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

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ABSTRACT:

Evidence suggests that excise taxes on tobacco improve fetal health. It remains unknown if smoke exposure causes lasting harm to children. I find that a one dollar increase in the state cigarette excise tax causes a 10% decrease in sick days from school, and a 4.5% decrease in the likelihood of having two or more doctor visits in the past 12 months. I find suggestive evidence for decreases in emergency room visits, hospitalizations, and asthma. This supports the hypothesis that in-utero exposure to smoking carries significant medium-term costs and excise tax policy can result in lasting intergenerational improvements in wellbeing.

JEL Codes: H71, I14.

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1. Introduction

Does in-utero exposure to cigarette smoke permanently harm children? I examine the long-term implications of in-utero smoke exposure for child health and health care utilization. I leverage cigarette tax hikes to circumvent the endogeneity of maternal smoking and second-hand exposure. Use of cigarette taxes sheds light on the viability of tobacco policy for improving health, decreasing health care costs, and stemming the intergenerational transmission of low socioeconomic status. 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 effect 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). 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 use this identification strategy 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

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[1] Cigarette taxes are not frequent enough to truly separate effects due to early life exposure from effects due to 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 after birth).

[2] 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

teenagers and high school dropouts (Gruber and 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 the United States Vital Statistics birth records 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 last 12 months. To investigate changes in health care utilization, I look at an indicator for having two or more doctor visits in the last 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

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teenage mothers and mothers between the ages of 25 and 34. Focusing on more recent tax hikes, Callison and Kaestner (2012) argue that the impact for the adult population as a whole is negative but not statistically significant in some specifications. However, Decicca and McLeod (2008) find large and statistically significant effects of recent tax hikes on adults between the ages of 45 and 59.

[3] The literature on smoking during pregnancy has focused on smoking participation rather than the number of cigarettes smoked. One reason for this is measurement error: 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.

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 controls for the cigarette tax faced at ages 1 through 5 (synonymous with cohort lags).[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. To my knowledge, the cigarette excise tax literature has not previously used the event study methodology.

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 the infant health effects of cigarette taxes 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 that policy interventions early in a child's life can result in disproportionately large returns (Almond and Currie, 2011).

2. Expected Effects

What expected maladies should result from in-utero smoke exposure? The most robust result in the medical literature is that smoking during pregnancy decreases birth weight (USDHHS, 2001). 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

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[4] As discussed in the methodology section, the tax rate faced at later ages also captures the tax rate faced by cohorts who have not yet been exposed to a hike and therefore represent pre-trends in cohort exposure to the tax.

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

reaches the fetus, resulting in decreased birth weight with effects strongest in the third trimester (USDHHS, 2001).[6]

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, 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.

Studies in the economics literature have offered causal evidence that cigarette smoke harms 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 the effect 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 the major papers in this literature. The impact of taxes on smoking has persisted over time even though the elasticity has fallen in more recent years. 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. Other studies show that a tax hike causes a "sharp" decrease in smoking (Lien and Evans, 2005).[7] I extend this literature by testing the hypothesis—for the first time of which I

[6] Because of this, I use the third trimester as the baseline for my analysis.
[7] 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.

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? Skeptics could argue that the influences of taxes on childhood outcomes should be too small or noisy to detect. However, the earlier literature has shown moderately large effects of cigarette taxes on birth weight. Economists have had success in showing the long-term effects of early life environment through tests of the fetal origins hypothesis (FOH). Given these motivations, I believe it is important and reasonable to test for long term effects of in-utero exposure to a cigarette tax.

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 family income and in-utero nutrition (Banerjee et al., 2010), malaria exposure (Barreca, 2010), food stamp introduction (Hoynes et al., 2012), as well as many others.[8]

Studies in Epidemiology and Economics offer insight into the FOH as it applies to smoking during pregnancy. Studies have found correlations with early

[8] 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. On the other hand, Royer (2009) looks at American cohorts using twin fixed effects and finds a statistically significant but much smaller effect of birth weight on education and the birth weight of one's children. 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.

life cigarette smoke exposure and test scores, labor market outcomes (Currie and Hyson, 1999), schooling (Harkonen et. all, 2012; Restrepo, 2012), occupational skills (Jackson, 2010), asthma (Stick 1996), stunting, childhood obesity, and overall child health (Stick 1996, Lessen, 1998). However, many of the epidemiology papers in this literature 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. My paper complements the epidemiology literature by offering a causal test of the lasting childhood health effects of smoke exposure.

This study is 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 than using a natural disaster because the estimated treatment could be implemented again in a policy setting. Further, most FOH studies skip over childhood and only look at adult outcomes.

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.[9] 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

[9] 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.

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 $0.10 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. From 2001-2005, tax hikes were implemented to increase general state revenues (Maag and Marriman, 2003; Decicca and McLeod, 2008). 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 2009.[10] The top panel of Figure 2 shows the number of tax hikes per year. This makes explicit the variation over time and the explosive increase in tax hikes in 2001. The bottom 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. These trends frame why it is important to control for unrestricted time in month fixed effects, something I do in all of my models.[11] 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

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[10] 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. 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.

[11] 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.

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.[12] 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 in the Sample Adult and Sample child questionnaires. I look at cohorts of children 24 months old (2 years) to 17 years old born between 1989 and 2009. 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.[13] I also drop all observations that are older than 17 to avoid introducing a bias due to selecting on young adults who have not yet left their parents' household. Since 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 exact gestational age and instead I must assume 9 months of gestation.[14] 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

[12] I accessed the restricted-use geocoded data through the Center for Disease Control's research data center in Maryland.

[13] For completeness, I have also run my baseline models including children 0–24 months old. Adding these observations does not significantly change my results.

[14] Markowitz et al. (2013) estimates the effect of taxes on gestational age and finds that a dollar tax hike has no effect on average weeks of gestation though it has some effect on the likelihood of being born full term. Within state taxes change relatively infrequently, so using a different gestational age is unlikely to result in a different tax rate being assigned.

of birth add a small amount of noise to the excise tax level variable. Measurement error in a right-hand-side variable attenuates its associated coefficient implying true effects that are somewhat larger than what I estimate.

According to the medical literature, the effect of maternal smoking on birth outcomes is strongest in the third trimester. Assigning treatment in the third trimester places infants in the first through third trimesters in the post period of a tax increase.[15] For my baseline results I attempt to capture the full range of health effects and therefore assign treatment in the third trimester; I merge in monthly state cigarette excise taxes from Orzechowski and Walker (2010).[16] I use outcomes from both the Person Core and Sample Child questionnaires.[17] Sample sizes are listed for each outcome in Table 2.

The doctor visits variable is aggregated into bins: (0, 1, 2-3, 4-9, 10-12, 13 +).[18] The aggregated nature of the data makes it natural to define the outcome variable as a dichotomous indicator for being above a threshold number of visits. A problem that arises in choosing the appropriate threshold is that increased utilization could be caused by either improved access to care or decreased health. 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

[15] By this assignment rule, those who have not yet been born are also placed in the "post" period of a tax increase.

[16] Orzechowski and Walker also have data on the average annual cigarette price (inclusive of tax) by state. There are a number of problems with using price data in this analysis. First, the data on price is annual instead of monthly. Annual prices makes it more difficult to assign timing as precisely as the month of the tax increase. Secondly, since variation in price reflects changes in demand and supply, price is less plausibly exogenous than a tax. Finally, the between state variation in cigarette price is almost entirely due to state excise taxes. This means using price largely adds time series variation and does not improve identification.

[17] I do not use the Sample Adult questionnaire. The adult file does not have retrospective questions on smoking during pregnancy. While the adult file does collect information on current adult smoking, since only one adult per household is interviewed, it would be a noisy proxy for a child's exposure to cigarette smoke.

[18] The question considers a doctor visit to be an in-person visit to a health professional. The question explicitly excludes overnight hospitalization, emergency room visits, and hospitalizations. The survey also direct interviewers not to count dental visits.

equal to one if the child had one or more doctor visits in the past 12 months. They argued that doctors recommend a child receive one visit every twelve months, implying that the move from zero to one doctor visits is associated with an improvement in access rather than a decline in health.[19]

I construct an outcome variable similar to the one used by Currie and Gruber, but I primarily want to evaluate whether there is a decline in child health rather than a change in access. Therefore, the indicator equals one 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 is more likely to capture a health effect relative to the move from zero to one visit.[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. I calculate mother's age at time of birth from information on mother's age and child's age in the NHIS. Unfortunately, the mother 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.

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[19] Another reason to investigate doctor visits as an outcome is that decreased health utilization is of direct interest. Decreased utilization unambiguously decreases spending on health care. Families care about the costs of doctor visits, emergency room visits, and hospitalization. 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.

[20] An analysis of the public use NHIS data in appendix table 1 supports this decision. Appendix table 1 shows that the likelihood of reporting poor health status decreases for those who have had one relative to zero doctor visits. Going from one to two visits (or more) is associated with an increase in the percent of children who report being in poor health. Sick days and asthma attacks stay constant or only increase slightly going from zero to one visit relative to two or more.

For placebo 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 boost the power of my tests following Kling et al. (2007); I 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.

I also use data from the U.S. 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 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.[22] 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. I always consider "time" to be the month and year the

[21] 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] I currently cannot look at cohorts born after 2004 in the vital statistics because the public-use data does not include state identifiers.

child begins the third trimester. 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 an 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.[23] 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, 36 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 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-differences 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, it is likely I would see my coefficients change as I add policy-related state-level controls.

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[23] To make sure my results are not sensitive to functional form, I also run a logit model for the dichotomous outcomes. The results do not change.

I add controls for the excise tax faced at later ages. This tests whether I am truly picking up an early life effect rather than a second hand exposure effect of taxes on child health. As discussed below, tax "leads" in age also capture how the tax level impacted cohorts born before a tax change. Therefore, controlling for the tax at later ages is similar to putting in tax-cohort lags. These lags also test whether there was not an effect of a tax on cohorts before a tax increase.

I also look at differences in child health outcomes across subgroups. When analyzing differences by subgroups, I estimate birth outcomes in the vital statistics and childhood health outcomes in the NHIS. For each, 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).[24]

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 away from 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. The case: j=0 indicates that a cohort is in the third trimester when the tax hike occurred. The case j=-1 indicates that the cohort is in

[24] They showed that the same demographic groups whose income increased due to the Earned Income Tax Credit (EITC) were those whose infants had birth weight increases.

the third trimester the quarter before the tax hike and will be born around the time of the tax hike. Unlike with standard difference-in-difference models, only observations that experience a policy intervention are 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.

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, 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). 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 established in the literature (see Almond et al., 2012). Without balancing, the event study could show biased trends through picking up the demographic changes from states entering and exiting the event window. I discuss my decisions related to constructing the event study in more detail 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 two or more doctor visits. 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.[25] A one-dollar tax increase causes a decrease of 0.31 sick days from school in the past 12 months. The coefficient on sick days is close to being significant at the 5% level with a p-value of 5.5. The p-value stays near this significance level across specifications, reassuring me that the sick day results are unlikely to be driven by type 1 error.

My first check is to test the sensitivity of my estimates to a wide range of state characteristics and policy controls. This is important because the coefficient on the cigarette tax is unbiased if there are no state-level changes in unobserved determinants of child health at the 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's 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, suggesting that changes due to state economic conditions do not influence my results.

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[25] I include child gender in my baseline specification as a control. One concern about this is that the Trivers-Willard hypothesis suggests that in-utero smoke exposure could lead to fewer males surviving to term making this an endogenous control. I checked this by dropping gender from my controls and my baseline results do not change.

[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.

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 smoking 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 is a substantial decline of 7.2% relative to the mean due to increasing cigarette taxes over this 29 year period.

One way to get an estimate for the effect of smoking during pregnancy is to divide the ITT 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 mothers who are swayed to quit smoking due to the policy. If we assume that mothers accurately report smoking during pregnancy, and that there is no effect from second-hand exposure, then this represents the true

[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).

TOT. However, if mothers lie or misreport smoking during pregnancy, then the estimated effect of taxes on smoking will be attenuated, causing the TOT 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. With this in mind, I calculate the TOT as a statistic of interest but with the caveat that it is likely an upper bound.

Using the vital statistics, I estimate a 0.31 percentage point decrease in probability of smoking during pregnancy (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 spuriously significant 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, 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 of the tax hike. The downward decline begins in period -1, reflecting some 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.

It is important to be able 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 cohort who was in its third trimester one quarter before the tax increase. They will be born around event time 0. If the event study shows an impact on these cohorts it reflects 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 later is the tax experienced in the third trimester by a cohort one year earlier. 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. If I am truly capturing an in-utero effect, either a downward pre-trend in child health or having my main coefficient be sensitive to controlling for "leads in age" can be seen as a failure of my empirical strategy.

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. This is shown as the "tax09 coefficient" in Figure 3.[28] The

[28] Additionally, I used the event study sample to estimate a difference-in-differences model. I assigned a dummy variable equal to one for those observations in the post-period of their state's cigarette tax hike. The coefficient on this dummy corresponds to the difference in sick days between the pre-period and post-period of the event study. I label this the "treat" coefficient and it

coefficient 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]

Table 4 presents the results of my preferred specification with the cigarette tax rates faced at later ages added to the model.[30] 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 "cohort time" 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 estimates 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

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has a value of -1.31. A visual check reveals that this magnitude matches the decrease shown in the event study.

[29] However, the standard errors more than triple in size due to losing a large portion of the sample.

[30] The sample size falls slightly when I add in the tax faced at age five. This change is due to limiting the tax variation through 2009. Due to the five year age-lead, children born in cohorts after 2004 would be exposed to tax variation past 2009 and are dropped from the sample. To make sure this is not driving my results, I ran my preferred regression specification on the smaller sample (without including age leads) and my baseline results did not change.

smoking coefficient to get a TOT estimate of 9.5 percentage points or approximately 15% of the mean (although this represents an upper bound).[31]

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.[32]

Table 6 shows the results on doctor visits when including the leads of the excise taxes.[33] 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 subgroups. The prior literature (Markowitz et al., 2011; Decicca and Smith, 2009; see Table 1) shows higher price elasticities for lower-

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[31]I also ran my results using an alternative methods of constructing the doctor visits variable. I use one or more doctor visits as the outcome and get a coefficient of -3.60 which is significant at the 5% level. 83% of the sample had one or more visit in the past 12 months so this is an effect of 4.3% relative to the mean. As expected, this shows a similar but attenuated effect relative to using two or more visits as the outcome.

[32] 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. Many of the children of ages 2-5 are dropped when balancing the event study due to being at the edge of the event study window. Because of the sample change, my regression results on the event study sample do not match the regression results on the full sample, making 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 are not shown here but are available upon request.

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

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, translating 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 days outcome. A similar pattern is followed for doctor visits. Children of mothers who are high school dropouts experience almost an 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 high school educated mothers 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 this fades out completely for college educated mothers, 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. 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 and maternal smoking, I perform my own estimates in order to more closely match up the cohorts and subgroups to the ones in my NHIS analysis. 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 graph 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).

Figure 4 shows such a scatterplot with the coefficients for the change in sick days on the y-axis and the change 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.[34] 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

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[34] The coefficient for Black children is off trend. Perhaps this is because the baseline incidence of low birth weight is higher for Black mothers, leading to larger 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 it showed a similar pattern.

the form of the birth weight impacts and later show up 3–17 years down the line in child health outcomes. 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.[35] That being said, the overall pattern is one of greater smoking elasticities correlated with greater childhood health gains.

Appendix Table A.2 breaks down the coefficients by different time periods. 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 reassuring; before 2000 expenditures on tobacco control programs were extremely small and only in 3 states (Carpenter and Cook, 2008). The fact that my coefficients are negative and significant for most years before 2000, suggests that anti-tobacco expenditures correlated with cigarette tax increases are not driving

[35] I have not run the NHIS regressions on only the states that have maternal smoking available in the vital statistics. Confidentiality issues for using the restricted-use NHIS means it is prohibited to break down state-level regressions into smaller groups of states without explicit permission. 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.

my results.[36] Likewise, 2001-2005 were years in which tobacco taxes were largely done to raise revenue. I get similar results when I run my models in this period as well; though the coefficient on sick days becomes slightly smaller in magnitude and loses significance.

Table A.3 shows results by child age. I stratified on age 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 in the coefficients across age subgroups. An important caveat of my results is that the baseline coefficients represent the average effect on children ages 2-17; however, some ages could be experiencing no effect and others could be experiencing much larger effects.

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.[37] 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 change in the likelihood of having an asthma attack in the past 12 months, an ITT of 15%

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[36] This robustness test follows Carpenter and Cook (2008) who were similarly worried that anti-tobacco program spending was driving their finding that tax hikes decreased teen smoking.

[37] I also ran regressions using a subjective measure of health (1–5 rating) as an outcome. For self-reported health, a score of 1 indicates excellent health and a score of 5 indicates poor health. The coefficient on cigarette taxes was small, positive, and statistically insignificant. However, self-reported health ratings are imperfect outcomes because they are less concretely measured than other health outcomes. Self-reported health may pick up noise because of differences in perceptions between families or differences in language connotations between families. These results are not shown in the paper due to the large number of outcomes reported but are available upon request.

of the mean. A $1 tax hike also causes a -0.40 percentage point change 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 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. Small changes in timing should not have much effect on the tax coefficient. This follows from the underlying identification being difference-in-differences: within a state observations are classified as coming either before or after a tax change. Changing the timing does not change this classification for the vast majority of observations and therefore will not have much effect on 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 that assigning treatment to different trimesters makes little difference to my results.[38] 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.

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[38] 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 much from the main results shown in Tables 3 and 5.

In Column 4 in Table 10, I include the excise tax faced in each trimester in the same regression model. This specification 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 in 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 encouraging.

7.2 Sample Robustness Checks

Appendix table A.4 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 day results and only a slight increase in the magnitude of the doctor visit results. I next drop all children who were missing an exact month or year of birth and could not be precisely assigned treatment. 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 1990s. To the degree that my coefficient estimates might be picking up the effects of Proposition 99, excluding California is one potential robustness check. The sick days coefficient decreases somewhat and loses significance, but it remains negative and is not statistically different from the coefficient from the baseline.[39]

7.4 Threat to Identification: Differential Fertility

One concern is that cigarette taxes may be correlated with state demographic changes. Alternatively smoke exposure may change the composition of births; for example, some demographic subgroups might be more likely to survive to term after a tax hike. To investigate this, I use the vital statistics data to construct outcome variables indicating fertility, gender of births, and percentage of births to different types of mothers within a state in a given year-month. Appendix table A.5. shows these results. There is no effect of the cigarette tax on total log births, the fraction female births, or the fraction of black births. There is an increase in the fraction of teen births. This could possibly be due to a culling effect: teen mothers are smoking less after a tax hike making their births more likely to survive to term. Regardless, the children born to teen mothers are less healthy on average; if teen pregnancies are increasing with the tax this will bias my result downwards.

7.5 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

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[39] 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 since changes in alcohol policy could impact child health and be correlated with changes in tobacco policy. 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 change in the standard errors, and the coefficients remained statistically significant.

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.

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.6. These calculations give only a rough sense of the monetary value of the benefits that I have estimated in my paper. [40] Row 1 of Table A.6 shows the cost related to each outcome I look at. For doctor's visits, I use the average cost of a doctor 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 average yearly expenditures for a child's 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).[41] 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.[42]

In Row 2 of Table A.6., I list the estimated treatment effect from my preferred specifications. I multiply the treatment effect by the cost of the

[40] These calculations are not meant to be a comprehensive cost-benefit analysis and I do not look at the full range of costs or benefits associated with a tax increase.

[41] The median earnings reported by the Social Security Administration came to roughly $26,000 a year in 2009.

[42] Recent research suggests that additional hours and days spent in school improves human capital as measured by test scores (Lavy, 2012; Hansen, 2011; Marcotte and Hemelt, 2008). I make the overly strict assumption that there are no sheepskin effects. However, this estimate is conservative in the sense that it ignores the forgone earnings of parents who take time off from work to care for sick children. Future work could loosen these assumptions to get more accurate estimates of the monetary benefits of a tax increase.

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.[43] 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 together. Instead I use the benefits associated with asthma since it is the smaller of the two. Adding together the benefits of forgone asthma treatment and sick days I get a total value of $1,962 per child.

How should we think about the size of these benefits? 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 dwarf the infant health costs to society of smoke exposure.

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 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

[43] 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.

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).[44] Using my preferred estimates, this increase caused a decrease in sick days of 0.27 of a day. If I assume the mechanism is 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 a TOT of 15% of the mean. For an average-sized cohort of four 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.[45]

This is one of the first studies to look at the childhood health effects of an 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. One novelty of this paper is that it examines medium term outcomes of an in-utero shock. As the cohorts effected by the tax hikes of the 1990s and 2000s mature into adulthood, future research can look at long term effects such as whether in-utero smoke exposure influences labor market outcomes and health in adulthood.

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[44] Derived from Orzechowski and Walker (2011).
[45] Average cohort size derived from the 1989-2004 vital statistics data.

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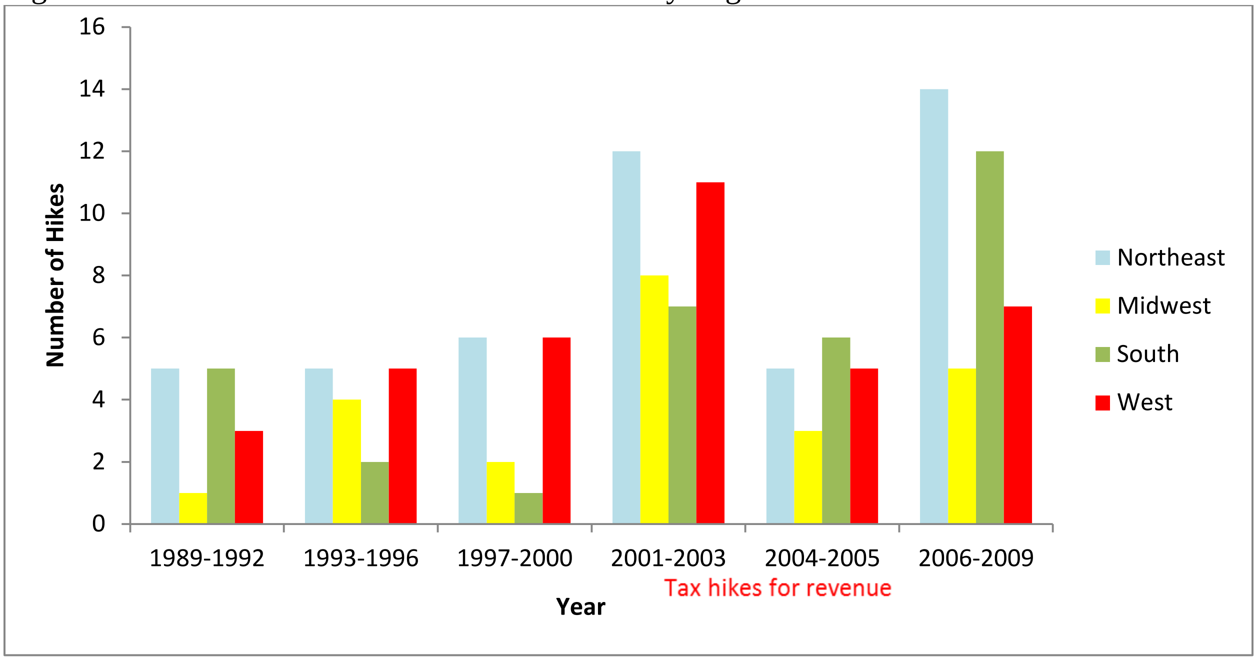
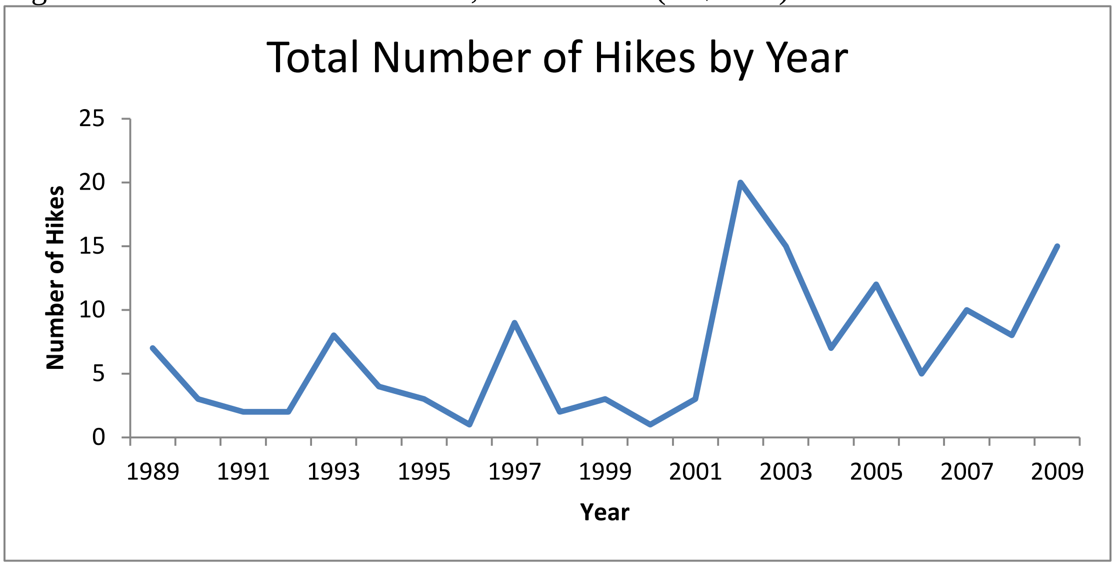
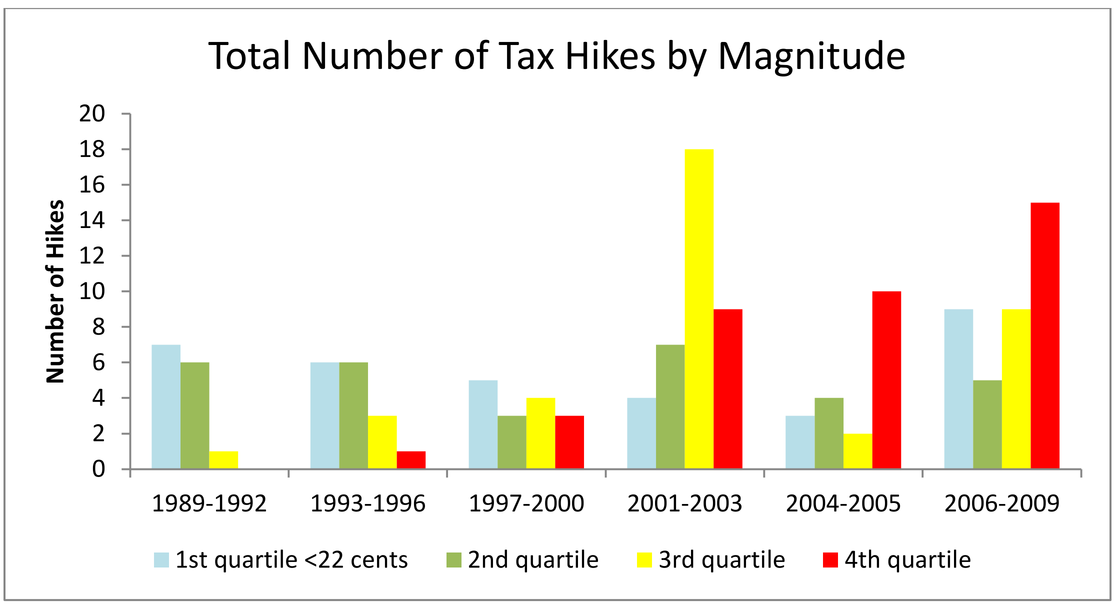
Figure 1: Number of Tax Hikes 10 Cents or More by Region



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. There were approximately 117 hikes that were10 cents or more from 1989 – 2007. Years from 2001-2005 represents a period when tax hikes were used to raise revenue in the wake of the 2001 recession.

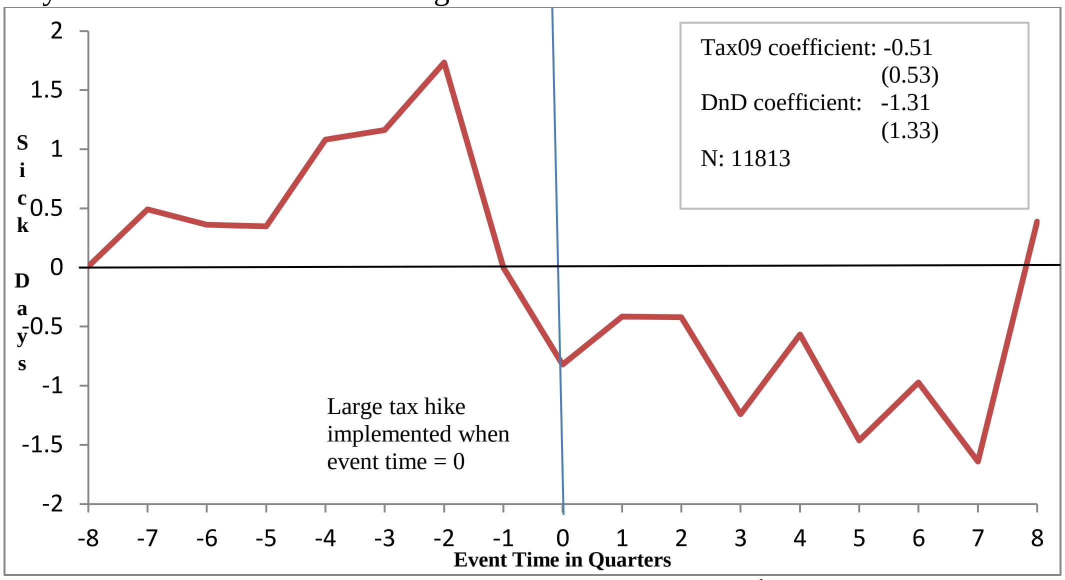
Figure 2: Tax Variation Over Time, 1989 – 2009 (in $2009)





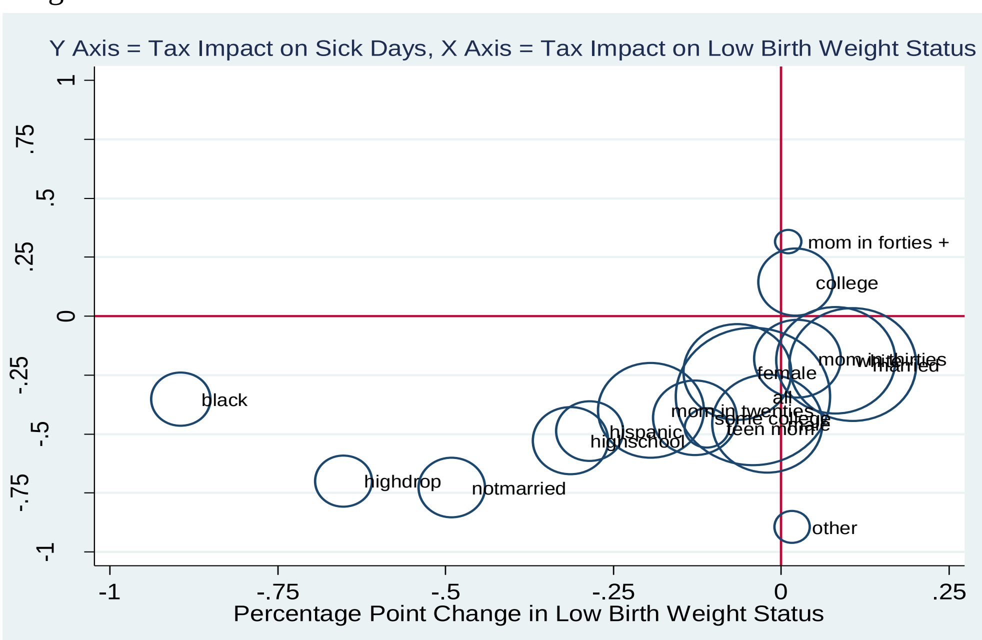
Compiled from excise tax data in "The Tax Burden on Tobacco", (Orzechowski and Walker, 2011). A tax hike is defined as any increase of 10 cents or more. All tax hikes are inflation adjusted to be in $2009.

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



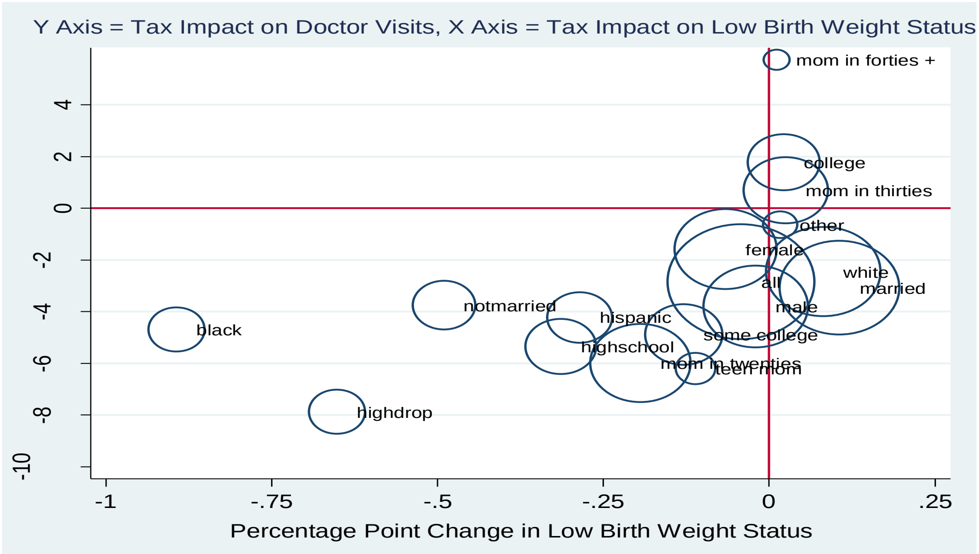
An event is defined as any cigarette tax increase equal or above the 85th percentile of all tax increases. NHIS child weights are used. All models include fixed effects for state, age-in-months, time, controls for race, gender, state policies, state unemployment rate, the Impacteen rating in bars and private work places, and the current cigarette tax.

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



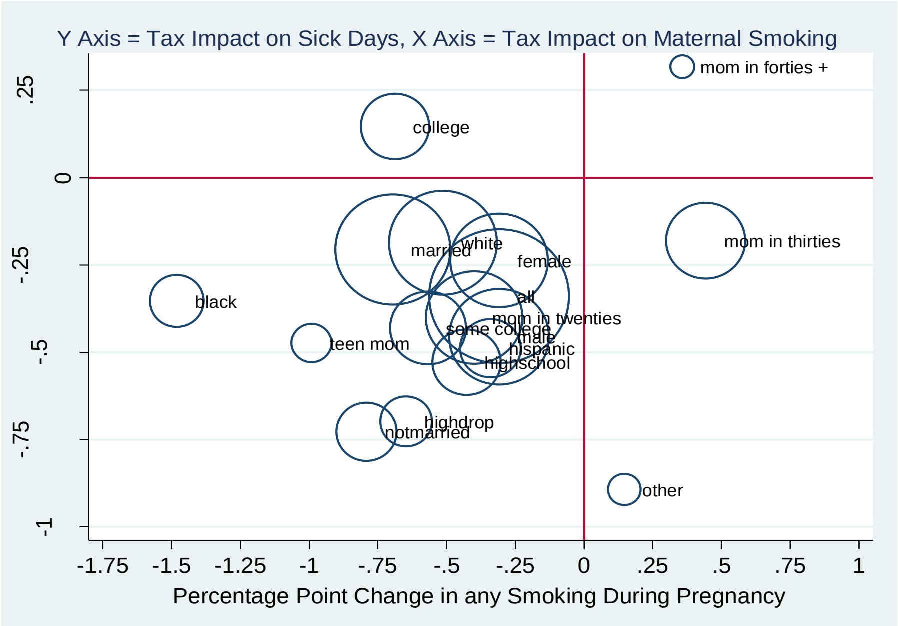
The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis plots the coefficients from a regression of cigarette taxes on number of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

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



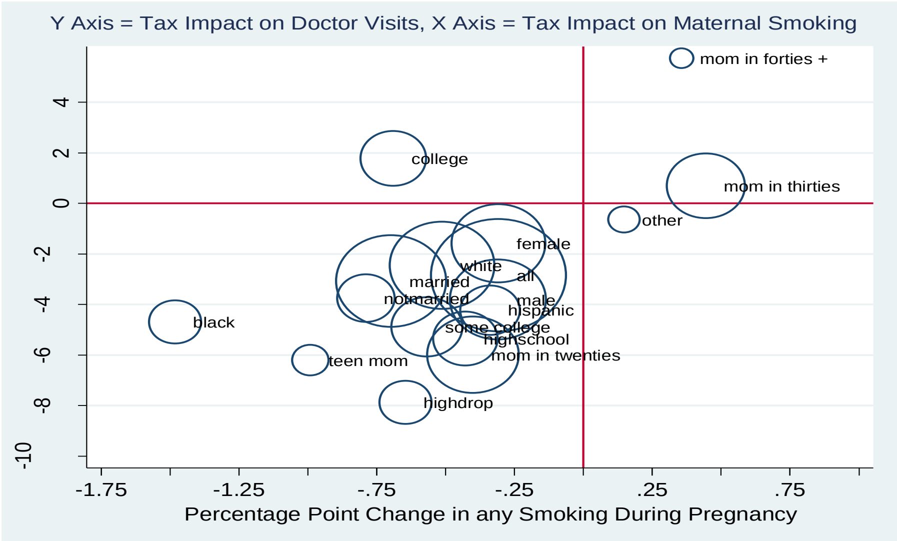
The points on the graph represent estimates for different maternal demographic subgroups. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis plots the coefficients from a regression of cigarette taxes on an indicator for having two or more doctor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

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



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

Figure 7: Subgroup Estimates of Cigarette Tax on Doctor Visits and Maternal Smoking



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

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 (2012)	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 past 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	NHIS Child	107227	20.53 %	2-17	1988-2007
Hospitalized over night	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

Notes: Sample weights are used for calculating all means. The means of dichotomous variables are multiplied by 100. I do not use vital statistics data for years 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 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)
P-values	[5.50]	[4.80]	[5.70]	[5.50]	[6.70]	[3.00]
Average increase in Excise Tax, 1980-2007	$0.80					
Mean sick days	3.43					
N	80063					
State policy controls	no	yes	yes	yes	yes	yes
Unemployment rate	no	no	yes	yes	yes	yes
Indoor smoking 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. P-values as a percent are in brackets below the standard errors. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 5-17. 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), mother's education (dropout, high school, some college, college+), mother's age categories (11-17, 18-25, 26-35, 36-45, 46+), 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 dataset used in this table. My sample includes children ages 5-17. All regressions use NHIS child weights. All models include fixed effects for state, age in months, and time, as well as controls for race, mother's education, mother's age, gender, state level policies, the state unemployment rate, the ImpacTeen indoor smoking law rating in bars and private work places, and the current cigarette tax. Model 1 adds a one year age lead for the cigarette tax. Model 2 includes tax leads through age five.

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
Indoor smoking law rating	no	no	no	yes	yes	yes
Current tax	no	no	no	no	yes	yes
State time trends	no	no	no	no	no	yes

Notes: Linear probability model coefficients are multiplied by 100 for ease of reading. 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 2-17. 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), mother's education (dropout, high school, some college, college+), mother's age categories (11-17, 18-25,26-35,36-45,46+), and gender. 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 2-17. 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, mother's education, mother's age, gender, state level policies, the state unemployment rate, the ImpacTeen indoor smoking law rating for 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. 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 2-17. 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, mother's education, mother's age, gender, state level policies, the state unemployment rate, the ImpacTeen indoor smoking law rating in bars and private work places, and the current cigarette tax. 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 Child's 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 2-17. 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, mother's education, mother's age, gender, state level policies, the state unemployment rate, the ImpacTeen indoor smoking law rating in bars and private work places, and the current cigarette tax. See the text for more details.

Table 9: **The Impact of Cigarette Taxes on Other Child Outcomes**

	(1)	(2)	(3)	(4)	(5)	(6)
	Asthma Attack in 12 Months					
Excise Tax (dollars)	-0.88*	-0.95*	-0.93*	-0.93*	-0.95*	0.43
	(0.48)	(0.48)	(0.49)	(0.52)	(0.54)	(0.60)
P-values	[7.3]	[5.7]	[6.4]	[7.7]	[8.2]	[47.9]
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)
P-values	[6.3]	[7.4]	[5.8]	[14.5]	[12.4]	[83.5]
mean	2.16					
N	236321					
	Emergency Room Visit 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)
P-values	[13.7]	[12.4]	[11.7]	[18.1]	[11.1]	[96.8]
mean	20.53					
N	107227					
State policy controls	no	yes	yes	yes	yes	yes
Unemployment rate	no	no	yes	yes	yes	yes
Indoor smoking 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 and p-values are in brackets. The 1997-2010 NHIS is the main dataset used in this table. My sample includes children ages 2-17. 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, mother's education, and mother's age. * 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 10: Impact of Cigarette Taxes on Sick days and Doctor Visits Using Different Timing Assumptions

Timing Assignment Model:	3rd trimester (base case)	2nd trimester	1st trimester	All trimesters
		Sick Days from School		
Excise Tax Coefficient :				
Tax in 3rd trimester	-0.40**			-0.61**
	(0.20)			(0.26)
Tax in 2nd trimester		-0.33		0.44
		(0.24)		(0.65)
Tax in 1st trimester			-0.34	-0.23
			(0.25)	(0.55)
Mean of dep. variable:	3.43			
N	76843			
	Doctor Visits, The Likelihood of Having Two or More Visits			
Excise Tax Coefficient:				
Tax in 1st trimester	-2.97**			-6.14**
	(0.94)			(2.21)
Tax in 2nd trimester		-2.31**		2.20
		(0.98)		(3.34)
Tax in 3rd trimester			-2.03 **	1.39
			(0.89)	(2.82)
Mean of dep. variable:	61.05 %			
N	103144			

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, mother's education, mother's age, state level policies, the state unemployment rate, the ImpacTeen indoor smoking law rating in bars and private work places, and the current cigarette tax. Sample size changes slightly relative to table 3 since I only include observations to which I can assign the cigarette tax for all three trimesters, which causes some of the latest births to be excluded. The baseline results are unaffected by this change.

Appendices

A Tables

Table A.1: Mean Health Outcomes by Number of Doctor Visits in the Past 12 Months

Outcome	Zero doctor visits	One doctor visit	Two visits	Three or more visits
Reported poor health	1.18	0.71	1.06	2.86
Sick days from school in 12 months	2.00	2.06	3.00	5.96
Asthma attack in 12 months	1.55	2.09	4.88	12.36

Notes: I use the public use national health interview survey from 1997-2010. I include all children ages 2-17. Means of the outcome multiplied by 100 are reported in each column. NHIS Sample child weights are used for calculating all means.

Table A.2: Outcomes by Tax Hike Era

	All years	1988 - 1995	1996 - 2000	2001 - 2005
		Sick Days		
Excise tax (dollars)	-0.34*	-0.47*	-0.76*	-0.23
	(0.18)	(0.26)	(0.39)	(0.37)
mean	3.43	3.56	3.20	3.15
N	80063	53041	20455	6557
		Doctor Visits, The Likelihood of Having Two or More Visits		
Excise tax (dollars)	-2.96**	-5.03*	-0.23	-6.72**
	(0.97)	(2.98)	(3.09)	(2.11)
mean	63.46	58.00	62.65	66.74
N	106401	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 2-17. 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, mother's education, mother's age, 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.3: 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	16848	23790	27816	16880	12715

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, mother's education, mother's age, state level policies, the state unemployment rate, the ImpacTeen indoor smoking 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.4: Sample Robustness Checks

	Original sample	Drop if missing mom	Drop if missing date of birth	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 smoking 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.5: Impact of Cigarette Taxes on Total Fertility and Composition of Births

	Log(births)	Fraction female	Fraction black	Mother age 11-19
Excise Tax (dollars)	-0.06	-0.01	0.57	0.82**
	(0.04)	(0.03)	(0.58)	(0.30)
mean	1.85	48.82	15.35	12.20
N	2078107	2078107	2078107	2078107

Notes: Each column is a separate model with the relevant dependent variable listed in the top row. To test the composition of births I look at the fraction of births to each demographic group. The coefficient in each column is the average yearly cigarette excise tax in 2009 dollars. Standard errors clustered on state are in parentheses. The vital statistics (1988-2004) is the main dataset used in this table. All models include fixed effects for state, age in months, and time, as well as controls for race (Black, White, Hispanic, Other), gender, mother's education, mother's age, state level policies, the state unemployment rate, and the ImpacTeen indoor air law rating in bars and private work places. See text for more details.

Table A.6: Monetized Benefits of a Dollar Tax Hike to Childhood Health

Outcome:	Doctor Visit	Sick Day from School	Treatment of Asthma
Average cost ($2009) of outcome	$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 ($2009) from tax hike	$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 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 Appendix

B.1 Restricted-Use Geocoded National Health Interview Survey

Roughly 6% of my sample is missing information on year or month of birth. I deal with observations 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. This is unlikely to affect my results since cigarette taxes do not change in high frequency within the same state. 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.

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 framework. To address variation in magnitudes across tax hikes, I take all (inflation-adjusted) tax hikes and assign them percentiles (un-weighted by state population). 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), that occurred in 1997 or later. This serves three purposes. First, the large-magnitude hikes are not pooled together into the same event study as the lower-magnitude hikes. Second, after limiting events to being at or above 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 1997–2005 period. Focusing on this later period is ideal for an event study because it leverages a large number of high-magnitude hikes that occur within a short time period of each other.

As discussed in the main text, 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 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. [1] 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 from the event study 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.

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 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. Due to the large number of observations in the vital statistics, this makes running regressions substantially faster and more 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). As an alternative birth outcome I use 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.

[1] 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 they showed no downward pre-trends.

[2] Only one event was excluded from the sick days event study due to having a hike in the pre-period.