for.

We believe the most important policies to control for are those that are changing at the same time as cigarette taxes and that have had the largest effects on maternal and/or child health.[27] During the 1990s there were expansions to Medicaid and the Children's Health Insurance Program (CHIP) which have had documented impacts on infant health, infant mortality and childhood health (Currie and Gruber, 1996a and 1996b; Howell and Kenney, 2012; Wherry and Meyer; 2015).[28] Welfare reform was also occurring in some states at this time which could have influenced child living arrangements, and through this channel, child health (Bitler, Gelbach and Hoynes, 2005 and 2006). In addition to the above, the unemployment rate has been shown to affect both smoking and child health (Ruhm, 2004; Dehejia and Lleras-Muney, 2004). Similarly if smoking and drinking are substitutes, then it could be important to also control for alcohol taxes. Given concerns over state trends in anti-smoking sentiment driving changes in cigarette taxes, DeCicca, Kenkel and Mathios (2002) and DeCicca et al. (2008) construct a state-year measure of anti-smoking sentiment that can be used to directly model and absorb one of the most intuitive sources of policy endogeneity. Controlling for anti-smoking sentiment is a common robustness test. To the degree that there is reverse causality (smoking trends causing changes in tobacco policy), one option is to include lead values of the tax rate. If the cigarette tax in the year after

[26] Alternatively, responses of "predetermined" demographic variables (or other variables that should not be affected by taxes) to tax changes sheds doubt on the exogeneity of tax variation.
[27] See Currie and Rossin-Slater (2015) for a comprehensive review of such policies that are changing at this time.
[28] Hoynes and Luttmer (2011) provide a detailed description in their data appendix on how to construct proxies for SCHIP and Medicaid eligibility.

birth has significant impact on health or smoking prior to birth (or if controlling for this changes the effect of taxes during pregnancy) then this is one sign of potential reverse causality.[29] Finally, it is important to control for other tobacco policies, which are likely to be correlated with taxes and directly affect smoking (and thus indirectly affect child health).[30] We discuss other policies in great detail in Section III.

One parametric method for taking into account differential trends is to include state-specific time trends (usually linear) in equations 1-3 above. Showing robustness to the inclusion of state-specific linear trends has become a standard practice in this literature. Nonetheless, there are drawbacks to this approach. Including these trends can absorb causal variation in taxes on smoking and health that would otherwise be used to identify the treatment effect (Hansen, Sabia Rees, 2015; Neumark, Salas and Wascher, 2014). This could be the case if a policy changes the post-policy trend in smoking and/or child health (and thus the average trend, which is captured by linear time trends). Additionally, the parametric assumption of linearity may be too strong in some cases; over a long time series, linear time trends may fail to absorb the influence of all confounding state-specific factors, especially in cases where underlying trends are nonlinear in nature. Ultimately, we turn to other additional tests for policy endogeneity and differential trends.

*C.v.b. Using "large" tax hikes in event study and synthetic control methods.* Some studies address the endogeneity of state policy by focusing on how a "large" tax increase in a single state (or several states) affects the outcomes of interest. Control states are selected out of the pool of states that did not have a tax increase during this time. One of the main benefits of focusing on a single policy change is that it allows researchers to graph pre-trends in the outcome variables relative to the occurrence of a tax hike. This approach (used in DeCicca and Smith (2012) and Lien and Evans (2005)) can be done through a matching estimator (such as propensity scores) where control states are chosen so that the average outcome between the tax increasing state and control states

[29] However, to the extent that changes in taxes are known ahead of time (e.g. if tax changes are announced ahead of implementation), statistically significant coefficients on leads of taxes may simply reflect anticipatory effects.

[30] Another related issue is that of smuggling, bootlegging and spillovers across tax jurisdictions, i.e. the idea that taxation in one area may drive a consumer to purchase cigarettes from neighboring areas. This possibility has been recognized and evidenced in a number of studies (Anger, Kvasnicka and Siedle, 2011; Baltagi and Levin, 1986; Carpenter 2009; Chaloupka, 1992; Chaloupka and Saffer, 1992; Chaloupka and Weschler, 1997; DeCicca, Kenkel and Liu, 2013b; Fleenor, 1996; Lovenheim, 2008; Ross and Chaloupka, 2004; Tauras, 2005b; Thursby and Thursby, 2000; Warner, 1982; Wasserman et al., 1991; Yurekli and Zhang, 2000).

are similar in the years leading up to the tax increase.[31] If the results from using this "single state" method are qualitatively similar to the two way fixed effect regression and additionally no differential pre-policy trends across treatment and control states are observed, then this approach provides a robustness check to the results from estimating equations 1-3 above. However, this strategy has several important limitations. First, it is not obvious what magnitude of a tax should be used to qualify a state as being "treated." Second, it is also unclear how to deal with multiple large tax hikes within a state. Relatedly, if the tax changes occur in a short time of each other (such that one tax increase exists in the pre- (or post-) period of another) it is unclear what constitutes a "pre" versus a "post" policy period.[32]

Simon (forthcoming) uses an approach that reformulates equations 1 and 2 as an event study. The sample is restricted to only states that enact hikes above various thresholds (25%, 50%, and 85%) and these tax increases are then treated as dichotomous policy events. The tax coefficient in equations 1-2 above are substituted with a vector of dummies that indicate the number of periods before or after the policy event. The coefficients on the dummy variables can then be plotted to show the evolution of the outcome variable relative to when the tax hike is implemented, providing a graphical test of the parallel trends assumption. It is also possible to test whether pre-period outcomes between the treatment and control groups are statistically different from each other. Because the sample is only composed of states that eventually implement a "large" tax hike, identification comes from only variation in the timing of such tax hikes rather than the selection into enacting large tax hikes. To deal with the issue of multiple policy changes within a single state, Simon uses as the event only the first tax hike above the specified threshold to occur in a state; and as an alternative event study specification replicates observations in each state by the number of events, assigns each replicated state to a different event, and then runs the regression down-weighting each observation by the number of times it was replicated.[33]

[31] Conceptually, this is very similar to controlling for state-specific time trends but has the added benefit of being able to more flexibly capture differential pre-trends (e.g. trends that are potentially nonlinear) and the disadvantage of limiting inference, as only a single treatment state is used.

[32] Moreover, when multiple tax hikes are examined it becomes unclear how to aggregate the results to provide a single, generalizable, average treatment effect. For example, Lien and Evans (2005) select control states to individually estimate tax hikes of 25 cents or more and then the average treatment effect across these experiments. However, because their methodology can use control states multiple times for each of the treatment states, averaging across the treatment effects gives more weight to control states used multiple times.

[33] See Simon (forthcoming) and the associated online appendix for details on how the event study was constructed and estimated.

An alternative way of approaching the issue of differential trends involves employing a recent econometric innovation, the synthetic control method (Abadie and Gardazabel, 2003). Abadie, Diamond and Hainmueller (2010) popularized the synthetic control approach by illustrating how an anti-tobacco program (proposition 99) in California decreased cigarette consumption. While in an early application of synthetic control methods were used to evaluate tobacco policy generally, they have not been employed to study other cigarette excise tax increases. Given a state that has experienced a tax increase, the synthetic control method selects control states whose trends in the time before the tax increase can be most efficiently weighted together to jointly match the trends in the treated state. This provides a data-driven method for selecting control groups, while directly adjusting for comparable pre-trends in the outcome variable. The weights assigned by synthetic controls are then used to project a policy counterfactual of the evolution of smoking (or child health) in the treated state if no tax increase had occurred. Unlike the matching method described above, which requires that the treatment state and each of the control states have matching pre-trends, the weighted "synthetic" control state is constructed in a way that it is guaranteed to have pre-trends matching the treatment state. Since all available control states are used in the synthetic control estimation, information from the entire sample is considered in the creation of a policy counterfactual.[34]

Synthetic control methods have yet to be used in the literature on taxes and maternal smoking and thus represent an opportunity. For example, one could compare the effects of major tax increases in the past 15 years estimated using the synthetic control method versus the more conventional two way fixed effects model. Nonetheless there are drawbacks to the synthetic control method. First, the approach remains limited in that it traditionally only considers a single treatment state at a time, though new work has begun to relax this restriction (Galiani and Quistorff, 2016). Second, it is unclear how to best control for observables. Finally, when evaluating multiple outcomes (say, maternal smoking and child health) synthetic control methods will often select different control groups across these outcomes.[35] Future research on the synthetic control methodology may help overcome these limitations and provide more guidance on its use for

[34] This approach recognizes that there are problems calculating inference when the number of units in the comparison group is small and provides guidelines to using placebo-based inference to get correct p-values, though this method does not produce standard errors (Abadie, Diamond and Hainmueller, 2010).
[35] Typically in synthetic control models, only a handful of control groups are given non-zero weights.

researchers; for now, this method remains a viable robustness check for the standard two way fixed effects model.

Altogether, the methods reviewed in this section represent a variety of tests that have emerged from the literature whose usefulness will depend on the exact nature of the study. Rather than having a single standard approach to address the potential policy endogeneity, we believe that this should be regarded as a toolkit of approaches that researches can draw upon.

*C.vi. Alternative mechanisms for the effects of tobacco taxes.* We now turn to investigating the potential for an excise tax to affect smoking (or child health) in a way that does not directly operate through increased prices. This does not change the reduced form interpretation of the tax as a policy effect but affects the interpretation of a tax as a price elasticity and potentially invalidates it as an instrument for maternal smoking behavior. One possibility is that the revenue raised from cigarette taxes is used to fund anti-smoking programs or social spending which then decreases smoking or improves health. In spite of being an intuitive channel by which taxes affect smoking and/or health, this has largely been overlooked by the literature. There is a relatively straightforward test of this channel; researchers can use data from the Regional Economic Information System (REIS) to estimate the effects of tax increases on (or control for) various forms of state spending.[36] In Appendix A, Table 2, we show coefficients from regressions of (per capita spending) on a variety of social programs on the state excise tax. The signs are mixed across models and not significant, except for the case of spending on public medical assistance. Still, this suggests there is some possibility that states are using tobacco tax revenue to fund medical spending and therefore indicates that researchers should control for state spending when using taxes as a way to alter outcomes exclusively through a price channel.[37]

Endogenous demographic change also poses a concern. For example, if there is migration in response to changes in taxes, this can lead to biased estimates of the effect of taxes on smoking and on child health. Alternatively, since most of the data sources described in Appendix B are restricted to the sample of live births, an increase in taxes may change the composition of mothers

[36] The REIS data is available online at: http://www.bea.gov/regional/downloadzip.cfm.

[37] Unfortunately the REIS does not contain information on state spending on anti-tobacco campaigns, though such information is available from the CDC's STATE database (http://www.cdc.gov/statesystem/). It would also be interesting to see how spending on other anti-tobacco policies interacts with taxes to lower maternal smoking. Unveiling the "black box" by which a tax impacts smoking has broad applications for the tax and smoking literature beyond just effects of taxes on smoking during pregnancy.

and births. This would happen if a tax increases the likelihood that more vulnerable children survive to term (in other words, if there is a culling effect of smoke exposure). To test whether maternal demographic characteristics respond to changes in taxation, it is possible to put time-varying state-level demographic indicators on the left hand side of equation 1. If the tax coefficients are small and not statistically significant for a wide range of characteristics, then this is evidence against endogenous migration and changes in sample composition.

An additional way in which taxes could affect child health without directly altering a mother's smoking behavior is through shifts in exposure to environmental tobacco smoke (ETS, also called second hand smoke exposure, or SHS). For example, if pregnant women's exposure to spousal or partner smoking falls as a result of tax hikes, then birth outcomes could improve independently of the tax impact on mothers' own smoking behavior. While there is relatively little evidence on the effect of taxes on the second hand exposure of pregnant women, there is some evidence that taxes significantly decrease ETS among non-smokers (Adda and Cornaglia, 2010). In light of these alternative pathways through which taxation can impact birth and other child health outcomes, we recommend caution when using taxes as an instrument for maternal smoking.

D. Empirical findings.

In this section we discuss the findings in the existing literature on taxes, maternal smoking and child health. We note that most studies find effects of taxes on maternal smoking; however, studies using more recent data find smaller elasticities than earlier studies. We evaluate several explanations for why these elasticities have fallen over time and conclude that the most likely reason is the changing composition of smoking mothers over time. We then discuss the evidence on how the timing of tax increases (relative to pregnancy) matters for the smoking behavior of mothers. We conclude with a review of the findings on the child health effect of taxes.

*D.i. Taxation and maternal smoking: declining participation elasticities*. Table 1 summarizes the estimates of participation elasticities. Across studies and over time, mothers have decreased smoking in response to an increase in cigarette prices. The first papers to examine the relationship between taxes and maternal smoking found fairly large effects. Evans and Ringel (1999) implement the two-way state year fixed-effects model using the Vital Statistics data for mothers giving birth from 1989 to 1992. Their preferred estimates give an elasticity of -0.49 to -0.52,

implying that a 10% increase in prices would decrease maternal smoking by about 5%. Later studies follow in the same vein and find similar results.[38] Three additional studies use the Vital Statistics data mothers giving birth from 1989 to the mid-1990s and find elasticities in the range of -0.35 to -0.70 (Gruber and Koszegi, 2001; Gruber and Zinman, 2001; Ringel and Evans, 2001)[39]; Colman, Grossman and Joyce (2003) use similar methods on the 1993-1999 PRAMS data and find an even larger elasticity of -0.91. Lien and Evans (2005) address the concern that the tax increases before 1996 were on average relatively small (less than 25 cents) by looking only at a series of larger cigarette tax hikes in the mid-1990s. They find similar results when focusing on large tax increases in four different states as separate experiments using a matching method that selected control states (without tax increases) using the pre-trends in smoking.[40] Finally, using a two-stage random effects model on panel data from the 1988 National Maternal and Infant Health Survey and 1991 follow up, Bradford (2003) finds that the elasticity of smoking intensity for pregnant women is -0.34.

Table 1 also shows that there is a departure between the first papers in the literature and later studies. Notably, starting with data from the latter half of the 1990s, Levy and Meara (2006) find much smaller elasticities stemming from a large national price increase that occurred in response to the 1998 Master Settlement Agreement (MSA).[41] The authors estimate the elasticity using two sources of variation: the national increase in prices associated with the MSA (in a pre- versus post-MSA comparison) and the relative effect the national price increase had on prices at the state level.[42] They find a tax elasticity of only -0.12, much smaller than the elasticity found in previous papers and statistically insignificant. However, it is worth noting that since their primary identification strategy relies solely on time-series variation, it is difficult to differentiate time series changes in smoking preferences (or other time-varying unobservable factors) from price driven

[38] For additional results by subgroup see Appendix A., Tables 3A-3B.

[39] Note that the elasticity reported in Gruber and Koszegi (2001) is for smoking intensity; the authors do not report the elasticity for smoking participation.

[40] The authors examined tax hikes of 15 cents or more in Arizona, Illinois, Massachusetts and Michigan and estimated a price elasticity of demand of about -0.49 on average. It is worth noting that, following the norms at the time this paper was published, the authors clustered standard errors at the state-year level and checked robustness to clustering at the state level but without additional corrections for a low number of clusters (between 6 and 11). Moreover, given the focus on one treated state per experiment, modern standards suggest that a permutation test would be a more appropriate way of getting standard errors.

[41] The Master Settlement Agreement refers to a lawsuit brought by attorney generals from 48 states on tobacco companies which resulted in a large increase in cigarette prices.

[42] For example, the 43.5 cent increase was a relatively large increase in states with low pre-MSA cigarette prices but relatively small in states with high pre-MSA prices. The authors assume that both state-level excise taxes and the national price increase were exogenous in the period from 1998 to 2000.

changes in demand.[43] Using standard two-way fixed effects models on data from 1999 to 2003, DeCicca and Smith (2012) estimate an elasticity of -0.14[44]; Markowitz et al. (2013) find an average elasticity of 0.06 using data from 1996-2008, but their results are not statistically significant.[45] Simon (forthcoming) directly compares the two time periods, finding a small response to taxes in the later period (post 2000) with an elasticity of -0.15 but a much larger estimated elasticity of -0.52 in the earlier period, averaging to -0.33 when estimated across the full period.

As a whole these results suggest that mothers are relatively price inelastic, but do respond to price changes.[46] Over the entire period, a 10% increase in price results in a 1.5% to 7% decrease in smoking during pregnancy. The large difference in magnitudes across studies seems to be driven primarily by whether the estimates are taken from looking at earlier birth cohorts (1989-1997) or from studies that focus on the cohorts of mothers in the late 1990s and later. Across more recent papers, the estimates are consistently about two to three times smaller than earlier estimates, suggesting that estimates of the participation elasticity of maternal smoking has fallen over time. While the declining elasticities of cigarette taxes have been discussed for adult and young adult smokers as a whole (Callison and Kaestner, 2014; Hansen, Sabia, and Rees, 2015), this is the first work that we know of to systematically confirm the decline in the population of pregnant women through cross study comparisons. In the next section we examine potential explanations for these trends.

*D.ii. Why are elasticities declining?* We consider four causes for the apparent decline in elasticities: a change in the methodologies used by researchers, a change in the price environment of tobacco products, increased measurement error in maternal smoking, and finally, a

[43] Leavy and Meara (2006) do examine the robustness of their estimates to several alternative assumptions about secular trends in maternal smoking. The authors also use relative price changes (and thus state-level heterogeneity in pre-MSA prices in addition to nationwide temporal variation in exposure to the MSA) as a secondary strategy and find that the coefficients on the post-MSA dummy and post-MSA time trend were not statistically significant.

[44] They find an average elasticity of -0.11 using a matching strategy using large tax hikes from 5 separate treatment states. The average is weighted according to the sample size from each state. The marginal effect of taxes on smoking participation is not significant for any state (significance levels not reported for elasticities).

[45] Elasticities for each age group are calculated using the following formula: $e_d = \alpha_1 \frac{\bar{p}}{\bar{s}}$; 100% pass through is assumed. The overall average elasticity is the weighted average of the estimated elasticities across various age groups (the authors do not report regression results for the full sample). The weights are based on the relative sample size of each age group. The only age group to yield a negative elasticity is mothers aged 35 and older.

[46] Relatedly, Adams et al. (2012) find moderate elasticities of quitting during pregnancy (0.34) with respect to price.

compositional change in the population of smokers.[47] In doing so, we divide studies into two types: those using data from an earlier period before the Master Settlement Agreement (MSA) to examine smoking during pregnancy before 2000 and those that use data from the post MSA period to analyze smoking during 2000 and later.

Due to advancements in empirical techniques, the higher elasticities in earlier studies may have been subject to bias not present in more recent papers. However this seems to be an unlikely explanation; many earlier papers (Evans and Ringel, 1999; Ringel and Evans, 2001; Lien and Evans, 2005) and later papers alike (DeCicca and Smith, 2012; Markowitz et al., 2013; Simon, forthcoming) all utilize some form of the same two way fixed effects strategy represented in equations 1-3 – the standard technique used in studies that estimate the effects of state policies – for their primary coefficient estimates. Furthermore, the estimates in these papers are robust to a wide range of controls (similar across most papers) and alternate specifications, so it also seems unlikely that the differences are due to specification choice.[48]

Levy and Meara (2006) do not rely on state level variation in tax changes, but instead exploit a large national price change caused by the MSA. Their primary identification strategy estimates the degree to which there are level and trend breaks in maternal smoking directly following the price increase from the settlement. Because the identification does not come from state tax law, their estimates may reflect a different LATE than the other studies mentioned above: one that encompasses a larger geographic scope and therefore has greater external validity. On the other hand, in relying on the national change as the primary source of exogenous variation in prices, Levy and Meara's study is also potentially more susceptible to estimates that are biased by unobserved confounders. Nonetheless, as discussed above, several more recent papers (DeCicca and Smith, 2012; Markowitz et al., 2013; Simon, forthcoming) employ the more traditional state level two-way fixed effects framework and also find smaller elasticities. This suggests that the decline in elasticities was not due to a methodological change.

[47] Table 1 shows that the datasets used in the literature have been fairly constant over time (as discussed in Appendix B, there are actually few national datasets with information on smoking during pregnancy), meaning that the decline in elasticities is not due to a difference in the datasets used.

[48] However, one important methodological innovation of the later literature is the treatment of preexisting differential trends across states though the inclusion of state-specific time trends in estimating equation 1; to the extent that the earlier literature is not able to fully address the issue of policy endogeneity related to potential differential trends, estimates from those papers may overestimate the response of maternal smoking to taxes. Still, this is also unlikely to be driving the difference in findings; Simon (forthcoming) and DeCicca and Smith (2012) estimate models without state linear trends (or matching) and still find small elasticities in the later period.

The second explanation for declining elasticities is that the price environment has changed over time. This could happen if the pass through rate of taxes by tobacco companies has declined in recent years.[49] While this explanation is promising, studies have found that the pass through rate has remained roughly constant at 100% over this time period. Studies on the pass through rate of cigarette taxes to prices in the late 1980s and early 1990s estimate that it has been slightly above 100%[50]; likewise a more recent study by DeCicca, Kenkel, and Liu (2013a) estimates full pass through on the overall population of smokers.[51]

Another way in which the price environment could have changed is if the price faced by consumers has become increasingly dependent on a more local price environment (relative to the state level). This change would occur if county or municipal taxes are playing a more prominent role over time in the final cigarette price paid by consumers. Similarly, if there has been an increase over time in the evasion of taxes through more cross border, internet or Indian reservation purchases, this could weaken the relationship between the final price paid by consumers and state level excise taxes. Pesko et al. (2016) investigate the role of price environment on (all) adult smoking. They construct measures of local prices paid for cigarettes using self-reported prices in the Tobacco Use Supplement of the Current Population Survey (CPS-TUS) and scanner data from Nielsen in 2006-7 and 2010-11. The authors show that when using prices at the county (or MSA) level, the effect of a dollar increase on smoking participation rises in magnitude from -0.07 to -0.4. Higher price elasticities (in absolute magnitude) between local prices versus states prices were only among states with Native American reservations, suggesting an important role for reservation purchasing in overall cigarette price responsiveness. Thus it seems that local prices are perhaps more influential for individual smoking behavior than state prices over this recent period in states with reservations. If the relationship between local prices and smoking has strengthened over time (and the relationship between state-level prices and smoking weakened over time), this could be one explanation why studies that use changes in state-level taxation yield smaller estimates in later periods relative to earlier periods.

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[49] We would like to thank an anonymous referee for raising this point. It is important to note that one way that the price environment could change is through inflation. However, studies in the literature typically adjust for inflation in the measurement of the tax, and the time dummies in equations 1-3 should absorb national inflationary changes.
[50] See Delipalla and O'Donnell (2001), Gruber and Koszegi (2001), Keeler et al. (1996) and Sumner and Ward (1981).
[51] That said, DeCicca, Kenkel, and Liu (2013a) did find that heavier smokers, smokers who engage in more price search and smokers who buy cartons pay a lower portion of the pass through; this heterogeneity in the pass through rate has never been investigated for pregnant women.

While Pesko et al. (2016) is highly suggestive, it contains some important limitations in explaining the declining elasticities in Table 1. Due to the availability of CPS-TUS, the local price environment is essentially measured during two waves: 2006 (to early 2007) and 2010 (to early 2011). This means that the effect of local price environment is essentially identified off of differences in smoking and prices between these two years. A simple comparison of prices in these two years makes it unclear if their results account for either pre-trends in smoking or a year-specific spurious change in the measurement of smoking that a richer comparison of 2006-2011 could identify.[52] In addition, their model only includes fixed effects at the state level and does not include a full vector of fixed effects for geographic units by which the authors measure local price.[53] This implies that the estimated elasticities including "local" price environment is at least partially identified off of within state cross-sectional differences in smoking, and these differences could potentially introduce bias of their own. Finally, Pesko et al. do not specifically analyze pregnant women, whose behavior and demographics may be considerably different from the adult population. A simple extension would be to investigate whether estimated elasticities for pregnant women in recent periods are larger once the local price environment is considered (by merging the MSA and county level prices in the CPS-TUS with the Vital Statistics data) and to compute similar estimates using local prices and birth outcomes (we discuss a similar decline in the effect of state taxes on birth weight in Section II D.iii.). In addition, sub-state fixed effects could be included in such a study to absorb within-state cross sectional differences in smoking, although at the expense of eliminating within-state price variation that may be causally linked to differences in smoking.

A third explanation for the decline in elasticities is increased measurement error in self-reported smoking during pregnancy. As discussed above, false negatives in a dichotomous left hand side variable attenuates estimated coefficients. If increased knowledge of the harmful effects of smoking and societal pressures not to smoke directly cause decreases in reported (relative to actual) smoking, this would lead to higher rates of false negatives. By nature, this hypothesis is difficult to test; however, one indirect way is to examine the changes in the association between

[52] While the authors look at 3 months of data in 2007 (and an additional 3 months in 2010), it is unlikely that the prices are changing monthly within this short time period. This means identification comes from the difference over time in price from 2007 to 2010.

[53] Local area-type fixed effects are included (for example an indicator for if the local environment is a county or core based statistical area); however, this will not directly capture within-locale changes in price. Additionally, the authors control for a number of demographic controls and two time-varying anti-smoking sentiment perception measures that vary at the local level. After adding these controls it is more likely that the state cross sectional omitted variable bias is low relative to casual price variation.

improved birth outcomes and tax increases over time. In birth certificate data, birth weight is typically measured and recorded by the hospital staff, making it less subject to the same type of measurement error as self-reported smoking. Table 2 displays the estimated effect of tax increases on birth weight (in grams) and LBW status for both the marginal effects reported in the paper and as tax elasticities of infant health that we have calculated. For both these coefficient estimates and the tax elasticity of infant health, it appears that the effect of taxes on birth weight and LBW status is larger and more statistically significant in the studies using data from the early 90s and before (Evans and Ringel, 1999; Lien and Evans, 2005) than in the studies using data from the mid-1990s onward (Markowitz et al., 2013; DeCicca and Smith, 2012; Simon, forthcoming). Finally, Simon (forthcoming) compares tax responsiveness of long term child outcomes for the period 1989-2000 and 2000 and onwards and finds that none of the post-2000 estimates are statistically significant and most of them are smaller in magnitude (though still economically significant and positive) than estimates from the earlier period. In sum, papers seem to show a declining impact of taxes on both smoking and birth outcomes; because the trends in the health impacts mirror the smoking effects, it seems unlikely that the declining elasticities are fully explained by increased measurement error in self-reported maternal smoking.[54]

One final interpretation of the trend over time in elasticities is that in the earlier period there were more light/casual smokers in the market but that remaining smokers in the post-MSA period are the heaviest and most addicted smokers, who are much less likely to respond to price increases. In other words, the trend in declining elasticities is consistent with the women most sensitive to taxation already having quit by the 2000s.[55] This explanation qualitatively matches the decline in smoking rates observed over this period (see Figure 1). Another piece of evidence is that the demographic characteristics of smokers has changed over time. Notably, the subgroups with the most elastic smokers in the early 1990s are the least likely to be smokers in the post-2000 period. Appendix Tables 3A-3B display estimated elasticities by subgroups across various studies.

[54] Observing parallel trends in the effects of taxes on smoking and birth outcomes does not definitively rule out measurement error as an explanation. For example, the trends observed in Table 2 may be due to a decreasing relationship between smoking and birth weight. However, we are not aware of any evidence that indicates smoking has become less harmful to birth weight in recent years relative to earlier years.

[55] Amongst smokers as a whole there has been documented considerable heterogeneity in this pattern among age groups. Specifically, the decline in elasticities has been focused on younger adults and teens (Hansen, Sabia, and Rees, 2015) whereas some groups of older adults still seem to be fairly responsive to prices, even in the 2000s (DeCicca and McLeod, 2008). That said, an earlier study does find that overall elasticities are smaller and less significant over time, even among older studies (Gallet and List, 2003).

For example, smoking among teen mothers and mothers aged 20-24 – who were the least responsive to taxes in the earlier period – has stayed roughly the same over time. On the other hand, smoking among older mothers (above 35) – who were the most responsive to taxes in the earlier period, with elasticities nearly double that of younger mothers – has declined more rapidly in both absolute and percentage terms (Ringel and Evans, 2001; Markowitz et al., 2013). As a consequence, the pool of smoking mothers has become increasingly dominated by younger, less price sensitive mothers (see Appendix Table 3A).[56]

Additional evidence on compositional changes comes from looking at the intensive margin effects of taxation. The earliest studies looking at the intensive margin find small (and statistically insignificant) effects of taxes on cigarettes per day conditional on smoking (elasticities of -0.04 to -0.01) (Evans and Ringel, 1999). On the other hand, later studies estimate stronger and statistically significant impacts on the conditional number of cigarettes, yielding elasticities much higher in magnitude (-0.14) and more statistically significant relative to earlier ones (Levy and Meara, 2006).[57] If participation elasticities are declining because the "heavier" smokers don't quit, then it is reasonable to expect a stronger intensive margin reaction over time as smokers stop responding to price increases by quitting completely but remaining smokers have more latitude to reduce smoking on the intensive margin.

We take the above points as promising evidence that demographic changes in the composition of smokers are a significant part of the reason why elasticities have declined over time. Yet, there is more work to do on this. Perhaps the most formal test would be to employ quantile methods to estimate differences in the effect of taxes on smoking across the distribution of smokers. Along these lines, Maclean and Weber and Marti (2014) find a U-shaped response to taxes in the adult population of smokers; light and heavy smokers do not respond to a price increase, while moderate smokers reduce smoking on the intensive margin. The lack of response among heavy smokers is consistent with the explanation of changing composition of smokers over

[56] By our calculations based on the figures reported in Ringel and Evans (2001) and Levy and Meara (2006), the proportion of young mothers (aged 24 or less) among smokers rose by 6.5 percentage points from the earlier period (1989-1995) to the later period (1996-2008). The proportion of older mothers (aged 35 and up) rose by only 2.5 percentage points over the same period. On the other hand, we do not observe the same trend for mothers by education subgroups. Mothers with a college degree or more are more price sensitive in the earlier period but they represent a similar share of smoking mothers across early and late periods; it is worth noting, however, that smoking rates among this group drop considerably (by over 47%) during this time period.

[57] This elasticity is for the period 15 months after the MSA. However, it is again worth noting that one difference across the Evans and Ringel (1999) and Levy and Meara (2006) studies is that the latter uses only temporal variation for its primary estimates while the former uses a more traditional two-way fixed effects model. Another more recent study using two-way fixed effects (Markowitz et al., 2013) finds no effect of taxes on the intensive margin of smoking as captured by 4 categories of smoking intensity.

time driving the observed decline in participation elasticities. A similar test could be performed using maternal smoking data in the Vital Statistics; researchers could analyze both smoking and birth outcomes and compare earlier to later periods.

In sum, it is unlikely that a change in measurement error, methodologies or the pass through rate is primarily responsible for the observed decline in elasticities shown in Table 1. Instead, the evidence seems most consistent with a shift in the composition of smokers, though we are unable to rule out the alternative theories of an increased emphasis on local price environment and/or better treatment of the issue of policy endogeneity. A fruitful direction for future empirical work would be to test these theories against each other in a more coherent framework.

*D.iii. Findings in literature: dynamics during pregnancy*. The findings in the literature can also shed light on when during a woman's life taxes matter for smoking, with a specific look at whether women respond more to taxes during pregnancy relative to before or after. The first papers in this literature found large elasticities of pregnant women relative to the population of female smokers. Early estimates of the price elasticity of participation for pregnant women are as high as -0.52 to -0.91 (Colman Grossman, and Joyce, 2003; Evans and Ringel, 1999)[58] compared to the female population elasticity of -0.09 to -0.39 (Farrelly and Bray, 1998; Lewit and Coate, 1982).[59]

Using data on multiple within-mother observations from the PRAMS, it is possible to compare whether price changes affect mothers more strongly prior to, during or following pregnancy. Assigning taxes at 3 months prior to pregnancy, Colman, Grossman and Joyce (2003) find that mothers are substantially more responsive to prices during pregnancy relative to before conception (an implied elasticity of -0.91 versus -0.30); the same pattern holds for quitting behavior (estimated elasticities of 0.66 and 1.04 before and during pregnancy, respectively). This result seems to hold using data from the later period as well; Adams et al. (2012) show that mothers are relatively price insensitive before pregnancy (with an estimated price elasticity of participation of -0.09) but are more responsive during pregnancy; a $1 price increase results in a 3.4% increase in the likelihood of quitting by the third trimester. The PRAMS also surveys mothers about their

[58] As already discussed, participation elasticities vary across studies. Even within studies, estimated elasticities are heterogeneous across various subgroups of pregnant women. For example, Lien and Evans (2005) find elasticities that range from -1.83 in Massachusetts to -0.1 in Illinois; Levy and Meara (2005) find that the full sample of mothers displays a lower price elasticity (-0.13) than the sample of teen mothers (-0.3).

[59] Bradford (2003) uses longitudinal data and finds no difference between maternal smokers and the population of smokers; however, a major limitation of this study is that Bradford's estimated tax coefficients are not adjusted to account with temporal trends or time fixed effects.

likelihood of relapse. While less than half of women who quit during pregnancy continue to abstain after delivery, such sustained quits are highly responsive to the post-delivery tax environment; price elasticities of sustained and long-term quits range from 0.74 to 1.5 (depending on the sample and time period studied)[60] and the elasticity of relapse is -1.0 (Colman, Grossman and Joyce, 2003; Adams et al., 2012). The combined effect of women being more likely to respond to taxes during pregnancy along with the persistence of these quits suggests that targeting women during the period of their pregnancy might be an effective way to ultimately reduce lifetime smoking among the adult female population as a whole.

Another area related to the dynamics of smoking is the forward-looking behavior of pregnant women (i.e. whether women who quit during pregnancy resume smoking afterward). Reinforcement, as defined in Becker and Murphy's (1988) theory of rational addiction, states that past and current consumption are complements. If mothers are forward looking, an increase in future prices will reduce current consumption. Indeed, some evidence suggests that pregnant mothers decrease consumption after a tax hike is announced but before it is implemented (Gruber and Koszegi, 2001). Relatedly, as smoking behavior is very persistent, there is scope for taxes faced during teenage years (when many smokers initiate) to affect smoking during pregnancy. Indeed, Colman, Grossman and Joyce (2003) find that smoking three months prior to pregnancy is influenced by taxes faced at age 14 and Gruber and Zinman (2001) find that smoking during pregnancy is heavily affected by both contemporaneous taxes and those faced during teen years (ages 14-17).

*D.iv. Effects of cigarette taxes on child health.* The primary justification for focusing on the smoking behavior of pregnant women is that pregnant women play a unique role in the intergenerational transmission of health capital. The medical literature offers a number of reasons for why we would expect to see effects of in utero smoke exposure on child health. The most mapped out biological mechanism is that smoking during pregnancy decreases birth weight. A birth weight effect comes from the intake of carbon monoxide and nicotine from smoking that restricts the flow of blood. This, in turn, reduces the amount of oxygen and nutrition that reaches the fetus, resulting in decreased birth weight. Effects are strongest in the third trimester because this is the time that the fetus is adding weight from nutrition (Surgeon General's Report, 2001).

[60] Sustained quits are defined as those who quit during pregnancy and have not relapsed after delivery. Long-term quits are those who smoke prior to pregnancy but who do not smoke after delivery (but who could have quit during pregnancy or after delivery).

Maternal smoking has potentially long-lasting impacts, as a growing literature in economics argues that in utero environment influences long term outcomes (Almond and Currie, 2011a; Currie and Rossin-Slater, 2014). Similarly, epidemiologists have extensively tracked the correlations between smoking during pregnancy and post birth outcomes including neonatal mortality, childhood asthma, childhood mental disabilities and general later life health (Lassen and Oei, 1998; Neuman et al. 2012, Shea and Steiner, 2008).

The first papers in the economics literature used models of household production (Becker, 1965) to attempt to estimate the underlying biological infant health production function. This involved leveraging cigarette prices along with other health care prices, parental education and family income as instruments to identify the impact of maternal smoking (and other inputs into health production) on birth weight. As is common in the earlier literature, some of the instruments used (namely parental education and family income) are no longer considered exogenous with respect to child health. Beyond this, also following the standards of the time, the exogeneity of the instruments were not tested as stringently in most modern papers (Rosenzweig and Schultz, 1983).[61]

More recently, the literature has focused on the reduced form relationship between taxes and birth outcomes rather than taking an instrumental variable approach (see Table 2).[62] Papers written on cohorts of mothers giving birth in the earlier period (before 2000) find moderate to large effects of taxes on low birth weight status, with reductions relative to the mean of 7.5% to slightly over 10% for a dollar tax increase (an absolute reduction of 0.45 to 0.75 percentage points; Evans and Ringel, 1999; Lien and Evans, 2005).[63] Table 2 also reports tax elasticities of infant health, which we construct from the information in these papers and through a modification of elasticity

[61] A number of papers within economics study the effects of maternal smoking on child health outcomes using methods other than instrumenting smoking with taxes. For example, Almond, Chay, and Lee (2005) use propensity score matching to adjust for selection into smoking by mothers in Pennsylvania and find that smoking during pregnancy reduces the incidence of low birth weight births by 3 to 4 percent. Yan (2013) used sibling fixed effects to show that smoking in the first trimester has little effect on infant health but that smoking in the second and third trimesters is particularly detrimental.

[62] This is in part because there are important caveats to using cigarette taxes as an instrumental variable. First, as discussed in Section II C cigarette taxes may affect child heath through channels other than maternal smoking (such as second hand smoke exposure, increased income to spend on other goods, and general equilibrium effects) which invalidates the use of taxes as an instrument for maternal smoking. Second, measurement error in maternal smoking (also discussed in Section II C) can attenuate first stage coefficients and thus inflate second state estimates. For these reasons, it is difficult to fully ascribe the health effects of a tax to smoking during pregnancy.

[63] These studies find more modest effects on average birth weight; a $1 increase in taxes leads to increases in birth weight ranging from 0.2 to 29 grams (at most a 1% effect relative to sample means).

formula discussed in Section II C.[64] These results show for the early period tax elasticity of LBW status of between -0.10 to -0.18. Studies from later periods generally yield smaller and sometimes insignificant effects of tax increases on birth outcomes.[65] DeCicca and Smith (2012) show that a dollar tax hike during this period reduces LBW by about 1-2% of the mean. While Markowitz et al. (2013) finds similar sized point estimates for effects of taxes on LBW, they are not statistically significant for any age group except teenagers.

While low birth weight status is one of the main benchmarks of child health used in the literature, other measures of infant birth have also been studied. Markowitz et al. (2013) find that increased prices decreased the probability of being born in a category of premature gestation by about 0.8 percentage points overall.[66] Perhaps the strongest indicator of health is infant mortality, where work has shown that increased taxes can decrease the mortality from SIDS. Using a two-way fixed effect model at the state level, Markowitz (2008) finds that a $1 increase in taxes leads to a reduction of about 8-9 annual SIDS cases (about a 40% reduction relative to the mean); her estimates imply that on the national scale, a 10% increase in taxes would lower the national death rate from about 0.53 to 0.52 per 1000 live births (in 2003). Using a similar model on a panel of countries, King, Markowitz and Ross (2015) find similar results for between cigarette prices and national SIDS cases; a $1 increase in the average cigarette price is associated with a reduction of between 12 and 13 SIDS deaths (about a 4.3% reduction over the mean).

The damage to a child's health from smoking during pregnancy potentially does not end at birth. Cognitive problems can occur when nicotine binds to neural receptors, leading to brain damage (Shea and Steiner, 2008). Nicotine acts as a sedative, slowing the development of the embryo, in turn impairing the development of the child's nervous system (Surgeon General's Report, 2010).[67] Many other harmful chemicals in cigarettes are believed to cause cellular damage,

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[64] Specifically, we multiply the marginal effects of taxes on infant health from the relevant study by the ratio of the average cigarette price during that time to the average birth outcome of interest. This tells us how a percentage change in price from a cigarette tax leads to a percentage change in infant health.

[65] Only one of the studies examined here (Markowitz et al., 2013) controls for mediating factors such as hypertension and diabetes; none of the studies control for alternate measures of child health at birth. Thus, we generally interpret all of the estimates as overall impacts of taxation (operating through any and all channels) and therefore differences in controls do not seem to explain the declining impacts of taxation over time.

[66] In a categorical model as being born in being born at less than 37 weeks, relative to 37 and more. The overall effect is calculated as the weighted average of the effects for each maternal age subgroup; weights are determined by the sample size of each group. Effects of taxes on likelihood of full term birth are statistically significant for babies born to all age groups except mothers aged 25-34.

[67] The harmful effects of nicotine itself (rather than carbon monoxide and other chemicals associated with smoking cigarettes) suggest that using nicotine replacements to reduce maternal smoking (such as chewing gum) may not be very effective in improving child health.

birth defects and other health complications that are not fully understood. Simon (forthcoming), using repeated cross sections from the national health interview survey, supplements the medical literature with causal evidence that exposure to a cigarette tax in utero has causal impacts on later life outcomes. A $1 cigarette tax while in utero decreases sick days from school for children ages 5–17 by about one-third of a day. Similarly, he finds negative effects of taxes on asthma attacks, emergency room visits and doctor visits. Bhai (2016) likewise shows that exposure to taxes during in utero decreases the incidence and severity of asthma and improves reported health status in children, with improvements focused on relatively low socioeconomic status families. Simon (forthcoming) and Bhai (2016) are examples of how the cigarette tax identification strategy can be used to look at the long lasting effects of smoke exposure. Smoking during pregnancy has also been linked to maternal health complications such as increased chances of ectopic pregnancy (Surgeon General's Report, 2001), but little has been done to link cigarette taxes with pregnancy-specific maternal health outcomes. As the children examined in the Simon and Bhai studies age into adulthood, additional work could more directly connect in utero smoke exposure to human capital development, such as long-term educational attainment and test scores.

## III. Clean Indoor Air Laws and Other Tobacco Policies

In this section, we review the literature linking other smoke-free policies to maternal smoking, birth outcomes and early childhood health. The emphasis of this section will be on so-called clean indoor air laws (CIALs), i.e. legislation that restricts or prohibits smoking in various venues. As indicated in the introduction, smoking bans and restrictions have become an increasingly prevalent policy tool (see Figure 2) and thus have the potential to impact a large number of pregnant women and children. While there are a number of studies that evaluate the general effectiveness of other anti-smoking policies (such as advertising campaigns, tobacco control spending, and youth access laws), very few of these studies specifically consider impacts of those policies on pregnant women and young children and so we do not focus on those studies here but rather discuss them in relation to studies of CIALs.

Following the format of Section II, we begin by outlining the basic identification strategies used in this area of the literature. Because there is a high degree of overlap in datasets used to study the effects of smoking related policies and those used to study the impact of cigarette taxes, we do not review commonly used datasets in this section. Instead we refer to the reader to

Appendix B. and Appendix A, Table 1A for a review of datasets containing information on child health outcomes and maternal smoking behavior. We describe datasets containing information on clean indoor air policies in Appendix C. In Section III B, we review and discuss the existing evidence that links smoke-free legislation to maternal smoking, birth outcomes and early childhood health. We also detail the mechanisms through which such legislation ultimately affects the health of infants and children. This is particularly important when examining the impacts of CIALs, as in contrast to changes in cigarette taxation, one of the primary ways through which we expect smoke-free laws to impact children and pregnant women is through exposure to second-hand smoke.

Note that there is a long line of related studies in the epidemiological literature that is largely descriptive in nature. In this review we will focus on the studies that aim to estimate causal effects of tobacco policy on maternal health and child outcomes but we refer the interested reader to Appendix D for a full discussion of related descriptive studies.

A. Identification

Similar to the literature that uses changes in tax policy to isolate exogenous variation in tobacco prices and maternal smoking behavior discussed in Section II A, the majority of existing empirical work studying the impact of other smoking-related policies exploits changes in laws over time and across geographical areas in a two-way fixed effects strategy:

$$5)\ Y_{i,s,t} = f\left(\beta_1 \tau_{s,t} + \boldsymbol{\beta_2} \boldsymbol{P}_{s,t} + \beta_X X_{i,s,t} + \mu_t + g(s,t;\theta) + \gamma_s + \varepsilon_{i,s,t}\right).$$

$Y_{i,s,t}$ is an outcome of interest for mother (or child) $i$ in area $s$ at time $t$; $\boldsymbol{P}_{s,t}$ is a vector of variables that capture the implementation of policies under study in area $s$ as at time $t$; $X_{i,s,t}$ are covariates at the child, mother, household and/or area level. Time-invariant determinants of $Y_{i,s,t}$ at the area-level are captured by area-fixed effects, $\gamma_s$, and aggregate changes in $Y_{i,s,t}$ over time are accounted for in time fixed effects, $\mu_t$. The inclusion of these two levels of fixed effects forms the basis of the generalized difference-in-difference strategy. $g(s,t;\theta)$ represents additional controls for secular temporal changes in outcomes, most often specified as area-specific linear time trends.

$\boldsymbol{\beta_2}$ are the parameters of interest and reflect the changes in outcomes associated with the policy vector, $\boldsymbol{P}_{s,t}$. Typically, $\boldsymbol{P}_{s,t}$ can capture several policy dimensions – namely, venue (e.g.

restaurants, workplace and childcare facilities), strength (e.g. full or partial), locality of implementation (e.g. municipal, county, state or nationwide) and timing relative to birth (e.g. pre-pregnancy, during various trimesters of pregnancy or post-birth).[68] See Section III B for a fuller discussion of parameterization of $\boldsymbol{P}_{st}$ and consequences for interpreting $\widehat{\boldsymbol{\beta}}_{\mathbf{2}}$. In practice, the functional form, $f(\cdot)$, used to estimate (5) depends on the outcome under study (e.g. binary, continuous or categorical).[69]

B. Methodological issues

Many of studies in this literature share common empirical issues with the tax studies discussed in Section II C. We do not review those issues in detail here and instead use this section to review the issues that are particular to studies evaluating anti-smoking policies other than taxes.

*B.i. Parameterizing anti-smoking policies and comparisons within and across studies*. One of the primary issues in this area of the literature is comparability. There are a plethora of differing smoking indoor restrictions, which vary based on the strength with which they are applied, the venue to which they apply, the level of government enforcement and the timing of implementation (relative to the outcome of interest, such as birth weight). Despite this sizeable variation in CIALs there is no standard method for parameterizing them, either across or within studies.

The early literature on the impact of smoking bans tended to favor indices of bans that collapse information on bans across various venues and differing strengths into a single index variable at the state and time level, $P_{(s,t)}$.[70] However, the main drawback of using such indices is that doing so requires strong parametric assumptions about the effects of ban stringency and venue;

[68] Clustering standard errors at the level of the policy under study is now the convention in the literature. For studies with a low number of clusters, now-common cluster-robust bootstrapping methods such as the wild cluster bootstrap (Cameron, Gelbach and Miller, 2008) are employed to avoid bias. Another issue that is often overlooked with regard to $(\varepsilon_{i,s,t})$ is that many studies in this area evaluate the impact of $P_{s,t}$ on an array of highly interrelated outcomes (for example, on multiple indicators of health at birth) but do not adjust for correlations in $(\varepsilon_{i,s,t})$ across outcomes. Shetty et al. (2010) highlight the importance of adjusting for arbitrary dependence across outcomes when making multiple comparisons (Hochberg, 1988).

[69] In some papers, special attention is also often paid to modelling outcomes as a set of ordered categories (e.g. levels of smoking participation); see for example Markowitz et al. (2013). This same methodology might be useful for some categorization of birth outcomes, such as very low birth weight (VLBW), low birth weight (LBW) and "normal" birth weight (neither VLBW nor LBW). The issue of censored outcomes (such as cigarettes per day) is rarely addressed in this area of the literature; an exception in related literature is Anger, Kvasnicka and Siedle (2011) who use tobit specifications to check for robustness to linear models.

[70] See, for example, Chaloupka and Weschler (1997), Ohsfeldt and Boyle (1999), Tauras (2004b), Wasserman et al. (1991) and Yurekli and Zhang (2000). A more recent paper (Bartholomew and Abouk, 2016) also collapses bans implemented across different venues into a single index by assigning values to combinations of ban venues and strengths. The authors argue that this categorization is reflective of differences in time spent across various venues by adults and so better captures exposure to a ban.

for example, a popular index based on reports by the U.S. Department Health and Human Services assigns areas with bans in private workplaces (regardless of the existence of other bans) the highest ban stringency (a value of 1), while areas with legislation that require restaurants to reserve at least 75 percent of seating for nonsmoking patrons fall into the next highest category (a value of 0.75), and areas with no restaurant or private workplace restrictions but with at least 4 other venue restrictions are given a value of 0.5.

In order to facilitate comparability across degrees of ban strength, many recent studies restrict attention to only 100% smoke-free regulations (Amaral, 2009; Adda and Cornaglia, 2010; Bharadwaj, Johnsen, and Løken, 2014); however since this approach classifies areas with partial CIALs as "control" areas, it potentially understates the impacts of CIALs. Others differentiate between full and partial bans (Adams et al., 2012; King, Markowitz and Ross, 2015; Markowitz et al., 2013) or even more nuanced degrees of stringency (Briggs and Green, 2012; Markowitz, 2008) to help account for the potentially nonlinear effect of ban strength.[71]

Another way in which the parametrization of indoor air laws can vary across studies is by the venue to which specific bans apply. Though most studies distinguish between bans in restaurants and bars, some collapse several venues into a single policy variable (e.g. a dummy for the existence of a ban in at least one of those venues). The justification for this aggregation is that the distinction between venues is often artificial and that bans in one venue can simply displace smoking to the other (Adda and Cornaglia, 2010). Others collapse all venues into a single CIAL "index" which is the number of venues in a geographic location that are subject to a ban (i.e. a simple sum of ban dummies across all venues). Collapsing ban information across venues leads to the same issues as collapsing information across ban strengths; most importantly, doing so imposes the strong parametric assumption that the marginal effect of a ban in an additional venue is the same for all venue types. Existing work has shown that bans in some venues (and some occupations, in the case of workplace bans) are more effective than in others (Bitler, Carpenter and Zavodny, 2010; Briggs and Green, 2012; Carpenter, 2009; Chaloupka and Saffer, 1992; Ross and Chaloupka, 2004; Tauras, 2005b). Given this heterogeneity, the majority of recent work

[71] There are two studies that address ban stringency in alternative ways. Ross and Chaloupka (2004) include controls for the type of punishment exacted in the case of CIAL violations (e.g. civil or criminal penalties or fines). Owyang and Vermann (2012) create a separate CIAL index for three different venue types (workplaces, restaurants and bars) and adjust the indices using the share of establishments in a given area that fall under the venue classification of each ban; for example, they adjust the restaurant exposure index to account for the proportion of establishments within each county that are restaurants. These methodologies have yet to be applied to study the impact of CIALs on mothers and children.

considers bans in restaurants and bars separately from bans in other venues such as private and public workplaces, schools, childcare centers, and other public places.

However, it varies greatly from study to study both *which* other venue-specific bans are included and *how* they are specified. Some studies include CIAL indicators across multiple venues within the same regression to estimate the partial effect of each individual CIAL (Adams et al., 2012; Adda and Cornaglia, 2010; Briggs and Green, 2012; Gruber and Zinman, 2001), but others claim that multicollinearity across CIALs prevents them from doing so and instead consider them in separate regressions (King, Markowitz and Ross, 2015; Markowitz, 2008; Markowitz et al., 2013). Surprisingly, only one study (Tauras (2004b)) tests for the joint significance of CIAL indicators to understand whether there is evidence of an overall effect of smoking restrictions; however this study does not focus on the effects of bans on mothers or children. It is not clear why studies do not perform such a joint test, which explicitly takes into account collinearity among measures.

Studies also vary in their parameterization of anti-smoking policies in the administrative level at which they consider policy enforcement. This distinction is important because the level of CIAL implementation will likely determine aspects of the policies such as enforcement and compliance. For example, local laws may enjoy greater community support and thus higher compliance rates and levels of enforcement than state laws. Alternatively, state laws may be more difficult to avoid (e.g. by traveling to an unregulated jurisdiction) than local laws and states may have more resources to devote to enforcement efforts than local governments. Most studies in this area of the literature focus on state-level regulations, though others consider national bans (Bharadwaj, Johnsen, and Løken, 2014; King, Markowitz and Ross, 2015) and still others consider municipal or county-level bans (Amaral, 20109; Bartholomew and Abouk, 2016; Briggs and Green, 2012).[72]

A number of studies attempt to combine policies from multiple levels of implementation by calculating the proportion of the population within each area (such as a state or county) that are subject to at least one restriction (workplaces, restaurants and bars); see, for example, Adda and Cornaglia (2010) and Farelly et al (2014).[73] One benefit of these measures is that they potentially

[72] Another related but more nuanced issue arises in understanding the heterogeneity in the effect of a state law in areas with and without preexisting local laws. To allow for this potential heterogeneity, Briggs and Green (2013) go one step further and consider municipal-level bans (in addition to state- and county-level bans) as well as the *interactions* between bans at all three levels.

[73] Whether these measures include partial bans (in addition to 100% smokefree policies) differs across studies.

reduce some measurement error in the application of bans because they account for (potentially much wider) sub-state variation in the implementation of local laws; this reduction in measurement error in the assignment of bans may lead to larger estimated impacts of smoke-free regulation.[74] These combined measures also help to capture some variation in the stringency of CIALs; for example local bans that are potentially easier to avoid will apply to a smaller number of people and thus receive a lower weight than state level bans which are potentially more difficult to avoid.

Measures of the proportion of the population subject to at least one restriction make two key assumptions: 1) that individuals respond to all bans in the same way (regardless of venue or level of enforcement) and 2) that there is no interactive effect of bans enforced in multiple venues or at multiple levels.[75] Both assumptions are troublesome. As mentioned above, earlier work has found that effectiveness and compliance varies across ban venue and occupation; other studies have found that the impacts of bans differ according to their level of enforcement (Amaral, 2009; Briggs and Green, 2012; Fleck and Hanssen, 2008). Regarding the assumption of no interaction effects, there is some evidence that comprehensively enforced bans (those that cover multiple areas) may have stronger impacts or better reduce second hand smoke exposure (Bartholomew and Abouk, 2016; Erazo et al., 2010; Fernández et al., 2009; Huss et al., 2010; Knudsen, Boyd and Studts, 2010; Ross and Chaloupka, 2004; Ward et al., 2013). Moreover, bans may have interactive effects when administered on multiple levels. In many cases, local policies precede statewide policies and so failing to account for the existence of local bans may understate the impacts of state-level policies. Conversely, there are some state laws which preemptively prohibit localities from implementing more restrictive policies. The interactions of local-, state-, and higher-level policies are not often addressed in this literature, and the few studies that do account for these interactions find doing so is important for evaluating the impact of bans on a variety of outcomes (Amaral, 2009; Briggs and Green, 2012; Fleck and Hanssen, 2008).

A final issue for parameterizing indoor smoking policies is that the way in which timing of policy implementation is matched to individual observations. Though some studies are careful to distinguish between exposure to policies in utero (for children) or during pregnancy (for mothers) versus at or after delivery (Adams et al., 2012; Briggs and Green, 2012; Markowitz et al., 2013),

[74] Indeed, a recent study (Pesko et al., 2016) finds that the estimated price elasticities approximately triple when using sub-state rather than state-level variation in cigarette prices.
[75] This latter assumption is made also by studies using separate indicators by venue and/or level of enforcement.

most match based on delivery date or date at which an outcome is observed.[76] Moreover, none of the current studies in this literature take into account the length of time a policy has been in place. This may matter if, for example, compliance becomes stronger over time, making newly enacted laws less comparable to well-established ones.[77] The dynamic effects of laws more generally – whether due to the age of the policy or to the predictions of models of rational addiction – are generally unexplored in this area of the literature. Tauras (2005a) explicitly brings the issue of the dynamic nature of smoking progression to light, claiming that (as of the time when the paper was written), no papers had studied the impact of policies on smoking progression beyond initiation. There are now papers that study cessation (even among mothers), though there are still none that look at the complete picture of smoking progression (initiation, progression and cessation) with respect to CIALs.[78] Moreover, there is only a single paper we are aware of that takes into account the length of time a policy has been in place when evaluating its effectiveness in terms of curbing maternal smoking and improving child outcomes (Briggs and Green, 2012); other studies in this area exclusively focus on the average effect of a ban across all post-ban periods. Event study-style methods (such as those used in Nikaj, Miller and Taurus (2016)) could be a useful addition to this literature, as it is possible that the effects of a policy either increase or fade over time.

While there are advantages and disadvantages to the choice of policy parameterization in each study, we believe there are a few standard practices that future studies would benefit from implementing. First, given the significant heterogeneity observed in the impacts of CIALs, we recommend avoiding indices that collapse information across ban venues. More recent literature has generally moved towards a more non-parametric approach, either by focusing on bans in a single venue or by allowing the effects of bans to vary separately by venue. We recognize that focusing on a single venue ignores interactive effects among policies and that including separate indicators for each venue in a single regression is subject to practical issues concerning collinearity. Nonetheless, we believe the benefits of advoiding both the strong parametric

[76] As discussed in great detail in Section II C.iv the maternal smoking response to anti-smoking policies may vary depending on the timing of exposure relative to pregnancy. Mothers are more sensitive to anti-smoking policies at some points than others; additionally, past, current and future policies are all important determinants of smoking behavior during pregnancy. Furthermore, impacts on birth outcomes and childhood health depend on *when* during pregnancy exposure occurs in addition to the extent of exposure.

[77] For example, Hamilton, Biener and Brennan (2008) find evidence that local tobacco regulations influence smoking norms and thus are likely to have impacts that may increase over time. The evidence on the dynamic effects of smoking bans on other outcomes are mixed; while Adams and Cotti (2008) find suggestive evidence that the impact of bans on drunk driving wanes over time, Fleck and Hanssen (2008) find no immediate or gradual (linear) effect of bans on restaurant sales over time.

[78] In the tax literature, the one study to do this is Colman, Grossman and Joyce (2003).

assumptions in the marginal effects of bans by venue and the bias due to omitting other policies outweighs these drawbacks. Relatedly, we recommend tests for the joint effect across multiple policy indicators that explicitly takes into account potential multicollinearity.

Second, researchers should also avoid using composite indices to capture ban strength. The ad-hoc way in which levels of stringency have been classified in the past means that there is no consistent index to reflect ban strength; thus estimates from indices in across studies are likely capturing different underlying policy parameters. Instead, we recommend restricting attention to bans of a single set strength – namely 100% smoke-free laws – or including measures of stringency in as non-parametric a way as possible (such as separate indicators for each strength category, e.g. as "full" and "partial" bans).[79] However, we acknowledge that focusing on only 100% smoke-free laws potentially biases the estimated effects of bans towards zero as discussed above, so researchers should be aware of this possibility when they interpret their results.

Finally, it is clear from the existing literature that it is important to take into account ban implementation at multiple administrative levels. Therefore, we favor either the inclusion of information on CIALs from all administrative levels separately or the use of policy variables that combine information from multiple levels, such as those that calculate the proportion of a population that is subject to a CIAL at any level. We recognize that the use of population-weighted measures that combine CIALs at multiple levels requires the strong assumption that the effect of bans is the same across all levels of implementation. Even so, we feel that it is more problematic to omit information on bans at other levels. Given the wide variation in the way that the existing literature has parameterized smoking restrictions, it would be a worthwhile addition to the literature to see how the estimates from existing studies compare once a more standard parameterization has been implemented.

*B.ii. Interpretation of policy effects and the role of second hand smoke exposure*. Another issue that plagues the literature on smoking bans and restrictions is how researchers should interpret the estimated policy parameters ($\boldsymbol{\beta}_2$ in equation 5). Studies concerned with the impact of cigarette taxes often focus on the direct impact of taxes on maternal smoking behavior. However, smoking bans are likely to affect birth outcomes and other measures of child health through at least two channels: the direct effect on maternal smoking behavior and the indirect effect on the exposure

[79] However, even the broad classification of "partial" bans is not always consistently defined across studies.

of young children and pregnant women to second hand smoke.[80] Several papers focus explicitly on the channels themselves, i.e. the effect of the policies on maternal smoking (Adams et al., 2012; Markowitz et al., 2013; Gruber and Zinman, 2001) or on exposure of children to smoke (Adda and Cornaglia, 2010; Nguyen, 2013). However, a number of papers look at the overall effect of smoking bans on child health or birth outcomes, but few do much to distinguish the direct and indirect channels from each other (the notable exceptions are Bartholomew and Abouk (2016) and Bharadwaj, Johnsen and Løken (2014), which separately estimate the effect bans on smoking and non-smoking mothers). This is sometimes due to lack of data on maternal or parental smoking behavior, though some studies ignore the issue altogether or simply assume that only one channel is at play.

Disentangling the two channels is especially important from a policy perspective. Given the scope for bans to displace smoking from public places to private homes, it is possible that the indirect effect of bans on child and newborn health through second hand smoke exposure may be to *increase* smoke exposure and worsen health (Adda and Cornaglia, 2010). In the presence of such displacement effects, we might expect the overall impact of bans to differ greatly across children from smoking and non-smoking families; by estimating an average effect, we would overlook any such heterogeneity. Given the importance of identifying these channels, we recommend that future work in this area estimate the effects of policies on smoking and non-smoking populations separately (when possible), a strategy that can be implemented even when cotinine level data (a direct measure of second hand smoke exposure) is not available.

*B.iii. Endogeneity of policies.* As discussed extensively in Section II C, the main econometric concern that arises in estimating a difference-in-difference specification as in equation (5) is the potential for policies to be endogenous with respect to maternal smoking or child health outcomes. Many of the endogeneity concerns – such as reverse causality/selection[81] and confounding

[80] Even within the direct effect on maternal smoking, there may be several underlying mechanisms at play. CIALs may the raise monetary cost of smoking (in terms of fines for disobeying the laws) or non-monetary cost of smoking if they increase the social stigma associated with smoking in restricted places or smoking more generally. Hamilton, Biener and Brennan (2008) find evidence in support of the latter mechanism; perceived anti-smoking sentiment rises in the presence of smoking bans, even after controlling for pre-ban norms.

[81] For example, previous work has shown that support for and existence of anti-smoking legislation (and the stringency with which such legislation is implemented) depend on individual smoking status as well as area-specific smoking prevalence, the composition, economic status and political power of local establishments, political preferences, public attitudes, pre-existing policies and perhaps even news coverage and state-level importance of tobacco production (Boyes and Marlow, 1996; Chaloupka and Saffer, 1992; Dunham and Marlow, 2000; Fleck and Hanssen, 2008; Gallet,

policies[82] – overlap to some extent with those in the tax literature (reviewed in Section II C), so in this section we focus most heavily on the issue of general equilibrium effects of polices which has specific applications to the evaluation of smoking restrictions. We then review some of the methods that have been used to account for these endogeneity concerns.

*General equilibrium effects of policies.* There are several general equilibrium aspects of policies that are important to keep in mind when interpreting the results of estimating equation (5): endogenous migration and other avoidance behavior, spillovers and overall impacts on government revenue and funding of other programs and facilities. Endogenous migration and general equilibrium effects of ban enforcement on public funding of other programs and facilities largely overlaps with the discussion in Section II C.v, so we do not repeat that discussion here.[83] However we note that evidence of avoidance behavior has been found in the contexts of bans. For example, Bharadwaj, Johnsen and Løken (2014) find that mothers who switch occupations following an extension of workplace bans to restaurants and bars are observably different from those who do not switch, suggesting that compositional changes in occupation in response to CIALs is likely in the medium- to long-term, even among the population of pregnant women.[84] Similarly, Adams and Cotti (2008) find that alcohol-related driving accidents *increase* following smoking bans in bars, suggesting that individuals are willing to drive further (into non-regulated jurisdictions) to avoid bans. Additionally, it is possible that bans have spillover effects across geographical areas and/or venues. For example, Adams and Cotti (2008) show suggestive

Hoover and Lee, 2006; Golden, Ribisl and Perreira, 2014; Hersch, Del Rossi, and Viscusi, 2004; Nikaj, Miller and Taurus, 2016; Ohsfeldt and Boyle, 1999; Pakko, 2006; Smith et al., 2008). Relatedly, Nikaj, Miller and Tauras (2016) find that municipality-level bans in restaurants and bars are implemented during times of robust bar sales for early but not late adopters, suggesting that the timing of legislation depends on the outcomes of interest. Fleck and Hansen (2008) find that failing to account for pre-existing trends in restaurant sales prior to implementation of the statewide ban in California potentially overstates the negative impact of the ban on restaurants.

[82] While at the national level the prevalence of smoking bans is positively correlated with cigarette taxes (see Figure 2) at the sub-national level bans, taxes, and other instruments may act as substitutes or complements; previous studies have found evidence consistent with both substitution and complementarity (Gallet, Hoover and Lee, 2006; Golden, Ribisl and Perreira, 2014). Many studies do not control for overall tobacco control spending or youth access restrictions, despite the possibility that these policies may directly or indirectly impact pregnant women. Indeed, Gruber and Zinman (2001) find that restrictions on youth access reduces smoking participation among teen mothers, even after controlling for cigarette prices and clean indoor air laws in several venues.

[83] We use the term "endogenous migration" loosely in this context to capture any ban avoidance behavior; for example, this could entail traveling to areas without smoking bans in certain venues or switching jobs to avoid workplace bans.

[84] Highly related to the issue of endogenous switching/migration is the choice of time window around the policy implementation to include in the estimation sample. The shorter the time window used, the less scope there is for endogenous migration/switching. For example, Bharadwaj, Johnsen and Löken (2014) do *not* find significant differences among switchers and non-switchers (nor evidence of compositional changes in mothers who work in venues affected by the ban) within a short (5-month) window around the reform.

evidence that drunk driving accidents rise not only in counties implementing a ban but also in neighboring counties where the ban was *not* implemented. The possibility of these types of spillover effects creates problems with interpreting the results of equation (5) as causal impacts of bans, as the difference-in-difference approach captures only the relative changes of the treatment (ban) group relative to the control (non-ban) group. If the bans indirectly affect the control group (in the same way as it affects the treatment group), this will likely understate the true impact of bans.

*Main measures taken to address policy endogeneity and general equilibrium effects.* The concerns about policy endogeneity pose serious threats to identifying the causal impact of bans. Thus most papers in this literature attempt to address these issues in a variety of ways. To reduce omitted variables bias and address other selection concerns many include a number of time-varying controls that may jointly determine outcomes of interest as well as smoke-free policies.[85] As part of this, many studies use lagged rather than contemporaneous policy variables (but usually continue to include contemporaneous taxes or prices). The underlying assumption behind this strategy is that individuals respond to policies only with a lag, but respond to changes in taxes and prices immediately. Finally, it has become standard practice to include area-specific linear (and sometimes quadratic) time trends to control for differential preexisting trends in the outcome variables that may induce adoption of smoke-free legislation.

Specific to the nature of dichotomous policies such as bans, one method for reducing concerns about selection into treatment is to vary the choice of the control group. For example, when examining the effect of a national ban on smoking in bars and restaurants in Norway, Bharadwaj, Johnsen and Løken (2014) show that their main results based on comparisons between mothers working in restaurant and bars (treated) and those working in retail establishments (control) before and after the ban is implemented are robust to considering various other control groups, such as all mothers employed in sectors other than restaurants and bars and those working in restaurants and bars but in periods well before the ban was implemented. As has already been discussed in Section II C.v, a relatively new approach that uses a more systematic manner of choosing appropriate (combinations of) controls is the synthetic control method (Abadie, Diamond

[85] See Section II C.v for a list of recommended policy controls. While some studies explicitly recognize the potential endogeneity of cigarette prices (e.g. Gruber and Zinman, 2001), none address the endogeneity of other policy instruments.

and Hainmueller, 2010; Abadie and Gardazabel, 2003), which has rarely been applied to the bans literature.

The choice of control group may also help address concerns about potential general equilibrium effects of bans. For example, it may help to use only "pure" control areas that do not border treatment areas and therefore are less likely to experience spillover effects; this approach has been used in several papers that study the impacts of cigarette taxation and which are concerned with smuggling and other cross-border effects (Anger, Kvasnicka and Siedle, 2011; Carpenter 2009; Chaloupka and Weschler, 1997) but has not yet been applied in the bans literature. Another option to correct for the general equilibrium effects of policies is to directly control for factors thought to influence cross-border activity (such as taxes/prices in neighboring states), a method that has also been implemented in the tax but not the ban literature (Baltagi and Levin, 1986; Chaloupka, 1992; Chaloupka and Saffer, 1992; Ross and Chaloupka, 2004; Tauras, 2005b; Wasserman et al., 1991; Yurekli and Zhang, 2000). Though this approach may be more relevant for taxation, it may be adapted to the study of bans by including additional controls such as (time-varying) indicators for bans or other policies in neighboring states. Finally, a way to investigate endogenous migration is to look for evidence of selection into or out of areas with bans. For example, one could check whether the number of or pre-determined characteristics of mothers changes in response to bans; if so, there may be reason to believe that the bans change the type of mothers who choose to become pregnant or who choose to remain in areas subject to bans.

*Other robustness checks and alternative strategies*. Several robustness checks are increasingly popular in studies of dichotomous policies using the strategy outlined in equation (5). One check is to examine the coefficients on the leads of policy variables; the idea is that policies should have an effect only after they are implemented and thus significant "impacts" of future policies on current behaviors may indicate reverse causality, omitted variables bias, differential preexisting trends across policy and non-policy areas and/or anticipatory effects (Adams and Cotti, 2008; Nguyen, 2013; Nikaj, Taurus and Miller, 2016). Though this has not been widely employed in papers that examine the impacts of smoking bans on pregnant women and children, there is no reason that it should not be.[86] Relatedly, researchers may want to consider both formal and

[86] It should be noted that since the ban under study in Bharadwaj, Johnsen and Löken (2014) is enforced at the national level, the falsification checks they employ that move the date of implementation earlier in the period (prior to the true implementation date) is similar in spirit to examining coefficients on leads of the policy variable.

informal tests of parallel pre-trends across treatment and control groups when possible (e.g. statistical tests or visual representations as in Carpenter, Postolek and Warman (2011) and Adams and Cotti (2008)).

Similar in spirit are a number of so-called falsification tests. Some studies estimate the effect of imposing "false" policy implementation dates; for example, both Bharadwaj, Johnsen and Løken (2014) and Kvasnicka (2010) estimate the effect of fictional bans put in place during a control (non-ban) period and show that there do not exist similar breaks in outcomes between treatment and control areas in periods other than those specific to ban implementation. Similar checks can be performed by falsely assigning "treatment" status to "control" areas without anti-smoking policies. Bharadwaj, Johnsen and Løken (2014) use exact randomization methods and permutation tests to show that both randomly assigned treatment and false assignment of treatment to control areas do not produce the same estimated effects as their main results. Another type of falsification tests make use of outcomes that should not be affected by smoke-free policies, such as infant deaths from motor vehicle accidents and drowning, other risky health behaviors and hip fractures among the elderly (Evans, Farelly and Montgomery, 1999; Markowitz, 2008; Shetty et al., 2010); these studies claim that the lack of impacts of CIALs on these outcomes indicates that CIALs are not capturing broader, unobserved trends or policies.

In some (rare) cases, panel data on mothers is available, allowing for the inclusion of mother fixed effects; these studies exploit only the variation in policy exposure across siblings born to the same mother and can therefore effectively control for unobservable characteristics otherwise not captured in equation (5) (Bharadwaj, Johnsen and Løken, 2014). Relatedly, Abrevaya (2006) and Abrevaya and Dahl (2008) use a matching algorithm based on maternal characteristics on U.S. federal natality data to link separate birth records to the same woman to estimate the direct effect of maternal smoking on child health. Though this method makes use of a small subset of the natality data, the resulting sample sizes (particularly with the addition of updated data that has since become available) are large enough to implement large sample inference.[87] Findings across these studies suggest that including mother fixed effects may improve the precision of estimates and reduce bias due to unobservable fixed factors at the mother level.

[87] Similarly, Nikaj, Miller and Tauras (2016) use establishment-level data to study the effects of bans on restaurants and bars and find that the increase in precision using fixed effects allows for the detection of impacts that would otherwise be undocumented with aggregate data.

Therefore researchers interested in the impact of bans may consider this matching algorithm and mother fixed effects strategy as one way to extend the current literature.[88]

Alternatively, others include additional levels of policy variation and pursue triple differences approaches. For example, Adda and Cornaglia (2010) exploit differences across individuals surveyed during the work week and those surveyed during the weekend to better understand the impacts of workplace, bar and restaurant bans. Though this involves additional assumptions (for example, that exposure to workplace bans is higher on week days than weekends while the converse is true for restaurant and bar bans; and, that there are no differential pre-trends between weekend and weekday smoking), it also allows the relaxation of others – most importantly, that the geographic selection into implementing smoking bans is exogenous. Similarly, Nguyen (2013) supplements the variation over time and across provinces in Canadian bans on smoking in cars with variation in policy applicability to specific age groups (as the bans only apply to vehicles carrying children under the age of 16). Specifically, he shows that the difference-in-difference effect of bans on second-hand smoke exposure estimated using province-by-year variation is significantly larger for those in the age groups targeted by the bans.

## C. Empirical findings

In this subsection, we review the empirical findings on the impacts of bans on maternal smoking, pregnancy and birth-related outcomes, and infant/early childhood health. We then discuss how the results can be used to understand the channels through which smoking bans ultimately impact infants and children. We are aware of only one existing meta-analysis of the effect of smoke-free legislation on perinatal and child health (Been et al., 2014), which summarizes the findings from 11 studies and largely focuses on studies from the epidemiology and public health literatures. We build upon their analysis in several key ways. First, in addition to birth and asthma-related outcomes, we also consider other measures of infant and early childhood health (such as cases of sudden infant death syndrome (SIDS)). Second, we carefully consider the mediating channels through which bans affect child health, namely maternal smoking behavior and second-hand

[88] A few papers in the early literature instrument for bans using variables such as per capita tobacco production, the percentage of the population that votes, state expenditures per capita, state political liberalism, interparty competition in state government, per capita state tobacco production and establishment size (for firm-level workplace bans) (Chaloupka and Saffer, 1992; Evans, Farrelly and Montgomery, 1999; Oshfeldt and Boyle, 1999). However, it is not clear that that these variables do not directly influence maternal smoking and/or child health outcomes and so we do not recommend IV strategies in this context.

smoke exposure (of both the mother and child, both pre- and post-natal). Finally, we include a number of additional studies not reviewed in Been et al. (2014).[89]

It is also worth noting that unlike in Section II D, we do not separate the empirical results into studies using data from an early versus a later period. This is largely because (as Figure 2 indicates) CIALs did not become widespread until the late 1990s.

*C.i. Maternal smoking behavior.* To date, there are five studies that look closely at the causal effect of smoking restrictions on maternal smoking behavior (see Table 3).[90] Bharadwaj, Johnsen and Løken (2014) find that following the Norwegian ban on smoking in bars and restaurants, women who entered pregnancy as smokers are 15 percentage points more likely to quit smoking if they work in an effected venue. By our calculations, this amounts to a 3 percentage point drop in maternal smoking (15.2% relative to the pre-ban mean). Similarly, Adams et al. (2012) find a smaller but statistically significant impact of US state-level, complete smoking, workplace bans on self-reported quits; the estimated effect is a 4-5 percentage point increase in quitting (or a reduction in smoking of 4.3% of the mean). This effect is roughly similar in magnitude to the effect of a $1.00 increase in cigarette prices/taxes[91]. It is interesting to note that the increase in quits due to full bans seems to be short-lived, as Adams et al. find that sustained quits (as of shortly after delivery) are no more likely for women exposed to the bans.

Gruber and Zinman (2001) find that teen mothers (ages 13-18) are most responsive to bans in restaurants (over bans in other venues) on both the intensive and extensive margin; restaurant bans reduce smoking participation by about 0.7 percentage points and cigarettes per day

[89] Ours is also not the only review of the effects of smoking bans more generally. The most recent study to do this that we are aware of is Levy and Friend (2003); earlier reviews are discussed in Levy and Friend (2003) and so we do not revisit them here. Our analysis differs from Levy and Friend (2003) in several ways, most notably that (i) their analysis does not focus specifically on the impacts of smoke-free laws on maternal and child health outcomes and (ii) their analysis is limited to studies published in 2002 and before; given that bans have increased in prevalence since then (and with a marked increase in coverage starting around 2003; see Figure 2) much of the literature in this area is still new and has been published in the intervening 14 years between their study and ours.

[90] There are several policies other than bans that have been studied with respect to maternal smoking behavior. Though Gruber and Zinman (2001) find no consistent impact of state-level youth access laws on smoking among teenage mothers in a traditional difference-in-difference framework, Yan (2014) finds that an increase in the minimum purchase age to 21 in Pennsylvania reduces daily prenatal cigarette consumption by 15 percent (2 percentage points on the extensive margin) by young mothers in an RD framework. Relatedly, Pesko, Seirup and Currie (2016) find that when minimum age purchase laws are enacted for e-cigarettes, traditional cigarette use among mothers in the age-affected group (i.e. those unable to legally purchase e-cigarettes after the law is in place) rises relative to mothers in the unaffected group (those above the legal purchase age). Evidence from randomized control trials indicates that prenatal smoking cessation interventions are successful in encouraging pregnant women to quit (Dolan-Mullen, Ramirez and Groff, 1994).

[91] The effect is much larger for mothers aged 19 or less, who increase third trimester quits by over 13 percentage points.

(conditional on participation) by about 0.6.[92] They also find some evidence that bans in other public areas (such as public transportation) are successful in lowering smoking participation among younger teen mothers (ages 13-16). The point estimates in Gruber and Zinman are much smaller than in both Bharadwaj, Johnsen and Løken and Adams et al., but the outcomes are not strictly comparable across the three studies; while Gruber and Zinman examine (unconditional) smoking participation among all pregnant teens, the other two studies examine quitting behavior among mothers who report smoking at the start of or prior to pregnancy. Across the three studies, it appears that bans can induce quits and reduce maternal smoking, particularly for younger mothers and when full bans are imposed in restaurants and worksites.

On the other hand, the remaining two causal studies find no impact of CIALs on maternal smoking. Bartholomew and Abouk (2016) analyze municipal-level bans in West Virginia over the period 1995-2010. While they find that the bans in workplaces, restaurants and bars are generally negatively related to unconditional prenatal smoking (self-reported at the time of delivery), the effects are not statistically significant.[93] Markowitz et al. (2013) look at the impacts of state-level restrictions and bans on smoking in restaurants using the PRAMS data. They find no consistent evidence that maternal smoking responds to restrictions, bans or taxes, on either the intensive or extensive margin.[94]

What can explain the inconsistencies in findings across studies? Studies that find negative and significant effects of bans on maternal smoking and those that find no or insignificant effects differ in at least three important ways. First, the time span for the data is generally shorter and more recent in the studies that find significant reductions in maternal smoking than in those that do not (see Table 3); this could be relevant because as seen in Figure 3, increases in ban coverage in the U.S. were relatively slow in the 1990s but increased rapidly starting in the early 2000s. Second, maternal smoking outcomes and samples are not strictly comparable across studies; those that find significant reductions in maternal smoking tend to use an indicator for quitting by the

[92] Gruber and Zinman (2001) capture state-level bans across 5 venue categories using indicators for each (simultaneously included in the regression models). However, it is not clear what level of ban strength they consider. They also find that bans in private workplaces actually increase smoking participation among teens.

[93] Bartholomew and Abouk (2016) collapse information on smoking restrictions across venues (restaurants, bars, and workplaces) and strengths (full, partial, none) to create four categories – comprehensive, restrictive, moderate and limited. The comprehensive classification (full bans in all three venues) is negatively associated with prenatal smoking in the full sample and all subsamples except for mothers aged 18-20. As in Gruber and Zinman (2001) and Adams et al. (2012), they find the largest point estimates for the youngest sample of mothers (under age 18).

[94] The finding that maternal smoking is not responsive to taxes is in contrast to most of the studies in the existing literature (see Section II D). The authors do note that when they exclude state-specific linear time trends and examine specific subgroups or subperiods, taxes and maternal smoking are significantly and negatively related.

third trimester conditional on smoking at the start of or prior to pregnancy, whereas the two studies that find no significant reductions in maternal smoking use an indicator for the unconditional probability of smoking for the full sample of mothers (rather than just prior smokers).[95]

Finally, both studies that fail to find significant effects of bans on maternal smoking control for state-specific linear time trends, while the three studies that do find significant effects do not include state-specific time trends (note, however, that Bharadwaj, Johnsen and Løken (2014) show that their results are not a function of differential trends for treated and control mothers). Given the potential for unobserved differential trends across ban and non-ban states discussed in Section III B, we consider this to be the most substantial shortcoming of the Gruber and Zinman and Adams et al. studies. Nonetheless, given the overall weight of the evidence (and the fact that Bharadwaj, Johnsen and Løken address the differential trends concern), we conclude that bans are likely to decrease maternal smoking – at least in the specific contexts found in these studies. Even so, given that the causal evidence in this area is still sparse, we consider this to be an open research question where additional analysis – for example, an examination of whether the results for conditional quit rates are robust to the inclusion of state linear time trends – would be useful.

*C.ii. Displacement effects, second hand smoke exposure and other channels of influence.* A number of studies have illustrated the strong correlation between second hand smoke exposure (both of mothers prenatally and of infants and children postnatally) and child health outcomes (see Cox et al. (2013) for a list of recent studies in this area). Smoking restrictions may increase or decrease overall exposure to second hand smoke. On the one hand, bans may reduce the exposure of pregnant women and children to environmental smoke in public places, workplaces and hospitality venues. On the other hand, if smoking restrictions in these venues lead to a reallocation of smoking towards private places such as homes and vehicles or to an increase in smoking intensity – so called displacement effects – this may increase second hand exposure for pregnant women and children in families with at least one smoking member.

Few studies in the economics and related literature address the effects of bans on maternal and child second hand smoke exposure and related health outcomes.[96] Shetty et al. (2010) find a

[95] Indeed, the results from Gruber and Zinman (2001) suggest that even when statistically significant effects of policies on the unconditional probability of smoking are detectable, they may be smaller in magnitude than effects on conditional quit rates.
[96] Carpenter, Postelek and Warman (2011) find no evidence that Canadian smoking bans on smoking in public and work places led to any displacement of smoking to homes, though their analysis is limited to adults.

rise in the prevalence of hospital admissions for asthma in those under the age of 18 when smoking restrictions are implemented.[97] This suggests that children's exposure to smoke increases when parents are not allowed to smoke at work or in bars and restaurants, i.e. that the detrimental displacement effects of bans outweigh any beneficial direct effects of reduced venue specific exposure. Adda and Cornaglia (2010) find that bans raise overall cotinine levels in children aged 13-19. Additionally, they find that while bans in bars and restaurants raise child cotinine levels for all days of the week for children in nonsmoking families, workplace bans raise levels only during the week and restaurant/bar bans only during the weekends for children in smoking families. This they take this as evidence consistent with smoking parents who are subject to a workplace (restaurant/bar) ban increasing their smoking at home during the work week (weekend). Nguyen (2013) is the only study to provide direct evidence on displacement effects, and finds no evidence of displacement of smoking into homes in response to a Canadian ban on smoking in cars when children are present.[98]

Both Bharadwaj, Johnsen and Løken (2014) and Bartholomew and Abouk (2016) indirectly address the role of second hand smoke exposure and displacement by examining the effects of smoking bans separately on smoking and non-smoking mothers. Bharadwaj, Johnsen and Løken find that while there are large positive effects of the ban on birth outcomes for smoking mothers, the effects for non-smoking mothers are small and statistically insignificant. In contrast, Bartholomew and Abouk find positive effects for both groups. It is possible that the discrepancy in these findings is a matter of how maternal smoking status is defined. Bharadwaj, Johnsen and Løken define smoking status as of the start of pregnancy, while Bartholomew and Abouk define smoking status as of delivery. Since Bharadwaj, Johnsen and Løken find one of the main impacts of the Norwegian ban is to induce third trimester quits among mothers who smoked at the start of pregnancy, it is possible that women classified as nonsmokers (as of delivery) in the Bartholomew and Abouk study would have been classified as smokers (as of the start of the pregnancy) by the Bharadwaj, Johnsen and Løken definition.

The results by maternal smoking status relate to second hand smoke exposure in two ways. First, a lack of findings for non-smoking mothers in the Bharadwaj, Johnsen and Løken study

[97] Note, however, that Shetty et al. (2010) find that the statistical significance of the results for child admissions for asthma disappears when they use the Hochberg procedure to account for testing of multiple hypotheses.
[98] The author notes, however, that given the categorical nature of his data, he is unable to detect small changes in smoking at home and thus cannot rule out small displacement effects. However, he also does not find evidence that smoke exposure is displaced to other public spaces, such as bus stops, restaurants, or parks.

suggests that either the ban was not successful in reducing second-hand smoke exposure of non-smoking pregnant women or that passive smoking has little effect on birth outcomes in that context. The results for nonsmoking mothers in Bartholomew and Abouk may suggest that the channel of second hand exposure is important for impact of municipal level bans on birth outcomes in West Virginia, though as previously mentioned, this finding may be due to the way they define non-smoking mothers. Second, the positive results for babies born to smoking mothers (and, in the case of Bharadwaj, Johnsen and Løken, the estimated reduction in maternal smoking itself) suggest that bans do not simply displace maternal smoking from the workplace, bars and restaurants to the home environment but lead to overall declines in maternal smoking and prenatal smoke exposure.

It is our opinion that the lack of consistent findings on displacement is likely related to two main factors: pre- versus post-natal exposure and ban venue. The two studies that find evidence of displacement effects focus on post-natal second hand smoke exposure (Adda and Cornaglia, 2010; Shetty et al., 2010). On the other hand, Bharadwaj, Johnsen and Løken (2014) and Bartholomew and Abouk (2016) who focus on pre-natal exposure find no evidence consistent with displacement or increases in prenatal exposure. As indicated in Section II D, pregnant women tend to be more responsive to taxes than the full population of female smokers; we believe the literature also suggests that pre-pregnancy smoking does not respond to smoking bans while smoking during pregnancy does. It is not unreasonable to think that bans are less likely to lead to displacement among pregnant mothers than among mothers more generally; in other words, bans may be more effective in reducing overall smoke exposure prenatally than postnatally. Ban venue may also play a key role in determining the size and scope for displacement. Specifically, one reason that Nguyen (2013) does not find evidence of displacement may be that the ban he studies is for cars carrying children under the age of 18. If parents spend little time in cars with their children – especially relative to the time they spend in cars without children or in hospitality venues (restaurants and bars) and in the workplace – workplace and hospitality bans may be more likely to displace parental smoking and lead to overall rises in children's exposure than bans on smoking in cars with children.

*C.iii. Pregnancy, birth-related, and early childhood outcomes.* The evidence on the effect of smoking bans on birth-related outcomes not only differs across studies and contexts but often

results are very heterogeneous within a single study (see Tables 4A-4B).[99] The strongest evidence of positive effects of smoking bans is presented in Bharadwaj, Johnsen and Løken (2014), who find that a national ban on smoking in restaurants and bars reduces the likelihood of very low birth weight status (VLBW, defined as under 1500 grams) and the risk of preterm birth but does not affect other birth outcomes (average birth weight, LBW status, APGAR scores, sex at birth or birth defects). Markowitz et al. (2013) find that state-level restrictions and bans on smoking in bars generally improve gestational length and birth weight (continuous and categorical measures) but the effects are small in magnitude (except for VLBW status) and statistically significant for only some subpopulations.[100]

Other findings are less consistent or less strong. On one hand, Bartholomew and Abouk (2016) find that comprehensive municipal-level bans (bans that cover workplaces, bars and restaurants) significantly increase average birth weight and gestational length and reduce VLBW and pre-term status in West Virginia. On the other hand, less stringent bans (that cover fewer venues) do not consistently improve health; in the case where only workplace bans are implemented, they find small decreases in average birth weight. Within California, Amaral (2009) finds no significant effects of local- or state-level ordinances on birth weight or gestation once she includes city-specific linear trends. Finally, Briggs and Green (2012) examine the effect of state-, county- and municipality-level bans on several birth outcomes. While they do find some evidence that bans affect child health, the results do not display any consistent patterns.[101]

Where does the current evidence leave us on the impact of CIALs on birth outcomes? Given the mixed findings of the literature, it is difficult to make unqualified conclusions. Still, we believe that the current empirical evidence generally supports the notion that smoking bans improve birth outcomes on average. The results in Bharadwaj, Johnsen and Løken (2014) provide the clearest and most robust evidence in favor of this conclusion, and the findings in Markowitz et al. (2013) and Bartholomew and Abouk (2016) – the other two studies that most convincingly

[99] Yan (2014) finds that an increase in the minimum purchase age for cigarettes in Pennsylvania leads to improved birth outcomes on a number of dimensions, including a reduction in LBW and premature birth and increases in gestational length and APGAR scores. There is also evidence that clinical trials that are successful in reducing prenatal smoking also increase birth weight (Dolan-Mullen, Ramírez and Groff, 1994).

[100] They find significant increases in gestational length for mothers aged 25-34 and improved birth weight categories for mothers aged 20-24, with more than high school education, on Medicaid and on private insurance.

[101] The estimated impact of bans is sometimes positive and at other times negative; some effects are statistically significant and others are not. The differences appear to depend on several key factors: outcome; ban venue, strength and administrative level; whether or not municipality-level bans are included as additional controls; and whether bans are captured by indicators or by the number of months of in utero-exposure. The overall the magnitude of the effects (regardless of sign or statistical significance) are generally quite small.

address the possibility of policy endogeneity – largely agree with this claim (though not always with statistical significance or consistency across subgroups). However, the findings from Amaral (2009) and Briggs and Green (2012) do underscore the importance of taking into account smoke-free policies at other administrative levels, something that the neither of the other two papers using U.S. data (Markowitz et al. and Bartholomew and Abouk) has done. Thus, we consider this area of the literature to be ripe for additional analysis. One way the literature might be extended is to make use of the population-based coverage data from the American Non-smokers' Rights Foundation, which contain information on policies implemented at multiple levels. Another useful addition to the literature would be an examination of these policy effects in developing countries, where smoking rates are generally much higher.[102] Finally, given the overwhelming evidence on the impacts of early life health on later life outcomes (see Almond and Currie (2011b) for an extensive review), a natural extension of this literature would be to examine how in utero exposure to CIALs influences outcomes in adulthood.

*Early childhood health outcomes.* Several studies examine early childhood health responses to bans on smoking. Shetty et al. (2010) find that smoking restrictions – particularly those imposed in the workplace – actually lead to increases in asthma admissions for children (aged 0-17).[103] As discussed in the previous section, these results may indicate potential for displacement effects, wherein parents may shift smoking from the workplace and public places to home in the presence of bans, thus exposing their children to more second-hand smoke. Two studies examine the relationship between smoking bans and cases of sudden infant death syndrome (SIDS). In a state level analysis, Markowitz (2008) finds that smoking restrictions in workplaces, restaurants and child care centers generally reduce the incidence of SIDS, though her results vary according to ban stringency and venue. In contrast, King, Markowitz and Ross (2015) find a positive but insignificant relationship between smoking restrictions and SIDS cases in a panel of 23 developed countries. Though neither study includes area-specific time trends – and thus cannot rule out the possibility that countries or states with and without bans are on differential preexisting trends –

[102] We are aware of only a single paper that examines the relationship between birth outcomes, maternal smoking, and CIALs. Harris, Balsa and Triunfo (2015) find that smoking cessation by the third trimester – instrumented using policy variables, including national bans on smoking in public spaces, public transport and workplaces – is positively associated with birth weight; however, we regard their results as largely descriptive as they are unable to distinguish between the effects of the national bans and general time trends.

[103] The authors admit that when they adjust for multiple comparisons using the Hochberg method, statistical significance of this result disappears.

Markowitz provides additional evidence that her results are not spurious; specifically, she shows that smoking bans are not related to non-smoking related infant fatalities (those involving motor vehicles or drowning). Thus, we regard the evidence in Markowitz as the more robust of the two studies, and conclude that the weight of the evidence favors the interpretation that bans can successfully improve early childhood health outcomes.

## IV. Discussion and conclusion

Are taxes or CIALs more effective in reducing maternal smoking and improving child health outcomes? For the many reasons discussed in the previous sections, it is difficult to compare effects across studies within the tax or ban literature, let alone across the two literatures. In particular, estimates from the bans literature are for differing levels of administration, ban strength and venue. With those caveats in mind, the early tax literature yields participation elasticities of about -0.5, while the later studies give estimates closer to about -0.12 (Table 1). Thus for a 10% increases in prices, we would expect about a 5% (1.2%) reduction in maternal smoking prevalence in the early (later) period. In contrast, the bans literature has found marginal effects of U.S. state-level bans on maternal smoking of about 4% when statistically significant (Table 3). Though we still regard the estimates from Bharadwaj, Johnsen and Løken (2014) as some of the most reliable for the effects of bans, we do not use their estimates as a point of comparison to the tax literature because they study the effect of a nationally enforced ban outside the U.S. Thus it seems that the effect of a state-level U.S. ban is roughly the same as a 10% increase in prices using early elasticity estimates, or about three times the effect of a 10% increase in prices using later elasticity estimates.

Next, we comment on some general trends as another means of comparison across the two literatures. We note that while participation elasticities in the tax literature have generally been declining over time, the opposite trend is observed in the bans literature. There are several potential explanations for these diverging trends. As discussed in Section II D, there is reason to believe that the composition of smokers is changing over time such that in more recent years, only the most addicted mothers (and thus those least responsive to changes in price) remain. In contrast, as postulated in Section III C, the effect of bans on encouraging quits among pregnant women may be growing over time. These relatively large impacts of bans persist even in the face of the changing composition of smoking mothers and occur during a time when the prevalence of CIALs has grown rapidly (around 2002-3 onward). Together, this suggests that the role of bans in

reducing maternal smoking may continue to rise, while the usefulness of taxes may continue to decline. That said, the use of bans as a policy tool (relative to taxes) is limited in at least two important dimensions. First, though policymakers can increase taxes indefinitely (in theory), once a full ban has been implemented there are no further actions to take in that policy space. Second, while taxes generally affect maternal smoking (and thus child health outcomes directly), smoking restrictions can have large impacts on exposure to second hand smoke. Given the possibility that such restrictions may simply reallocate smoking from banned venues to private spaces (such as homes) where children are more likely to be present, there are potential negative consequences of CIALs on children that do not exist with taxation.

In both the tax and bans literatures, the reduced form relationship between birth outcomes and policy is more muted than the relationship between maternal smoking and policy. This is perhaps surprising, given the wealth of existing evidence that links smoking during pregnancy to poor health at birth. However, it is important to keep in mind that while taxes and CIALs do seem to reduce maternal smoking, the absolute magnitudes of these effects are fairly small. Accordingly, the reduced form impact of these policies on average birth outcomes for the entire population (across smoking and non-smoking mothers) is also small. In that sense, quantile estimation methods that separately identify the effects of policy over the entire distribution of health may be an especially fruitful direction for further research, as those babies at the bottom of the health distribution are also the ones who are most likely to be born to mothers who smoke and thus to be affected by smoking policies. In contrast, the evidence that ties taxes and smoking restrictions to health after birth – such as SIDS-related deaths and childhood asthma – is somewhat stronger. Given the cumulative nature of health, it may be the case that the full health effects of smoking-related policies grows over time as children age, an important consideration to keep in mind when evaluating the effectiveness of such policies.

Throughout this review, we have indicated a number of avenues for future research in this area. Of these, we think that the largest contributions can be made in the following areas: (i) better understanding the source of decline in price elasticities over time; (ii) taking a more standardized approach to parameterizing CIALs and reexamining effects on maternal smoking and birth outcomes using data from the U.S.; (iii) perhaps most importantly, investigating the medium- and long-term effects of both taxes and bans on subsequent health and other outcomes. As of yet, there are relatively few studies that link taxes to post-birth childhood health and none that do so for

CIALs. There is currently no evidence of the medium-term impacts of smoking policies on important cognitive and behavioral outcomes for children (such as test scores or incidence of behavioral problems) or the longer-run impacts on adult outcomes (such as final educational attainment and labor market success). Finally, the literature on the effect of tobacco policies other than taxes and bans on maternal smoking and childhood health is extremely limited. Many of the datasets and methods we reviewed could be applied to these other policies. For example, making e-cigarettes illegal for teens may increase teen smoking during pregnancy (Pesko and Currie, 2016); this could be investigated in state-level Vital Statistics data using a regression discontinuity approach exploiting pregnant women aging into the legal purchase age for e-cigarettes.

Given the ability of smoking policies to impact maternal behavior – and thus the intergenerational transmission of human capital – as well as the long line of research showing the importance of the in utero and early life environment in determining the long run success of individuals, it will be important to understand whether and how smoking policies contribute to adult well-being.

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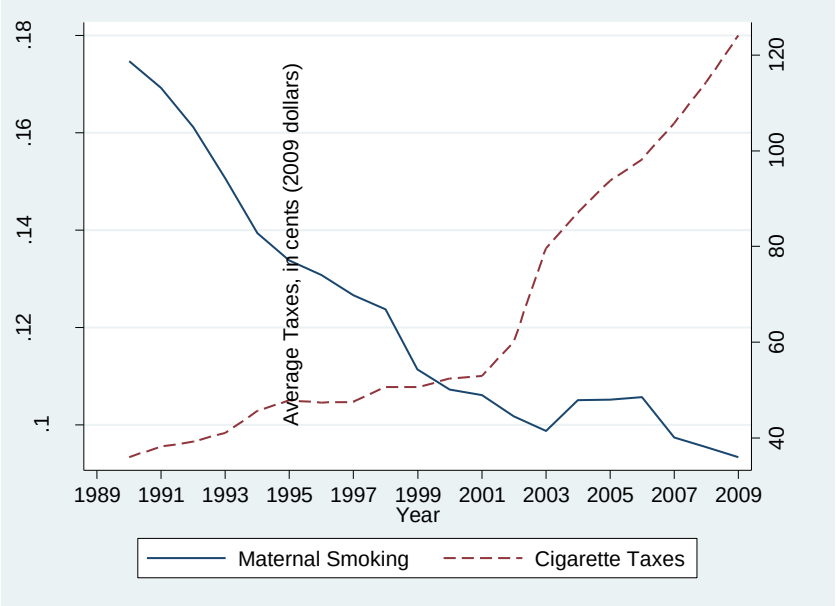
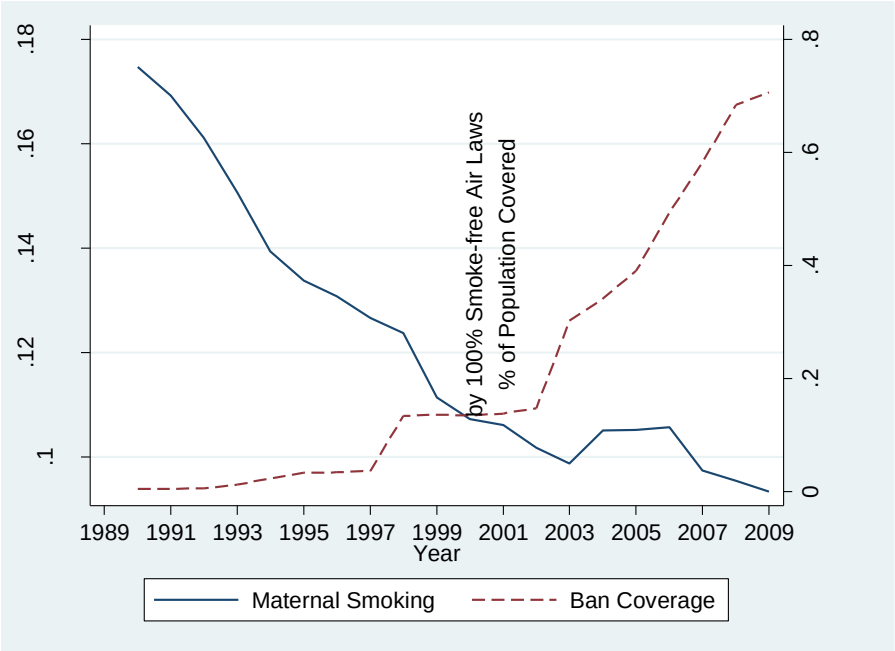
## VI. Figures and Tables

Figure 1: Maternal Smoking and Cigarette Taxes (1989-2009)



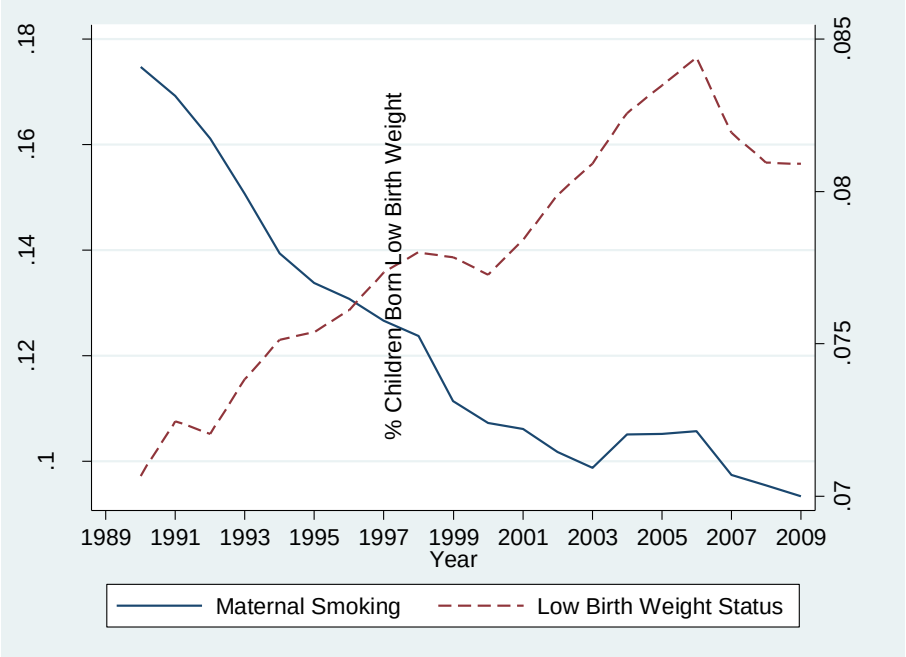

Notes: Data from the National Center for Health Statistics Natality birth files (1997-2009). Maternal smoking is defined as the proportion of mothers who report smoking during pregnancy in the Vital Statistics. Cigarette taxes are defined as the national average of state cigarette excise taxes in cents (in 2009 dollars).

Figure 2: Maternal Smoking and Ban Coverage (1989-2009)



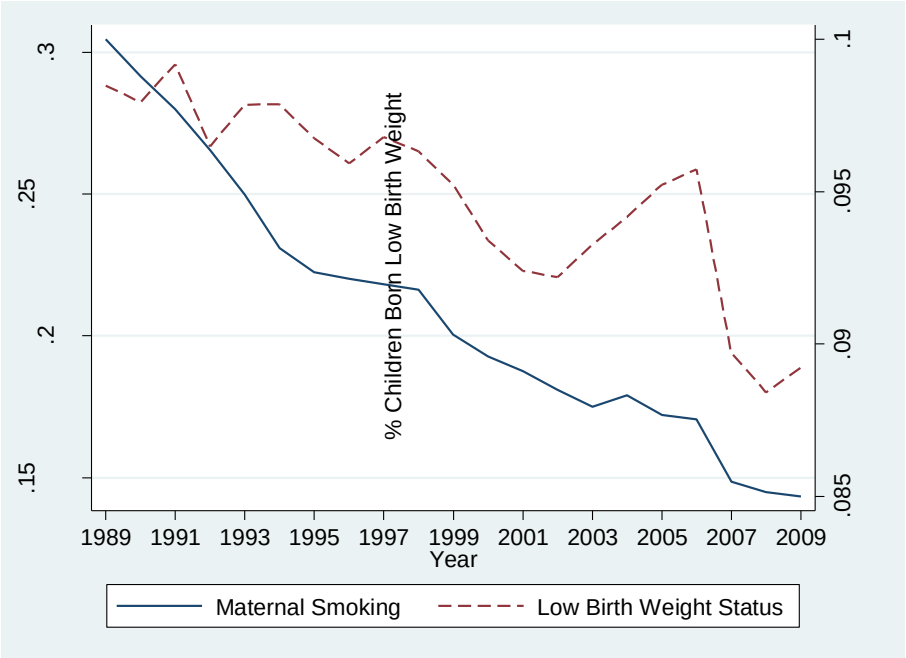

Notes: Data from the National Center for Health Statistics Natality birth files (1997-2009). Maternal smoking is defined as the proportion of mothers who report smoking during pregnancy in the Vital Statistics. Ban coverage is defined as the proportion of the national population covered by at least one 100% smoke-free indoor air law as reported by the American Non-smokers Rights Foundation.

Figure 3a: Maternal Smoking and Low Birth Weight Status, Full Sample (1989-2009)




Notes: Data from the National Center for Health Statistics Natality birth files (1997-2009). Maternal smoking is defined as the proportion of mothers who report smoking during pregnancy in the Vital Statistics. Low birth weight status is defined as weighing less than 2500 grams at birth.

Figure 3b: Maternal Smoking and Low Birth Weight Status, High School Dropouts (1989-2009)




Notes: Data from the National Center for Health Statistics Natality birth files (1997-2009). Maternal smoking is defined as the proportion of mothers who report smoking during pregnancy in the Vital Statistics. Low birth weight status is defined as weighing less than 2500 grams at birth. Sample is restricted to children born to mothers with less than a high school degree.

## VII. Tables

**Table 1: Maternal Smoking Participation and Cigarette Taxes**

| Period Classification | Study | Cohort Years | Dataset | Sample Notes | Real Price per Pack† | Proportion of Smoking Mothers | Participation Elasticity | Strategy |
|---|---|---|---|---|---|---|---|---|
| Studies using data from early period | Evans and Ringel (1999) | 1989-1992 | Vital Statistics | All | $2.77 | 0.171 | -0.52*** | Two-way fixed effects (probit model) |
| | Gruber and Koszegi (2001) | 1989-1996 | Vital Statistics | All | $2.73 | 0.162 | -0.35***†† | Two-way fixed effects |
| | Gruber and Zinman (2001) | 1989-1996 | Vital Statistics | Mothers aged 24 and older | $2.73 | 0.158 | -0.60*** | Two-way fixed effects |
| | Ringel and Evans (2001) | 1989-1995 | Vital Statistics | All | $2.73 | 0.165 | -0.70*** | Two-way fixed effects (probit model) |
| | Bradford (2003) | 1988, 1991 | National Maternal and Infant Health Survey | All | $2.35 | 0.231 | -0.34***†† | Two-stage random effects (logit, FGLS) with individual and time effects |
| | Colman, Grossman and Joyce (2003) | 1993-1999 | PRAMS | All in 10 select states | $2.92 | 0.150 | -0.91*** | Two-way fixed effects |
| | Lien and Evans (2005) | 1990-1997 | Vital Statistics | All mothers in AZ, IL, MA, and MI | $2.76 | 0.175 | -0.49***? | Two-way fixed effects, matching |
| | Simon (forthcoming) | 1989-1999 | Vital Statistics | All | $2.96 | 0.145 | -0.52**‡‡ | Two-way fixed effects |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Significance for the marginal effect of taxes/prices are reported; typically papers do not report the significance of the elasticity.

† Prices are in real 2009 dollars and are inclusive of taxes. For most papers real prices were not reported. Instead, we calculated prices using *The Tax Burden on Tobacco* publication to get the state average pack price for a year averaged over the sample years.

†† Elasticity is for the total number of cigarettes per day; a participation elasticity is not reported.

? Descriptive statistics and participations elasticities were only reported for certain subgroups or states. To get a value for the whole sample, we estimate a weighted average (according to sample size) across the values reported for subgroups or states. We do not report significance for the average results except when this is reported (for the average marginal effect) in the paper.

? ? Adams et al. report an elasticity of quitting, such that a positive elasticity reflects a decrease in maternal smoking. Since quit elasticities are conditional on smoking before pregnancy, they are larger than participation elasticities.

‡‡ When pooling early and late periods, Simon (forthcoming) weights the sample to reflect the same cohort composition of children as in the 1997-2010 NHIS, such that the years 1996-2000 receive extra weight. Elasticities are not reported in the paper so we estimate these directly using a log-log regression on his data (with the same covariate set he uses).

**Table 1 (continued): Maternal Smoking Participation and Cigarette Taxes**

| Period Classification | Study | Cohort Years | Dataset | Sample Notes | Real Price per Pack† | Proportion of Smoking Mothers | Participation Elasticity | Strategy |
|---|---|---|---|---|---|---|---|---|
| Studies using data from late period | Leavy and Meara (2006) | 1996-2000 | Vital Statistics | All | $3.29 | 0.127 | -0.12‡ | Time series with linear trend |
| | DeCicca and Smith (2012) | 1999-2003 | Vital Statistics | All | $4.35 | 0.119 | -0.14*** | Two-way fixed effects |
| | | 1999-2003 | Vital Statistics | All | $4.35 | 0.116⁈ | -0.11⁈ | Two-way fixed effects, matching |
| | Markowitz et al. (2013) | 1996-2008 | PRAMS | All in 29 select states plus New York City | $4.10 | 0.140⁈ | 0.06⁈ | Two-way fixed effects |
| | Adams et al. (2012) | 2000-2005 | PRAMS | All in 29 select states plus New York City | $4.51 | 0.15 | 0.335*⁈ ⁈ | Two-way fixed effects |
| | Simon (forthcoming) | 2000-2015 | Vital Statistics | All | $4.66 | 0.10 | -0.15‡‡ | Two-way fixed effects |
| Data from both early and late periods | Simon (forthcoming) | 1989-2009 | Vital Statistics | All | $3.72 | 0.14 | -0.33***‡‡ | Two-way fixed effects |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level. Significance for the marginal effect of taxes/prices are reported; typically papers do not report the significance of the elasticity. Elasticities were not reported in Markowitz et al. (2013) and therefore we impute these elasticities using formula $e_d = \frac{\alpha_1}{\frac{\partial p}{\partial \tau}} \frac{\bar{p}}{\bar{s}}$.

† Prices are in real 2009 dollars and are inclusive of taxes. For most papers real prices were not reported. Instead, we calculated prices using *The Tax Burden on Tobacco* publication to get the state average pack price for a year averaged over the sample years.

†† Elasticity is for the total number of cigarettes per day; a participation elasticity is not reported.

⁈ Descriptive statistics and participations elasticities were only reported for certain subgroups or states. To get a value for the whole sample, we estimate a weighted average (according to sample size) across the values reported for subgroups or states. We do not report significance for the average results except when this is reported (for the average marginal effect) in the paper.

⁈ ⁈ Adams et al. report an elasticity of quitting, such that a positive elasticity reflects a decrease in maternal smoking. Since quit elasticities are conditional on smoking before pregnancy, they are larger than participation elasticities.

‡ Elasticity is for the time period 15 months following the MSA. Statistical significance is for the difference in smoking rates with and without MSA; statistical significance of the elasticity not reported.

‡‡ When pooling early and late periods, Simon (forthcoming) weights the sample to reflect the same cohort composition of children as in the 1997-2010 NHIS, such that the years 1996-2000 receive extra weight. Elasticities are not reported in the paper so we estimate these directly using a log-log regression on his data (with the same covariate set he uses).

**Table 2: Birth Weight and Cigarette Taxes**

| Period Classification | Study | Cohort Years | Dataset | Sample Notes | Marginal Effects on Birth Weight (grams)† | Tax Elasticity of Birth Weight‡ | Marginal Effects on Low Birth Weight Status† | Tax Elasticity of LBW‡ | Strategy |
|---|---|---|---|---|---|---|---|---|---|
| Early Studies | Evans and Ringel (1999) | 1989-1992 | Vital Statistics | All | 0.208*** (0.006%) | 0.01 | -0.00045 (-0.008%) | -0.10 | Two-way fixed effects |
| | Lien and Evans (2005) | 1990-1997 | Vital Statistics | All births in AZ, IL, MA, and MI | 29.0†† (0.9%) | 0.02 | -0.0075†† (-10.3%) | -0.18 | Two-way fixed effects, matching |
| | Simon, forthcoming | 1989-1999 | Vital Statistics | All | -- | -- | -0.002‡‡ -- | -- | Two-way fixed effects |
| Later Studies | DeCicca and Smith (2012) | 1999-2003 | Vital Statistics | All | -- | -- | -0.00136** (-1.1%) | -0.03 | Two-way fixed effects |
| | | | | | -- | -- | -0.00242†† (-2.0%) | -0.06 | Two-way fixed effects, matching |
| | Markowitz et al. (2013) | 1996-2008 | PRAMS | All | 9.29†† (0.3%) | 0.01 | -0.00128‡ (-1.86%) | -0.08 | Two-way fixed effects |
| | Simon, forthcoming | 2000-2009 | Vital Statistics | All | -- | -- | 0.0008‡‡ -- | -- | Two-way fixed effects |
| Both Early and Late Periods | Simon, forthcoming | 1989-2009 | Vital Statistics | All | -- | -- | -0.003*‡‡ (-3.19%) | -0.12 | Two-way fixed effects |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

† Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.

†† Estimates are an average of estimates across the individual marginal effects for each treatment state or age group, weighted by sample size for each group; statistical significance of the average/pooled elasticity is not reported.

‡ To get tax elasticities, we multiply the marginal effects of taxes on infant health from the relevant study by the ratio of the average cigarette price during that time to the average birth outcome of interest. This tells us how a percentage change in price from a cigarette tax leads to a percentage change in infant health.

‡‡ In Simon (forthcoming) low birth weight estimates are indirectly reported in figures 4 and 5. We use Simon's data to derive exact point estimates and significance levels. For the early and late periods, the proportion of low birth weight babies was not reported so we cannot calculate elasticities.

**Table 3: Maternal Smoking Participation and CIALs**

| Study | Cohort Years | Dataset | Sample Notes | Restriction Venue | Restriction Strength‡‡ | Restriction Level (Administrative) | Marginal Effect on Maternal Smoking/Quitting† | Strategy |
|---|---|---|---|---|---|---|---|---|
| Gruber and Zinman (2001) | 1991–97 | Vital Statistics | All mothers aged 13-18 | Workplaces | Not disclosed | State | 0.011** (6.11%) | Two-way fixed effects |
| | | | | Government | Not disclosed | State | -0.002 (-1.11%) | |
| | | | | Restaurants and Bars | Not disclosed | State | -0.007* (-3.89%) | |
| | | | | Schools | Not disclosed | State | 0.004 (2.22%) | |
| | | | | Other | Not disclosed | State | -0.006 -3.33% | |
| Adams et al. (2012) | 2000-2005 | PRAMS | All in 29 select states plus New York City who smoke at the start of pregnancy | Workplaces | Partial | State | -0.038 (Quitting) 0.010 (Smoking††) (3.91%) | Two-way fixed effects (probit model) |
| | | | | Workplaces | Full | State | 0.044*** (Quitting) -0.011 (Smoking††) (-4.30%) | |
| Markowitz et al. (2013)‡ | 1996-2008 | PRAMS | All in 29 select states plus New York City | Restaurant | Partial | State | 0.010 (7.19%) | Two-way fixed effects (probit model) |
| | | | | Restaurant | Full | State | 0.006 (4.32%) | |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

† Effects are for maternal smoking, unless otherwise indicated. Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.

†† Marginal effect on smoking is imputed using the reported estimated effect on quitting by the third trimester/end of pregnancy and the pre-ban/pre-pregnancy proportion of quitters and smokers.

‡ Estimates are an average across all age groups, weighted by sample size for each age group no statistical significance reported for average/pooled effect.

‡‡ A full restriction refers to a complete ban on smoking in a given area. A partial restriction requires designated smoking areas or designated smoking areas with separate ventilation.

**Table 3 (continued): Maternal Smoking Participation and CIALs**

| Study | Cohort Years | Dataset | Sample Notes | Restriction Venue | Restriction Strength‡‡ | Restriction Level (Administrative) | Marginal Effect on Maternal Smoking/Quitting† | Strategy |
|---|---|---|---|---|---|---|---|---|
| Bharadwaj, Johnsen and Løken (2014) | January–May 2004 and November 2004–March 2005 | | All mothers working in restaurants, bars and retail stores who smoke at the start of pregnancy | Restaurants and Bars | Full | National | 0.150** (Quitting)<br>-0.030 (Smoking††)<br>(-15.2%) | Difference-in-difference (time X employment sector) |
| Bartholomew and Abouk (2016) | 1995-2010 | State Birth Certificate Data | All mothers resident in West Virginia | Index | Comprehensive (full in workplaces, restaurants, bars) | County | -0.015<br>(-5.75%) | Two-way fixed effects (probit and logit models) |
| | | | | Index | Restrictive (full in workplaces, restaurants; none in bars) | County | 0.007<br>(2.68%) | |
| | | | | Index | Moderate (full in workplaces; partial in restaurants; none in bars) | County | 0.005<br>(1.92%) | |
| | | | | Index | Comprehensive (partial in workplace; any in restaurants; none in bars) | County | -0.014<br>(-5.37%) | |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

† Effects are for maternal smoking, unless otherwise indicated. Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.

†† Marginal effect on smoking is imputed using the reported estimated effect on quitting by the third trimester/end of pregnancy and the pre-ban/pre-pregnancy proportion of quitters and smokers.

‡ Estimates are an average across all age groups, weighted by sample size for each age group no statistical significance reported for average/pooled effect.

‡‡ A full restriction refers to a complete ban on smoking in a given area. A partial restriction requires designated smoking areas or designated smoking areas with separate ventilation.

**Table 4A: Birth Outcomes and CIALs - Study Descriptions**

| Study | Cohort Years | Dataset | Sample Notes | Strategy |
|---|---|---|---|---|
| Amaral (2009) | 1988-1999 | Vital Statistics | Births in California Only | Difference-in-difference (time periods X ever had local ordinance) |
| Briggs and Green (2012) | 2000-2005 | PRAMS | All in 29 select states plus New York City who smoke at the start of pregnancy | Two-way fixed effects (probit model) |
| Markowitz et al. (2013) | 1996-2008 | PRAMS | All in 29 select states plus New York City | Two-way fixed effects (probit model) |
| Bharadwaj, Johnsen and Løken (2014) | January–May 2004 and November 2004–March 2005 | | All mothers working in restaurants, bars and retail stores who smoke at the start of pregnancy | Difference-in-difference (time X employment sector) |
| Bartholomew and Abouk (2016) | 1995-2010 | State Birth Certificate Data | All mothers resident in West Virginia | Two-way fixed effects (probit and logit models) |

**Table 4B: Birth Outcomes and CIALs - Marginal Effects**

| Study | Restriction Venue | Restriction Strength? ? | Restriction Level (Administrative) | Marginal Effects† Birth Weight (grams) | Low Birth Weight Status | Very Low Birth Weight Status | Gestation (weeks) | Preterm/ Full Term |
|---|---|---|---|---|---|---|---|---|
| Amaral (2009)†† | Workplaces | Full | County and City | -1.83 | 0.0009 | -0.0003 | 0.035 | -- |
| | | | | (-0.05%) | (1.65%) | (-3.75%) | (-0.01%) | -- |
| | Workplaces | Full | State | -2.45 | -0.00001 | -0.0003 | -0.064 | -- |
| | | | | (-0.07%) | (-0.02%) | (-3.75%) | (-0.02%) | -- |
| Briggs and Green (2012)‡ | Workplaces | Partial | State | 1.11 | -0.001** | -- | -0.018** | -- |
| | | | | (0.04%) | (-1.25%) | -- | (-0.05%) | -- |
| | Workplaces | Full | State | -13.1*** | 0.000 | -- | 0.032** | -- |
| | | | | (-0.04%) | (0.00%) | -- | (0.08%) | -- |
| | Restaurants | Partial | State | -23.0*** | 0.003*** | -- | -0.020** | -- |
| | | | | (-0.70%) | (-3.75%) | -- | (-0.05%) | -- |
| | Restaurants | Full | State | 5.61*** | 0.001 | -- | -0.011 | -- |
| | | | | (0.17%) | (1.25%) | -- | (-0.03%) | -- |
| | Bars | Partial | State | 3.28 | -0.002 | -- | -0.95*** | -- |
| | | | | (0.10%) | (-2.50%) | -- | (-2.45%) | -- |
| | Bars | Full | State | -1.82 | 0.000 | -- | -0.035*** | -- |
| | | | | (0.05%) | (0.00%) | -- | (-0.09%) | -- |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

† Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.

†† Reported estimates are from regressions that include state-specific linear trends.

‡ Reported estimates for the county within state sample. For some ban venues (work places and restaurants), there is an additional ban strength indicator (qualified) but due to space constraints results for that indicator are not reported in this table.

‡‡ Estimates are an average across all age groups, weighted by sample size for each age group no statistical significance reported for average/pooled effect.

? Estimates are for the marginal effect of a **full term** birth.

? ? A full restriction refers to a complete ban on smoking in a given area. A partial restriction requires designated smoking areas or designated smoking areas with separate ventilation.

**Table 4B (continued): Birth Outcomes and CIALs - Marginal Effects**

| | | | | Marginal Effects† | | | | |
|---|---|---|---|---|---|---|---|---|
| Study | Restriction Venue | Restriction Strength? ? | Restriction Level (Administrative) | Birth Weight (grams) | Low Birth Weight Status | Very Low Birth Weight Status | Gestation (weeks) | Preterm/ Full Term |
| Briggs and Green (2012)‡ | Workplaces | Partial | County | -0.039 | 0.000 | -- | 0.013*** | -- |
| | | | | (-0.01%) | (0.00%) | -- | (0.03%) | -- |
| | Workplaces | Full | County | 3.06 | -0.002 | -- | -0.027* | -- |
| | | | | (0.10%) | (-2.50%) | -- | (-0.07%) | -- |
| | Restaurants | Partial | County | 5.08*** | -0.002*** | -- | -0.006 | -- |
| | | | | (0.15%) | (-2.50%) | -- | (-0.02%) | -- |
| | Restaurants | Full | County | -8.11 | 0.002 | -- | 0.105*** | -- |
| | | | | (-0.24%) | -2.50% | -- | (0.27%) | -- |
| | Bars | Partial | County | 3.92 | -0.002 | -- | -0.315*** | -- |
| | | | | (0.12) | (-2.50%) | -- | (-0.81%) | -- |
| | Bars | Full | County | -4.74 | -0.001 | -- | -0.160*** | -- |
| | | | | (-0.14%) | (-1.25%) | -- | (-0.41%) | -- |
| Markowitz et al. (2013)‡‡ | Restaurants | Partial | State | 5.78 | -0.003 | -0.0009 | 0.066 | 0.006? |
| | | | | (0.17%) | (-3.75%) | (-9.0%) | (0.17%) | (0.65%) |
| | Restaurants | Full | State | 8.40 | -.003 | -0.0006 | 0.083 | 0.002? |
| | | | | (0.25%) | (-3.75%) | (-6.0%) | (0.21%) | (0.22%) |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level
† Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.
†† Reported estimates are from regressions that include state-specific linear trends.
‡ Reported estimates for the county within state sample. For some ban venues (work places and restaurants), there is an additional ban strength indicator (qualified) but due to space constraints results for that indicator are not reported in this table.
‡‡ Estimates are an average across all age groups, weighted by sample size for each age group no statistical significance reported for average/pooled effect.
? Estimates are for the marginal effect of a **full term** birth.
? ? A full restriction refers to a complete ban on smoking in a given area. A partial restriction requires designated smoking areas or designated smoking areas with separate ventilation.

**Table 4B (continued): Birth Outcomes and CIALs - Marginal Effects**

| Study | Restriction Venue | Restriction Strength? ? | Restriction Level (Administrative) | Marginal Effects† Birth Weight (grams) | Low Birth Weight Status | Very Low Birth Weight Status | Gestation (weeks) | Preterm/ Full Term |
|---|---|---|---|---|---|---|---|---|
| Bharadwaj, Johnsen and Løken (2014) | Restaurants and Bars | Full | National | 54.9 | -0.0001 | -.018** | -- | -0.025* |
| | | | | (1.59%) | (-7.34%) | (-78.3%) | -- | (-39.1%) |
| Bartholomew and Abouk (2016) | Index | Comprehensive (full in workplaces, restaurants, bars) | County | 28.8* | -0.005 | -.004* | 1.64 | -0.015 |
| | | | | (0.87%) | (-7.34%) | (-44.4%) | (0.60%) | (-13.7%) |
| | Index | Restrictive (full in workplaces, restaurants; none in bars) | County | -2.77 | 0.002 | -.002* | 0.046 | 0.003 |
| | | | | (-.08%) | (2.94%) | (-22.2%) | (0.02%) | (2.73%) |
| | Index | Moderate (full in workplaces; partial in restaurants; none in bars) | County | -23.3** | 0.008 | 0.001 | -0.355 | 0.004 |
| | | | | (-0.71%) | (11.75%) | (11.1%) | (-0.13%) | (3.64%) |
| | Index | Limited (partial in workplace; any in restaurants; none in bars) | County | -5.58 | -0.001 | -0.001 | 0.202 | 0.001 |
| | | | | (-0.17%) | (-1.47%) | (-11.1%) | (0.74%) | (0.91%) |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level
† Effects relative to the mean of the dependent variable reported in parentheses below marginal effects in absolute terms.
†† Reported estimates are from regressions that include state-specific linear trends.
‡ Reported estimates for the county within state sample. For some ban venues (work places and restaurants), there is an additional ban strength indicator (qualified) but due to space constraints results for that indicator are not reported in this table.
‡‡ Estimates are an average across all age groups, weighted by sample size for each age group no statistical significance reported for average/pooled effect.
? Estimates are for the marginal effect of a **full term** birth.
? ? A full restriction refers to a complete ban on smoking in a given area. A partial restriction requires designated smoking areas or designated smoking areas with separate ventilation.

# VIII. Appendices (For Online Publication)

## A. Appendix A – Appendix Tables

**Appendix Table 1A: Datasets on Maternal Smoking and Child Health Outcomes**

| Dataset | Survey Years | Sample Notes | Maternal Smoking Behavior | Birth Outcomes | Childhood Health Outcomes | Geographical Identifiers |
|---|---|---|---|---|---|---|
| Vital Statistics National Birth Certificate Data | 1968-2014 | Includes all women who give birth during a given year | (1) # of cigarettes smoked per day during pregnancy<br>(2) # of cigarettes smoked per day each trimester (beginning in 2013) | Birth weight; APGAR score; abnormal conditions and congenital anomalies | -- | State and county (restricted access after 2004) |
| State Birth Certificate Data | Varies by state | May allow for multiple pregnancy matching by mother | Varies by state, may include data such as # of cigarettes smoked per day during pregnancy | Varies by state, usually includes birth weight and some information on health status | -- | Varies by state; can get as detailed as zip code (may be restricted access) |
| Pregnancy Risk Assessment Monitoring System (PRAMS) | 1988-2013 | Not every state participates in every year; number of participating states varies from 3 to 28 in a given year (avg=19 states) | (1) # of cigarettes smoked pre-pregnancy<br>(2) # of cigarettes smoked each trimester<br>(3) # of cigarettes smoked per day | Alive at birth; birth defect; birth weight | -- | State; urban/rural |
| National Health and Nutrition Examination Survey (NHANES) | 1999-2016 | Other similar surveys available for years 1959-1998 | (1) Cotinine level<br>(2) Mother smoked when pregnant (self-reported)<br>(3) Other smokers in household (# of people & # of cigarettes, self-reported) | Birth weight | Underweight/overweight (self-reported, told by a doctor) | State; county; Census block group |
| National Survey of Family Growth (NSFG) | 1973, 1976, 1982, 1988, 1995, 2002, 2006-2010, 2011-2013 | Only surveys women aged 15-44 (expanded to 49 in 2015) | (1) Amount smoked 6 months before mother knew she was pregnant<br>(2) Amount smoked during pregnancy<br>(3) Mother smoked at all once she knew she was pregnant | Alive at birth; birth weight | -- | -- |

**Appendix Table 1A (continued): Datasets on Maternal Smoking and Child Health Outcomes**

| Dataset | Survey Years | Sample Notes | Maternal Smoking Behavior | Birth Outcomes | Childhood Health Outcomes | Geographical Identifiers |
|---|---|---|---|---|---|---|
| National Health Interview Survey (NHIS) | 1963-2016 | Pregnancy & Smoking Supplement administered in 1985, 1990, 1991, 1998, 2010 | Only in the pregnancy & smoking supplements: (1) Smoke at all during pregnancy (2) # of cigarettes smoked a day before learning she was pregnant (3) # of cigarettes smoked a day after learning she was pregnant | Retrospective birth weight for some children | Many health conditions disabilities, vaccinations | U.S. Regions (Northeast, North Central, South, West) and MSA Size. State and county can be available in the restricted use version of the data |
| National Maternal and Infant Health Survey and Longitudinal Follow-Up | 1988, Follow-Up in 1991 | Groups at risk for adverse pregnancy outcomes were oversampled; Sampled from 48 states, D.C., and New York City | (1) Told to stop/cut down smoking at a prenatal visit (2) Smoked in the 12 months prior to delivery (3) # of cigarettes smoked a day before learning she was pregnant (4) # of cigarettes smoked a day after learning she was pregnant (5) Quit smoking for at least a week during pregnancy | Alive at birth; birth weight | Health events after birth (in follow-up) | U.S. Regions |
| National Pregnancy and Health Survey | 1992-1993 (one wave) | Sampled from 37 hospital clusters, including 60 hospitals | (1) Mother smoked at all (2) # of days per week smoked (3) # of cigarettes smoked per day (evaluated at different points during pregnancy, verified by medical records and urine samples) | Alive at birth; birth weight | -- | -- |

**Appendix Table 1B: Datasets on Tobacco Prices and Policies**

| Dataset | Survey Years | Sample Notes | Policy Variables | Venues | Geographical Identifiers |
|---|---|---|---|---|---|
| Tax Burden on Tobacco Publication | 1970-2014 | Data can be obtained from STATE System | (1) Gross tax revenue from cigarettes, in dollars<br>(2) Average cost per pack, in dollars<br>(3) Pack sales per capita<br>(4) Federal & State tax as % of price<br>(5) Federal & State tax per pack, in dollars<br>(6) State tax per pack, in dollars | -- | State |
| Tobacco Use Supplement of the Current Population Survey (CPS-TUS) | 1992-1993, 1995-1996, 1998-1999, 2000, 2001-2002, 2003, 2006-2007, and 2010-2011 | CPS Sample | (1) Smoking status and amount smoked<br>(2) Smoking history and quitting behavior<br>(3) Cost of cigarettes and purchase location<br>(4) Workplace and home smoking restrictions<br>(5) Attitudes toward smoke-free policies | -- | State, County, MSA (as in usual CPS data) |
| Nielsen Retail Data | 2006-2016 | 35,000 stores; Alaska and Hawaii not included | For each UPC code:<br>(1) Units sold<br>(2) Price<br>(3) Price multiplier<br>(4) Baseline units<br>(5) Baseline price<br>(6) Feature & display indicators | Food stores, drug stores, mass merchandisers, others stores (e.g. liquor, convenience) | State, Sub-State |
| State Tobacco Activities Tracking and Evaluation (STATE) System | 1995-2016 | -- | (1) Taxation<br>(2) Smoke-free laws (indoors & schools)<br>(3) Retail licensing laws<br>(4) Youth access laws<br>(5) Preemptive laws<br>(6) Tobacco control spending | 25 unique categories (e.g. bars, restaurants, gov't worksites, private worksites, etc.) | State |

**Appendix Table 1B (continued): Datasets on Tobacco Prices and Policies**

| Dataset | Survey Years | Sample Notes | Policy Variables | Venues | Geographical Identifiers |
|---|---|---|---|---|---|
| American Non-Smokers' Rights Foundation, U.S. Tobacco Control Laws Database | 1985-2015 (records back to 1914) | 13,000 laws in over 5,000 municipalities | (1) 100% smoke-free air laws (local & state)<br>(2) Sales/distribution laws (state & local)<br>(3) Youth access laws (state & local)<br>(4) Advertising laws (local)<br>(5) Excise tax laws (local)<br>(6) Conditional Use Permits (local) | Workplaces, restaurants, bars, gambling, private clubs, private vehicles, residences, foster care, other public places | State, County, Municipality |
| ImpacTeen State Level Tobacco Control Policy and Prevalence Database | 1991-2008 | Includes 50 states and Washington D.C. | (1) Price, taxes, associated revenue<br>(2) Tobacco control program funding<br>(3) Youth access laws<br>(4) Clean air laws<br>(5) Preemptive laws | Healthcare facilities, worksites, child care centers, restaurants, bars, recreational & cultural facilities, public transit, shopping malls, schools | State |

**Appendix Table 2: Impact of Taxes on State Spending**

## Impact of Cigarette Taxes on Per Capita State Transfers to Individuals (2009 Dollars)

| | Total transfers | SSI benefits | Transfers to nonprofits |
|---|---|---|---|
| Excise Tax (dollars) | 122.10 | 4.63 | -0.16 |
| | (89.01) | (5.37) | (0.19) |
| mean | 3748.07 | 107.54 | 18.30 |
| *N* | 1071 | 1071 | 1071 |
| | Retirement and disability insurance benefits | Supplemental nutrition assistance programs | Public medical assistance |
| Excise Tax (dollars) | 18.82 | -1.16 | 136.6* |
| | (20.37) | (5.09) | (78.04) |
| mean | 1490.03 | 76.42 | 704.15 |
| *N* | 1071 | 1071 | 1071 |
| | Family income assistance | State unemployment benefits | |
| Excise Tax (dollars) | -4.70 | 10.13 | |
| | (5.30) | (6.59) | |
| mean | 69.98 | 104.12 | |
| *N* | 1071 | 1071 | |

Notes: Each reported coefficient is from a separate model with the dependent variable listed in the row above. The coefficient in each column is the average yearly cigarette excise tax in 2009 dollars. Standard errors clustered on state are in parentheses. The REIS data on government transfers to individuals (1988-2007) is the main dataset used in this table. All models include fixed effects for state and year.

**Appendix Table 3A: Maternal Smoking and Cigarette Taxes - By Age Groups**

| Study | Cohort Years | Dataset | Strategy | Demographic Group | Proportion of Smoking Mothers | Participation Elasticity† | Relative Size of Group (among Smoking Mothers) †† |
|---|---|---|---|---|---|---|---|
| Gruber and Zinman (2001) | 1991-1997 | Vital Statistics | Two-way fixed effects | Mothers aged 13-18 | 0.180 | -0.35*** | 0.196 |
| | | | | Mothers aged 17-18 | 0.127 | -0.38 | 0.273 |
| | | | | Mothers aged 13-16 | 0.164 | -0.24*** | 0.531 |
| Ringel and Evans (2001) | 1989-1995 | Vital Statistics | Two-way fixed effects (probit model) | Mothers aged <=19 | 0.184 | -0.50*** | 0.149 |
| | | | | Mothers aged 20-24 | 0.200 | -0.55*** | 0.319 |
| | | | | Mothers aged 25-29 | 0.158 | -0.58*** | 0.277 |
| | | | | Mothers aged 30-34 | 0.138 | -1.18*** | 0.182 |
| | | | | Mothers aged 35-39 | 0.128 | -1.13*** | 0.064 |
| | | | | Mothers aged >=40 | 0.111 | -1.02*** | 0.009 |
| Leavy and Meara (2006)‡,‡‡ | 1996-2000 | National Maternal and Infant Health Survey | Two-stage random effects (logit, FGLS) with individual and time effects | Mothers aged 15-19 | 0.164 | -0.30*** | 0.170 |
| | | | | Mothers aged 20-34 | 0.113 | -0.06 | 0.733 |
| | | | | Mothers aged 35-44 | 0.086 | -0.30** | 0.097 |
| Markowitz et al. (2013) | 1996-2008 | PRAMS | Two-way fixed effects | Mothers aged < 20 | 0.180 | 0.002 | 0.178 |
| | | | | Mothers aged 20-24 | 0.190 | 0.021 | 0.354 |
| | | | | Mothers aged 25-34 | 0.110 | 0.152 | 0.370 |
| | | | | Mothers aged 35+ | 0.100 | -0.130 | 0.097 |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

Participation elasticities by subgroups were often not directly reported in the paper. When elasticities were not reported we imputed their values using formula $e_d = \frac{\alpha_1}{\frac{\partial p}{\partial \tau}} \frac{\bar{p}}{\bar{s}}$.

† Significance is for the marginal effect of taxes/prices reported; statistical significance of the elasticity is not reported.

†† Relative size of group refers to the proportion of smoking mothers represented by each demographic group. This stands in contrast to the column the "Proportion of Smoking Mothers," which reports the proportion of mothers within each demographic group that smoke.

‡ Proportions are regression-based estimates for 15 months following the MSA.

‡‡ Elasticity is for the time period 15 months following the MSA. Statistical significance is for the difference in smoking rates with and without MSA; statistical significance of the elasticity not reported.

**Appendix Table 3B: Maternal Smoking and Cigarette Taxes - By Maternal Education**

| Study | Cohort Years | Dataset | Strategy | Demographic Group | Proportion of Smoking Mothers | Participation Elasticity | Relative Size of Group (among Smoking Mothers) †† |
|---|---|---|---|---|---|---|---|
| Ringel and Evans (2001) | 1989-1995 | Vital Statistics | Two-way fixed effects (probit model) | Mothers with less than HS educ | 0.271 | -0.30*** | 0.346 |
| | | | | Mothers with HS degree | 0.198 | -0.49*** | 0.440 |
| | | | | Mothers with some college | 0.117 | -0.86*** | 0.149 |
| | | | | Mothers with college degree or more | 0.036 | -3.39*** | 0.042 |
| | | | | Mothers education not reported | 0.175 | -1.14 | 0.023 |
| Leavy and Meara (2006)‡,‡‡ | 1996-2000 | Vital Statistics | Time series with linear trend | Mothers with less than HS educ | 0.199 | -0.15* | 0.360 |
| | | | | Mothers with HS degree | 0.153 | -0.05 | 0.435 |
| | | | | Mothers with some college | 0.087 | -0.04 | 0.165 |
| | | | | Mothers with college degree or more | 0.019 | -0.18 | 0.041 |
| DeCicca and Smith (2012) | 1999-2003 | Vital Statistics | Two-way fixed effects | All | 0.119 | -0.14*** | 0.738 |
| | | | | Mothers with less than HS educ | 0.206 | -0.24*** | 0.262 |
| | | | Two-way fixed effects, matching | All | 0.119 | -0.11⁈ | 0.346 |
| | | | | Mothers with less than HS educ | 0.206 | -0.25⁈ | 0.440 |

*** significant at the 1% level; ** significant at the 5% level; * significant at the 10% level

Participation elasticities by subgroups were often not directly reported in the paper. When not reported they we imputed elasticity values using formula $e_d = \frac{\alpha_1}{\frac{\partial p}{\partial \tau}} \frac{\bar{p}}{\bar{s}}$.

† Significance for the marginal effect of taxes/prices reported; statistical significance of the elasticity not reported.

†† Relative size of group refers to the proportion of smoking mothers represented by each demographic group. This stands in contrast to the column the "Proportion of Smoking Mothers," which reports the proportion of mothers within each demographic group that smoke.

‡ Proportions are regression-based estimates for 15 months following the MSA.

‡‡ Elasticity is for the time period 15 months following the MSA. Statistical significance is for the difference in smoking rates with and without MSA; statistical significance of the elasticity not reported.

⁈ Estimates are an average across treatment states, weighted by sample size for each state; no statistical significance reported for average/pooled elasticity.

## B. Appendix B -- Commonly used datasets: maternal smoking and child health.

To study maternal smoking and child health, a potential dataset would ideally contain the following elements:

1) *Pregnancy and child health outcomes*: A minimum requirement is to have a dataset that either interviews pregnant women or collects information retrospectively on mothers during the course of their pregnancy. Ideally, this includes information on the timing of conception and/or birth, medical health inputs (such as prenatal care use, procedures, diagnostics), behavioral health inputs (such as cigarette and alcohol consumption), birth outcomes (such as birth weight and gestational age), pregnancy complications, and background information on mothers (such as demographics and health insurance status).
2) *Smoking behavior and smoking dynamics*: Though some datasets include biological markers for nicotine intake (such as cotinine levels), most datasets rely on survey responses to smoking questions where underreporting is a major source of measurement error. Studies show that up to 20-30% of smoking mothers report that they did not smoke during pregnancy (Brachet, 2008; Dietz et al., 2011; Ford et al., 1997; Klebanoff et al., 2001). Even under the assumption of classical measurement error, such error can lead to bias and inconsistency when the outcome variable is dichotomous (Aigner, 1973). Therefore, failure to accurately report smoking could mask the effects of the tax on smoking.

   In addition, very few datasets offer a reasonable look at the within-pregnancy dynamics in maternal smoking over time. The ideal data would include accurate information on smoking in the periods prior to, during and following the pregnancy. Smoking even early in a pregnancy is associated with cognitive damage (Falk et al., 2005; Roza et al., 2007), while smoking during the second or third trimester is shown to be a stronger predictor low birth weight than smoking earlier in the pregnancy (Ahlsten, Cnattingius and Lindmark, 1993; Juarez and Merlo, 2013; Lieberman et al., 1994; MacArthur and Knox, 1988; Ohmi, Hirooka, and Mochizuki, 2002; Yan, 2013; Yan and Groothius, 2015). Likewise, women who quit smoking upon learning they are pregnant may revert after birth[104]; since postnatal smoking is also harmful to infant health, measuring smoking behavior only during pregnancy may neglect to account for the full health effects of a tax change. Without longitudinal data or a pseudo panel of mothers, it is difficult to capture these dynamics.[105]
3) *Sample sizes*: Over the past 15 years, 13.5% of mothers report smoking during pregnancy and of those who smoke, only a subset are potentially influenced by tax changes. Accordingly, large data sets are typically required for sufficient statistical power to identify impacts on in maternal smoking using state-year variation in tax laws.
4) *Geographical and temporal identifiers*: Matching state excise tax to maternal and child data requires information on the state women were living in leading up to conception and/or birth, as well as the year and month of conception and birth.

With these general characteristics in mind, we now describe the commonly used datasets in this literature in turn.

*Vital Statistics Natality Birth Certificate Data* (henceforth referred to as the Vital Statistics): These data include the census of all women who give birth during a given calendar year in the United States. Birth certificates include information on the state, county, and month of birth; mother's education and race; and child's birth weight. Birth certificate data is collected and maintained by the federal government through the National Center for Health Statistics (NCHS). Most state birth certificates also provide information on smoking during pregnancy and the

[104] Indeed, Colman, Grossman and Joyce (2003) report that 51% of women who quit smoking during pregnancy resume between 2-6 months postpartum.

[105] A related concern is that there are currently no data for evaluating long term trends in smoking during pregnancy. The earliest data available are from the 1967 National Natality Survey. Smoking questions were not added to birth certificate data until 1989 (Surgeon General's Report, 2014).

number of cigarettes smoked.[106] After 2004, access to county and state identifiers has been restricted, though the application process for the restricted use data is free and relatively simple.[107] The key advantage of the birth certificate data is the sheer size of the dataset; there are approximately 3.9 million births per year in the United States, most of which are recorded in the vital statistics data. This data set is also the most commonly used to estimate the price elasticity of smoking and the effect of taxes on birth outcomes.

One issue with using these data is that prior to 2003 the questions on smoking did not specify the number of cigarettes smoked per day nor the timing of smoking relative to the pregnancy. In addition inhibiting the identification of the effects of the timing and intensity of smoking on birth and child outcomes, the ambiguity of the questionnaire text could contribute substantially to measurement error in maternal smoking status. For example, the questionnaire asks about "tobacco use during pregnancy," but it is unclear whether this refers to any tobacco use during the entire course of pregnancy or specifically to heavy tobacco use at the end of the pregnancy. Different mothers, nurses and hospitals will interpret this question in different ways, and this will add additional measurement error.

Since 2003, the standard birth certificate was revised to include a great deal of additional information, and this revised certificate has gradually been adopted at the state level. Of particular interest to this review is that the revised birth certificate gives information on average number of cigarettes smoked during pregnancy by trimester.

*State Birth Certificate Data:* An alternative to using the national Vital Statistics data is to directly obtain state level birth records. Petitioning states for these data can be more time consuming and expensive but allows researchers access to more detailed information than is recorded in the federal data due to confidentiality concerns. Such additional information includes geographical identifiers as specific as the hospital, exact birthdates (as opposed to month and year of birth, as reported in the national data), and the mother's full name. Notably, Abrevaya and Dahl (2008) use birth certificate data from Washington and Arizona which contain mother identifiers, allowing them to match mothers within a state across multiple births; Yan (2013) uses restricted birth certificate information from Pennsylvania and Washington that matches multiple births to the same mother based on the mother's name and date of birth, birth parity, etc. One benefit of such matching is that it allows for the estimation of sibling fixed effects models. However, using state-specific data naturally limits the geographic scope for purposes of cross-state variation in policy.[108]

Identification strategies that exploit the more detailed state-level birth certificate data warrant more exploration. So far, no papers use matched mother identifiers to compare within-mother smoking habits in response to a tax change. Isolating the variation to within-mother changes can cleanse the estimates from changing state demographics, or selection into first time births, either of which might be correlated with cigarette taxes and infant health. A panel data approach from matching mothers across birth certificates combined with data on smoking intensity, and focusing on the outcomes of the second birth to a mother, allows for a design that tests/controls for heterogeneity in a mother's disposition towards smoking intensity. Mothers who smoke a lot during their first pregnancy are the ones who have a revealed preference and are likely to be the least receptive to a price change. Measures of the availability and price of cigarettes at the zip code level could be used for geographic variation at the sub-state level. As Pesko et al. (2016) demonstrate in the adult smoking literature, the local price environment of cigarettes has become increasingly important in explaining smoking but has not yet

[106] Not all states report smoking during all years of the data. Notably, California, Indiana, Nebraska, New York, Oklahoma, and South Dakota do not report smoking for some years. Using these states in a two-way fixed-effect model leads to an unbalanced cross section, which can bias estimation results (Kennedy, 2003). One way of addressing this concern is to run the models after balancing the states on reports of smoking to make sure the results are not sensitive to this change; another is to check whether the inclusion of smoking questions is nonrandom across state-year observations.
[107] A researcher must get approval from a review board at the National Health Association for Public Health Statistics and Information Systems (NAPHSIS). This involves completing a project review form and emailing it with related materials to the NAPHSIS contact. The process is estimated to take around 2 months. Details can be found online at: http://www.naphsis.org/programs/vital-statistics-data-research-request-process.
[108] Nonetheless, the state data can be used to implement alternative identification strategies. For example, using exact date of birth, Yan (2014) employs a regression discontinuity approach using maternal age at conception to study the effect of being able to legally purchase cigarettes (at age 21) on smoking among pregnant women and subsequent birth outcomes.

been analyzed for pregnant women (we discuss the local price environment in more detail in Section II D.ii). Yan (2014) uses a regression discontinuity approach exploiting maternal birth date available in state level birth certificate data and the legal age for purchasing cigarettes.

*Pregnancy Risk Assessment Monitoring Systems (PRAMS):* The PRAMS is a survey collected by different states and directly linked to a sample from a state's birth certificate. The survey asks mothers extended questions about their health, demographic backgrounds, behaviors, and attitudes. Questions are asked about benchmark periods in a woman's pregnancy, with the first interview 3 months before delivery. During this initial interview, questions are asked about the current state of the pregnancy, as well as retrospective questions about the period three months before the pregnancy. There is then a follow up survey given to the women 2 to 6 months after delivery. Because of the state-centered design of the PRAMS, not all states participate in all years and therefore the survey is not nationally representative and coverage is potentially non-random.[109] Moreover, even for participating states, PRAMS surveys a sample of pregnant mothers rather than the universe of pregnant mothers as in the Vital Statistics, resulting in much smaller sample sizes. The design of the PRAMS has allowed a number of recent research papers to gain an understanding of the dynamics of price elasticities of smoking before, during and after pregnancy (Adams et al., 2012; Colman, Grossman and Joyce, 2003; Markowitz et al., 2011; Markowitz et al., 2013). One unique aspect of the PRAMS is that it contains detailed information about attitudes towards the pregnancy such as whether the pregnancy was intentional, as well as health insurance status, reports of domestic abuse and information on alcohol consumption. Future research in the PRAMS could explore how policy interacts with attitudes going into the pregnancy. The information on maternal drinking has not been used either and there is room for a paper on the interactions between smoking (perhaps using taxes as an instrument) and drinking during pregnancy, along with subsequent implications for child health.

*National Health and Nutrition Examination Survey (NHANES)*: The NHANES sets the gold standard for accurately measuring tobacco use in survey data. Examination labs present at the time of interview take biological samples of blood and saliva from the participants which is then used to analyze cotinine levels (the metabolite of nicotine). The medical literature has identified cotinine levels in excess of 10 ng/ml as evidence of current smoking (Adda and Cornaglia, 2010). Likewise, lower levels (for example, in young children) can be used as a proxy for environmental tobacco smoke exposure. Cotinine levels are more likely to accurately reflect maternal and passive smoke exposure, reducing concerns related to measurement error and underreporting.

*National Survey of Family Growth* (*NSFG*): The NSFG interviews respondents on topics related to their family, fertility, contraception, and health. Survey rounds are not conducted at regular intervals, though there have been multiple waves (1973, 1976, 1982, 1988, 1995, 2002, 2006-2010, 2011-2013; forthcoming waves in 2013-2015 and 2015-2019). The NSFG asks women ages 15 – 44 detailed questions about smoking during pregnancy. Women are asked the amount smoked before pregnancy, whether they smoked at all after they learned they were pregnant and (if they did smoke) the amount smoked during pregnancy. In sum, this survey provides much more detailed information than birth certificate data, with the tradeoff of smaller sample size (though roughly 5,000 to 12,000 women are interviewed in each year, not all of whom are pregnant at the time of the survey). Given the comprehensive information the data contain on female health as well as that fact that pregnancy is a time when many women quit smoking (Tong et al., 2013), this survey could be used to look at the long term health of the mothers themselves. However, access to detailed geographic information (at the state, county or zip code level) – which is necessary to link cigarette price or tax data – requires restricted access through a census or CDC research data center.

[109] The number of sample states ranges from 3 to 31, depending on the year; this number includes Washington, D.C. and South Dakota Tribal as "states."

*National Health Interview Survey (NHIS):* The NHIS is one of the largest government surveys used to monitor health trends in the United States, interviewing approximately 35,000 – 40,000 households per year. The typical annual survey in the NHIS asks questions on adult smoking behavior but does not specifically ask questions on whether mothers smoked during pregnancy. However, there are intermittent years which contain supplemental smoking questions, including questions on smoking during pregnancy as well as smoking initiation and relapse behavior.[110] One major advantage of the NHIS is that it contains rich information on child mental and physical health. Use of these data can shed light on the child health effects of cigarette taxes. For example, Simon (2015) uses data on the state, year and month of birth to link in utero exposure to a tax with later life health in children. Future work in this data set could be used to look at the childhood health effects of exposure (in utero or otherwise) to other types of tobacco policy such as smoking bans and media campaigns. Papers on adult smoking have used the NHIS to create long term individual smoking histories (De Walque, 2010), but this has not yet been extended to the long term smoking behavior of mothers.

*National Maternal and Infant Health Survey (1988) and the National Maternal and Infant Health Survey Longitudinal Follow-up (1991)*. The 1988 survey contains 26,000 women with detailed information on the health and behavior of women who had a pregnancy-related experience (live births, late fetal deaths and infant deaths). Additionally, enumerators contacted hospitals and physicians associated with the births to verify the survey information and to gather richer details. There was a 1991 longitudinal follow up on 8,000 of these women and (when appropriate) their children. During the follow up, enumerators obtained medical records on health events the child experienced after birth. A major limitation of these data are that they only include one birth cohort, making it difficult to control for temporal trends in smoking and child health. However, because mothers are of different ages, there is variation in the timing and length of time each mother has been exposed to her state's tobacco policy which could potentially be leveraged to identify the effects of policy exposure on mother and child. In addition, a researcher could use the information on still births; no study has yet looked at the possibility of taxes preventing miscarriages. Arguably pregnancy in of itself is a powerful intervention that discourages female smoking, but for the most part the few studies that have looked at quit and relapse behavior are limited to the PRAMS (which follows up less than a year after pregnancy). These data have not yet been used to study the association between childhood health outcomes and in utero smoke exposure or between maternal relapse in smoking and tobacco policies.

*National Pregnancy and Health Survey*. This survey was produced by the United States Department of Health and Human Development in order to estimate the number of women who use legal and illegal drugs prior to giving birth. It is perhaps the only available survey with detailed questions on the use of alcohol, amphetamines, cocaine, heroin, marijuana, and other drugs during pregnancy. To help insure accuracy in reporting, the data were compared with and verified against mother and infant medical records and regular urine specimens collected by an obstetric team. Unfortunately, this dataset is limited to one wave (1992-3), making an identification strategy based on state-year variation (as is typical in this literature) impossible (though variation in exposure to tax policy by state and cohort is still possible as suggested for the National Maternal and Infant Health Survey and follow up). However, beyond the smoking literature, very little is known about the impacts of policy on substance abuse during pregnancy and birth outcomes.

A number of additional datasets exist that can be used to study the relationship between taxes, smoking and child health that do not contain details on smoking during pregnancy specifically: the American Time Use Survey (ATUS), the Current Population Survey Tobacco Use Supplement (CPS-TUS), the Behavioral Risk Factor Surveillance System (BRFSS), the National Survey of Household Drug Abuse (NHSDA), the National Survey on Drug Use and Health (NSDUH), the Early Child Longitudinal Study (ECLS), and the National Survey of Children's Health (NSCH).

[110] Specifically, these questions are asked in the NHIS in the following years: 1985, 1990, 1998, 2005 and 2010.

## C. Appendix C: Commonly used datasets: taxes and other smoking policies

The major elements required to link smoking related policies to maternal and child outcome data are details concerning the timing, location, and strength of implementation and well as the venue targeted by the policy. Restrictions on smoking in the U.S. date back to the Prohibition era, when some states banned cigarette sales; they surfaced again in the 1970s and 1980s in the form of mandated non-smoking sections in restaurants and other venues. By the 1990s they took the form of municipality-level (and later, state-level) bans on smoking in public areas, workplaces, and other venues. This has led to significant variation in the exposure pregnant women and young children to smoking-related policies.

The main source for tax and cigarette price data is the Tax Burden on Tobacco, which is used in virtually all studies that include price and/or tax data. There are two additional sources of data for sub-state prices: the Tobacco Use Supplement of the CPS (CPS-TUS) and Nielsen Retail data. There are several widely used sources of data that track the enactment of clean air policies as well as other related policies. These are each described in turn in Appendix Table 1B and below.

*State Tobacco Activities Tracking and Evaluation (STATE) System.* These data are published by the Center for Disease Control (CDC) and contain state-level information on a variety of smoking-related legislation for the period 1995-2005, including the following: taxation, indoor air laws, retail licensing, youth access, smoke-free school campuses, and preemptive laws (state-level laws that preemptively limit the ability of counties and municipalities to implement more restrictive smoking-related ordinances). The datasets contain basic information about the policies, including enactment and effective dates, venues to which they apply (25 unique categories), what specifically they mandate, etc. The current interface also includes links to other state-level datasets on topics such as smoking prevalence, smoking-related mortality and morbidity, and utilization of quit line services.

*American Non-smokers' Rights Foundation.* The American Non-smokers' Rights Foundation collects and publishes data on a variety of smoking-related policies. The most often used is the database on the adoption of 100% smoke-free laws applying to non-hospitality workplaces, restaurants, and freestanding bars, which covers municipality, county, and state-level ordinances beginning in 1990 and current through October 2015. The database also includes an estimate of the (2007) population covered by each law.

*ImpacTeen State Level Tobacco Control Policy and Prevalence Database.* These data provide information on both smoking-related policies and tobacco use prevalence at the state level (including Washington, DC) for the period 1991-2008. The policies covered in the database include the following: price and taxes (and associated revenue), tobacco control program funding, youth access laws, clean indoor air laws, and preemptive laws. A useful feature of these data is that they also contain a measure of the intensity of clean indoor air laws as well as very specific classifications of venues to which they apply (12 unique categories).

## D. Appendix D -- Descriptive studies linking smoking restrictions to maternal smoking and child health

The descriptive approaches in this area of the literature vary from study to study but generally take the following form[111]:

$$A1)\ Y_{i,t} = f\left(\beta_1 P_t + \beta_2 P_t * t + \beta_X X_{i,t} + g(t;\theta) + \varepsilon_{i,t}\right).$$

[111] Several studies (including Charrier et al. (2010), Franchini et al. (2008) and Jarvis et al. (2011)) are purely descriptive in nature and do not employ regression-based techniques.

where $Y_{i,t}$ is an outcome of interest for mother or child $i$ at time $t$, such as maternal smoking behavior or a birth outcome; $P_t$ captures the policy under study, usually a dummy variable that takes the value of 0 before the policy is enacted and 1 in all subsequent periods; $X_{i,t}$ are covariates at the child, mother, and/or household level[112]; and $g(t;\theta)$ is a function of the time variable, intended to represent underlying trends in the outcome that are unrelated to the policy (most commonly linear). $\beta_1$ and $\beta_2$ are the parameters of interest and reflect the change in outcomes associated with the implementation of the policy, $P_t$, both in terms of a level shift ($\beta_1$) and a break in trend ($\beta_2$).

*Maternal smoking behavior.* As indicated in equation A1, much of the literature outside of economics is based on a comparison of outcomes before versus after a particular policy change[113]; for example, many the studies focus on national bans on smoking in public spaces and workplaces enacted in the United Kingdom between 2006 and 2007. While the majority of these studies find a reduction in self- and child-reported maternal (and sometimes paternal) smoking (Franchini et al., 2008; Holliday, Moore and Moore, 2009; Kabir et al., 2009; Mackay et al., 2012; Moore, Holliday and Moore, 2011; Page et al., 2009)[114], magnitudes and statistical significance of impacts vary greatly across studies. Two studies find no change in parental smoking after ban implementation (Charrier et al., 2010; Ho et al., 2010).[115] Still, the weight of the descriptive evidence supports the notion that smoking restrictions reduce measures of parental smoking.[116] Note that the results discussed here concern parental smoking rates; we discuss measures of second-hand exposure of children (related to cotinine levels and also child reports of the intensity and location of parental smoking) in the following section.

*Displacement effects, second hand smoke exposure and other channels of influence.* A number of studies across the epidemiological literature estimate the effect of bans on the postnatal second hand smoke exposure of children in a variety of different contexts and most find overall reductions in cotinine levels and self-reported exposure to smoke following the implementation of smoke-free legislation (Ahktar et al., 2007; Ahktar et al., 2009; Ahktar et al., 2010; Holliday, Moore and Moore, 2009; Jarvis et al., 2011; Moore, Holliday and Moore, 2011).[117] Since all of these studies focus on bans on smoking in public places and workplaces but measure either overall exposure (cotinine levels) or self-reported exposure at home, they serve as descriptive evidence that any displacement effects of bans are outweighed by the reductions in exposure to smoking adults in public and commercial spaces and at home.[118] The one exception is Ho et al. (2010), who find that the proportion of primary school students reporting exposure to second hand smoke rises after a ban in Hong Kong, both in and outside the home. Two

[112] A number of studies that examine the responses of child outcomes to CIALs include measures of parental smoking in $X_{i,t}$ (Geidenberger, 2011; Hade, 2011; Ho et al., 2010; Kabir et al., 2009; Kabir et al., 2013; Page et al., 2012). This may be problematic for two main reasons. First, it is unclear whether or not parental smoking behavior is exogenous with respect to pregnancy, birth, and early childhood outcomes under consideration. Second, in some cases, parental smoking is one of the potential channels through which we expect a smoking related policy to influence outcomes. Studies that include parental smoking in $X_{i,t}$ do not allow parental smoking behavior to respond to policies.

[113] Page et al. (2012) is the only study to attempt a difference-in-difference design but the authors use only a single treatment and control area; such pairwise comparisons can often lead to misleading results that overstate general effects (Shetty et al., 2010). This leads us to regard the results presented in Page et al. as largely descriptive.

[114] Magnitudes range from about 1 percentage point to 15 percentage point reductions in parental smoking, but results are statistically significant at the 5% level in only 2 studies (Kabir et al., 2009; Moore, Holliday and Moore, 2011). Holliday, Moore and Moore (2009) find that smoking inside the home by parents (but not overall parental smoking status) fell significantly after a ban in Scotland.

[115] Charrier et al. (2010) does not explicitly test whether maternal smoking during pregnancy fell after a ban in Italy but comparisons of means pre- and post-ban do not indicate that there was a drop in smoking prevalence or intensity during pregnancy. While Ahktar et al. (2009) find an increase in child-reported voluntary restrictions on smoking within the home after a Scottish ban on smoking in public spaces, a similar study of the same legislation and population finds no reduction in parental smoking rates (Ahktar et al., 2007).

[116] To our knowledge, there is one descriptive study that examines the impact of smoke-free policies on maternal smoking in developing countries. Harris, Balsa and Triunfo (2015) find that national bans on smoking in public places, workplaces, and public transport were associated with increased probability of third trimester quits in Uruguay.

[117] Some studies note that the magnitude of the fall in second hand smoke exposure is heterogeneous and depends on factors such as the number smoking parents, pre-ban exposure and socioeconomic status (Ahktar et al., 2007; Ahktar et al., 2009; Ahktar et al., 2010; Holliday, Moore and Moore, 2009; Jarvis et al., 2011; Moore, Holliday and Moore, 2011).

[118] Additionally, Ahktar et al. (2007) and Moore, Holliday and Moore (2011) find no statistically significant evidence of displacement to smoking in cars.

studies explore changes in pregnancy outcomes among pregnant women before and after smoking bans and find some evidence that outcomes improve even for women that never smoke, suggesting that bans may also lower second hand smoke exposure for pregnant women (Kabir et al., 2009; Mackay et al., 2012).[119]

*Pregnancy, birth-related, and early childhood outcomes.* There are a number of descriptive studies examining the association between smoke-free legislation and child health outcomes. Note that here we only discuss birth and early childhood health outcomes, as we have already discussed the literature concerning direct measures of childhood exposure to second hand smoke. The majority of descriptive studies find significant declines in the risk of preterm birth (defined as delivery before 37 weeks of gestation) associated with the implementation of smoke-free laws, though magnitudes vary greatly across studies (Cox et al., 2013; Kabir et al., 2009; Mackay et al., 2012; Page et al., 2012).[120] However, others find no effect of bans on preterm births (Hade, 2011; Geigenberger, 2011). The evidence on low birth weight status and small for gestational age status (SGA, defined as less than the 5$^{th}$ or 10$^{th}$ percentile of birth weight for a given gestational age and sex) is similarly mixed.[121] While some find a reduction in LBW and SGA status (Kabir et al., 2013; Mackay et al., 2012), others find no statistically significant impact of bans (Hade, 2011; Geigenberger, 2011; Page et al., 2012) and one study even finds a rise in LBW rates following the implementation of a ban in Ireland (Kabir et al., 2009).

[119] There is some evidence from the medical literature that clinical interventions can lessen second hand exposure of pregnant women and subsequently improve birth outcomes (El-Mohandes et al., 2010). Charrier et al. (2010) find that pregnant women's exposure to smoke in the workplace falls after Italy's national ban but that women are still prenatally exposed to smoke in their homes; the authors do not comment on whether overall second hand exposure rises or falls in response to the ban.

[120] The results in Kabir et al. (2009) are very sensitive to the inclusion of birth weight as a covariate. Mackay et al. (2012) find a significant decline in pre-term birth rates three months *prior* to the implementation of the legislation.

[121] Definitions of SGA vary across studies; some consider SGA status for births under the 5$^{th}$ percentile, while others consider SGA status for births under the 10$^{th}$ percentile.