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

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May 2015

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 while in-utero 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 exposure to smoking in utero and the first months of life carries significant medium-term costs. My results also suggest that excise tax policy can lead to lasting intergenerational improvements in wellbeing.

JEL Codes: H71, I14.

I am indebted to my dissertation chairs, Hilary Hoynes and Doug Miller. I would like to thank Marianne Page, Colin Cameron, seminar participants at UNC-Greensboro, the UC-Davis brown bag series, and the All-CA labor economics conference for their great feedback on this paper. Joe Roessler provided excellent research assistance. I would like to thank Patricia Barnes and the staff at the Center for Disease Control Research Data Center in Maryland for support in accessing my data. The findings and conclusions in this paper are those of the author and do not necessarily represent the views of the National Center for Health Statistics or the Centers for Disease Control and Prevention. The Russell J. and Dorothy S. Bilinski Fellowship Fund provided me with irreplaceable financial support. Finally, I would like to thank Donald S. Kenkel for providing me with data on state anti-smoking sentiment.

1. Introduction

Does early life exposure to cigarette smoke permanently harm children? I examine the long-term implications of smoke exposure for child health and health care utilization. I leverage cigarette tax hikes to circumvent the endogeneity of maternal smoking. 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 early life 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. Larger elasticities have been uncovered for some subgroups, notably 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; Adams et al., 2012).

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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 will sometimes use the term in-utero to refer either to in-utero or in early life (up to roughly six months after birth).

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.

One caveat with using cigarette taxes is that the tax induced cessation may result in mothers not resuming smoking after pregnancy. In this case the child health effects of a tax could include both reduced smoke exposure during the in-utero period and the accumulated effects of reduced exposure throughout the child's life. A number of tests reveal that even if there is a second hand smoke effect, my results are identified off a relative decline in smoke exposure in the pre-natal and early life period versus exposure in the years after birth. While I cannot fully rule out a second hand smoke effect, the brunt of the evidence is that smoke exposure during the early life/in-utero period is primarily driving my coefficient estimates.

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 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.

In addition, I use an event study to explicitly test the assumption that there are no differential pre-trends between treatment and control states in my outcome variables. In my model pre-trends capture both general trends in child health and the effect of second hand smoke on older children (I discuss why this is the case in more detail in section 5.4). Therefore, these two factors are not separately identified in an event study. However, the event study does show whether there is a discrete impact of smoke exposure during the in-utero period relative to second hand exposure in childhood. An event study can also confirm the identification assumption that the results are not driven by improving trends in child health. 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 early life environment on medium-term childhood outcomes.[2] 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 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 the nicotine and carbon monoxide in

[2] Hoynes et al. (2012) and Nilsson (2008) are other important examples.

cigarettes restricting the flow of blood vessels through the mother's body. Restriction of blood vessels reduces the amount of oxygen and nutrition that reaches the fetus, resulting in decreased birth weight with effects strongest in the third trimester (USDHHS, 2001).

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 decrease in smoking soon after the tax hike, rather than there being a

gradual decline in smoking rates (Lien and Evans, 2005).[3] I extend this literature by testing the hypothesis—for the first time of which I know—that exposure to a cigarette tax hike while in-utero and early life 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.

Studies in Epidemiology and Economics offer insight into the FOH as it applies to smoking during pregnancy. Studies have found correlations with early life cigarette smoke exposure and test scores, labor market outcomes (Currie and

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[3] 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.

Hyson, 1999), schooling (Harkonen et. all, 2012; Restrepo, 2012), asthma, 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, 2012; 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.[4] I analyze tax hikes for

[4] 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.

cohorts born from 1989 to 2008.[5] State cigarette taxes have experienced massive increases over time. In the 2011 fiscal year, state taxes generated more than $17 billion, representing a rise from $4 billion in 1980 and a growth of roughly 333% (Orzechowski and Walker, 2011). Figure 1 shows the variation in the number of states that enacted a tax hike of $0.10 or more (in 2009 dollars) by region over the cohorts in my sample.

A state's legislature is responsible for approving the state budget and passing laws for enacting taxes, including cigarette excise taxes. Though policies and processes can vary across states, typically the state House of Representatives (or larger chamber of the state) has exclusive power to propose tax laws. After a tax increase is passed in the House of Representatives by a majority vote it then goes to the senate, where it must also pass with a majority vote. Then, if signed by the governor, the proposed tax becomes law. Most states have a department of revenue or taxation who is responsible for regulating and enforcing tax law (National Conference of State Legislatures, 2014).

Given the legislative process behind cigarette tax increases: which state legislatures pass tax hikes and why? Traditionally, the primary purpose of state cigarette taxes was to increase state revenue. The price elasticity of smoking is inelastic across most demographics group, making taxing cigarettes a stable source of revenue that can also be implemented at a low administrative cost. Since the 1950s and 1960s knowledge about the adverse health effects of smoking has increased, and in response states have also used taxes to reduce cigarette consumption. Reducing cigarette consumption is politically popular given that even though taxes are inelastic, those who do end up quitting in response to a tax

[5] 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.

have improved health which in turn defrays long term public medical expenditures (Gruber 2001). Because elasticities are highest among teens, public opinion also typically supports taxes as a way of preventing addiction: "polls often find support for cigarette excise increases among American voters, even smokers (Chalolupka and Warner 2000, pg. 1566)." As shown in appendix Table B-1, sometimes cigarette taxes are earmarked for a specific purpose, however most of the time the revenue goes directly into the general state fund.

Naturally, states where the tobacco industry is stronger, or in which the population is more resistant to taxation, are less likely to increase cigarette taxes. This can be seen in Figure 1 which shows that the Midwest and Southern states have had fewer tax hikes. However, Figure 1 also shows that these states were just as likely to raise taxes when faced with revenue short falls during the recession of the early 2000s. Maag and Merriman (2003) document that raising tobacco taxes was a favorite response to revenue short falls at this time even among states that typically had low excise tax levels. Therefore, over the years of my sample almost every state has increased their taxes at least once. Given that state fixed effects absorb any constant state characteristics, the near ubiquitousness of tax hikes across states, and the use of hikes for spending mostly on areas other than health; I believe this suggests that the mechanisms associated with a state's decision to increase taxes do not directly impact child health.

If cigarette tax revenue is spent on health programs such as Medicaid or emergency care services then my results could be driven by increases in the use of health care rather than a reduction in smoke exposure. To see if this is biasing my results I use a database compiled by the American Lung Association that records all earmarks for cigarette tax revenue. I created a table from this database, included below as Appendix Table B-1, summarizing how tax revenue was earmarked in each state with a focus on health related spending. The first column of Table B-1 shows the current amount (in cents) of the cigarette tax in each state. I then break

down the amount spent into several major categories related to health: public insurance, health care and emergency assistance, mental health programs (includes substance abuse clinics), research related to tobacco disease and cancer, and other/general health related spending. Tax laws across states are not always easily comparable and I include in the data appendix notes I made on how I assigned spending when it was not explicitly clear which category an earmark should be assigned to. I also include in Table B-1 a "general spending" category which shows the amount of taxes that either went into the state general fund or were not specifically earmarked for programs related to health or child outcomes; indeed, the majority of the revenue falls into this category. Of the health related spending, it is unlikely that cancer research is directly improving childhood health around the time of the tax increase. Otherwise, 23.6% of the remaining tax revenue goes to either public health insurance, health care, or unspecified health spending. While not all of the 23.6% of cigarette tax revenue spent on health will be used in ways that improve child outcomes, these earmarks could potentially lead me to overstate the health benefits of a tax. To see if this health spending is biasing my results I directly control for state level transfers to individuals using the Regional Economic Information System (REIS) database as a robustness check in section 7.

4. Data

The primary datasets I use are repeated cross sections from the 1997–2010 NHIS. The public-use NHIS does not provide state geographic identifiers, making it necessary for me to access the restricted-use version of the data. The restricted-use, geocoded NHIS includes 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.[6] 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, 2008 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.[7] Ideally, I would have the state of birth for each child, but this is not consistently available due to roughly 8% of the sample not reporting state of birth, so for most of my models I assume that state of interview is the same as state of birth. Making assumptions about gestational age and state of birth add a small amount of noise to the excise tax variable. Measurement error in a right-hand-side variable attenuates its associated coefficient implying true effects that are somewhat larger than what I estimate. I confirm this is the case for state of birth by running my main specifications on the smaller sample matched on state of birth and getting (if anything) slightly larger coefficients.

According to the medical literature, the effect of maternal smoking on birth outcomes is strongest in the third trimester. For my baseline results I attempt to

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

[7] 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.

capture the full magnitude of health effects and therefore assign treatment in the third trimester; I merge in monthly state cigarette excise taxes from Orzechowski and Walker (2010).[8] I use outcomes from both the Person Core and Sample Child questionnaires. Sample sizes along with some basic summary statistics 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 +).[9] The questionnaire considers a doctor visit to be an in-person visit to a health professional excluding dental visits. 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 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 visits is associated with an improvement in access rather than a decline in health.[10]

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[8] Orzechowski and Walker also have data on the average annual cigarette prices (inclusive of tax) by state. There are a number of problems with using price data in this analysis. First, the data on prices are annual instead of monthly. Annual prices makes it impossible to assign timing as precisely as the month of the tax increase. Secondly, since variation in price reflects changes in both 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.

[9] 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 directs interviewers not to count dental visits.

[10] 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.

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. Appendix Table B-2 supports this decision by showing that the likelihood of reporting poor health decreases for those who have had one relative to zero doctor visits. That being said, I also run my models using one or more visits and four or more visits as the outcome and I get similar results.

I merge mother and family demographic information onto 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 roughly 1.21% of my sample and for all of survey year 1997. I include the unmatched observations in my regressions by controlling for a missing mother indicator.

For placebo tests, I use reports on the following conditions: chicken pox, chronic headaches,[11] anemia, an indicator for reporting an injury in the past six months (reduced to 3 months after 2004), and food allergies. Since headaches, anemia, food allergies, and injuries are relatively 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 four, again normalized to have a standard deviation of one.

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[11] 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).

I also use data from the restricted NCHS natality files for years 1989–2008, as compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program. The Vital Statistics natality data is a yearly census of births in the United States collecting 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 the same cohorts in the NHIS sample. For most outcomes this covers children born between 1989 and 2008. When looking at the sick days from school outcome the cohorts in my sample only go through 2005 since children must be five years of age or older to have data on school days. Since 2010 is the last NHIS data year in my sample, 2005 is the last full cohort with data on sick days from school in my sample. 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 Public Use Vital Statistics stops reporting state identifiers after 2004. I applied for access to the restricted use version of the vital statistics data through the National Association of Public Health Statistics and Information Systems (NAPHSIS). Researchers can apply directly to the NAPHSIS for a version of the data with state identifiers. As a final note on the vital statistics, not every state reports data on smoking during pregnancy in every year. This means that including all states in a regression model with year and state fixed effects leads to an unbalanced panel. Unbalanced panels can bias results (Kennedy, 2003). The following states do not report smoking during pregnancy for the majority of the pregnancies at some point during the middle of my sample: California, Florida, Indiana, New York, and South Dakota. To address this, I balance the panel by

dropping these states from the smoking during pregnancy regressions. When using an unbalanced panel I get similar results for the sick day from school cohorts (1989-2005) and similar but slightly smaller coefficients for the cohorts that correspond to doctor visits (1989-2008).

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 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.[12] 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

[12] 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.

month/year of interview fixed effects. For all models, I cluster the standard errors on the state of interview.

My empirical strategy can also be modified to address concerns about second hand smoke and confounding demographic trends. To do this, I add controls for the excise tax faced at later ages as well as the taxes faced before birth. Including the tax faced at later ages is one way to test for a health effect of second hand smoke exposure. However, the coefficients on taxes faced at later ages can also be driven by general health trends across cohorts. To see this, first note that I assign to the tax09 variable the tax level experienced by a child in their third trimester. Now, imagine a tax increase from $1.00 to $1.50 in Michigan in the second quarter of 1991. Children in their third trimester in the second quarter of 1991 are in the post period of the tax hike and are assigned the higher tax level of $1.50. Children who are in their third trimester in the second quarter of 1990 are assigned a tax level of only $1.00 but they will be exposed to the increase when they are one year old. So, the coefficient on the tax at age one picks up both the effects of exposure to the tax on one year olds and any trends in health associated with the Michigan cohort born one year before the tax increase. Similarly, I can add to my model the tax faced at earlier ages: the tax level faced a year before birth, two years before birth, and so on. This is a test of the robustness of my model, assuming a tax on a child who is in their third trimester in 1989 should not have a discrete impact on a child in their third trimester in 1990 (conditional on correctly specifying the 1990 tax in the model).[13]

5.2. Policy controls

[13] I would like to thank the editor and two anonymous referees for helping me clarify this language and suggesting I add lags to my model.

I test the sensitivity of my initial results to additional state-level characteristics and policy controls. This is important because my model is similar to standard difference-in-differences models which are identified off of 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. In this case it is likely that I would see my coefficients change as I add policy-related state-level controls. The earlier literature has identified two main types of policies that are of concern: state level expansions to safety net programs that impact child outcomes, and state demographic or policy changes that affect smoking and smoke exposure.

Currie and Rossin-Slater (2014) give an extensive review of the state policies that target a child's early life environment and impact later life outcomes. Importantly, they note that Medicaid and the State Children's Health Insurance Program (SCHIP) were expanding during the time of my sample. Gruber (2003) shows that state expansions in public insurance were usually done as an increase in the income eligibility thresholds above a percent of the poverty line. I leverage Gruber's earlier work by including these eligibility thresholds as a control for state level Medicaid and SCHIP generosity. Beyond public insurance expansions, some states also implemented welfare reform during this time. Previous work has shown that welfare reform impacted health and child living arrangements (Bitler, Gelbach, and Hoynes, 2005 and 2006). With this in mind, I add to my models a dummy variable that equals one if the state had reformed its welfare system by the given year.

The other type of confounding variation I am concerned about are state level changes that affect smoke exposure other than the cigarette tax. The main such policy changing during my sample were in-door smoking laws. These laws limit the venues in which smoking is allowed and through this may change child smoke exposure. Impacteen, an organization dedicated to research on youth health, tracks

and rates the stringency of state indoor air laws and how those laws have changed over time. Their rating is one of the measures used in the literature (Carpetner and Cook, 2007), and I control for this rating in my main specification. Finally, there is evidence that smoking and infant health change with the business cycle (Ruhm 2004; Dehejia and Lleras-Muny, 2004), because of this I also control for the unemployment rate.

While the above represent the primary state and policy variables I am concerned about, I have done a number of robustness tests to address other potentially confounding state level changes. If states jointly increase "sin" taxes, then alcohol tax changes could be correlated with cigarette tax hikes. This would introduce bias if drinking affects child health. Therefore, I test the robustness of my results to controlling for the beer tax (which typically proxies for state excise taxes on alcohol). The beer tax data was collected from the Brewer's Almanac (available online at http://www.beerinstitute.org/br/beer-statistics/brewers-almanac). Controlling for the beer tax has no effect on my results. Similarly, an index on anti-smoking sentiment was developed by DeCicca et al. 2006. This index was created out of the concern that changes in smoking laws are driven by state level changes in tobacco sentiment. If changes in tobacco sentiment are correlated with childhood health then excluding this control will bias my coefficients. Controlling for the anti-smoking sentiment index does not substantively change my results.

It is worth taking a moment to discuss other policies observed in the literature that one might be concerned about. Federal policies such as WIC and food stamps impact child outcomes, however the state level variation in these policies occurred during their roll out which ended well before the first year of my sample. Otherwise the effects of federal policies such as food stamps, WIC, head start, and public health campaigns will be absorbed by the year-month fixed effects in my models. Policies that have been shown to have limited impacts on children

will not be correlated with the outcomes I look at and therefore will not bias the excise tax coefficient. For example, state family leave policies are changing at this time but are limited in scope (only five states and D.C. have these laws) and evidence suggests that these policies have little to no impact on child health (Currie and Rossin-Slater, 2014). Lastly, at this time universal pre-school programs are starting to be offered by about 40 states. While there is some evidence that universal pre-K may improve cognitive outcomes, there is also evidence that it may hurt child health because children grouped together get exposed to more sickness and disease (Baker et al., 2008). Given Baker's results not controlling for universal pre-K would bias my results downward in magnitude.

While I attempt to be as comprehensive as possible in controlling for confounding variation related to policy and state level changes, it is not always possible to control for all of the specific changes that are happening during this time. To this end, the event studies provide a useful check. Any policy or change that systematically occurs before a cigarette tax hike will show up as a pre-trend in the event studies. The event studies show either no pre-trend or a worsening pre-trend in child health before the early life period. If this worsening pre-trend is due to some unaccounted for policy variation, then not controlling for this policy biases my results towards zero.

5.3. Maternal smoking and subgroup regressions

As part of my empirical strategy 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. (2015).[14]

My first stage results rely on using data from the U.S. Vital Statistics birth certificate data. When working with the vital statistics I collapsed the data down to cells by the year-month of the third trimester, state, education, race, marital status, gender, mother age and birth certificate version (2003 or 1989). I then weight the regressions by the size of the cells. This is done for convenience; the sample size of the birth certificate data is large enough that running regressions at the cell level is substantially faster and easier to work with. Running weighted regressions at the cell level gives me the same results as at the individual level.

I specifically estimate the model below:

$$M_{j,s,c} = \beta_1 T_{s,c} + \beta_2 X_{j,s,c} + \theta_j + \gamma_s + \eta_c + \varepsilon_{j,s,c}$$

Where $M_{j,s,c}$ is the smoking rate of mothers in demographic group j, state s, and cohort c. $T_{s,c}$ is the tax the mother faced in the third trimester of the pregnancy. $X_{j,s,c}$ includes all of the relevant demographic controls that were used in my second stage regressions: mother's education, race dummies, dummies for mother's age and marital status. Because not every state reports smoking during pregnancy in every year of the vital statistics, I balance my results by only including states that consistently report smoking throughout the sample. I also show the robustness of these results to including the same state demographic and policy controls as I do when looking at later life child health outcomes. I cluster the standard errors on state.

5.4. Event Study Methodology

[14] 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.

Event studies are used to test the assumption that there are no differential trends between treatment and control groups. 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. For my initial specification, I define event time in quarters. Specifically, I include one event dummy for each quarter extending up to two years before and after the event. In some specifications I aggregate event time into 6-month bins. The advantage of using 6-month rather than quarterly bins is that more aggregated dummies are more precisely estimated. Using 6-month bins therefore reduces noise and makes the pattern of the coefficients in the event study smoother. The case j=0 indicates that a cohort is in the third trimester when the tax hike occurs. I normalize all event studies such that the Y-axis is equal to 0 in period j=-1 (in other words period -1 is the excluded dummy in the regression).

Unlike with standard difference-in-difference models, only observations that experience a policy intervention are included in the event study. Plotting the coefficients on the event dummies makes explicit how the difference between the treatment and control groups evolves relative to the policy. This helps ensure the validity of the research design.

Since the X-axis of the event study tracks cohorts in their third trimester relative to a tax increase, the pre-trends are a combination of cohort specific trends in child health and the effect of second hand exposure to cigarette smoke. To see this, imagine the case where there is only one tax increase in my sample: for example in Michigan in the second quarter of 1991. Children in their third trimester in the second quarter of 1991 would be assigned event time zero. At event time -1 the X-axis corresponds to children in their third trimester the quarter before the tax increase (the first quarter of 1991). These children will be born in the second

quarter of 1991, making them exposed to the tax around their time of birth. If the event study shows an impact on the cohort in their third trimester in period -1 it could reflect a cohort specific trend in health. Alternatively, an improvement in health for this cohort could be due to the tax reducing second smoke exposure at the time of birth.

The event studies are still helpful for two reasons. First, an event study shows when exposure to the tax matters. Specifically, an event study shows if there is a sudden improvement in child health from in utero exposure to the tax relative to post-natal exposure.[15] Second, if the event study does not show a long term trend of improving health in the years leading up to the hike, then that is evidence against differential trends driving my results.

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. To address this issue I define a discrete tax hike event as any tax hike greater than or equal to 25 cents. This has the advantage of building on the earlier literature: Lien and Evans (2004) showed that tax hikes of 25 cents or higher produced detectible effects on smoking during pregnancy, whereas they did not find detectible effects on a lower hike of 14 cents. After using 25 cents as a cutoff for an event, I then take all tax hikes and assign them percentiles (un-weighted) based on the amount of the hike. I show that the event studies follow the same general pattern when I define an event as a tax hike at or above the 85th, 50th, 25th, or 0th percentile of all hikes.

In a traditional event study there is only one event per unit analyzed. When there is more than one event per state it becomes unclear exactly how the event study should be estimated. To deal with the issue of multiple events per state I run

[15] I would like to thank the editor for helping me clarify this point.

the event study counting only the first tax hike (above the event cutoff) in each state as an event. However, in data appendix C I discuss the robustness of my results to another specification where I include every event in the event study and perform a reweighting scheme. Both methods give similar results.

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., 2011). Without balancing, the event study could show biased trends through picking up the demographic changes from states entering and exiting the event window. In data appendix C, I discuss additional details on the decisions I made in constructing the event study.

6. Results

I look at two of the higher 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 (62%) 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.[16] A one-dollar tax increase causes a decrease of 0.32 sick days from school in the past 12 months.

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

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

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.[17] 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. As discussed above changes in unemployment is associated with changes in health. Also, controlling for 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.

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. The ImpacTeen rating of 1–5 based on the strictness of state's smoke-free indoor air laws is one of the controls for indoor smoking bans used in the literature (see Carpenter and Cook, 2008; Bitler et al., 2009). 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. Column 5 shows that a dollar tax decreases sick days from

[17] For Medicaid and SCHIP income eligibility thresholds, I use the same data as Hoynes and Luttmer (2011) who in turn compiled it from Gruber (2003), the National Governor's Association, Kaiser Family Foundation, and the Center of Budget and Policy Priorities.

school by 0.35 of a day. 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 0.35 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 10% decrease in sick days for a dollar tax hike. Given that between 1980 and 2009 state cigarette taxes increased by $0.80 on average, this suggests a substantial decline of 8.2% relative to the mean.

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 -0.56. However, my event study analysis sheds light on why this is the case.

Figure 2 shows the event study for sick days. The event study reveals a decline in sick days around the time of the tax hike. It is important to be able to interpret the x-axis of the event study in birth cohort time. Referring back to Figure 2, 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. Therefore, moving into earlier periods of the event study potentially shows an effect of smoke

exposure on older children. The downward decline from the pre-period average begins around period -1 or so, reflecting some effect on children around the time of birth or even slightly after.

The event study is encouraging overall. Ideally, an event study shows a flat pre-period. Here, if anything, there is an upward trend beginning roughly a year and a half before the child is born. This is followed by a relatively steep decline in sick days for children exposed to the tax hike between the third trimester and their first months of life. The decline beginning around the prenatal/early life period suggests that children are affected by smoke exposure in early life/in-utero relative to later life exposure (though later life exposure could still have its own impact). An upward pre-trend is consistent with Table 3, in which including state linear trends increases the magnitude of the coefficient on the excise tax.

Overall, the pre-trend in the event study is a combination of cohort specific trends and the effect of second hand exposure. Decreased second hand exposure should improve child health, so this graph suggests a worsening cohort specific pre-trend until exposure to the tax in early life. After that point, there is an improvement in health continuing from the first months of life through the in-utero period. While the event study is encouraging on the whole, the standard errors on the event time coefficients are large. At times in the event study the coefficients jump around which also could be due to noise. Putting event time in six month bins results in a smoother event study graph. I show this in Appendix Figure A-1 which matches Figure 2 but is done with more aggregated 6 month event time dummies. I proceed by following Appendix Figure A-1 and presenting robustness tests that aggregate the event time dummies into six month bins.

In Figure 3, I show the robustness of the event study results to changing the cutoff to only including tax hikes that are at or above different percentiles of all hikes in the sample. Specifically, in the four panels of Figure 4, I draw the cutoffs at the 85th percentile, 50th percentile, 25th percentile, and 0th percentile. Here the 0th

percentile includes all hikes of 10 cents or more. The event studies in Figure 3 follow the same overall pattern as the earlier graphs. Across these range of different cutoffs there is an increase in sick days leading up to the tax hike and a discrete decline in sick days coinciding with exposure between birth and the first several months of life. As would be expected, the effect becomes slightly larger and more distinct when drawing the event cut off at higher percentiles.

Since the sample changes when doing an event study (due to balancing and limiting the sample to only those states that passed a hike of the requisite size), I re-ran my baseline regression model limiting the observations to only those that are also in the event study sample. These regression results are shown in the as the "tax09 coefficient" in the box in Figures 2 and 3. Across the different event studies the "tax09 coefficient" is roughly the same as the coefficient on the excise tax in Table 3. This suggests that the change in the sample is not driving the event study result. It also confirms that my results hold when limiting my control group to only being states that eventually pass a large tax hike.

Table 4 presents results using my preferred specification from Table 3 but adding controls for the tax level faced at different times before or after a child's birth. Column 1 of Table 4 controls for the tax that was implemented one year before the child was born. Column 2 adds values of the tax each year up to five years before the child was born. Of the different coefficients on the five tax levels faced before birth, all are of mixed signs and only one is significant. As expected, this suggests that a tax before the child is born does not have an effect on child health. Similarly, columns 3 and 4 include the tax faced when the child was one to five years old. Accounting for the tax faced at ages one to five 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 as shown in the event studies. The fact that the taxes faced at later ages do not change the coefficient on the in-

utero tax supports my hypothesis that my results are not driven by smoke exposure at later ages. Finally, column 5 includes all of these controls in the same model for a complete set of five year lags and leads. When jointly controlling for the taxes faced five years before and after birth, the coefficients are of mixed signs and almost always substantially smaller than the in-utero coefficient. This test helps confirm that the decrease in sick days in my baseline regression model occurs around the in-utero/early life period and not before or after.

Results for two or more doctor visits are shown in Table 5. My preferred 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 2.8 percentage points. The ITT coefficient represents a 4.5% impact relative to the mean.

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.[18] I also run my results using alternative methods of constructing the doctor visits variable. I use one or more doctor visits as the outcome and get a coefficient of -2.98 percentage points 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 3.5% relative to the mean. As expected, this shows a similar but attenuated effect relative to the mean compared to using two or more visits as the outcome. I get similar effects (though not significant)

[18] I also ran an event study on doctor visits. Unfortunately, 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.

when using four or more visits as the outcome. See Appendix Table B-3 for these results.

Table 6 shows the results on doctor visits when including controls for the tax faced up to five years before or after birth. As with sick days, the coefficient on the tax in the third trimester does not significantly change regardless of how I control for taxes that occur later or earlier. Several of the coefficients on the tax faced at later ages are statistically significant. This could possibly be evidence of an independent effect of second hand smoke. However, because the tax faced at later ages does not change the in-utero tax coefficient, this suggests that second hand smoke exposure is not driving my main results. Table 6 also shows that a tax before the child is born has no impact on doctor visits, which is an important placebo test supporting the validity of my natural experiment.

6.2 First Stage Analysis in Vital Statistics

To look for further evidence of my hypothesis, I estimate how cigarette taxes impact maternal smoking using the U.S. Vital Statistics. The first row of Table 7 shows this first stage for children born between 1989 and 2005. These cohorts correspond to the sick days from school sample in the NHIS. The second row of Table 7 shows results for children born between 1989 and 2008, which matches the doctor visits cohorts. The first column of Table 7 includes the cigarette tax along with state and year-month fixed effects, and reveals a moderately large, negative, and significant impact of cigarette taxes on maternal smoking. Moving from column 1 to column 2 shows the importance of including state specific linear trends in models of smoking during pregnancy. Controlling for linear trends roughly cuts the coefficient on the excise tax in half, though this could be due to either absorbing a pre-trend or through absorbing some of the results. Regardless, even with state trends the coefficient on the tax remains negative and significant.

State linear trends also reduce the standard errors, suggesting that these trends explain maternal smoking in ways that are unrelated to the state excise tax. Because of the importance of state-linear trends, I include these trends in my remaining models.

Columns 3-6 show that after adding state linear trends the coefficient on taxes stays stable to other specifications. Across these columns, I gradually add the same demographic and policy controls that I use in my child health regressions. I also control for a dummy variable for the adoption of the 2003 revised standard birth certificate. The results from my preferred specification (column 6) show that a dollar tax hike decreases smoking during pregnancy by -0.5 to -0.7 percentage points. The reported mean smoking over this full time period is around 14% and average prices were $6.50 in 2001 (Orzechowski and Walker, 2010). From these prices and smoking rates I calculate an elasticity of between -0.23 to -0.33. This fits in the range of elasticities previously found in the literature.

One way to get an estimate for the effect of smoking during pregnancy and doctor visits is to divide my coefficient estimates by the percentage point decrease in maternal smoking. This gives the treatment on the treated (TOT), which measures the effect of a cigarette tax hike on the children of those mothers who quit smoking during pregnancy due to the tax. 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 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. Dividing sick days by the change in smoking of 0.5 to 0.7 percentage points gives a treatment on the treated (TOT) estimate of

roughly 0.4 to 0.6 of a day sick from school or around 15% of the mean. Similarly, for two or more doctor visits dividing by the maternal smoking coefficient gives a TOT estimate between 4.3 to 6 percentage points or approximately 8% of the mean.

To supplement my regression results, I show an event study on smoking during pregnancy in Figure 4. As in Figure 2, here an event is defined as any tax increase of 25 cents or higher. As with sick days, there is an increase in smoking during pregnancy leading up to a tax increase. The subsequent decrease in smoking is strongest for mothers exposed to the tax throughout their entire pregnancy (periods 1 onward). However, effects begin even for those mothers exposed at the end of pregnancy (period 0 or -1). The slight decline beginning around period -1 could also reflect anticipation of the hike. Gruber and Kozeghi (2001) show that pregnant women exhibit forward-looking behavior by decreasing smoking in the months after a tax is announced but before it is implemented. This small decrease is more likely just due to mean reversion after the increase in smoking leading up to the tax. Regardless, the decline is fairly discrete with no evidence of being driven by a long term improvement in health in the years before the tax law was passed by the state congress. In turn, Figure 5 jointly graphs the maternal smoking and sick day event studies. Figure 5 shows that the pattern of the coefficients on these two series are strikingly similar.

6.3 Results by Subgroup

My results can be further investigated by estimating the same models while stratifying on various demographic subgroups. The prior literature (Markowitz et al., 2011; Decicca and Smith, 2009; see Table 1) shows higher price elasticities for lower-educated and younger women. Thus, I examine subgroups using mother's age at a child's time of birth and maternal education at time of interview. Table 8 stratifies my results by mother's education. Unfortunately, for the sick day outcome, dividing the sample by subgroups reduces the sample size and results in

larger standard errors such that none of the coefficients are significant. That being said, the pattern of the coefficients follows what would be expected. This pattern shows that the largest effects are concentrated on 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. However, there is limited power after stratifying the sample on education for the sick days' outcome and these results need to be interpreted with caution given the large standard errors.

A similar pattern in education is followed for doctor visits though in this case there is more statistical power. 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 5.45 percentage points. There are still significant gains for mothers with some college education (-4.2 percentage points), but this fades out completely for college educated mothers, who experience a positive but statistically insignificant increase of 1.4 percentage points.

Table 9 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 9 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 0.46 and a decrease in the probability of having two or more doctor visits of 6.19 percentage points. On the other hand, children of mothers 40 and older experience no decrease in doctor visits or sick days. I take Tables 8 and 9 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.

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 6 shows such a scatterplot. Figure 6 reveals a strong correlation between being in a subgroup that gained birth weight as an infant and having fewer sick days later in life.[19] Figure 7 does the same exercise but for doctor visits: the patterns of results are strikingly similar. I take Figures 6 and 7 together as strong evidence that the gains to child health correspond directly to the early life birth weight effects found in the previous literature. The health impact of tax hikes can first be seen in early life in the form of the birthweight 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 8 and 9. 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 6 and 7. 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

[19] 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.

statistics whereas my sick days and doctor visits coefficients are from the full NHIS sample of all 50 states and DC. Another reason Figures 8 and 9 might be noisier then Figures 6 and 7 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. That being said, the overall pattern is one of greater smoking elasticities correlated with greater childhood health gains.

Appendix Table B-4 shows coefficients from regressions stratified by different time periods. The fact that my coefficients are negative and significant for the years before 1996 suggests that anti-tobacco expenditures, which were extremely small in these years, are not driving my results.[20] For both periods the coefficient on the tax does not substantially change when I add state linear trends. The coefficients on sick days and doctor visits decrease in the later period, and in the case of sick days loses significance. This matches Levy and Meara (2004) who found that in this later period pregnant women were less responsive to cigarette taxes. The coefficients on the in-utero tax in both periods are still fairly large and the coefficient on the tax from the pooled sample falls between the two.

Appendix Table B-5 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. Finally, Appendix Table B-6 shows effects by gender. Across all outcomes the improvements in health from exposure to the cigarette tax are greater for boys

[20] 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.

relative to girls. This fits with male fetuses being more vulnerable to in-utero shocks and therefore having more to gain from the sheltering effects of a cigarette tax.

6.4 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 10 for emergency room visits, overnight hospitalizations in the last 12 months, and having an asthma attack in the last 12 months.[21] For the first specification (Column 1) of Table 10, the coefficients are negative across all outcomes. A $1 tax hike causes a -0.94 percentage point change in the likelihood of having an asthma attack in the past 12 months, an ITT of 16% of the mean. A $1 tax hike also causes a -0.35 percentage point change in having an overnight hospitalization (ITT of 15% of the mean) and a 1.74 percentage point decrease in having an emergency room visit in the past 12 months. Looking across the columns of Table 10 these results are also quite robust. Particularly, asthma attacks and hospitalizations show consistently strong effects on child health in most specifications. However, not all of these results are statistically significant, and their signs become positive (though statistically insignificant) when state linear trends are added.

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[21] 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.

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 11 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.[22] Table 11 shows the largest effects in the third trimester (especially for doctor visits), but none of the estimates are statistically different from each other.

In Column 4 in Table 11, 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

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[22] As with looking at the tax at later ages, 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. For each of the trimesters, I estimate my main model on the smaller sample. The results do not change from the main results shown in Tables 3 and 5. This helps assure me that the change in sample is not driving the results of the robustness test in Table 11.

that the coefficient on the third trimester tax remains negative and significant is encouraging.

7.2 Sample Robustness Checks

Appendix Table B-7 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 run my results merging in the tax based on the state of birth data available in the NHIS. As discussed in my data section, I lose about 8% of my sample when I do this due to observations missing information on state of birth. My results become slightly larger and more precise when matching on state of birth suggesting that if anything, using state of residence attenuates my results due to classical measurement error.

7.4 Threat to Identification: Differential Fertility and other Confounding Mechanisms

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. Table 12 shows these results. There is no effect of the cigarette tax on total log births, the fraction female births, the fraction of black births, or the fraction of births to

married mothers. 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.

A remaining concern is that Appendix Table B-1 shows that some of the revenue from cigarette taxes is used on state health spending and this spending could directly improve child outcomes. However, I can test this using data from the Regional Economic Information System (REIS) database. The REIS data contains a detailed profile of the economic conditions of states. Included in the REIS data is information on transfer payments to individuals from state and local governments. Many of the categories of transfers in this dataset correspond to government spending known to affect child health such as public medical programs and food stamps. The data on state transfers to individuals is available online through the US bureau of economic analysis.[23]

I run models controlling for the categories of transfers that are likely to affect child health outcomes. My first specification matches my preferred specification on sick days and doctor visits; however, I additionally control for total aggregate state spending on health related transfers. In my second and third specifications I replace aggregate health transfers with more specific categories of transfers. In the second specification, I control for dollars of transfers to low income families in the form of Medicaid, SCHIP, and other insurance programs. The second specification also controls for spending on supplemental nutritional benefits such as food stamps, which have been shown to improve child health

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[23] The REIS data is available online at: http://www.bea.gov/regional/downloadzip.cfm. Go to "interactive data" and look under state personal income accounts where there is an option to download tables on current transfer receipts. Alternatively go to "quarterly state personal income" and there is an option to download the data.

(Almond et al 2011, Hoynes et al. 2014). In my third specification I add controls for transfers related to family income assistance, SSI benefits, unemployment benefits, and education and training benefits. I show these results for sick days and doctor visits in Table 13. Looking at Table 13, the coefficient on the excise tax for both sick days and doctor visits does not substantially change regardless of how I control for the different categories of transfers. The robustness of my coefficients to controlling for transfers suggests that state spending on these programs is not driving my results.

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 the NHIS on chicken pox, anemia, chronic headaches, food allergies, injuries and a constructed index of all the low-incidence outcomes (anemia, injuries, headaches, and allergies). Table 14 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 B-8. These calculations give only a rough sense of the monetary value of the benefits that I have estimated in my paper.[24] Row 1 of Table B-8 shows the cost of each outcome. For doctor's visits, I use the average cost of a doctor visit for children ages 5–17 which comes to $606 (Agency for Health Care Research and Quality, 2009). Similarly, for the

[24] 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.

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., 2003); I take 7% of the 2009 median earnings reported by the Social Security Administration, which came to roughly $26,000 (Social Security Administration, 2011). 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.[25]

In Row 2 of Appendix Table B-8, I list the estimated treatment effects from my preferred specifications. I multiply the treatment effect for each outcome by the cost of the associated child health ailment to get the monetary benefit per child per year of a $1 tax hike. In Row 6, I multiply this monetary benefit by the years I estimate treatment effects over (i.e., 13 years for sick days from school and 15 years for the other outcomes), to get the full value of health benefits over the course of childhood.[26] 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

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[25] 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.

[26] 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.

the two. Adding together the benefits of reduced incidences of asthma and sick days from school, 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 a useful tool to ameliorate the harm of this exposure. If pregnant women permanently quit smoking due to a tax increase, then part of my results may be due to reduced smoke exposure throughout life. However, my investigation of the timing of a tax provides at least suggestive evidence that it is exposure in early life that is driving my results. Finally, my results show that the birth weight effects of smoking found in the earlier literature are only the beginning: the health costs of early life exposure 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).[27] Using my preferred estimates, this increase caused a decrease in sick days of 0.28 of a day. If I assume the mechanism is maternal smoking, scaling by the change in maternal smoking represents a TOT of around 0.5 days or 15% 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 2.8 percentage points for the population or a TOT of around 8% of the mean. For an average-sized cohort of four million children and using the health benefit per child from Table B-8, a $0.80 tax increase amounts to a savings of $6.3 billion.[28]

This is one of the first studies to look at the childhood health effects of an early life policy-generated improvement to health. My work demonstrates that policies designed to shield infants from smoke can potentially have large societal returns. These returns can come in the form of lasting improvements in child outcomes that 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 early life smoke exposure influences labor market outcomes and health in adulthood.

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

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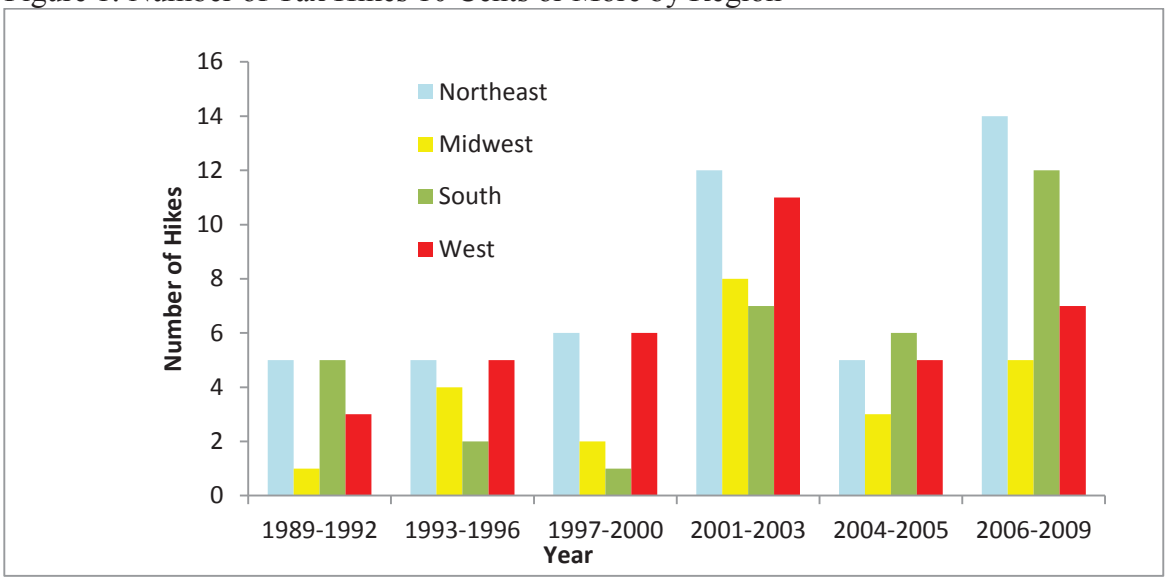
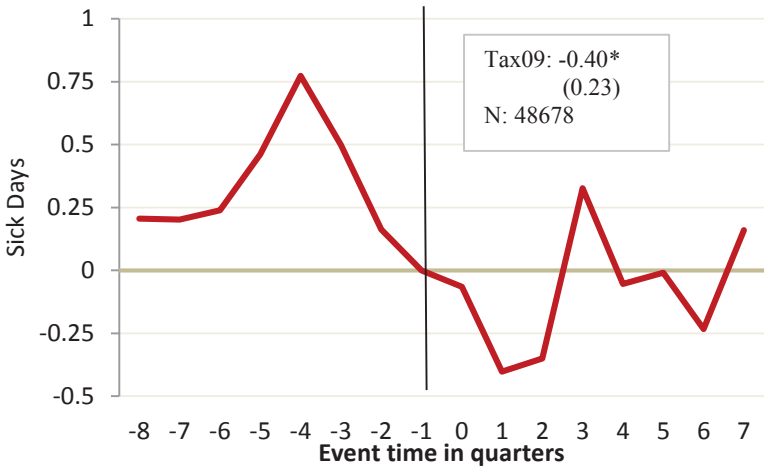
Figure 1: Number of Tax Hikes 10 Cents or More by Region



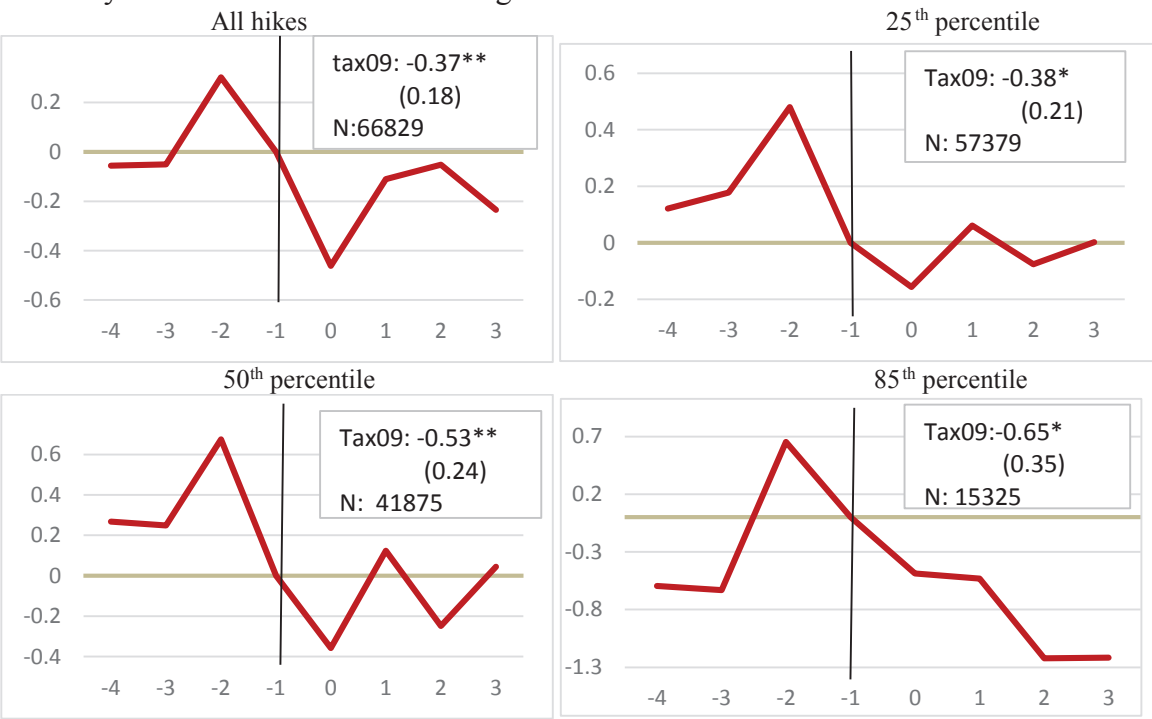
Compiled from excise tax data in "The Tax Burden on Tobacco" (Orzechowski and Walker,2011). All tax hikes are inflation adjusted to be in $2009.

Figure 2: Event Time Estimates of In-Utero Exposure to a 25 Cent or Higher Cigarette Tax Hike on Sick Days from School.



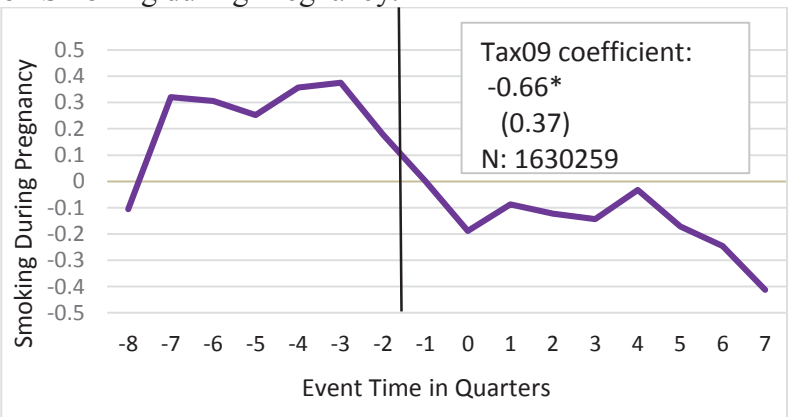
Note: An event is defined as any cigarette tax increase equal or above 25 cents. The sample includes children ages 5-17. NHIS child weights are used. All models include fixed effects for state, age-in-months, time, controls for race, gender, state policies, state unemployment rate, the Impacteen rating in bars and private work places, and the current cigarette tax. The reported Tax09 coefficient is the coefficient on the excise tax from running my regression model on my event study sample. Event time tracks the number of quarters before or after a tax hike during which a cohort is in their third trimester. For example, when event time is -1 this corresponds to a cohort being in their third trimester the quarter before a tax hike. This cohort will in turn be born around the time of the hike. Therefore, if the event study shows an impact on cohorts during event time -1 or -2, this could represent an "early life" effect of exposure to the tax around the time of birth or the first several months of life. Therefore, the pre-trends in the event study capture both state trends in child health before a tax increase and the effect of a change in second hand smoke exposure after birth.

Figure 3. Robustness of Event Time Estimates of In-Utero Exposure to Alternative Cutoffs on Sick Days from School for Children Ages 5 – 17.



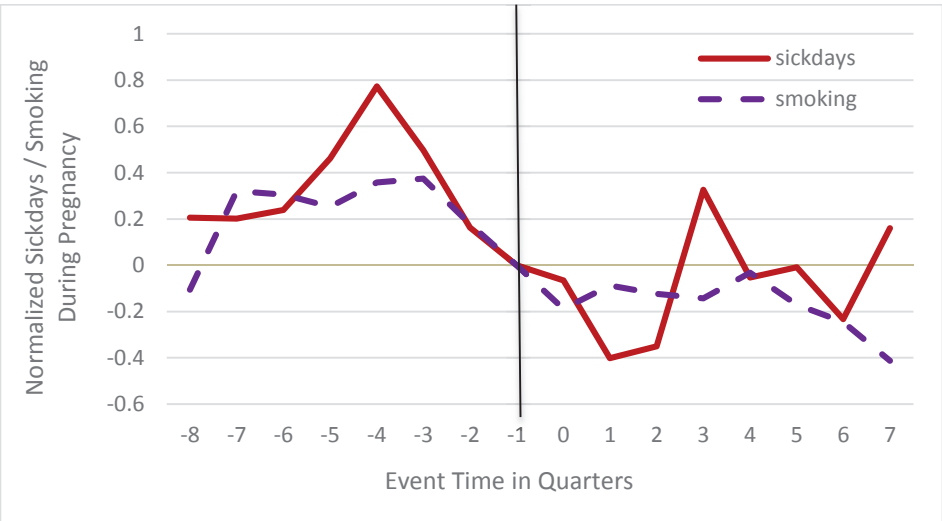
Note: An event is defined as any cigarette tax increase equal or above the percentile indicated in the figure. Event time in six month bins is on the X-axis of each graph. The reported Tax09 coefficient is the coefficient on the excise tax from running my regression model on my event study. Event time tracks the number of 6 month intervals before or after a tax hike during which a cohort is in their third trimester. For example, when event time is -1 this corresponds to a cohort being in their third trimester 6 months before a tax hike. This cohort will in turn be born around the time of the hike.

Figure 4: Event Time Estimates of In-Utero Exposure to a 25 Cent or Higher Cigarette Tax Hike on Smoking during Pregnancy.



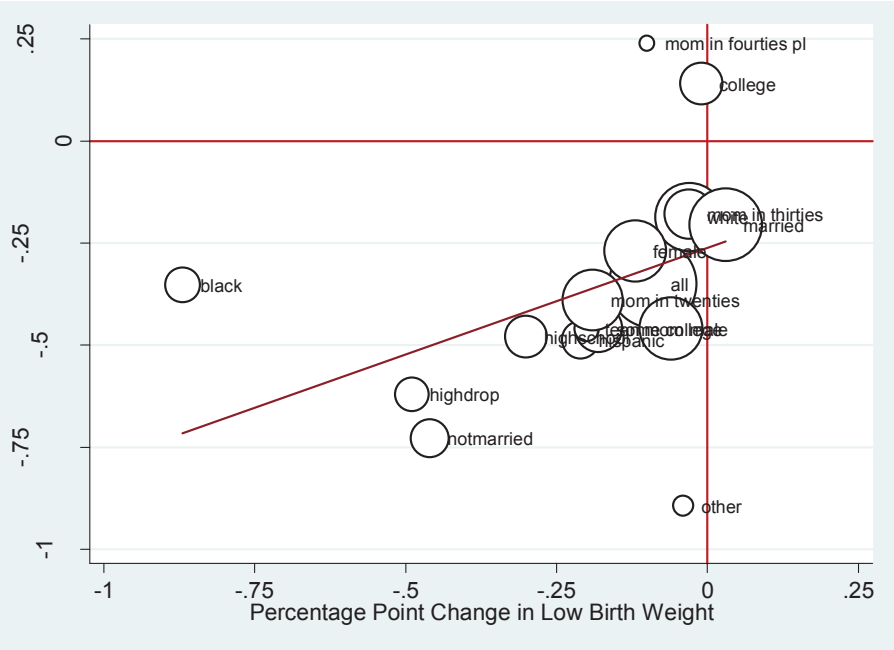
Note: All models include fixed effects for state, time, race, gender, state policies, state unemployment rate, the Impacteen rating in bars and private work places, the current cigarette tax, state-linear trends and an indicator for adopting the 2003 birth certificate. Standard errors are clustered on state. The reported Tax09 coefficient is the coefficient on the excise tax from running my regression model on my event study sample.

Figure 5: Event Time Estimates of In-Utero Exposure to a 25 Cent or Higher Cigarette Tax Hike on Sick Days from School and Smoking during Pregnancy.



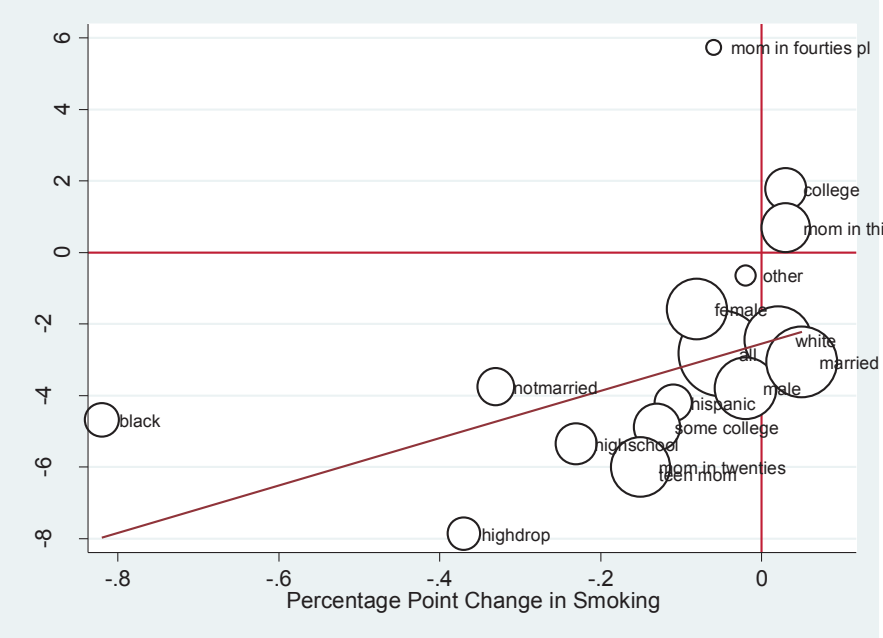
Note: Here an event is defined as any cigarette tax increase equal or above 25 cents. Both series are normalized to equal zero in period -1 (the excluded event dummy). See the text for details.

Figure 6: Subgroup Estimates of Cigarette Taxes on Sick Days and Low Birth Weight Status



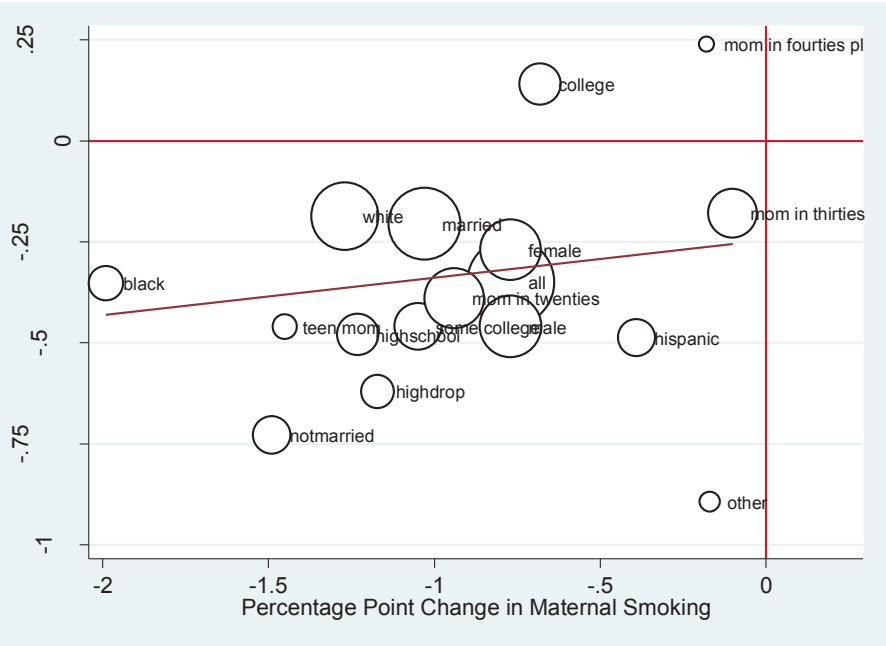
The points on the graph represent estimates for different subgroups based on demographic and maternal characteristics. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis plots the coefficients from a regression of cigarette taxes on number of sick days from school in the last 12 months. The size of the dot represents its relative sample size. Because subgroups are sometimes overlapping the fitted line should not be interpreted as the best linear fit to the population.

Figure 7: Subgroup Estimates of Cigarette Tax on Doctor Visits and Low Birth Weight Status.



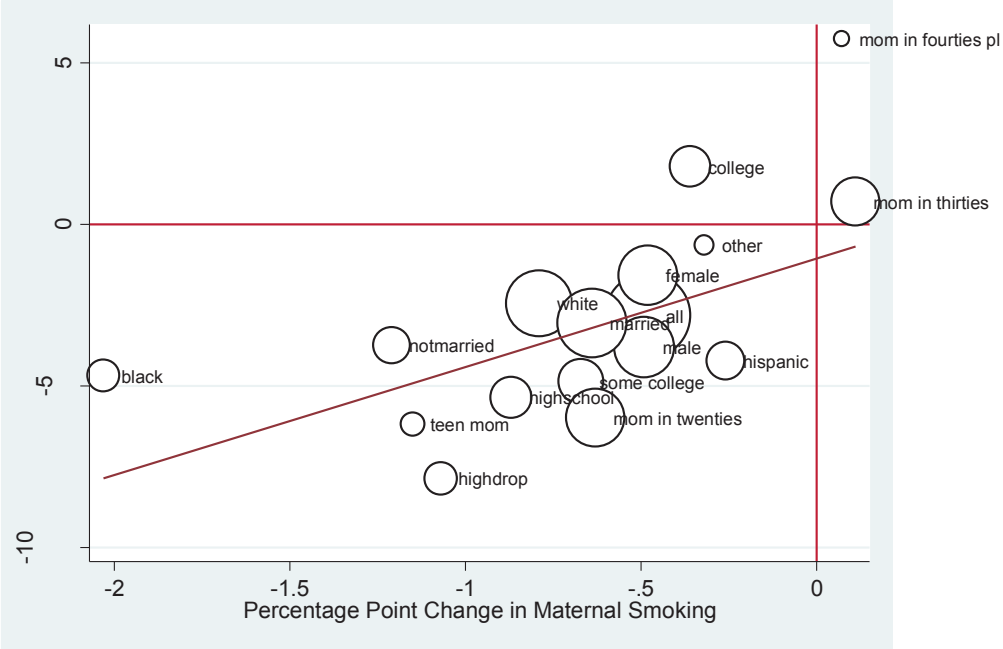
The points on the graph represent estimates for different maternal demographic subgroups. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis plots the coefficients from a regression of cigarette taxes on an indicator for having two or more doctor visits in the last 12 months. Because subgroups are sometimes overlapping the fitted line should not be interpreted as the best linear fit to the population.

Figure 8: 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. Because subgroups are sometimes overlapping the fitted line should not be interpreted as the best linear fit to the population.

Figure 9: 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. Because subgroups are sometimes overlapping the fitted line should not be interpreted as the best linear fit to the population.

Table 1: Smoking Elasticities of Pregnant Women by Study

Study	Cohort Years	Data Set	Demographic Group	Elasticity	% Smoker
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	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 elasticities of engaging in any smoking behavior during the pregnancy. For Lien and Evans (2005); I took the simple average of the four different state elasticities they estimated.

Table 2: Sample Statistics

Outcome Variables	Survey	N	Cohort Years
Sick days from school in past 12 months	NHIS Child	85117	1988-2005
Two or more doctor vsits in 12 months	NHIS Child	118826	1988-2008
Asthma attack in 12 months	NHIS Child	120169	1988-2008
Emergency room visit	NHIS Child	119747	1988-2008
Hospitalized over night	NHIS Person	262599	1989-2008
Any smoking during pregnancy	Vital Statistics	1833409	1989-2008
Low birthweight	Vital Statistics	1986654	1989-2008

NHIS Demographic Variables	mean	NHIS Demographic Variables	mean
Mother Dropout	14.21%	Mother's age	36.14
	(34.91)		(7.44)
Mother Highschool or GED	23.11%	Child's age	8.19
	(42.16)		(4.41)
Some College	27.71%	Black	15.11%
	(44.76)		(35.82)
College Educated	23.01%	Hispanic	19.32%
	(42.09)		(39.48)
Mother Married	66.53%		
	(47.08)		

Notes: Sample weights are used for calculating all means. The means of dichotomous variables are multiplied by 100. 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)	-0.32**	-0.31**	-0.31**	-0.35**	-0.35**	-0.56**
	(0.15)	(0.14)	(0.15)	(0.17)	(0.18)	(0.26)
Average increase in Excise Tax, 1980-2007	$0.80					
Mean sick days	3.43					
N	85117					
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. The 1997-2010 NHIS is the 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.

Table 4: Taxes on Sick days with Controls for the Tax in the Years before and after Birth

	(1)	(2)	(3)	(4)	(5)
Excise Tax (dollars)	-0.40 *	-0.50*	-0.59**	-0.57**	-0.72 **
	(0.20)	(0.29)	(0.24)	(0.24)	(0.33)
Tax: one year before birth	-0.11	-0.07			-0.03
	(0.34)	(0.38)			(0.37)
Tax: two years before birth		0.48			0.59
		(0.36)			(0.35)
Tax: three years before birth		-0.88**			-0.88**
		(0.38)			(0.37)
Tax: four years before birth		0.60			0.55
		(0.38)			(0.39)
Tax: five years before birth		0.26			0.26
		(0.26)			(0.29)
Tax: one year after birth			0.32	0.24	0.16
			(0.20)	(0.26)	(0.26)
Tax: two years after birth				0.08	0.27
				(0.29)	(0.32)
Tax: three years after birth				0.06	0.04
				(0.27)	(0.27)
Tax: four years after birth				0.06	-0.13
				(0.21)	(0.22)
Tax: five years after birth				-0.15	-0.07
				(0.19)	(0.21)
N	78348	46328	85107	84946	46167

Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. My sample includes children ages 5-17. The sample size falls slightly when I add in the taxes faced at later and earlier ages due to limiting the tax variation through 1989-2008. This change in sample does not change my baseline results

Table 5: The Impact of Cigarette Taxes on Doctor Visits

	(1)	(2)	(3)	(4)	(5)	(6)
Excise Tax (dollars)	-2.93***	-2.95***	-2.94***	-3.15***	-2.82***	-2.35**
	(0.88)	(0.82)	(0.83)	(0.81)	(0.84)	(1.03)
Average increase in Excise Tax, 1980 - 2007	$0.80					
Mean of the dep. variable	61.88					
N	118826					
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. Standard errors clustered on state are in parentheses. 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: Taxes on Doctor Visits with Controls for the Tax in the Years before and after Birth.

	(1)	(2)	(3)	(4)	(5)
Excise Tax (dollars)	-3.95**	-3.06**	-2.84 **	-3.58**	-4.36**
	(1.22)	(1.09)	(1.42)	(1.41)	(1.72)
Tax: one year before birth	1.17	0.43			-0.51
	(1.52)	(2.21)			(2.46)
Tax: two years before birth		0.61			1.52
		(2.96)			(3.35)
Tax: three years before birth		0.45			1.36
		(3.03)			(3.75)
Tax: four years before birth		0.93			1.84
		(2.94)			(4.20)
Tax: five years before birth		-0.57			0.27
		(2.29)			(2.71)
Tax: one year after birth			0.28	2.28	3.10
			(2.00)	(2.19)	(2.37)
Tax: two years after birth				-3.37**	-3.24*
				(1.65)	(1.74)
Tax: three years after birth				1.60	1.46
				(1.34)	(1.39)
Tax: four years after birth				-2.14	-2.84*
				(1.48)	(1.44)
Tax: five years after birth				2.18**	2.66**
				(0.99)	(0.88)
N	111996	78959	118826	113509	73642

All regressions use NHIS child weights. The sample size falls slightly when I add in the taxes faced at later and earlier ages due to limiting the tax variation through 1989-2008. This change in sample does not change my baseline results

Table 7: Impact of Taxes on Any Smoking During Pregnancy

	(1)	(2)	(3)	(4)	(5)	(6)
Cohorts 1989-2004 (sickday cohorts)						
Tax 2009	-1.87*	-.66*	-0.65*	-0.68*	-0.68*	-0.70*
	(1.01)	(0.36)	(0.35)	(0.35)	(0.38)	(0.38)
Mean of Dep. Variable	14.01					
Number of Cells	1644017					
Cohorts 1989-2008 (Full Sample)						
Tax 2009	-1.15*	-0.61**	-0.50	-0.52*	-0.52*	-0.50*
	(0.63)	(0.28)	(0.31)	(0.30)	(0.30)	(0.29)
Mean of Dep. Variable	13.66					
Number of Cells	2082485					
State and Time FE	x	x	x	x	x	x
State linear trends		x	x	x	x	x
Demographic Controls			x	x	x	x
Policy Controls				x	x	x
Unemployment rate					x	x
Indoor air laws						x

Notes: Linear probability model coefficients are multiplied by 100 for ease of reading. Standard errors clustered on state are in parentheses. All models include fixed effects for state, time and an indicator for adopting the 2003 birth certificate. Vital Statistics data balanced by dropping states with more than 50% of the data missing in one of the years of the sample due not reporting smoking on the birth certificate. See text for details.

Table 8: The Impact of 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)	-0.62	-0.48	-0.46	0.14
	(0.68)	(0.33)	(0.48)	(0.16)
mean	3.52	3.55	3.72	2.92
N	14096	20050	23545	17385
		Doctor Visits, The Likelihood of Having Two or More Visits		
Excise Tax (dollars)	-8.05**	-5.45***	-4.16***	1.43
	(2.77)	(1.80)	(1.56)	(1.54)
mean	53.48 %	63.74 %	63.73 %	67.13 %
N	20028	27408	32160	24412

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. See the text for more details.

Table 9: The Impact of 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)	-0.46	-0.39*	-0.18	0.24
	(0.56)	(0.24)	(0.22)	(1.15)
mean	3.51	3.414	3.45	3.32
N	7054	37961	27495	3010
		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. 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 10: The Impact of Cigarette Taxes on Other Child Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Asthma Attack in 12 Months					
Excise Tax (dollars)	-0.94*	-1.00**	-0.99*	-0.98*	-1.00*	0.12
	(0.49)	(0.49)	(0.50)	(0.50)	(0.54)	(0.55)
P-values	[6.2]	[5.0]	[5.6]	[6.4]	[6.8]	[82.0]
mean	5.78					
N	120169					
	Overnight Hospitalizations in 12 Months					
Excise Tax (dollars)	-0.35*	-0.33*	-0.33*	-0.25	-0.27	0.13
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.27)
P-values	[5.3]	[6.9]	[6.0]	[15.2]	[14.3]	[64.6]
mean	2.29					
N	23632					
	Emergency Room Visit in 12 Months					
Excise Tax (dollars)	-1.74	-1.75	-1.79	-1.73	-1.97	-0.14
	(1.22)	(1.21)	(1.21)	(1.234)	(1.28)	(1.37)
P-values	[16.1]	[15.42]	[14.3]	[20.4]	[13.0]	[92.2]
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. 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 11: Impact of the Cigarette Tax 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	-0.42**			-0.72***
	(0.19)			(0.26)
Tax in 2nd trimester		-0.332		0.47
		(0.23)		(0.64)
Tax in 1st trimester			-0.33	-0.15
			(0.23)	(0.53)
Mean of dep. variable:	3.43			
N	81547			
		Doctor Visits, The Likelihood of Having Two or More Visits		
Excise Tax Coefficient:				
Tax in 1st trimester	-3.08***			-5.15**
	(0.86)			(2.22)
Tax in 2nd trimester		-2.49***		1.23
		(0.98)		(3.41)
Tax in 3rd trimester			-2.12 **	1.96
			(0.89)	(2.75)
Mean of dep. variable:	62.03 %			
N	115216			

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, born from 1988 to 2008. 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 tables 3 and 5 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.

Table 12: Impact of Taxes on Total Fertility and the Composition of Births

	Log(births)	Fraction female	Fraction black	Mother age 11-19	Married
Excise Tax (dollars)	-0.11	-0.014	0.64	0.81***	0.92
	(0.070)	(0.023)	(0.46)	(0.18)	(0.0068)
mean	1.83	49.00	16.00	12.00	67.00
N	2269504	2269504	2269504	2269504	2269504

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 (1988-2008) 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 13: Robustness of Results to Controlling for State Transfers to Individuals

Table R1-2: Robustness to Controlling for State Transfers to Individuals

	Sickdays			2 or More Doctor Visits		
	(1)	(2)	(3)	(1)	(2)	(3)
3rd Trimester Excise Tax (dollars)	-0.34**	-0.41**	-0.33***	-0.25**	-0.26**	-0.23***
	(0.17)	(0.15)	(0.13)	(0.74)	(0.91)	(0.87)
Medical Benefits	X			X		
Public Medical Assistance		X	X		X	X
Food Stamps		X	X		X	X
Family Income Assistance			X			X
SSI benefits			X			X
Unemployment Benefits			X			X
Education and training			X			X

Notes: Data on state transfers to individuals comes from Regional Economic Information System (REIS). Medical benefits includes all spending on public medical programs, and Vet insurance benefits. Public medical assistance includes all means tested insurance programs in the state such as Medicaid and SCHIP. 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

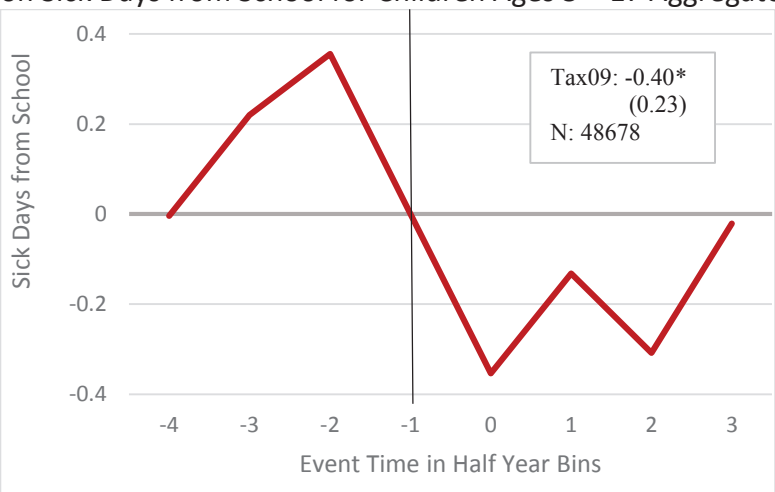
Table 14: Impact of Cigarette Tax on Placebo Outcomes

Dependent variable	Chicken Pox	Chronic Headaches	Anemia	Food Allergy	Injured	Placebo Index
	-0.23	0.30	- 0.07	-0.01	-0.07	0.01
	(1.08)	(0.45)	(0.17)	(0.58)	(0.17)	(0.03)
mean	37.22	5.13	1.13	3.97	2.48	0.03
N	118602	105915	119552	119432	263696	105709

Note: Each column represents a different regression on a different placebo outcome. Standard errors clustered on state are in parentheses. NHIS child weights are used in all models. All models include fixed effects for state, age, and time as well as controls for race, gender, state and tobacco policy variables, and the current cigarette tax. The Placebo Index takes all of the low incidence placebo outcomes (headaches, anemia, allergy, and injuries) and combines them into an index with mean zero and standard deviation 1.

Appendix A. Figures

Appendix Figure A-1: Event Time Estimates of In-Utero Exposure to a 25 Cent Cigarette Tax Hike on Sick Days from School for Children Ages 5 – 17 Aggregated to 6 Month Event Time Bins.



Note: An event is defined as any cigarette tax increase equal or above 25 cents. 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. The reported Tax09 coefficient is the coefficient on the excise tax from running my regression model on my event study. Event time tracks cohorts who were in their third trimester when exposed to an event. Cohorts who were in period -1 were in their third trimester before the tax hike and are born around the time of the tax hike.

B. Appendix Tables

Table B-1: Cigarette Tax Revenue and Health Earmarks, Total Tax and Spending in Cents (2014 Dollars).

State	tax in cents	General (non health services)	Public insurance	Health care / health services	Mental health	Unspecified/other health	tobacco and cancer research	% Spent on Health Excluding Research
Alabama	42.5	13.3	26		3.2			68.7%
Alaska	200	200						0.0%
Arizona	200	108	69	16			7	42.5%
Arkansas	115	115						0.0%
Colorado	84	84						0.0%
Connecticut	340	340						0.0%
Florida	134	23.5		110			0.5	82.1%
Georgia	37	37						0.0%
Hawaii	320	240				80		25.0%
Idaho	57	52.3					4.7	0.0%
Illinois	98	98						0.0%
Indiana	99.5	69	3		0.5	27		30.7%
Iowa	136					136		100.0%
Kansas	79	79						0.0%
Kentucky	60	58					2	0.0%
Louisiana	36	27			2		7	5.6%
Maine	200	200						0.0%
Massachusett	351	301				50		14.2%
Michigan	200	124	65			11		38.0%
Mississippi	68	68						0.0%
Montana	170	95.2	74.8					44.0%
Nebraska	64	64						0.0%
New Hampsh	178	178						0.0%
New Jersey	270			268.4			1.6	99.4%
New Mexico	166	148					18	0.0%
New York	435	435						0.0%
North Dakota	44	44						0.0%
Ohio	125	125						0.0%
Oklahoma	103	50.5	39	6	2		5.5	45.6%
Oregon	131	97		34				26.0%
Rhode Island	350	350						0.0%
South Dakota	153	136		17				11.1%
Tennessee	62	60		2				3.2%
Texas	141	141						0.0%
Vermont	275					275		100.0%
Virginia	30			30				100.0%
Washington	44	44						0.0%
Wyoming	60	60						0.0%
%	100%	75%	5%	9%	0%	10%	1%	23.8%

Note: Notes on the construction of this table are in the data appendix. Data compiled from the American Lung Association on 12/18/2014: http://www.lungusa2.org/slati/states.php. There are only 38 states in this table due to 6 states not having passed a cigarette tax increase during the years of my sample and 6 states not having information on them in the American Lung Association database.

Table B-2: Mean Health Outcomes by Number of Doctor Visits in the Past 12 Months

Outcome	Zero doctor visits	One doctor visit	Two to three visits	Four 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 to 2010. I include all children ages from 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 B-3: Impact of the Cigarette Tax on Doctor Visits across Alternative Cutoffs

Dependent variable	One or more doctor visits	Two or more doctor visits	Four or more visits
	-2.98**	-2.83***	-1.18
	(0.96)	(0.92)	(1.11)
Mean doctorvisits	83.59	63.46	28.58
Effect as % of mean	3.56%	4.46%	4.13%
N	118817		

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 dataset used in this table. My sample includes children ages 2-17. 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.

Table B-4: Results across Different Periods of the Tax Increase

	1989-1995	1989-1995	1996-2008	1996-2008
		Sick Days		
Excise tax (dollars)	-0.92**	-1.05***	-0.38	-0.57
	(0.36)	(0.38)	(0.31)	(0.36)
mean	3.56	3.56	3.23	3.23
N	53354	53354	31763	31763
	Doctor Visits, The Likelihood of Having Two or More Visits			
Excise tax (dollars)	-7.66**	-6.37*	-2.21**	-2.38
	(3.25)	(3.51)	(1.04)	(1.54)
mean	57.69	57.69	65.54	65.54
N	58090	58090	60750	60750
State linear time trends	no	yes	no	yes

Notes: Excise tax is in 2009 dollars. Standard errors clustered on state are in parentheses. 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, and the ImpacTeen indoor smoking law rating in bars and private work places

Table B-5: 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: My sample includes children ages 2-17, born from 1988 to 2008. All regressions are. There are no sick day outcomes for children ages 2-4 because these children are too young to have entered school.

Table B-6: Impact of the Cigarette Tax by Gender

Dependent variable	Sick Days	2+ Doctor Visis	Asthma Attack	Hospitalization	Emergency Room Visit
Male	-0.461**	-4.171***	-2.013***	-0.482*	-2.393
	(0.212)	(1.237)	(0.757)	(0.265)	(1.813)
mean	3.381	61.206	6.839	2.484	21.725
N	43748	60969	61653	134623	61426
Female	-0.268	-1.117	0.071	-0.086	-1.583
	(0.223)	(1.082)	(0.558)	(0.254)	(1.198)
mean	3.472	62.584	4.673	2.103	19.036
N	41369	57871	58516	127976	58321

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. 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.

Table B-7: Sample Robustness Checks

	Original sample	Drop if missing mom	Drop if missing date of birth	Match Tax on State of Birth
		Sick Days		
Excise tax (dollars)	-0.35**	-0.34*	-0.33*	-0.41***
	(0.15)	(0.18)	(0.19)	(0.15)
N	85117	70859	74276	78835
		Doctor Visits		
Excise tax (dollars)	-2.82***	-3.222**	-2.50**	-2.52***
	(0.84)	(0.94)	(1.11)	(0.72)
N	118826	93432	99176	111256

Notes: 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 matches the excise tax on the state of birth for those observations for which it is available in the data. See the text for more details.

Table B-8: 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	

Notes: 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.

C Data Appendix

C.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 models. 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 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.

C.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 define my discrete tax hike event as any take hike greater than or equal to 25 cents. I then take all tax hikes and assign them percentiles (un-weighted by state population). I show that I get approximately similar figures when looking at the 85th, 50th, 25th, percentile or 0th percentile (all hikes).

I use two modifications of the event study techniques for addressing the fact that at lower the cutoffs there are two (or even three and four) events per state. My main technique for addressing the multiple events per state, and the one I present in my paper, is to just run the event study counting only the first tax hike in each state as the event. This has the advantage of being a simple and transparent way of choosing an event. However, one drawback to this approach is that using the first hike as opposed to later ones is a relatively arbitrary choice. My second approach is to include every event in the event study and perform a reweighting scheme to account for multiple events per state. When there are multiple large hikes within a state I duplicate the observations and assign each set of observations a different event. I then down-weight the observations by the number of events per state. For example, if there were three tax hikes large enough to be considered events in

Michigan, I would duplicate the observations in Michigan three times, assign each a different event, and then down weight each of the sets of observations by 1/3rd. The down weighting insures that none of the original observations has a weight of more than 1. This is more complicated than the first method, but is also richer and allows for the incorporation of multiple events per state into the event study.

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. 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. Only one event was excluded from the sick days' event study due to having a hike in the pre-period. Because my event study sample changes from my main regression model, I re-estimate the preferred regression specifications on only the event study sample.

C.3 Notes on Tax Revenue Earmarks

To check how wide spread taxes earmarked for health spending were, I constructed a table showing how much money from cigarette taxes in each state were earmarked for health related spending. These results were shown in Appendix Table B-1. It is important to note that the laws earmarking tax revenue can be complicated and are not always easily compared across states. Here I include my notes on how I assigned the tax earmark when it was not clear which category of spending an earmark should be assigned to. I also include notes on whether or not a spending earmark for health occurred outside the period of my sample (and was therefore not relevant and not included in the table).

Alabama — For 26 cents of the tax, $2 million goes to local governments and the remainder is earmarked for spending on Medicaid. Using year 2013 state cigarette tax revenue, a back of the envelope calculation suggests that Medicaid spending is 24 cents of the tax.

Colorado — During times of state fiscal emergencies some of the cigarette tax money has been dedicated to health program spending.

Hawaii — 80 cents of the tax goes to health spending some of which is cancer research, the division between cancer research and other health spending is not clear, so I classified all of this as "general" health spending.

Illinois	In 2012 additional money from the tax was earmarked to healthcare spending. There are no cohorts born in 2012 in my sample, so this does not apply to my study.
Indiana	Money earmarked to the Indiana checkup plan trust fund goes to providing health care services.
Kentucky	For Kentucky It was not clear exactly how much of the tax is earmarked for cancer research but it was reported as being a "small amount." I ended up assigning 2 cents of the tax to cancer research.
Massachusetts	In 2013, legislation was passed such that an unspecified portion of the tax goes to support the Mass. health insurance system; however, this earmark began outside of the years my sample so I did not count an additional portion as going towards public health insurance.
Michigan	The state legislature has power to override any earmarks and does so regularly. For example, additionally, tax revenue was sent by the state to the "Healthy Michigan fund" which was largely not used on public insurance but cigarette cessation funding.
New Jersey	The first 1 million deposited from the tax goes to cancer research (I treated this as 0.6%), the next 150 million goes to health care (99.4%).
New York	Before July 2010, cigarette taxes were not earmarked for spending related to public insurance or health services (http://codes.lp.findlaw.com/nycode/TAX/8/171-a). Since none of the cohorts in my sample were born after 2008, I do not count the New York tax as being earmarked for health spending in these areas.
Oklahoma	Some of the Oklahoma tax is earmarked to the health employee and economy improvement fund which includes Medicaid/SCHIP, so I counted this as Medicaid spending in my table.
South Dakota	The formula for distributing the tax is complicated. In 2013, 11% of the tax revenue went to health services. As a percent of the tax this is 16.83 cent