Does Early Life Exposure to Cigarette Smoke Permanently Harm Childhood Health? Evidence from Cigarette Tax Hikes

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ABSTRACT:

Research has shown that smoking during pregnancy decreases birth weight, but is the harm to childhood health permanent? I examine how in-utero and early life exposure to smoking affects a range of child health outcomes. Identification comes from cigarette tax hikes, a plausibly exogenous source of variation. By leveraging cigarette tax hikes, I shed light on the ability of these taxes to change health behavior in a way that improves long-term child outcomes. In-utero exposure to a tax hike leads to large and significant improvements to a child's wellbeing. I find that a dollar tax hike (in 2009 dollars) causes roughly a 9% decrease in sick days from school and a 4.6% decrease in the likelihood of having two or more doctor visits in the past 12 months. I also find suggestive evidence that early life exposure to a cigarette tax hike decreases hospitalizations and asthma attacks. The validity of my research design is confirmed through a number of falsification tests and event studies. Additionally, I estimate the long-term effects of cigarette tax hikes on a wide range of different demographic subgroups. Comparing these to the corresponding reductions in birth weight and maternal smoking shows that those who experienced the largest later life health gains are the ones who saw reductions in maternal smoking and improvements in birth weight.

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1. Introduction

Does in-utero exposure to cigarette smoke permanently harm childhood health? In this study, I examine the long-term implications of in-utero smoke exposure for child health and health care utilization. To this end, I leverage cigarette tax hikes to circumvent the endogeneity of maternal smoking and second-hand exposure. Use of cigarette taxes, in turn, sheds light on the viability of tobacco policy for improving health, decreasing health care costs, and stemming the intergenerational transmission of low socioeconomic status. To answer this question, I make use of the restricted-use geocoded National Health Interview Survey (NHIS). Access to these data allows me to examine medium-term childhood health outcomes not commonly used in the economics literature.

A study of the childhood health effects of in-utero exposure to cigarette taxes is particularly timely given the large number of state excise tax hikes in recent years.[1] Between 1980 and 2009, state taxes on cigarettes have increased by approximately $0.80 on average. Focusing on the past 15 years, there were over 80 tax hikes of $0.25 or more with roughly 2.5 tax hikes per state (Orzechowski and Walker, 2011). This increase was driven by a series of large cigarette tax hikes starting in the mid-1990s. State excise taxes continue to increase, making them a relevant policy to evaluate. At the same time, the variation from tax hikes is old and large enough that it is feasible to study medium term childhood outcomes.

Past work on cigarette taxes has shown negative price elasticities of smoking for adults, teenagers, and—particularly relevant for my work—pregnant mothers.[2] Larger elasticities have been uncovered for some subgroups, notably teenagers and high school dropouts (Gruber and

[1] Cigarette taxes are not frequent enough to truly separate effects due to early life exposure from in-utero exposure. For brevity, I use the term in-utero to refer either to in-utero or in early life (up to roughly six months of age).

[2] In general, past research has found effects of tax hikes on smoking for pregnant mothers (Ringel and Evans, 2001) and teenagers (Gruber and Zinman, 2000). More recent work by Markowitz et al. (2011) shows that for tax hikes implemented from 2000 to 2005, the effects for mothers are focused on teenage mothers and mothers between the ages of 25 and 34. Callison and Kaestner (2012) argue that the most recent tax hikes on the adult population as a whole are negative but not statistically significant in some specifications. However, Decicca and McLeod (2008) find large and statistically significant effects on adults between the ages of 45 and 59.

Zinman, 2000; Decicca and Mcleod, 2008). Recent studies also confirm that the relationship holds with tax hikes in the 2000s (Carpenter and Cook, 2008; Markowitz et al., 2011).[3]

With this in mind, excise taxes are a viable exogenous shifter for early life smoke exposure. Further, the largest effects should be focused on the children of mothers who have the highest tax elasticities: namely, the children of teenage mothers and mothers who are high school dropouts. While I draw upon the previous literature for the "first stage" of my study, I also confirm its findings by using vital statistics to directly estimate the impact of taxes on maternal smoking and infant health.

My primary dataset is repeated cross sections from the restricted-use geocoded NHIS from 1997 to 2010. The NHIS contains childhood health outcomes, including sick days from school in the last 12 months and having had an asthma attack in the past 12 months. To investigate changes in health care utilization, I look at an indicator for having two or more doctor visits in the past 12 months. I also examine emergency room visits and hospitalizations.

My empirical strategy involves regressing various child wellbeing outcomes on the state excise tax faced by a child in-utero while including state and year-month fixed effects in the model. Such a model generalizes the standard difference-in-differences model to account for tax hikes having varying magnitudes and occurring multiple times within most states. The coefficient of interest is identified by the changes in state excise taxes over time, comparing child outcomes across states and birth cohorts. I test the robustness of my results by controlling for a range of state policy variables, implementing placebo tests, and saturating my model with leads of the tax variable.[4] In addition, I evaluate the identification assumption in my model by constructing an event study. Event studies test the assumption of difference-in-difference models that there are no differential trends between treatment and control states. This is the first

[3] The literature on smoking during pregnancy has focused on smoking participation rather than the number of cigarettes smoked because it is harder to measure the intensive margin as mothers may not remember the exact number of cigarettes smoked. Additional evidence suggests that mothers who do not quit as a result of a tax increase smoke more intensely (Adda and Cornaglia, 2006), which could also confound the health effects of the intensive margin.

[4] As discussed in the methodology section, leads of the tax variable also capture health outcomes for cohorts who have not yet been exposed to a hike and therefore represent pre-trends in cohort exposure to the tax.

time of which I know that the event study empirical model has been used in the cigarette excise tax literature.

This study is among the first to look at the impact of a policy intervention that improves in-utero environment on medium-term childhood outcomes.[5] It also represents one of the only extensions of the literature on cigarette taxes and infant health to childhood health outcomes. My findings suggest that sheltering children from in-utero smoke exposure can have large and lasting effects on health. This supports evidence from earlier studies on health economics suggesting that policy interventions early in a child's life can result in disproportionately large returns (Almond and Currie, 2011).

2. Expected Effects

What conditions are associated with in-utero exposure to cigarette smoke? The most robust result in the medical literature is that smoking during pregnancy decreases birth weight (USDHHS, 2001).[6] This is due to carbon monoxide in cigarettes restricting the flow of blood vessels through the mother's body. Restriction of blood vessels reduces the amount of oxygen and nutrition that reaches the fetus, resulting in decreased birth weight with effects strongest in the third trimester (USDHHS, 2001).[7]

Beyond birth outcomes, the medical literature provides evidence that harm from smoke is widespread and lasting. Nicotine binds to neural receptors in the developing fetus, potentially leading to brain damage (Shea and Steiner, 2007). Nicotine also hinders the movement of the embryo, which could retard the development of the child's nervous system (USDHHS, 2010). Finally, along with nicotine, there are more than 100 other harmful chemicals in cigarettes, which are believed to cause cellular damage through changes in cell structure and hormone levels (Dempsey and Benowitz, 2001). This could result in birth defects as well as additional health complications that are not fully understood.

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[5] Almond et al. (2011) and Nilsson (2008) are other important examples. .

[6] Medical studies have also consistently found a strong correlation between smoking and pre-term births, although the biological mechanisms are not as well understood (USDHHS, 2010).

[7] This is the effect traditionally measured by papers on smoking and infant health. Because of this, I use the third trimester as the baseline for my analysis.

Studies in the economics literature have offered causal evidence for cigarette smoke harming a child's health at birth. Evans and Ringel (1999) first used across state variation in cigarette taxes as an instrument to obtain two-stage least squares (2SLS) estimates of smoking on birth weight. A one-dollar tax increase resulted in a 32% reduction in smoking during pregnancy and a 5% reduction in low birth weight births. A number of other studies support Evans and Ringel's initial finding. Table 1 gives details on some of the major studies. Notably, Table 1 shows that there are larger price elasticities of smoking for some demographic groups. I leverage this finding by stratifying my estimates by these subgroups. Table 1 also shows that the impact of taxes on smoking has persisted over time even though the elasticity has fallen in more recent years.[8] Other studies show that a tax hike causes a "sharp" decrease in smoking (Lien and Evans, 2005).[9] Further, while pregnant mothers who quit are likely to relapse after the pregnancy is over, those who quit due to a tax increase are less likely to relapse (Colman et al., 2003). I extend this literature by testing the hypothesis—for the first time of which I know—that exposure to a cigarette tax hike while in-utero results in lasting improvements to childhood health.

Should I expect the biological impacts discussed above to surface in childhood outcomes available in survey data? Previously, economists have had success in measuring the long-term effects of early life environment through tests of the fetal origins hypothesis (FOH). Originally ascribed to David J. Barker, the FOH states that negative shocks faced by a fetus can alter the developmental course of an infant's body, resulting in chronic conditions in adulthood. Almond and Currie (2011) provide a review of how the FOH has been applied by economists to look at economic outcomes such as wages, employment, and mortality. Natural experiments used in this literature include the effects of the 1918 influenza pandemic (Almond, 2006), blights to French vineyards that shifted in-utero nutrition and family income (Banerjee et al., 2010), malaria

[8] This decline is most likely due to smoking during pregnancy becoming less prevalent.
[9] Lien and Evans (2005) looked specifically at four individual states, each of which implemented a tax hike in the mid to late 1990s. They used propensity score matching to match tax hike states with states that did not implement a tax hike but had similar trends in smoking.

exposure (Barreca, 2010), food stamp introduction (Hoynes et al., 2012), as well as many others. In sum, the literature has extensively documented how health shocks in early life lead to measurable changes in later life outcomes.[10]

Epidemiology offers some insight into the FOH as it applies to smoking during pregnancy. Studies have found correlations with early life cigarette smoke exposure and test scores, asthma, stunting, childhood obesity, and overall child health (Stick, 1996; Lessen, 1998). One problem with these studies is that they do not fully account for omitted variable bias. Low socioeconomic status (SES) mothers are on average less healthy and may be more likely to have unhealthy children. Since low SES mothers are also on average more likely to smoke during pregnancy, this could result in a spurious relationship between smoking and childhood health outcomes. In turn, omitted variables correlated with low SES status could result in an upwards bias to the estimates of the cited studies. This study complements the epidemiology literature by offering a causal test of the lasting childhood health effects of smoke exposure.

This study is also among the first to examine the FOH in the context of a positive shock caused by a policy intervention on intermediate-term childhood outcomes. Most economic papers testing the FOH focus on negative health shocks identified through natural disasters (Hoynes et al., 2012; and Nilsson, 2008; are two of the exceptions). Studying the FOH in the context of tax hikes is arguably more relevant because the estimated treatment effect is from a policy that could be implemented again in the future. Many FOH studies also only look at adult health (particularly adult health post–reproductive age). However, a shock that causes an individual to suffer as an adult has smaller welfare implications than if that individual was

[10] A debate related to the FOH has developed among labor economists on whether or not birth weight matters. Black et al. (2007) estimated twin fixed effects for the impact of birth weight on educational attainment and earnings. They found large effects for the 1977–1986 Norwegian cohorts. For these cohorts, low birth weight infants are 5% more likely to drop out of high school and have a 15% reduction in labor market experience. On the other hand, Royer (2009) finds a statistically significant but much smaller effect of birth weight on education and the birth weight of one's children for American cohorts using twin fixed effects. Since smoking during pregnancy is the most preventable cause of low birth weight, this paper could indirectly offer evidence on the long-term effects of having low birth weight. However, since smoke exposure could affect health through channels other than birth weight, I cannot make any strong conclusions.

affected throughout his/her childhood as well. For cigarette tax hikes, I document that health benefits throughout childhood make up a substantial portion of policies' welfare value.

3. Policy Background

Taxes on cigarettes are levied at the federal, state, and municipal levels. Following the majority of the literature, I focus on state excise taxes.[11] State cigarette taxes have experienced massive increases over time. In the 2011 fiscal year, state taxes generated more than $17 billion, representing a rise from $4 billion in 1980 and a growth of roughly 333% (Orzechowski and Walker, 2011). Figure 1 shows the variation in the number of states that enacted a tax hike of $0.25 or more (in 2009 dollars) by region over the cohorts in my sample. The northeast and western regions of the United States enacted a large number of the hikes, but they are not alone; the mid-western and southern regions have also regularly implemented increases at times. Figure 1 also shows that the period from 2001 onward represents the greatest activity and geographic diversity of tax increases.

Tax hikes in the early to mid-1990s were largely done to reduce tobacco use (DeCicca et al., 2008). In the later part of the 1990s tax hikes were partially inspired by the Master Settlement Agreement (MSA). The MSA was a lawsuit by states against tobacco companies to recoup insurance claims from illnesses caused by tobacco use (Sloan and Trogdan, 2004). On the other hand, during the recession of the early 2000s, tax hikes were implemented to increase general state revenues. This resulted in a greater number of tax hikes, larger-magnitude hikes, and more widely spread geographic variation in hikes.

I analyze tax hikes for cohorts born from 1989 to 2007.[12] The top panel of Figure 2 takes these tax hikes and splits them into quartiles based on real values over the full period. I then plot the number of tax hikes within each quartile over time with the largest and most regular changes occurring after 2001. The bottom panel of Figure 2 shows the number of tax hikes per year. This

[11] It is difficult to separately identify federal tax changes from national trends in smoking and child health. Municipal taxes are uncommon, and there is no comprehensive dataset documenting them.
[12] Here and throughout the paper, I define a tax hike as any increase in taxes of $0.10 or more (in 2009 dollars). Virtually every legislated tax change was at least $0.10. Further, defining a tax hike as being at least $0.10 helps separate a policy increase from any small annual changes in the real tax due to inflation.

makes explicit the variation over time and the explosive increase in tax hikes in 2001. These trends frame why it is important to control for unrestricted time in month fixed effects, something I do in all of my models.[13] Because states might be implementing taxes at different times and for different reasons, it is also important to control for state fixed effects and to check the robustness of my results to including state linear time trends.

4. Data

The primary datasets I use are repeated cross sections from the 1997–2010 NHIS. The public-use NHIS does not provide state geographic identifiers, making it necessary for me to access the restricted-use version of the data.[14] The restricted geocoded NHIS interviews include a cross section of households each year, gathering demographic and health data on each household member into the person-core questionnaire. One adult and one child are also randomly sampled from each household and asked more detailed questions. I look at cohorts of children 3–17 years old born between 1989 and 2007. I limit my sample to children who are 24 months or older in part to focus on long-term effects and in part to avoid capturing noise from very young children going to the doctor often for well-baby visits.[15] I also drop all observations that are older than 17 to avoid introducing a bias due to selecting young adults who have not yet left their parents' household. Since I assign treatment in the third trimester and 2010 is the latest survey year available, 2007 is the last complete birth cohort year.

Date of birth and geography variables in the restricted-use geocoded NHIS jointly allow me to assign to each child a cigarette excise tax level roughly corresponding to the state, month, and year the child was in-utero. The timing of trimester is not precise since I do not have information on gestational age. Ideally, I would have the state of birth for each child, but this is not available, so I assume that state of interview is the same as state of birth. Making assumptions about gestational age and state of birth add a small amount of noise to my right-

[13] Throughout this paper I consider "time" to be the month and year the child begins the third trimester. This is calculated by counting three months backwards from the month and year of birth.
[14] I accessed the restricted-use geocoded data through the Center for Disease Control's research data center in Maryland.
[15] For completeness, I have also run my baseline models including children 0–24 months old. Adding these observations does not significantly change my results.

hand-side variable. Measurement error in a right-hand-side variable attenuates its associated coefficient implying that the true effects that are somewhat larger than what I estimate. The medical literature (as discussed above) suggests that the effect of maternal smoking on birth outcomes is strongest in the third trimester, while the effects of maternal smoking on other areas of child development can accumulate throughout the entire pregnancy. For my baseline model, I attempt to capture the largest health effects suggested by the medical literature by assuming nine months gestation and treatment in the third trimester. I merge onto the NHIS the monthly state cigarette excise taxes from Orzechowski and Walker (2010).

I use outcomes from both the person core and child sample questionnaires.[16] Sample sizes are listed for each outcome in Table 2. I look at outcomes related to child health, sick days from school in the past 12 months, and having had an asthma attack in the past 12 months.[17] I also look at variables capturing medical care utilization: doctor visits[18], hospitalizations, and emergency room visits. The prevalence of missing values varies somewhat across outcomes.

A problem that arises with constructing a health-utilization outcome is that increased utilization could be caused by either improved access to care or decreased health.[19] In their paper on the effect of Medicaid on the utilization of health care, Currie and Gruber (1996) dealt with this issue by constructing a dichotomous variable equal to 1 if the child had one or more doctor visits in the past 12 months. I construct an outcome variable similar to the one used by Currie and Gruber, but I primarily want to study how smoke exposure changes doctor visits by

[16] I currently do not use the sample adult questionnaire. The adult detail file does not have retrospective questions on smoking during pregnancy. While the adult detail file does collect information on current adult smoking, the research data center charges additional fees to merge in additional datasets. Further, since only one adult per household is interviewed, it would be a noisy proxy for a child's exposure to cigarette smoke. Since I have limited grant money, I have not pursued this and have instead estimated maternal smoking in the vital statistics for similar cohorts of children that are in the restricted-use geocoded NHIS.

[17] The NHIS child sample file also has information on whether a child was ever diagnosed with asthma. Due to the large number of outcome variables, I do not show results on asthma diagnosis, but it produced qualitatively similar results as having an asthma attack in the past 12 months.

[18] The exact wording of the question is "During the past 12 months, how many times has {child's name} seen a doctor or other health care professional about {his/her} health at a doctor's office, a clinic, or some other place?" The question explicitly excludes overnight hospitalizations, emergency room visits, home visits, and telephone conversations. The survey also directs the interviewers not to count dental visits.

[19] A further complication with the NHIS doctor visits variable is that the number of doctor visits in the past 12 months is grouped into bins. For most survey years the questionnaire used the following categories: none, 1, 2–3, 4–9, 10–12, 13 or more.

improving health. Therefore, I construct an indicator variable equal to 1 if the child had two or more doctor visits in the last 12 months. The move from fewer than two visits to two or more visits is more likely to capture children who have improved health rather than decreased access to care.[20]

I merge mother and family demographic information into each observation in order to control for important covariates, such as mother's education, marital status, and age. It also allows me to stratify on these variables and mother's age at time of birth. I calculated mother's age at time of birth from information on mother's age and child's age in the NHIS. Unfortunately, this identifier is missing for a number of observations and for all of survey year 1997. Of the 143,141 observations in my base sample, I am able to match 118,271 observations to their mothers. I include the unmatched observations in my regressions by controlling for a missing mother indicator variable that is equal to 1 for the unmatched observations.

For falsification tests, I use reports on the following conditions: chicken pox, chronic headaches,[21] anemia, and food allergies. Since headaches, anemia, and food allergies are low incidence, I attempt to boost the power of my placebo tests by pooling these together into a single index. I follow Kling et al. (2007), and normalize each outcome variable to have a mean of zero and a standard deviation of one and to be signed such that a decrease in the index represents increased health. The index is the average of the three, again normalized to have a standard deviation of one.[22]

I also use data from the vital statistics for years 1989–2004. The vital statistics is a census of births in the United States, which collects data on birth weight; mother demographic

[20] Another reason to investigate doctor visits is that increased health utilization is also of direct interest because it unambiguously increases spending on health care. Families care about the costs of doctor visits, emergency room visits, and hospitalization. Further, expenditures in these areas might be substitutes for other child spending. Economists are also concerned about documenting health utilization because due to insurance markets and public health insurance, these costs could be born outside the household.

[21] I believe this is a valid placebo variable since the vast majority of chronic headaches in children are migraines (Abu-Arefeh and Russell, 2003), and genetic factors play a leading role in determining the incidence of migraines (Russell et al., 1996).

[22] No single variable is weighted more heavily than another when constructing the index. This assumes that no placebo provides "more information" than another placebo. I use the child detail file weights when calculating the variable means and performing the placebo regressions.

information; and maternal health behaviors, including smoking during pregnancy. The vital statistics allow me to interpret my findings in the NHIS by adding "first-stage" estimates of taxes on maternal smoking and infant health to my analysis. I update the previous studies that have looked at the effect of taxes on maternal smoking by looking at birth certificate data on cohorts of children from 1989 to 2004.[23] I then estimate the same equations that I use in the later life analysis but with maternal smoking and low birth weight status as the outcome variables. This allows me to compare the magnitudes of the impact of taxes on early life and later life outcomes for different subgroups. The details of constructing the vital statistics outcome variables are also discussed in Appendix B.

5. Empirical Methodology

5.1 Difference-in-Differences

My principal empirical strategy uses linear regression models with state and time fixed effects. When discussing my empirical strategy, I always consider "time" to be the month and year the child begins the third trimester. Thus, these fixed effects hold constant fixed differences across states and over birth cohorts. Specifically, I estimate the following regression equation:

$$Y_{isc} = \beta_1 T_{sc} + \beta_2 X_{isc} + \gamma_s + \eta_t + \varepsilon_{isc}$$

Y_{isc} indicates a health or educational outcome for child i born in state s whose cohort was in-utero at time c. This is calculated by counting three months backward from the month and year of birth. The cigarette excise tax to which a child is exposed is T_{sc} (measured in 2009 dollars), and β_1 is the coefficient of interest. I also control for state fixed effects γ_s as well as time fixed effects η_t. X_{isc} is a vector of additional demographic and state policy controls.[24] In my initial specification, I include in X_{ist} dummies for mother's age at the time of interview (11–17, 18–25, 26–35, 46 and older), mother's education at the time of interview (dropout, high school, some college, college and beyond), child's race (White, Black, Hispanic, Other), child's gender, and a

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[23] I currently cannot look at cohorts past 2004 in the vital statistics because the public-use vital statistics do not include state identifiers. I have applied for access to the geocoded vital statistics data.

[24] Some of my outcomes are dichotomous. To make sure my results are not sensitive to functional form, I also run a logit model when the outcome variable is two or more doctor visits, asthma attacks, or hospitalizations. My results do not change.

full set of fixed effects for a child's age in months. Jointly, the child's age in months fixed effects and the month/year of treatment fixed effects subsume the month/year of interview fixed effects. For all models, I cluster the standard errors on the state of interview.

I test the sensitivity of this model to additional state-level characteristic and policy controls. This is important because my model is similar to standard difference-in-difference models. Difference-in-differences is identified off of the variation in the timing and size of changes in taxes across states and over cohorts. If the states that increase their taxes also implement other policies that improve child health, my coefficients could be biased. If this is the case, I should see my coefficients change as I add policy-related state-level controls.

I also look at differences in child health outcomes across subgroups. When analyzing differences by subgroups, I estimate birth related outcomes in the vital statistics and childhood health outcomes in the NHIS. For each group, I graph the impact of taxes on birth weight or maternal smoking on the x-axis and childhood outcomes on the y-axis. This tests whether the groups experiencing the early life effect of a tax also experience the childhood effects. In doing this, I follow a method similar to Hoynes et al. (2012).[25]

5.2 Event Study Methodology

Any systematic pre-trends in child outcomes across tax-hike states will be revealed by an event study. Therefore, event studies are good for explicitly testing the assumption that there are no differential trends between treatment and control states. A typical event study is modeled by constructing a vector $\sum_{j=-J}^{J} e_{sj}$ of dichotomous indicators, each of which is equal to one when an observation is j periods before or after some discrete policy event. These event time dummies replace the treatment variable in the regression model. I include one event dummy for each quarter extending up to two years before and after the event. The event dummy directly before the policy takes place (j = -1) is the excluded indicator variable. Unlike with standard difference-in-difference models, only observations that experience a policy intervention are

[25] They similarly show that the same demographic groups whose income increased due to the Earned Income Tax Credit (EITC) were those whose infants had higher birth weight increases with EITC expansions.

included. Plotting the coefficients on the event dummies makes explicit how the difference between the treatment and control groups evolves over time relative to the policy. This helps ensure the validity of the research design.

Unfortunately, my excise tax variation does not fit neatly into the standard event study approach. Differing magnitudes of excise taxes means that the policy cannot be simply characterized as a dichotomous treatment. Further, the majority of the states in my sample had two to three tax hikes, and some of these hikes occurred in adjacent years. This makes it difficult to separately analyze a single hike within a given state. To address the variation in magnitudes across tax hikes, I take all (inflation-adjusted) tax hikes and assign them percentiles (unweighted). I then define my discrete tax hike event as any tax hike greater than or equal to the 85th percentile ($0.72 in 2009 dollars). I discuss this, and other decisions related to the event studies, in more details in data appendix B.

6. Results

I first look at the two highest-prevalence outcomes in the NHIS: sick days from school and two or more doctor visits. The majority of children (71%) had at least one sick day in the past 12 months and, on average (61%), had more than one doctor visit. Because these outcomes are "high incidence," they are more likely to have the statistical power needed to test my hypothesis. Table 3 shows results for sick days from school in the past 12 months as the dependent variable. My initial specification includes only demographic controls and fixed effects. A one-dollar tax increase causes a decrease of 0.31 in sick days from school in the past 12 months. As discussed in the empirical methodology section, my first check of these results' validity is to test the sensitivity of my estimates to a wide range of state characteristic and policy controls. This is important because the coefficient on cigarette taxes is unbiased if there are no state-level changes in unobserved determinants of child health at the same time a tax hike is implemented

I begin by adding a core set of state policy controls: the state's income threshold for pregnant women to qualify for Medicaid, an indicator for the state having implemented welfare reform, and the State Children Health Insurance Program (SCHIP) income eligibility threshold based on the child's age and state at the time of interview.[26] As shown in Column 2 of Table 3, adding these controls has little effect on my results. I next add the state unemployment rate at birth, the motivation for which is the literature documenting changes in health behavior and mortality based on the unemployment rate (Ruhm, 2000; Stevens et al., 2011; Dehejia and Lleras-Muney, 2004). The unemployment rate should also help account for relative increases in state spending due to tax hikes in response to the 2001 recession. There is virtually no change to the coefficient in Column 3 of Table 3, which suggest that changes due to state economic conditions do not influence my results.

In Column 4 of Table 3, I next control for the state's "ImpacTeen" rating for smoke-free indoor air laws in bars and private work places.[27] Controlling for these laws is important because they could affect child health by reducing a mother's second-hand exposure to smoke. I include the cigarette tax at the time of the child's interview to capture the impact of a current tax change on child health independent of the in-utero effect. Neither the "ImpacTeen" controls nor the current cigarette tax significantly change my estimates. My preferred specification is Column 5, which includes all of the previous state-level controls, the unemployment rate, indoor air bans, and the current cigarette tax. The robustness of my results to these controls suggests that the coefficients are not driven by unobserved state-level changes correlated with the tax hikes.

How should the magnitude of a 0.31 decrease in sick days be interpreted? This is the intent to treat (ITT) impact of a tax hike and represents the effect distributed across the entire population. The ITT does not take into account that only a subset of the population reacts to the cigarette tax change. Dividing by the mean signifies an approximately 9% decrease in sick days for a dollar tax hike. Given that between 1980 and 2009 state cigarette taxes increased by $0.80 on average, this suggests a substantial—though not overly large—impact.

[26] For Medicaid and SCHIP income eligibility thresholds, I use the same data as Hoynes and Luttmer (2011) who in turn compiled it from Gruber (2000), the National Governor's Association, Kaiser Family Foundation, and the Center of Budget and Policy Priorities.

[27] ImpacTeen is an organization that rates each state-year from 1–5 based on the strictness of various smoke-free indoor air laws. This is one of the more common controls for indoor smoking bans used in the smoking literature (see Carpenter and Cook, 2008; Bitler et al., 2009).

One way to get an estimate for the effect of smoking during pregnancy is to divide the change in sick days by the percentage point decrease in maternal smoking. This gives the treatment on the treated (TOT), which measures the effect of a cigarette tax hike on the children of those mothers who are swayed to quit smoking due to the policy. If we assume that mothers accurately report smoking during pregnancy in the vital statistics, and that there is no effect from second-hand exposure, then this represents the true TOT. However, if mothers lie or misreport smoking during pregnancy, then the estimated effect of taxes on smoking will be attenuated, causing the treatment effect to be overstated (Brachet, 2008). While these assumptions are likely to be overly restrictive, smoking during pregnancy is still intuitively the primary mechanism by which fetuses are exposed to cigarette smoke. Using the vital statistics, I estimate a 0.31 percentage point decrease in probability of smoking during pregnancy at all (an elasticity of -0.23) from a $1 tax hike. This estimate falls into the range estimated by the earlier literature (see Table 1). Dividing sick days by this coefficient gives a treatment on the treated (TOT) estimate of roughly one sick day or 29% of the mean for a $1 increase in taxes.

Finally, I test the robustness of my estimates to including state linear time trends. Linear trends help account for differences in pre-trends in infant health for high–cigarette tax states relative to low-tax states. Of particular concern is the fact that my results are driven by unobserved factors causing child sick days to trend downward before states implement a hike. In this case, adding state linear trends would absorb the spurious coefficient on sick days. Column 6 shows the result when I include state linear time trends. The coefficient retains sign and significance after controlling for trends, which is further evidence that my results are not driven by unobserved factors. Including the linear trends does cause the magnitude of the coefficient estimates to increase to -0.58. However, my event study analysis sheds light on why this is the case.

Figure 3 shows the event study for sick days. The event study reveals a sharp decline in sick days around the time the tax hike is implemented. The downward decline begins in period -1, reflecting an effect on children around the time of birth. The event study is encouraging overall. Ideally, the event study shows a flat pre-period. Here, if anything, there is an upward trend. An upward pre-trend in the event study is consistent with Table 3, in which including state linear trends increases the magnitude of the coefficient on the excise tax. The upward pre-trend could reflect the effect of inflation decreasing the real value of a cigarette tax before a legislated change in the tax. Correcting for an upward pre-trend driven by inflation is an argument for including trends in sick day models; however, I remain conservative by excluding linear trends from my baseline estimates.[28]

Since the sample changed due to balancing the event study two years before and after the event, I re-ran my baseline regression model on the event study sample. The point estimate on excise tax reported in the box on Table 3 is roughly the same as the coefficient on excise tax in Column 5 of Table 3. This suggests that the change in the sample is not driving the event study result.[29] This is shown as the "tax09 coefficient" in Figure 3.[30]

It is important to interpret the x-axis of the event study in birth cohort time. Referring back to Figure 3, the treated cohort is in their third trimester at event time zero. If we move to the left to event time -1, we are looking at the cohorts who were in their third trimester one quarter earlier before the tax increase. They will be born around event time 0, so if the event study shows an impact on these cohorts, it could reflect an "early life" effect. Likewise moving into earlier periods of the event study potentially shows an effect of smoke exposure on older children. The excise tax faced by a child one year after the third trimester is also the tax level faced by a cohort one year before the current tax was implemented. Therefore, an alternative way to investigate pre-trends (or an effect of smoke exposure at older ages) is to include future excise tax "leads in age" in the model. Given the assumption of my model that I am capturing an in-utero effect, either a downward pre-trend in child health or significant negative coefficients on the "leads in age" can be seen as a failure of my empirical strategy.

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[28] The upward pre-trend could also represent an attempt to offset a negative trend in child health.
[29] However, the standard errors more than triple in size due to losing a large portion of the sample and tax variation.
[30] I used the event study sample to estimate a difference-in-differences model. I assigned a dummy variable for the treatment of those observations in the post-period of their state's cigarette tax hike. The coefficient should correspond to the difference in sick days in the pre-period and post-period of the event study. This coefficient is -1.31 and is shown in the box in Figure 3. A visual check reveals that it matches the decrease shown in the event study.

Table 4 presents the results of my preferred specification with leads of the taxes added to the model. [31] Column 1 of Table 4 includes the in-utero tax as well as the tax faced one year after a child is in the third trimester. The coefficient on the one-year age lead is small and positive and lacks any statistical significance. Accounting for the tax faced at a later age causes the coefficient on the in-utero tax to increase in magnitude and significance. This matches the results with linear trends (Table 3, Column 5) and reflects a similar adjustment for an upward pre-trend. The second column of Table 4 includes the leads of the taxes each year up to five years past the third trimester. Again, I find no statistically significant effect of these tax hikes. This further confirms that I am picking up an early life effect and that these results are not driven by a downward pre-trend.

Results for two or more doctor visits are shown in Table 5. My baseline coefficient estimate shows that increasing the excise tax while in-utero by $1 (in 2009 dollars) decreases the likelihood of seeing a doctor twice or more in 12 months by almost 3 percentage points. The ITT coefficient represents a 4.6% impact relative to the mean. As above, it is possible to divide by the maternal smoking coefficient to get a TOT estimate of 9.5 percentage points or approximately 15% of the mean (although as discussed above, this represents an upper bound).[32]

As with sick days, there is little effect of adding various state-level controls as is shown in Columns 2–5. After adding state linear trends in Column 6, the coefficient on the cigarette tax remains negative and significant. Interestingly, the doctor visits outcome is less sensitive to state linear trends.[33]

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[31] The sample size falls slightly when I add in the tax faced at age five. This result is due to limiting the tax variation through 2007. Some of the children born in a later cohort fall outside this window and are dropped. To make sure this was not driving my results, I ran my preferred regression specification on the smaller sample (without including age leads). I got the same results as shown for sick days in Table 3.

[32] I also impute doctor visits into the midpoint of the bin (rounding to the nearest whole number) and run a poission model. The results have a negative sign and a similar magnitude relative to the mean as with two or more sick days, but are not statistically significant. The added noise from using the poission may come from noise added by capturing the marginal effect of moving from zero visits to one visit, which captures a change in access rather more than a change in health. Future work will test this hypothesis using an ordered probit. The poission results are available upon request

[33] I also run an event study on doctor visits. Unfortunately, I lose a larger fraction of my sample when balancing the event study for doctor visits because I look at children as young as 24 months old in the baseline sample. Many of these younger children are dropped when balancing due to being at the edge of the event study window. Because of

Table 6 shows the results on doctor visits when including the leads of the excise taxes.[34] Including the tax at one year of age does not significantly change the coefficient on the tax experienced in the third trimester. Further, the coefficient on the tax faced one year later is small and statistically insignificant. The results in the second column are similar, but the lead at age two is statistically significant and negative. The overall pattern of the leads suggests that they are not as important for infant health as the in-utero effect. Table 6 also suggests that my main specification is not being driven by an effect on children of older ages or a pre-trend.

6.2 Results by Subgroup

My results can be further investigated by estimating the same models on various demographic and child subgroups. The prior literature (Markowitz et al., 2011; Decicca and Smith, 2009; see Table 1) shows higher price elasticities for lower-educated and younger women. Thus, I examine subgroups using mother's age at a child's time of birth and maternal education at time of interview. Table 7 divides the subgroups by mother's education and shows that the largest effects are concentrated in less-educated mothers. For sick days, the coefficient on mothers who are high school dropouts is -0.70. This is more than twice as large as the coefficient for the entire sample. This translates into an ITT estimate of roughly 21% of the mean. The coefficient for the children of college-educated mothers is small, positive, and insignificant, suggesting no effect for this group. That being said, there is limited power after stratifying the sample on subgroups for the sick day outcome. A similar pattern is followed for doctor visits. Children of mothers who are high school dropouts experience almost a full 8 percentage point drop in the probability of having two or more doctor visits in the past 12 months: a 15% decrease relative to the mean. The children of mothers who only have high school education experience a smaller but substantial decrease of 5.35 percentage points. There are still significant gains for mothers with some college education (-4.9 percentage points), but

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that, my regression results on the event study sample do not match the regression results on the full sample, which makes it difficult to draw any definitive conclusions. That being said, it is reassuring that I found no pre-trend in the doctor visits event study. Results for the doctor visits event study (as well as the event studies on the other outcomes) are not shown here but are available upon request.

[34] As with sick days above, I lose some of my sample when adding leads, but I get similar results when running my preferred regression on the smaller sample.

this fades out completely for mothers with a college education, who experience a positive but statistically insignificant increase of 1.8 percentage points.

Table 8 divides the subgroups based on mother's age at time of birth. I calculated mother's age at time of birth from information on mother's age and child's age in the NHIS. Following the earlier literature, teen mothers have the highest price elasticity of smoking. Likewise, the overall results from Table 8 follow a pattern of children of mothers younger than 30 experiencing the largest child health gains from in-utero cigarette tax exposure. Children of teen mothers experience a decrease in sick days of -0.47 and a decrease in the probability of having two or more doctor visits of 6.12 percentage points. On the other hand, children of mothers 40 and older experience no decrease in doctor visits or sick days. I take Tables 7 and 8 as suggestive evidence that the same subgroups experiencing the largest "first-stage" effects of cigarette tax hikes are also experiencing the greatest later life health gains.

To look for further evidence for this pattern, I turn to vital statistics natality data from 1989 to 2004. While a number of previous studies have estimated the impact of cigarette taxes on birth outcomes, I perform my own estimates in order to more closely match up the cohorts and subgroups to the ones in my NHIS analysis. I use the vital statistics to estimate the impact of taxes on low birth weight status and maternal smoking. My model is the same as my preferred specification from the NHIS: it includes all state policy variables, clean indoor air law ratings, unemployment rate, and state and cohort fixed effects. Besides maternal age and education, I look at subgroups based on child's race (Black, White, Hispanic, Other), mother's marital status, and child's gender. I then plot a scatterplot with the vital statistics treatment effects on low birth weight on the x-axis and the later life child health treatment effects on the y-axis. I include estimates for the entire sample as one of the points on the graph. The size of the points on the graph reflects relative subgroup size (using NHIS weights). I multiply the coefficients on each outcome variable by -1, which places the scatterplot in the first quadrant. This method is similar to that used by Hoynes et al. (2012) to compare subgroups whose income and children's birth weight were being increased by the EITC.

Figure 4 shows such a scatterplot with the coefficients for the reduction in sick days on the y-axis and the reduction in low birth weight status on the x-axis. Figure 4 reveals a strong correlation between being in a subgroup that gained birth weight as an infant and having fewer sick days later in life.[35] Figure 5 does the same exercise but for doctor visits: the patterns of results are strikingly similar. I take Figures 5 and 6 together as strong evidence that the gains to child health correspond directly to the early life birth weight effects found in the previous literature. The health impact of tax hikes can first be seen in early life in the form of the birth weight impacts and later show up 3–17 years down the line in child health outcomes. However, to be clear, I cannot conclude that the childhood health gains are *due* only to the improvement in birth weight since cigarette smoke may separately harm both birth weight and later life health.

I perform the same analysis with the vital statistics outcome of "any maternal smoking during pregnancy" as the dependent variable. These results are shown in Figures 7 and 8. The general pattern is that a larger decrease in maternal smoking is correlated with a larger decrease in sick days and doctor visits. However, the results are noisier than for Figures 5 and 6. The additional noise could come from the under reporting of maternal smoking on birth certificates (Brachet, 2008). Further, smoking data are only included in some states in the vital statistics whereas my sick days and doctor visits coefficients are from the full NHIS sample of all 50 states and DC.[36] That being said, the overall pattern is one of greater smoking elasticities correlated with greater childhood health gains.

Appendix Table A.1 breaks down the coefficients by tax hike "era." Estimating by "era" is important because tax hikes were implemented for different reasons during different years. Each column of Table A.2 represents a different collection of years over which there were tax

[35] The coefficient for Black children is off trend. Perhaps this is because the base low birth weight incidence for Black mothers is higher, leading to large marginal gains. The point for children born to "other races" is also off trend; however, the sample size for the "other" category is small meaning that it has little influence on my net NHIS results. I did a similar scatterplot using average birth weight and got a similar pattern.

[36] I have not run the NHIS regressions on only the states that have maternal smoking available in the vital statistics. Confidentiality issues for the restricted-use NHIS means it is prohibited to break down state-level regressions into smaller groups of states without explicit permission. I received this permission for the event studies but have not yet gotten permission to examine this for states that report smoking on birth certificates. Another reason Figures 7 and 8 might be noisier then Figures 5 and 6 is because second-hand smoke exposure could contribute to both birth weight effects and later life effects while not being fully picked up by the "any maternal smoking" variable.

hikes. The pattern of coefficients is such that the effects are not consistently concentrated at the beginning or end of the entire sample period. This is somewhat reassuring since tax hikes in the mid-1990s were driven by tobacco control policy whereas tax hikes in the 2000s were done for revenue. This suggests that neither one of these associated events is driving my results. That being said, the coefficient on sick days becomes slightly small in magnitude and loses significance in the later period.[37]

6.3 Other Outcome Variables

The NHIS contains additional information on childhood health and medical utilization outcomes. Unlike sick days and doctor visits, these outcomes tend to either be more extreme events, such as emergency room visits, or of lower incidence. In spite of this, I find some evidence of effects for many of these outcomes. The results are shown in Table 9 for emergency room visits, overnight hospitalizations in the last 12 months, and having an asthma attack in the last 12 months.[38] For the first specification (Column 1) of Table 9, the coefficients are negative across all outcomes. A $1 tax hike causes a -0.9 percentage point decrease in the likelihood of having an asthma attack in the past 12 months, an ITT of 15% of the mean. A $1 tax hike also causes a -0.40 percentage point decrease in having an overnight hospitalization (ITT of 18% of the mean) and a 1.71 percentage point decrease in having an emergency room visit in the past 12 months. Looking across the columns of Table 9 these results are also quite robust. Particularly, asthma attacks and hospitalizations show consistently strong effects on child health through most specifications. However, not all of these results are statistically significant, and the sign on

[37] Table A.2 shows results by child age. This was done because symptoms related to smoke exposure may evolve over time, or only get noticed at certain ages. However, I did not see any consistent patterns across age subgroups.

[38] I also ran regressions using a subjective measure of health (1–5 rating) as an outcome. These results are not shown in the paper due to the large number of outcomes reported. The results on subjective health were small, positive, and statistically insignificant. However, subjective health ratings are imperfect outcomes because they are less concretely measured than other health outcomes. These outcomes may pick up differences in perceptions between types of families or differences in language connotations between families. The NHIS also contains information on "ever having been diagnosed with asthma" as well as asthma attacks. I ran my regressions on this variable, and it produced similar results to having an asthma attack in the past 12 months. Finally, I am exploring grade retention and disability status outcomes using data from the American Community Survey (ACS). This work is in progress. Using the ACS involves significantly greater data challenges than the ACS because exact year and month of birth are not reported.

asthma attacks becomes positive (though statistically insignificant) when state linear trends are added.

7. Robustness Checks

7.1 Test of the timing assumptions

All results shown have assumed that the beginning of the third trimester of pregnancy is when the cigarette tax matters for infant health. This decision was motivated by the medical literature suggesting that the birth weight effects of smoking during pregnancy are likely to matter most in the third trimester. Small changes in timing should not have significant effects on the tax coefficient because moving a few observations around the cutoff from the treatment to the control groups is unlikely to change the results. Further, if the timing is wrong, the results are likely to be underestimates due to attenuated coefficients. Regardless, I test the timing assumption shown in Table 10 by looking at the first, second, and third trimesters. Each column represents a different regression. As expected, I find little difference in which trimester treatment is assigned to my results.[39] Table 10 shows the largest effects in the third trimester (especially for doctor visits), but none of the estimates are statistically different from each other.

In Column 4 in Table 10, I include the excise tax faced in each trimester in the same regression model. Controlling for the excise tax in each trimester simultaneously represents a horse race between the three trimesters to see which has the strongest impact. Including excise tax by trimester is pushing the data because excise taxes within a state do not change much within such a short period of time. As this would suggest, the standard errors increase when I add all three excise tax variables in the same regression, making it difficult to draw a conclusion about which trimester matters the most. However, the fact that the coefficient on the third trimester tax remains negative and significant is an encouraging robustness check, which supports using the third trimester as the baseline for timing assignment.

[39] As with looking at the age leads, the sample shifts slightly when assigning treatment in the first trimester. This comes from fixing the tax variation to be no earlier than 1988, causing some of the earlier cohorts to be excluded from the sample. I estimate each of the trimesters on the smaller sample with the third trimester model matching the same sample as my main model. The results do not change from the “main” results shown in Tables 3 and 5.

7.2 Sample Robustness Checks

Appendix table A.3 presents results that test the sensitivity of my main estimates on sick days and doctor visits to the assumptions I made when constructing the sample. I first drop all children that were missing a mother identifier in the NHIS data. Previously, these children were included in the regressions with a missing mother dummy in place of mother demographic controls. This causes virtually no change to the sick days and only a slight increase in the magnitude of the doctor visits variable to -3.22 percentage points. I next drop all children who were missing an exact date of birth and could not be precisely assigned treatment.[40] My results are fully robust to excluding this group. I finally drop the state of California from my sample. The logic behind this is that California implemented Proposition 99, the earliest and largest anti-tobacco media campaign of the late 1990s. To the degree that my coefficient estimates might be picking up the effects of Proposition 99, excluding California is a good robustness check. The sick days coefficient decreases somewhat and loses significance, but it remains negative and is not statistically different from the coefficient on the original sample.[41]

7.3 Placebo Tests

If my models are picking up spurious trends in child health, I would expect significant effects on outcomes that are not related to exposure to cigarette smoke. To this end, I perform a number of falsification tests using health data in the NHIS on chicken pox, anemia, chronic headaches, food allergies, and a constructed index of low-incidence outcomes (anemia, headaches, and allergies). Table 11 shows these placebo tests. The coefficients are all small in magnitude and statistically insignificant.

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[40] As discussed in the data section (Section 4), I assigned these children the year of birth as birth year = year of interview − age.

[41] While not shown in my main results, I also perform a number of additional robustness checks. I test the sensitivity to controlling for the ImpacTeen indoor air rating on restaurants instead of bars as well as to controlling for a dummy variable for a complete indoor air ban instead of the ImpacTeen rating. I run a model controlling for the alcohol tax in case changes in alcohol policy are correlated with changes in tobacco policy and are confounding my results. I collapse the sample down to the state and cohort year-month cell level and rerun my models. Finally, I add a control for state anti-smoking sentiment developed in the literature as a measure for the degree of anti-tobacco sentiment within a state at a given time (DeCicca et al., 2008). Across all of these checks, there was no substantial change to the coefficients on the cigarette tax for sick days and doctor visits. Likewise, there was no substantial change in the standard errors, and the coefficients remained statistically significant.

8. Economic Significance

To get a sense of the monetary value of my findings, I perform some back-of-the-envelope calculations as shown in Appendix Table A.4. These calculations are rough, hinge on several assumptions, and are meant to provide a sense of the magnitude of my treatment effects rather than to be wholly conclusive results. Row 1 of Table A.4 reflects the costs related to each case of a child health outcome. For doctor's visits, I use the average cost of a visit for children 5–17 years old which comes to $606 (Agency for Health Care Research and Quality, 2009). Similarly, for the cost of having an asthma attack, I use estimated average expenditures in a year for medical services related to asthma which comes to $1359 (Agency for Health Care Research and Quality, 2009). I quantify the costs of a sick day from school by estimating the forgone wages of missing a day of education. Assuming a year of education is worth 7% of wages (Harmon et al., 2011); I take 7% of the 2009 median earnings from the ACS (Social Security Administration, 2011).[42] Using the national average school year of 180 days, the value of a day of school is roughly $400 over a 40-year work life.[43]

I next estimate the monetized childhood health benefits of a $1 tax hike. In Row 2 of Table 4.A, I list the estimated treatment effect from my preferred specifications. I multiply the treatment effect by the cost of the associated child health ailment to get the monetary benefit per child per year of a $1 tax hike. In Row 6, I multiply this monetary benefit by the years I estimate treatment effects over (i.e., 13 years for sick days from school and 15 years for the other outcomes) to get the full health benefits over the course of childhood.[44] In total, the benefit from reducing doctor visits comes to $255 per child; for sick days from school, it comes to $1,768 per child; and for asthma treatment, it comes to $194 per child. Because children might be going to doctor visits to be treated for asthma, it would be inappropriate to add these two measures

[42] The median earnings reported by the Social Security Administration came to roughly $26,000 a year in 2009.
[43] This makes the overly strict assumption that there are no sheepskin effects for a year of education and that all days of education are equally valuable to human capital. On the other hand, this ignores the forgone earnings of parents who take time off from work to care for sick children.
[44] Note that for doctor visits I make the assumption that the reduction in the probability of having two or more doctor visits is comparable to reducing the probability of having an additional doctor visit by my treatment effect.

together. Instead, I add together the benefits of forgone asthma treatment and sick days to get a total value of $1,962 per child.

How should we think about the size of these amounts? One way is to compare them to the value of reducing low birth weight births. Using Almond et al. (2005) as a benchmark, the cost of moving a birth from 1,500 grams to more than 2,500 grams saves $25,137 in excess hospital costs. The estimated effect on low birth weight status of a $1 tax hike is -0.004. Therefore, the value per child of a $1 tax hike in terms of reducing birth weight costs comes to roughly $12 per child born. In this context, the long-term costs of early life exposure to cigarette smoke dwarf the costs to society of low birth weight births.

9. Conclusion

This paper documents the effect of early life exposure to cigarette smoke on childhood health. The restricted-use geocoded NHIS allows me to estimate the effects of state cigarette tax policies on a variety of childhood health outcomes; rarely examined in this way by health or labor economists. I have shown that there are large and persistent effects of early life smoke exposure and that cigarette taxes can be a useful tool to ameliorate the harm of this exposure. Furthermore, the birth weight effects of smoking found in the earlier literature are only the beginning: the health costs of smoking during pregnancy extend at least through childhood.

Based on my estimates, what can be said about the economic benefits of increasing cigarette taxes? Between 1980 and 2009, state cigarette taxes increased by $0.80 on average (in 2009 dollars).[45] Using my preferred estimates, this increase caused a decrease in sick days of 0.27 of a day. If I assume the mechanism is entirely due to maternal smoking, scaling by the change in maternal smoking represents a TOT of 1.1 days or 32% of the mean, though this is likely an upper bound. Similarly, the average tax hike decreased the probability of having two or more doctor visits by 2.3 percentage points for the population or (scaling by maternal smoking) 11 percentage points which comes to 15% of the mean. For an average-sized cohort of four

[45] Derived from Orzechowski and Walker (2011).

million children and using the health benefit per child from Table A.4, a $0.80 tax increase amounts to a savings of $6.1 billion.[46]

This is one of the first studies to look at the childhood health effects of a positive, in-utero, policy-generated improvement to health. My work demonstrates that policies designed to shield pregnant mothers can potentially have large societal returns. These returns can come in the form of lasting improvements in child outcomes and may not be fully captured by increased health at birth. This paper only examines the effect of an in-utero exposure to tax hikes on childhood outcomes. As these cohorts age into adulthood, future research can look at how in-utero smoke exposure influences labor market outcomes and health in adulthood.

[46] Derived from the 1989-2004 vital statistics data.

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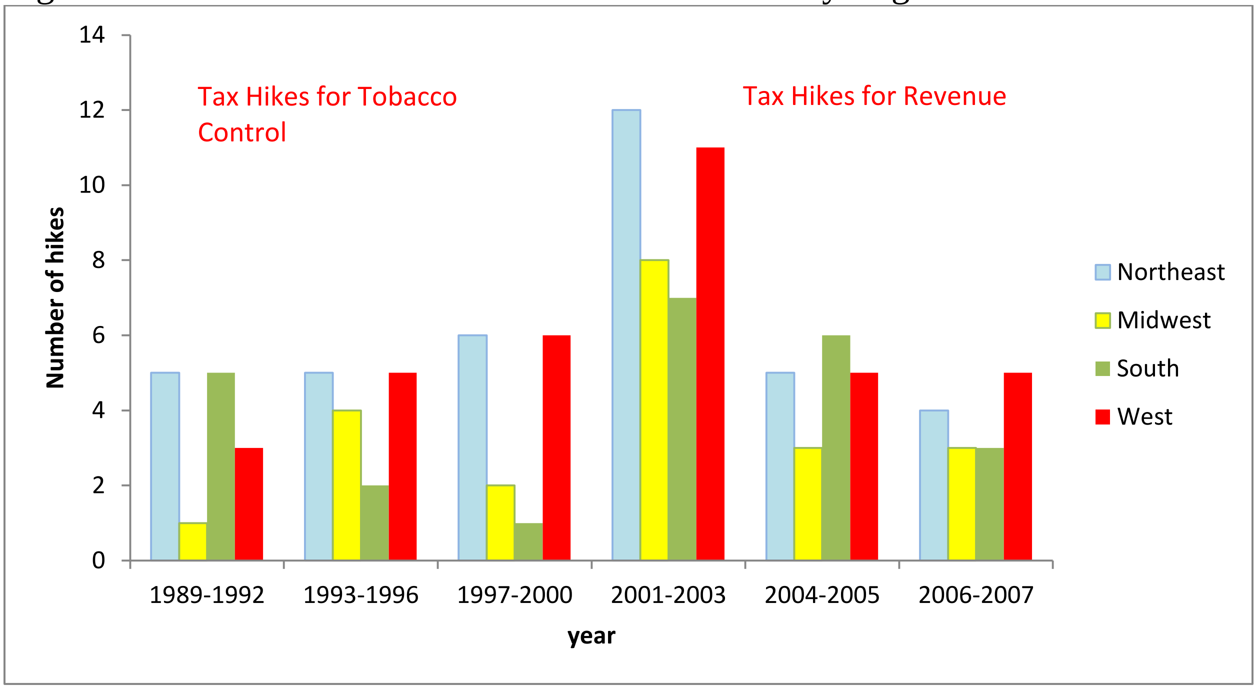
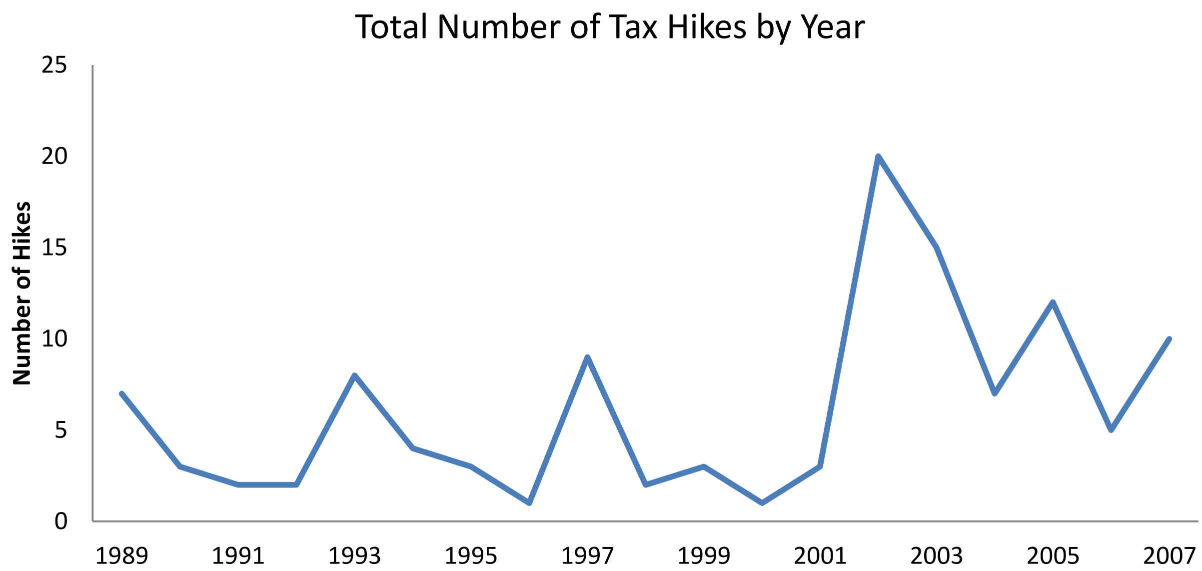
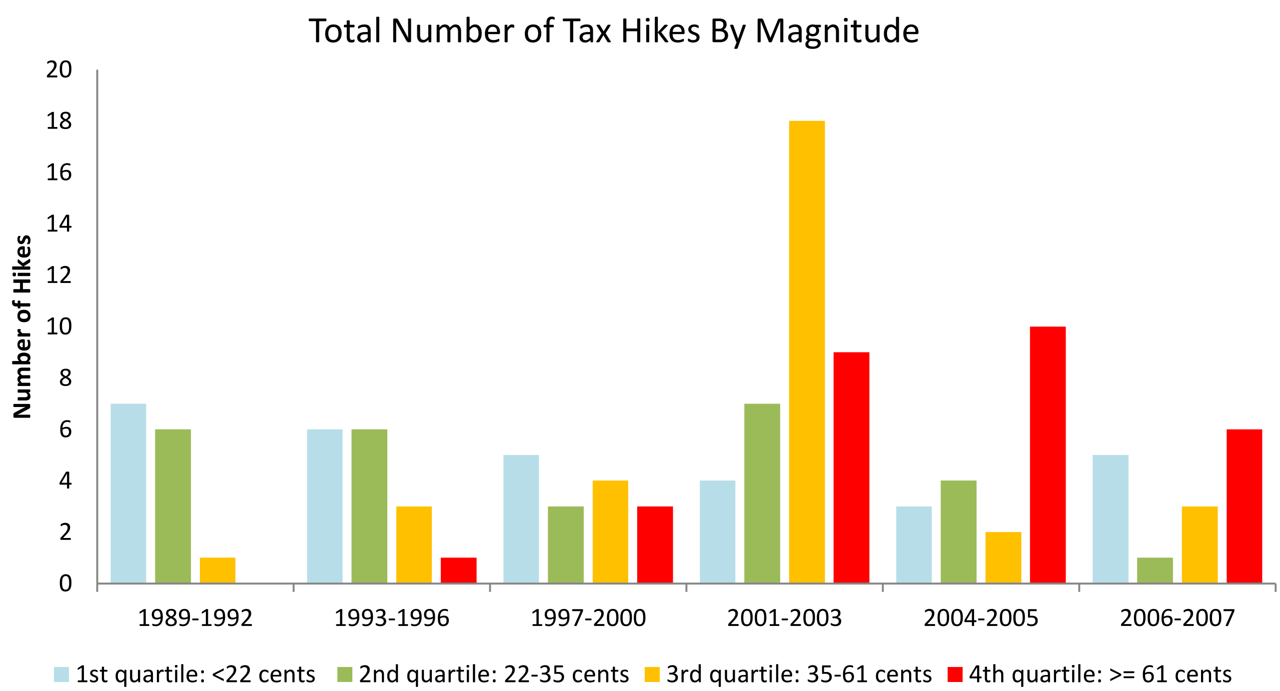
Figure 1. Number of Tax Hikes $0.25 Cents or More by Region



Compiled from excise tax data in "The Tax Burden on Tobacco" (Orzechowski and Walker, 2011). All tax hikes are inflation adjusted to be in 2009 dollars. There were approximately 117 hikes of more than $0.25 between 1989 and 2007. The years from 1993 to 2000 represent a period when tax hikes were largely done to reduce smoking. From 2001 to 2007, tax hikes were used to raise revenue.

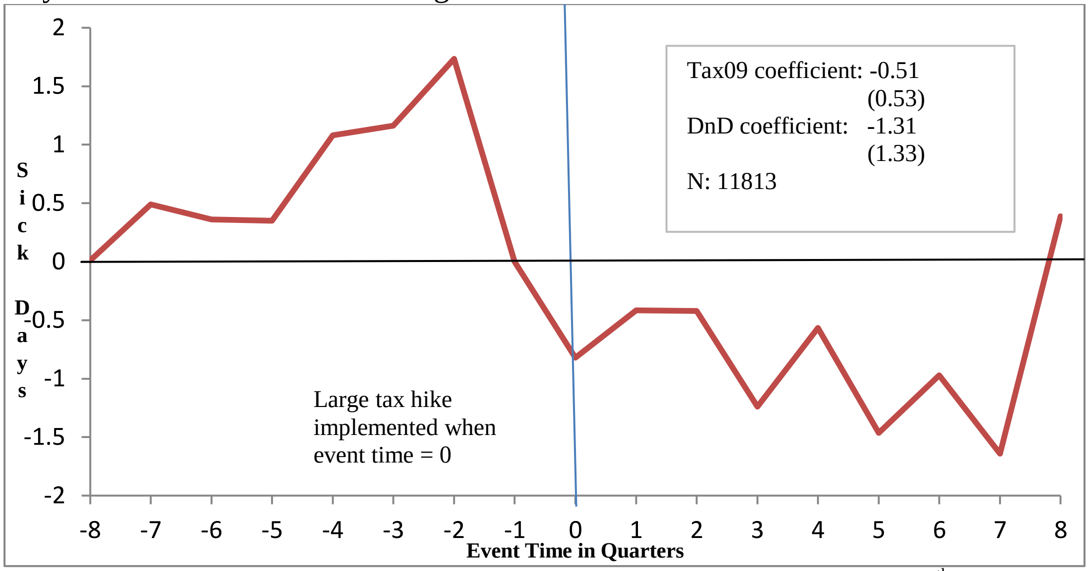
Figure 2. Tax Variation Over Time, 1989 – 1007 (in $2009)





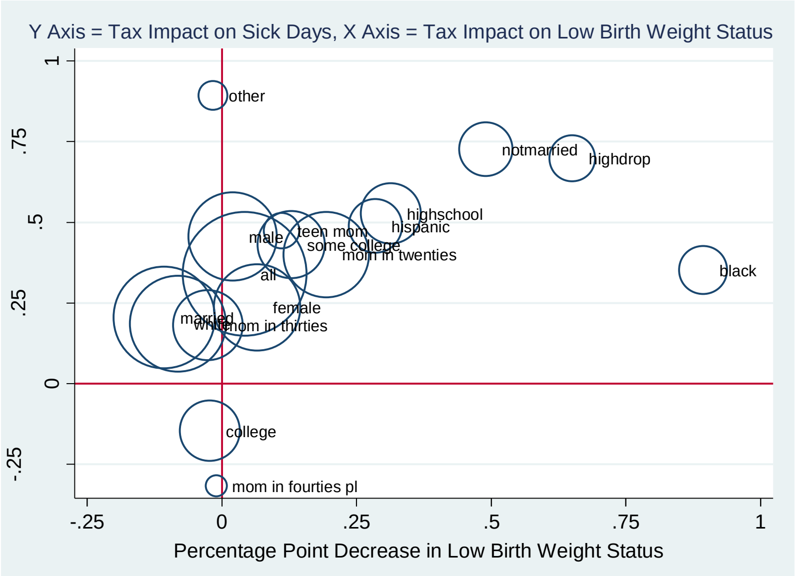
Compiled from excise tax data in "The Tax Burden on Tobacco", (Orzechowski and Walker, 2011). A tax hike is defined as any increase of 10 cents or more. All tax hikes are inflation adjusted to be in 2009 dollars.

Figure 3. Event Time Estimates of In-Utero Exposure to a Large Cigarette Tax Hike on Sick Days from School for Children Ages 5 – 17



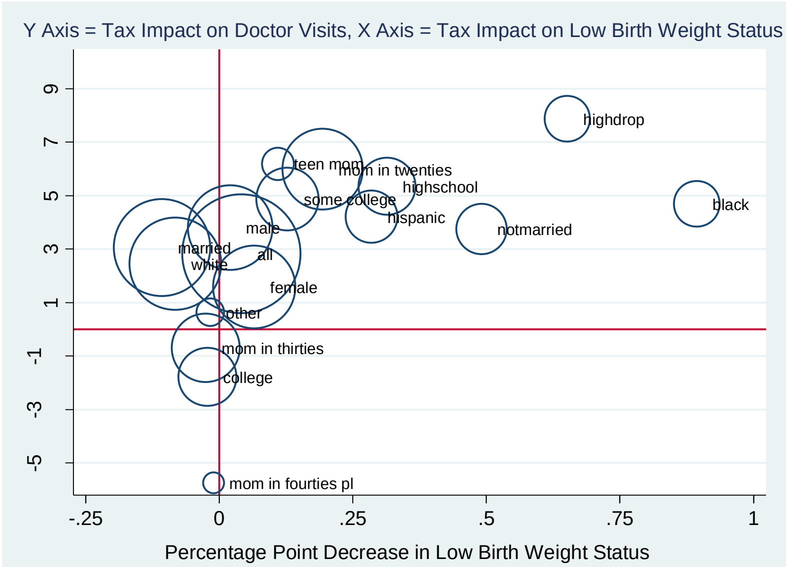
For the purpose of implementing an event study any cigarette tax increase above the 85th percentile is treated as a dichotomous event (see text for more details). NHIS child weights are used to weight the event study regression. All models include fixed effects for state, age-in-months, and time as well as controls for race, gender, state policies, the state unemployment rate, the ImpacTeen clean air rating in bars and private work places, and the current cigarette tax.

Figure 4. Subgroup Estimates of Cigarette Taxes on Sick Days and Low Birth Weight Status



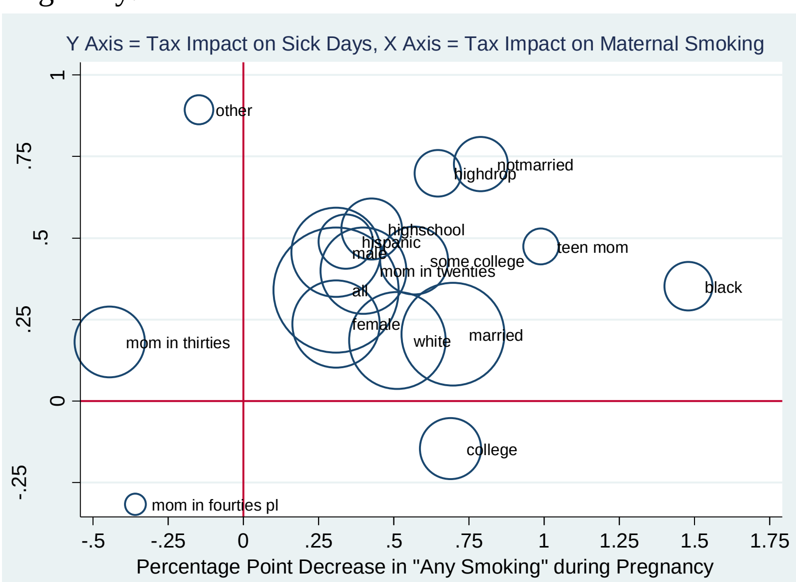
The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of low birth weight status on the state cigarette tax. The y-axis plots the coefficients from a regression of sick days from school in the last 12 months on the state cigarette tax.

Figure 5. Subgroup Estimates of Cigarette Tax on Two or More Doctor Visits and Low Birth Weight Status.



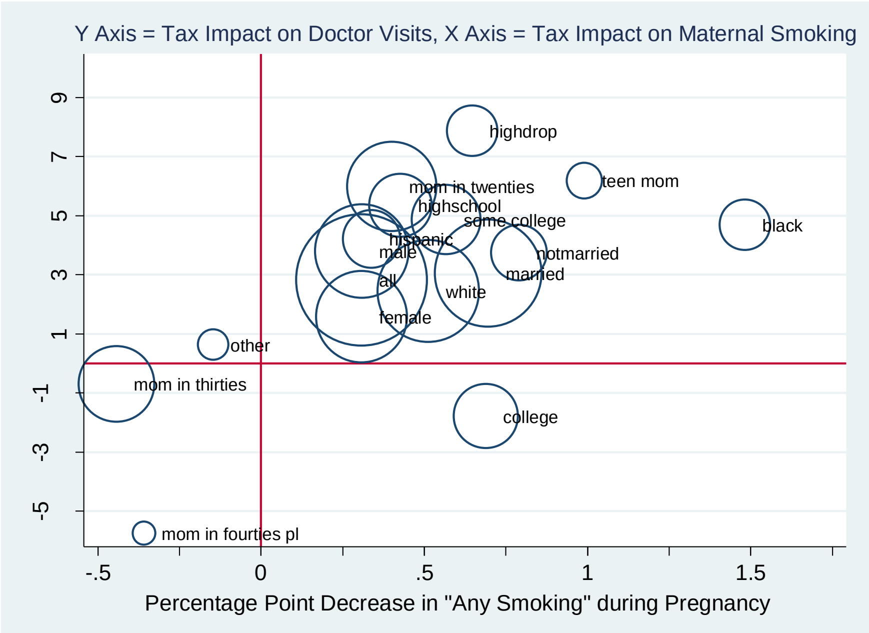
The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis plots the coefficients from a regression of an indicator for having two or more doctor visits in the last 12 months on the state cigarette tax.

Figure 6: Subgroup Estimates of Cigarette Taxes on Sick Days and Maternal Smoking during Pregnancy.



The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of an indicator for the mother having smoked at all during the pregnancy on the state cigarette tax. The y-axis plots the coefficients from a regression of number of sick days from school in the past 12 months on the state cigarette tax.

Figure 7: Subgroup Estimates of Cigarette Tax on Having Two or More Doctor Visits and Maternal Smoking during Pregnancy



The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of an indicator for the mother having smoked at all during the pregnancy on the state cigarette tax. The y-axis plots the coefficients from a regression of an indicator for having two or more doctor visits in the last 12 months on the state cigarette tax.

Table 1: Smoking Elasticities of Pregnant Mothers by Study

Study	Cohort Years	Data set	Demographic Group	Elasticity	% Smokers
Markowitz et al (2011)	2000 - 2005	PRAMS	Teen Mothers	-0.81	18 %
			Mom age 20 - 24	-0.23	19 %
			Mom age 25-34	-0.59	10 %
			Mom age 35 +	-0.13	10 %
Decicca and Smith (2009)	1999 - 2003	Vital Stats	All Mothers	-0.14	12 %
			Mom dropout	-0.24	21 %
Lien and Evans (2005)	1990 - 1997	Vital Stats	Mothers in Arizona, Ill., Mass. and Michigan	ave: -0.62	17.6 %
Gruber and Zinman (2000)	1991 - 1999	Vital Stats	Mom Age 13 -15	-0.24	13 %
			Mom Age 17 - 18	-0.37	8 %
Ringel and Evans (2001)	1989 - 1995	Vital Stats	All Mothers	-0.7	17 %
			Black	-0.55	14 %
			White	-0.79	19 %
			Hispanic	-0.64	6 %
			Other Race	-0.54	12 %
			Married	-1.12	13 %
			Unmarried	-0.37	25 %

Notes: All elasticities reported are the price elasticity of engaging in any smoking behavior during the pregnancy. Lien and Evans (2005) estimated separate elasticities for Arizona, Illinois, Mass. and Michigan; I took the simple average of these four elasticities together to get an elasticity of -0.62. See the text for details.

Table 2: Outcome Variables by Survey

Outcome	Survey	N	Mean	Ages	Cohort Years
Sick days from school in 12 months	NHIS Child	80063	3.43	5-17	1988-2005
Two or more doctor vsits in 12 months	NHIS Child	106415	60.88 %	2-17	1988-2007
Asthma attack in 12 months	NHIS Child	107609	5.87 %	2-17	1988-2007
Emergency room visit in 12 months	NHIS Child	107227	20.53 %	2-17	1988-2007
Hospitalized over night in 12 months	NHIS Person	236321	2.16 %	2-17	1989-2007
Any smoking during pregnancy	Vital Statistics	1833409	13.68 %	newborn	1989-2004
Low birthweight	Vital Statistics	1986654	7.48 %	newborn	1989-2004
Birthweight	Vital Statistics	1986654	3319.98	newborn	1989-2004

Notes: Sample weights are used for calculating all means. The means of dichotomous variables are multiplied by 100. I currently do not use vital statistics data later than 2004 due to state identifiers not being available in the public use files. See the text for details.

Table 3: The Impact of Cigarette Taxes on the Number of Sick Days from School in the Past 12 Months

	(1)	(2)	(3)	(4)	(5)	(6)
Excise Tax (dollars)	-0.31*	-0.29**	-0.30*	-0.35*	-0.34*	-0.58**
	(0.15)	(0.14)	(0.15)	(0.17)	(0.18)	(0.26)
Average increase in Excise Tax 1980 - 2007	$0.80					
Mean of the dep. variable	3.43					
N	80053					
State policy controls	no	yes	yes	yes	yes	yes
Unemployment rate	no	no	yes	yes	yes	yes
Clean air law rating	no	no	no	yes	yes	yes
Current tax	no	no	no	no	yes	yes
State linear time trends	no	no	no	no	no	yes

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 5-17, born from 1988 to 2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time as well as controls for race (Black, White, Hispanic, Other), and gender. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Table 4: The Impact of Cigarette Taxes on Sick Days from School with Age Leads

	(1)	(2)
Excise Tax (dollars)	-0.56**	-0.55**
	(0.25)	(0.25)
Tax: one year lead (dollars)	0.30	0.18
	(0.19)	(0.24)
Tax: two year lead (dollars)		0.13
		(0.28)
Tax: three year lead (dollars)		0.04
		(0.28)
Tax: four year lead (dollars)		0.05
		(0.21)
Tax: five year lead (dollars)		-0.11
		(0.25)
N	79891	

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 5-17, born from 1988-2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. Model 1 additionally includes a one year age lead for the cigarette tax. Model 2 includes leads through age five for the cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. The sample size changes relative to table 3 since I only include observations to which I can assign the tax up to 5 years ahead for both models, which causes some birth cohorts to be excluded. The baseline results are unaffected by this change. See the text for more details.

Table 5: The Impact of Cigarette Taxes on Doctor Visits, The Likelihood of Having Two or More Visits in the Past 12 Months

	(1)	(2)	(3)	(4)	(5)	(6)
Excise Tax (dollars)	-2.96***	-2.95***	-2.96***	-3.15***	-2.83***	-2.02**
	(0.97)	(0.92)	(0.93)	(0.90)	(0.92)	(0.96)
Average increase in Excise Tax 1980 - 2007	$0.80					
Mean of the dep. variable	63.46					
N	106401					
State policy controls	no	yes	yes	yes	yes	yes
Unemployment rate	no	no	yes	yes	yes	yes
Clean air law rating	no	no	no	yes	yes	yes
Current tax	no	no	no	no	yes	yes
State linear time trends	no	no	no	no	no	yes

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988-2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time as well as controls for race (Black, White, Hispanic, Other), and gender. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Table 6: The Impact of Cigarette Taxes on Doctor Visits with Age Leads

	(1)	(2)
Excise Tax (dollars)	-2.99**	-3.00**
	(1.49)	(1.48)
Tax: one year lead (dollars)	-0.06	2.49
	(1.49)	(2.44)
Tax: two year lead (dollars)		-3.80**
		(1.88)
Tax: three year lead (dollars)		1.36
		(1.44)
Tax: four year lead (dollars)		-1.92
		(1.50)
Tax: five year lead (dollars)		1.85*
		(1.07)
N	102286	

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988-2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. Model 1 additionally includes a one year age lead for the cigarette tax. Model 2 includes leads through age five for the cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. The sample size changes relative to table 3 since I only include observations to which I can assign the tax up to 5 years ahead for both models, which causes some births to be excluded. The baseline results are unaffected by this change. See the text for more details.

Table 7: The Impact of Cigarette Taxes on Sick Days and Doctor Visits by Mother's Education at Time of Interview

	Dropout	High school grad	Some college	College grad
		Sick Days from School		
Excise Tax (dollars)	-0.70	-0.53	-0.43	0.15
	(0.68)	(0.36)	(0.50)	(0.17)
mean	3.39	3.55	3.70	3.01
N	13128	18753	22043	16468
		Doctor Visits, The Likelihood of Having Two or More Visits		
Excise Tax (dollars)	-7.86**	-5.35***	-4.87***	1.79
	(2.66)	(1.75)	(1.40)	(1.78)
mean	52.31 %	58.93 %	63.23 %	66.50 %
N	17718	24449	28700	21896

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988-2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate,the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Table 8: The Impact of Cigarette Taxes on Sick Days and Doctor Visits by Mother's Age at Time of Birth

	Teen mom: age 12-19	Age 20 to 29	Age 30 to 39	Age 40 plus
		Sick Days from School		
Excise Tax (dollars)	-0.47	-0.40	-0.18	0.32
	(0.56)	(0.25)	(0.18)	(1.15)
mean	3.40	3.41	3.45	3.46
N	6573	35480	25907	2853
		Doctor Visits, The Likelihood of Having Two or More Visits		
Excise Tax (dollars)	-6.19**	-5.99***	0.70	5.74
	(3.04)	(1.24)	(1.17)	(5.15)
mean	57.52 %	60.26 %	62.10 %	61.54 %
N	8835	47230	33608	3730

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988-2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Table 9: The Impact of Cigarette Taxes on Other Childhood Health Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Asthma Attack in 12 Months					
Excise Tax (dollars)	-0.87*	-0.88*	-0.95*	-0.93*	-0.95*	0.43
	(0.48)	(0.48)	(0.48)	(0.52)	(0.54)	(0.60)
mean	5.87					
N	107609					
	Overnight Hospitalizations in 12 Months					
Excise Tax (dollars)	-0.40*	-0.39*	-0.41*	-0.30	-0.30	0.01
	(0.21)	(0.22)	(0.21)	(0.20)	(0.21)	(0.29)
mean	2.16					
N	236321					
	Emergency Room Visits in 12 Months					
Excise Tax (dollars)	-1.71	-1.76	-1.80	-1.72	-1.94	-0.05
	(1.13)	(1.12)	(1.13)	(1.27)	(1.19)	(1.28)
Average increase in the Excise Tax 1980 - 2007	$0.80					
mean	20.53					
N	107227					
State policy controls	no	yes	yes	yes	yes	yes
Unemployment rate	no	no	yes	yes	yes	yes
Clean air law rating	no	no	no	yes	yes	yes
Current tax	no	no	no	no	yes	yes
State linear time trends	no	no	no	no	no	yes

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 5-17, born from 1988-2007. NHIS child weights are used in models where the dependent variable is having an asthma attack or an emergency room visit. NHIS person weights are used in models where the dependent variable is having an overnight hospitalization. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Appendices

A Tables

Table A.1: Outcomes by Tax Hike Era

	1988 - 1995	1996 - 2000	2001 - 2005
		Sick Days	
Excise tax (dollars)	-0.47*	-0.76*	-0.23
	(0.26)	(0.39)	(0.37)
mean	3.56	3.20	3.15
N	53041	20455	6557
		Doctor Visits, The Likelihood of Having Two or More Visits	
Excise tax (dollars)	-5.03*	-0.23	-6.72**
	(2.98)	(3.09)	(2.11)
mean	58.00	62.65	66.74
N	59032	29484	15074

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988 to 2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. * indicates significance at the 10% level. ** indicates significance at the 5% level. *** indicates significance at the 1% level. See the text for more details.

Table A.2: Outcomes by Child Age

	Ages 3-4	Ages 5-7	Ages 8-11	Ages 12-14	Ages 15-17
			Sick Days		
Excise tax (dollars)		-0.55**	0.19	-0.38	-0.19
		(0.27)	(.32)	(0.40)	(1.20)
mean		3.34	3.27	3.54	3.85
N		22989	27688	16790	12594
		Doctor Visits, The Likelihood of Having Two or More Visits			
Excise tax (dollars)	-4.53***	-0.20	-2.60	-8.99**	.015
	(1.69)	(2.00)	(2.97)	(3.95)	(7.37)
mean	74.54	69.13	56.82	53.59	51.89
N	8355	16850	27819	16881	12718

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 3-17, born from 1988 to 2007. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. There are no sick day outcomes for children ages 3-4 because these children are too young to have entered school.

Table A.3: Sample robustness checks

	Original sample	Drop if missing mother id	Drop if missing birth date	Drop California
		Sick Days		
Excise tax (dollars)	-0.34*	-0.34*	-0.33*	-0.22
	(0.18)	(0.18)	(0.19)	(0.17)
N	80053	70859	74276	67680
		Doctor Visits		
Excise tax (dollars)	-2.96***	-3.222**	-2.50**	-2.74**
	(0.97)	(0.94)	(1.11)	(1.01)
N	106401	93432	99176	90041

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. The NHIS is the main dataset used for all models in this table. All regressions are weighted using NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, state level policies, the state unemployment rate, the ImpacTeen indoor air law rating in bars and private work places, and the current cigarette tax. The first column is the original sample estimated in table 3 column 5. The second column drops observations that are missing the mother identifier and therefore cannot be matched to a mother. The third column drops observations missing date of birth. The fourth column drops the state of California. See the text for more details.

Table A.4: Monetized Benefits of a Dollar Tax Hike to Childhood Health

Adverse Outcome:	Doctor Visit	Sick Day from School	Treatment of Asthma
Average cost of outcome ($2009)	$606	$400	$1,359
Treatment effect (ITT) per year	-0.0280	-0.3400	-0.0095
Years of health effects	15 years	13 years	15 years
Childhood benefits from tax hike ($2009)	$255	$1,768	$194
Total decrease in health costs per child (ignoring potential double counting)		$ 1,962	

All benefits are in 2009 dollars. The cost of a doctor visit is the average cost of visiting a doctor for children ages 5-17. The cost of treatment for asthma is the average expenditures on asthma treatment services. Both doctor visit and asthma values were calculated by the Agency for Healthcare Research and Quality (The Center for Financing, Access and Cost Trends) from the Medical Expenditure Panel Survey (2009). The cost of a sick day from school is the forgone wages of missing a day of education. This assumes that a year of education increases wages by 7 % and uses the median household earnings in 2009 to approximate the value of a day of education. See the text for more details.

B Data

B.1 Restricted-Use Geocoded National Health Interview Survey

Roughly 6% of my sample is missing information on year and/or month of birth. I deal with data missing year of birth by using a simple assignment rule: year of birth = year of interview − age of child. Fewer children were missing the month of birth. I assign these to being born in June, the midpoint of the year. Since cigarette taxes do not change in high frequency within the same state, this is unlikely to affect my results. I check this by dropping all of the observations missing date of birth and re-running my baseline observations. I also perform a second check for which I randomly impute the birth date over the possible years and months a child was born based on year of interview and age. Neither of these robustness checks changes my baseline results.

The ideal method for dealing with observations missing exact birth date would be to use multiple imputations. This involves randomly imputing the date of birth multiple times, calculating different coefficient estimates for each imputation, and making adjustments for the noise added to the estimates by the imputation. Unfortunately, since I use the restricted-use NHIS, I am limited in both time in the RDC) computer lab and output that I can bring out of the lab. This kept me from easily doing the multiple imputation method. Instead, I decided to implement the methods and robustness checks described above. Checking my results using the multiple imputation method is on my agenda for future trips to the RDC.

In the child detail file of the NHIS, there is some birth weight data. At first, it seemed promising to estimate birth weight in the same sample as I estimate the childhood health outcomes. Unfortunately, the birth weight data appears to be of low quality compared to the vital statistics. The NHIS birth weight variable is retrospective, which is likely to be noisier than the administrative vital statistics data. More importantly, when comparing low birth weight status in the NHIS to the administrative vital statistics data, the NHIS consistently overstates the fraction of low birth weight births by several percentage points. Due to these issues, I rely on the higher-quality administrative data.

B.2 Details on the Construction of the Event Study

I make several adjustments to a traditional event study so that it fits with the cigarette excise tax policy. To address variation in magnitudes across tax hikes, I take all (inflation-adjusted) tax hikes and assign them percentiles (un-weighted). I then define my discrete tax hike event as any tax hike greater than or equal to the 85th percentile ($0.72 in 2009 dollars). This serves three purposes. First, the largest-magnitude hikes are not pooled together into the same event study as the lower-magnitude hikes. Second, after limiting events to the 85th percentile and balancing the event study, I have only one hike per state. Finally, this adjustment means virtually every event in my sample is now in the 2001–2005 period.[1] Focusing on this later period to control for systematic trends is ideal for leveraging a large number of high-magnitude hikes that occur within a short time period of each other.

I balance the event study such that events are only included if there are two full years in both the pre-period and post-period. Balancing event studies has been previously well

[1] The one exception is Alaska, which had a $0.94 hike in 1997. Michigan, which implemented a $0.72 hike in 1994, also had a hike before 2001. During my trip to the RDC, Michigan was initially dropped from the event study because its hike was from a much earlier period than the other hikes. On my next visit to the RDC I plan on adding Michigan back in and re-running the event study to make sure the results are robust to the inclusion of Michigan.

established in the literature (see Almond et al., 2012). Without balancing, the graphic depiction of the event study could pick up demographic changes from states entering and exiting the event window. I also exclude any events in which there was a cigarette tax hike in the same state within the two-year pre-period before that event occurred. This preserves the pre-trends from showing a spurious trend due to an earlier hike, although very few events were censored due to this.[2] Because my event study sample changes from my main regression model, I re-estimate the preferred regression specifications on only the event study sample[3].

B.3 Vital Statistics

I look at birth certificate data from 1989 to 2004. Ideally, I would have birth certificate data through 2007, but the public-use natality data only includes state identifiers through 2004. I have applied for the restricted-use data for 2005–2007. I have not yet gotten access to this data. I start with the 1989 cohort instead of the 1988 cohort because smoking is not reported in the vital statistics before 1989. I collapse observations down to the state, cohort year and month, race (White, Black, Hispanic, Other), gender, age, marital status, and maternal education (dropout, high school some college, college) cell. I then weight my regressions by cell size to get population-level estimates. This makes running the regressions more practical and efficient.

For the birth weight, outcomes I follow the standards in the literature and construct an indicator for low birth weight status (birth weight < 2,500 grams) and use reported birth weight in grams. For smoking outcomes, I follow the literature and construct a dummy variable equal to 1 if the mother reported smoking at any point during the pregnancy. As with my other outcome variables, Table 2 lists the sample size and means of the outcomes I use from the vital statistics.

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[2] Only one event was excluded from the sick days event study because of this.

[3] For completeness, I also run the event studies unbalanced and on each quartile of the tax hike. As expected, introducing lower-magnitude hikes, unbalancing the event time window, and including multiple hikes per state significantly increases the noise of the event studies. I do not show these results, but they are available upon request. While the event studies were noisy, I was reassured that studies showed no downward pre-trends.