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

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*Evidence suggests that excise taxes on tobacco improve fetal health. However, it remains unknown if smoke exposure in early life causes lasting harm to children. I find that in utero exposure to a dollar increase in the state cigarette tax causes a 10 percent decrease in sick days from school and a 4.7 percent decrease in having two or more doctor visits. I present additional evidence for decreases in hospitalizations and asthma. This supports the hypothesis that exposure to cigarette smoke in utero and infancy carries significant medium-term costs, and that excise taxes can lead to lasting intergenerational improvements in well-being.* (*JEL* H25, H71, I12, J13)

Does early life exposure to cigarette smoke permanently harm children? This question is relevant to several related literatures in economics. While smoking during pregnancy is known to damage infant health, there have been few attempts to causally link early life smoke exposure with longer term outcomes. A growing literature shows that early life environment predicts success in adulthood. Still, little is known about how health capital at birth influences childhood—an important period in the lifecycle for human capital accumulation. Furthermore, smoking is a behavior associated with low socioeconomic status families. For those interested in the intergenerational transmission of inequality, cigarette smoke could be one channel by which health and human capital are passed from parent to child. Finally, this research question has implications for tobacco policy and smoking cessation programs. The potential for long-term improvements in child health should be considered in cost-benefit analyses of these policies.

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I examine the long-term implications of smoke exposure in utero and through the first few months of life on child health and health care utilization.[1] I leverage cigarette tax hikes to circumvent the endogeneity of maternal smoking and secondhand exposure. Use of cigarette taxes sheds light on the viability of tobacco policy for improving health, decreasing health care costs, and stemming the intergenerational transmission of low socioeconomic status. I make use of the restricted-use geocoded National Health Interview Survey (NHIS) from 1997 to 2010. Access to these data allows me to examine medium-term childhood health outcomes not commonly used in the economics literature. Specifically, the NHIS contains information of sick days from school in the last 12 months and having had an asthma attack in the last 12 months. For health care utilization, I look at an indicator for having 2 or more doctor visits in the last 12 months, as well as emergency room visits and hospitalizations.

A study of the childhood health effects of cigarette taxes is particularly timely given the large number of state excise tax hikes in the past 20 years. Between 1980 and 2009, state taxes on cigarettes have increased by approximately \$0.80 on average. 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 women. I update the findings of the previous literature by directly estimating the impact of taxes on smoking during pregnancy. Many of the earlier papers on this topic, reflecting the norms during the period in which they were published, use heteroskedastic robust standard errors. I show that the relationship between taxes and smoking during pregnancy becomes weaker relative to the earlier literature when clustering on state. However, after fully exploiting the rich nature of the available data, the impact of taxes on smoking during pregnancy is negative, moderately sized, and highly significant for the birth cohorts in my study.

My empirical strategy involves regressing various child well-being 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. The tax induced cessation may result in mothers not resuming smoking after pregnancy such that the child health effects of a tax could include the accumulated impact of reduced exposure throughout the child's life. However, a number of tests reveal my coefficient estimates are identified off a decline in smoke exposure in the prenatal period and through the first several months of life, relative

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

to exposure in later years. Specifically, I use an event study to explicitly show that there is a discrete, contemporaneous impact of smoke exposure in early life relative to secondhand 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 positive, policy generated intervention that improves early life environment on intermediate-term childhood outcomes.[2] It also represents one of the only extensions of the literature on the infant health effects of cigarette taxes to child health outcomes. My findings suggest that sheltering children from smoke exposure in utero, relative to exposure in the years after birth, 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 (Currie and Almond 2011).

## I. Expected Effects

What maladies should result from in utero smoke exposure? The most robust result in the medical literature is that smoking during pregnancy decreases birth weight. This is due to the nicotine and carbon monoxide in cigarettes restricting the flow of blood vessels through the mother's body. Restriction of blood vessels reduces the oxygen and nutrition that reaches the fetus, resulting in birth weight effects that are strongest in the third trimester (United States Department of Health and Human Services (USDHHS) 2001).

Beyond birth outcomes, the medical literature provides evidence that harm from smoke exposure is widespread and lasting. Nicotine binds to neural receptors in the developing fetus, potentially leading to brain damage (Shea and Steiner 2008). Nicotine also hinders the movement of the embryo, which could retard the development of the child's nervous system (USDHHS 2011). There are more than 100 other harmful chemicals in cigarettes, which are believed to potentially 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. Similarly, postnatal exposure in the months after birth is associated with later life asthma, the need for emergency care, and respiratory difficulties (Sabia 2008).

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 1 dollar tax increase resulted in a 32 percent reduction in smoking during pregnancy and a 5 percent reduction in low birth weight births. A number of additional studies support Evans and Ringel's initial finding (Gruber and Zinman 2000; Ringel and Evans 2001; Markowitz et al. 2011; and DeCicca and Smith 2012). Additionally, later studies have shown that a

[2] Hoynes, Schanzenbach, and Almond (2016) and Nilsson (forthcoming) are other important examples.

tax hike causes a decrease in smoking that occurs contemporaneously with the hike, rather than resulting in a gradual decline in smoking rates in the years after the tax increase (Lien and Evans 2005).

Should I expect the biological impacts discussed above to surface in the childhood outcomes available in survey data? Skeptics could argue that the influence 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 also 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 early life 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 later in life. Currie and Almond (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), state food stamp introduction (Hoynes, Schanzenbach, and Almond 2012), as well as many others.

Studies in epidemiology and economics offer insight into the long term effects of the FOH in the context of smoking during pregnancy. These studies have found correlations between early life cigarette smoke exposure and test scores, labor market outcomes (Currie and Hyson 1999), schooling (Härkönen et al. 2012; Restrepo 2012), asthma, stunting, childhood obesity, and ratings of overall child health (Stick 1996, Lassen and Oei 1998). However, many of these papers do not use policy variation to help correct 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 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 could result in an upwards bias to the estimates of the studies cited above. My paper complements the existing epidemiology literature by offering a causal test of the lasting childhood health effects of early life smoke exposure.

In the strictest sense the FOH refers only to shocks in the in utero period. The treatment effects I discover likely capture the impact of smoke exposure both in utero and during the first months of life. However, after investigating different mechanisms, I show evidence that smoking during pregnancy is the primary behavior driving my results and in that sense this work speaks to the FOH literature. With that in mind, most economic papers on the FOH focus on health shocks identified through natural disasters. Tax hikes are arguably more relevant than using a natural disaster because the estimated treatment could be implemented again in a policy setting. Most FOH studies focus on negative events, whereas this paper quantifies the impact of a positive shock. Further, most FOH studies skip over childhood and only look at adult outcomes. However, health in the medium term could be an important portion of total lifetime welfare.

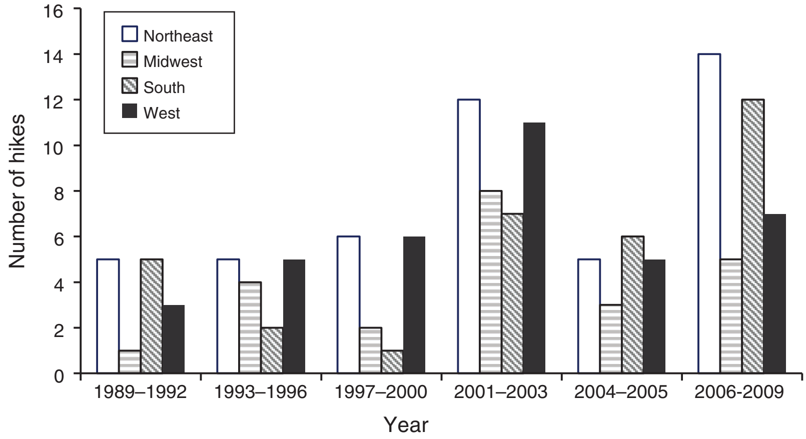


Figure 1. Number of Tax Hikes 10 Cents or More by Region

*Notes:* Compiled from excise tax data in Orzechowski and Walker (2011). All tax hikes are inflation adjusted to be in 2009 dollars.

## II. 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.[3] I analyze tax hikes for cohorts born from 1988 to 2009. State cigarette taxes have experienced massive increases over time. In the 2011 fiscal year, state taxes generated more than $17 billion in revenue, representing a rise from $4 billion in 1980 and a growth of roughly 333 percent (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.[4]

If cigarette tax revenue is spent on health programs such as Medicaid or public emergency services, then my results could be driven by increases in the use of health care rather than a reduction in smoke exposure. Online Appendix Table A-1 shows for each state how much revenue from cigarette taxes are earmarked for different categories of public health spending. This table reveals that the majority of the revenue raised from cigarette taxes goes into either the state's general fund or is earmarked for non-health-care-related spending. After excluding cancer research, which is unlikely to improve health immediately following a tax increase, only 23.8 percent of state tax revenue is allocated for health spending. While not all of this revenue will be used in ways that improve child health, these health related earmarks could potentially lead me to overstate the benefits from reduced cigarette smoke exposure. To address this, I directly control for state level transfer payments to individuals using the Regional Economic Information System (REIS) database as a robustness check in Section VII.

[3] It is difficult to separately identify federal tax changes from national trends in smoking and child health. Municipal taxes are less common and there is no comprehensive dataset documenting them.

[4] 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.

To access the exogeneity of state cigarette taxes it is important to understand who decides to increase these taxes and why. State tax laws are passed after they receive a majority vote in the state congress and are signed by the governor. Traditionally, legislatures passed tobacco taxes in order to increase state revenue; however, as the knowledge of the adverse health effects of smoking have spread, states also have used taxes to reduce cigarette consumption (Gruber 2001). States where the tobacco industry is stronger, or in which the population is more resistant to taxation, are less likely to increase their cigarette taxes. That being said, Figure 1 shows that the Midwest and Southern states, while slow to adopt taxes before 1997, were just as likely to raise taxes when faced with revenue short falls during the recession of the early 2000s. It can take several years for a tax law to go into effect after the initiation of a campaign for a tobacco tax increase (Gruber and Kozeghi 2001). This delay means that at the time the tax is enacted the tax is unlikely to be correlated with short-term changes in antismoking sentiment and therefore is also likely to be independent of immediate changes in health caused by changes in antismoking sentiment. I review in more depth the institutional details behind cigarette tax legislation in the online Appendix in Section B.1.

## III. Data

The primary datasets I use are repeated cross sections from the 1997–2010 NHIS. The restricted-use, geocoded NHIS gathers geographic 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 to 17 years old born after 1987. 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. Since survey year 2010, which includes some early 2011 interviews, is the latest survey year available, the latest birth cohorts I examine were born in early 2009. When looking at school related outcomes, the children must be older than five, making 2005 the latest birth cohort in these models. Additionally, cohort in the NHIS is a function of age and survey year, restricting me to only observing two-year-olds when looking at cohorts born in 2009 (for non-school related outcomes). For children born to the later cohorts, there will be disproportionately fewer (and younger) children in the NHIS. Overall, this means a greater portion of the sample, roughly 80 percent, comes from children born from 1988 to 1999.

Date of birth and state of interview variables in the NHIS jointly allow me to assign to each child a state cigarette excise tax level corresponding to the 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 nine months of gestation.[5] Ideally, I would have the state of birth for each child, but this is not consistently available due to missing data on state of birth for roughly 8 percent of

[5] 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

the sample and all of interview year 1997. Instead, I assume 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 implying true effects that are somewhat larger than what I estimate. However, I check the robustness of my results to these decisions in Section VII.

In the NHIS, information on number of doctor visits is aggregated into bins: (0, 1, 2–3, 4–9, 10–12, 13+), which makes it natural to define a doctor visit's outcome variable as a dichotomous indicator for being above a threshold number of visits.[6] Currie and Gruber (1996), in their paper on the effect of Medicaid expansions on children's health care utilization, dealt with this issue by constructing a dichotomous variable for if a child had 1 or more doctor visits in the past 12 months. I construct an outcome variable similar to the one used by Currie and Gruber, but one designed to evaluate whether there is a decline in child health rather than a change in access.[7] Therefore, my indicator equals 1 if the child had two or more doctor visits in the last 12 months.

I merge mother and family demographic information onto each observation in order to control for covariates such as mother's education, marital status, and age. The mother identifier is missing for roughly 1.21 percent 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. I merge in monthly state cigarette excise taxes from Orzechowski and Walker (2011). Sample sizes along with summary statistics are listed for each outcome and demographic covariate in Table 1.

To estimate the effect of taxes on smoking, I primarily rely on the restricted use Vital Statistics Natality files for years 1989–2009.[8] The natality data is a yearly census of birth certificates in the United States collecting data on birth weight; mother demographic information; and maternal health behaviors, including smoking during pregnancy. I collapse the vital statistic data into cells of means by state, time, and demographic group. I then weight each cell by the number of observations in the cell and the relative proportion that cohort is observed in the NHIS. Collapsing observations to the cell level is done for convenience in order to make the sizable dataset easier to work with. The weighted cell level regressions give the same results as when run at the individual level. I construct the dependent variable as a dichotomous indicator for having reported smoking during pregnancy. When investigating the effect of taxes on birth weight, I use a dichotomous indicator for low birth weight status: any child born weighing less than 2,000 grams is considered "low birth weight." I balance the vital statistics by dropping states that do not report smoking

state taxes change relatively infrequently, so using a different gestational age is unlikely to result in a different tax rate being assigned.

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

[7] 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.

[8] As compiled from data provided by the 57 vital statistics jurisdictions through the Vital Statistics Cooperative Program. Before 1989 the Vital Statistics did not collect information on smoking during pregnancy.

Table 1—Sample Statistics

| | Survey | Mean | Observations | Cohort years |
|---|---|---|---|---|
| *Panel A. Outcome variables* | | | | |
| Sick days from school in past 12 months | NHIS child | 3.43 | 85,117 | 1988–2005 |
| | | (6.11) | | |
| Two or more doctor visits in 12 months | NHIS child | 61.88% | 113,719 | 1988–2009 |
| | | (48.30) | | |
| Asthma attack in 12 months | NHIS child | 5.80% | 120,169 | 1988-2009 |
| | | (23.37) | | |
| Emergency room visit (12 months) | NHIS child | 21.19% | 119,747 | 1988–2009 |
| | | (40.87) | | |
| Hospitalized over night (12 months) | NHIS person | 2.30% | 262,599 | 1989–2009 |
| | | (14.88) | | |
| Smoking during pregnancy | Vital statistics | 14.79% | 10,765,009 | 1989–2009 |
| | | (21.01) | | |
| Low birth weight status | Vital statistics | 9.45% | 2,383,417 | 1989–2009 |
| | | (18.88) | | |
| | Mean | | | Mean |
| *Panel B. NHIS demographic variables* | | | | |
| Mother dropout | 14.21% | Mother's age | | 36.14 |
| | (34.91) | | | (7.44) |
| Mother high school 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 in the NHIS. The vital statistics data is weighted to be representative of the proportion of cohorts in the NHIS sample (see the text for more details). The vital statistics sample size is in state-year-demographic group cells. Dichotomous variables are multiplied by 100 for ease of reading.

during pregnancy throughout part of the sample years (for more details on the above decisions, see the online Data Appendix Section B.2).

I supplement the birth certificate results using an alternative data source on smoking—aggregate per capita cigarette consumption (in packs). I gather this data from tables of state retail cigarette sales provided by Orzechowski and Walker (2011). I divide annual aggregate per capita cigarette sales by 12 and merge this data onto the monthly cigarette tax data. While data on per capita cigarette sales does not only reflect the smoking behavior of pregnant women, it does have several advantages. Because this data comes from retailers, cigarette consumption does not suffer from the same stigma and underreporting issues as self-reported smoking during pregnancy. Similarly, the intensive margin of number of cigarettes smoked is better measured in this data as well. Aggregate cigarette consumption also captures postnatal maternal smoking during the first six months of life,[9] and may reflect shifts in secondhand and environmental tobacco smoke that could affect the health of mothers

[9] Capturing postnatal exposure is important because my reduced form event studies suggest that child health is improving due to reduced smoke exposure in the first several months of life (as well as in utero). Further, since

and their children. Given these advantages, I use this data as corroborative evidence for my findings in the vital statistics.

## IV. Empirical Methodology

### A. *Difference-in-Differences*

My principal empirical strategy uses linear regression models with state and time fixed effects.[10] I always consider "time" to be the month and year the child began their third trimester. In modeling time as the start of the third trimester, I follow the medical literature, which shows that the effect of maternal smoking on birth outcomes is strongest during this period. Specifically, I estimate the following regression equation:

$$Y_{isc} = \beta_1 T_{sc} + \boldsymbol{\beta}_2 \mathbf{X}_{isc} + \gamma_s + \eta_t + \varepsilon_{isc}. \tag{1}$$

$Y_{isc}$ indicates an outcome for child $i$ born in state $s$ whose cohort was in utero at time $c$. The cigarette excise tax level at the time of the third trimester is $T_{sc}$ (measured in 2009 dollars), and $\beta_1$ is the coefficient of interest. I also control for state fixed effects $\gamma_s$ as well as time fixed effects $\eta_t$. $\mathbf{X}_{isc}$ is a vector of additional demographic and state policy controls. I initially include in $\mathbf{X}_{ist}$ dummies for mother's age at the time of interview (11–17, 18–29, 30–39, 40 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 the interactions between each of these demographic indicators (for each separate pair of covariates $x_i$ and $x_j$, I include the interaction between the two). I also include a full set of fixed effects for a child's age in months. In all models I cluster the standard errors on state.

As an additional control, I test the robustness of all my baseline estimates to include 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 possibility that my results are driven by unobserved factors causing child health to be improving before states implement a hike. In this case, adding state linear trends would help absorb a spuriously significant coefficient.

If mothers do not resume smoking after the pregnancy, then the coefficient on the tax could reflect the benefits of reduced secondhand smoke exposure throughout childhood. To help address this issue, I extend the model by including as controls the cigarette excise taxes faced at later ages. If cigarette taxes shift smoke exposure, then leveraging taxes at later ages is a way to test how secondhand exposure might be driving the results.

The coefficients on the taxes at later ages should be interpreted as a combination of the effect of secondhand exposure and cohort pre-trends in health. Taxes at later

70 percent of mothers who quit smoking during the period of pregnancy resume smoking after birth, this could be a nontrivial margin by which taxes decrease smoke exposure (Colman, Grossman, and Joyce 2003).

[10] 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.

ages reflect cohort trends in health because, due to only observing repeated cross sections, a child exposed to a tax at age one must be from a cohort who was born at least a year before any given tax increase. To illustrate this, imagine that there is only one tax increase (from $1.00 to $1.50) in the sample occurring in Michigan in the second quarter of 1991. Children who are born in the second quarter of 1990 are assigned a tax level in utero of only $1.00, and they will be exposed to the increase when they are one-year-old. These children will be assigned a tax at age one of $1.50. Children who are born in the second quarter of 1989 will be the first children to be assigned a tax at age two of $1.50. Likewise children born in the second quarter of 1988 will be the first children to receive a tax at age three of $1.50. Exposure to the tax change in Michigan at later ages directly corresponds to observations born to earlier cohorts. Generalizing this example across many states and tax hikes, implies that the coefficient on taxes at later ages captures cohort trends in health relative to a tax change.

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 an impact on a child in their third trimester in 1990. Which is true, conditional on correctly specifying the 1990 tax level in the model.[11]

### B. *Maternal Smoking Regressions*

I estimate an analogous model for the effect of taxes on smoking during pregnancy. Here, I include all of the relevant demographic controls that were used when modeling the impact of taxes on later life child health in Section VA. In addition, I add a number of controls available in the vital statistics that predict maternal smoking: indicators for the number of prenatal care visits, an indicator for the version of the birth certificate form used (1989 or 2003), father's age category dummies, father's race category dummies, and the interactions between these variables. Adding these additional variables has little effect on the coefficients but reduces the standard errors. Adding state linear trends also reduces the noise in the model, decreasing the standard errors on the tax coefficients substantially. I believe this is due to the trends predicting smoking in ways that are largely unrelated to the tax, and because of this I include these trends in all of my baseline maternal smoking models.[12] Following Gruber and Kozeghi (2001) and Brachet (2008), I interpret the 1989 birth certificate question on smoking to refer to smoking in the month directly before birth, and I assign the tax based on the year and month of birth. As with the NHIS model, small changes in timing make little difference for my results. I use the same basic model as described above when estimating the effect of taxes on per capita cigarette consumption. The per capita cigarette data does not have information for including demographic controls, though I do include state linear trends in all of these models.

[11] 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.

[12] Smoking rates have steeply decreased over the past 15 years. I take the strong reduction in standard errors (with little change in the coefficient) from adding trends as evidence that state linear trends help explain this decline independent of changes in the tax.

### C. *Policy Controls*

My coefficient estimates will be biased if I fail to control for state policies that improve child health that are implemented with cigarette taxes. I am particularly concerned about policies that either: affect a child's early life environment and have long-term impacts on health; or tobacco policies that affect childhood smoke exposure for reasons other than higher cigarette taxes.

I address the first concern through a review of the literature (for an extensive review, see Currie and Rossin-Slater 2015). Specifically, earlier studies have shown that early life exposure to Medicaid and the State Children's Health Insurance Program (SCHIP) improves health. Gruber (2003) demonstrated that state expansions in these programs were usually done as an increase to the income eligibility threshold above a percent of the poverty line. I include these eligibility thresholds as a control for state level Medicaid and SCHIP generosity. Medicaid eligibility for pregnant women is assigned based on state and time of birth.[13] SCHIP eligibility for children is assigned based both on state and age of child at the time of interview. 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, 2006). Therefore, I also add to my models a dummy variable that equals one if the state had reformed its welfare system by the given year. Jointly public insurance and welfare reform represent the major state level policies that impact child health that were changing with the birth cohorts in my sample.

The other main type of confounding variation of concern are state level policy changes that affect smoke exposure other than the cigarette tax. Smoking indoor air laws limit the venues in which smoking is allowed and through this may change childhood exposure to cigarette smoke. ImpacTeen, a research organization on youth health, rates the stringency of indoor air laws annually by state. I control for this rating in my main specification (following Carpenter and Cook 2008). There is also evidence that smoking and infant health changes with the business cycle (Ruhm 2005; Dehejia and Lleras-Muney 2004), and because of this I also control for the unemployment rate. Controlling for the unemployment rate additionally helps account for relative increases in state taxes in response to the 2001 recession. Finally, as a robustness check, I control for the state beer tax and a measure of state level antismoking sentiment constructed by the earlier literature (DeCicca et al. 2008). These two controls are less commonly looked at in the literature but changes in both could be correlated with changes in cigarette taxes, and have an independent effect on smoke exposure.

It is worth taking a moment to discuss the other policies I am not able to control for which could potentially affect child health. Exposure to federal policies such as WIC and food stamps have been shown to have long-term impacts on health, however the state level variation in these policies occurred during their roll out, which ended well before the first year of my sample. More recent federal variation in these policies will be absorbed by the year-month fixed effects in my models. State family

[13] This data on SCHIP and Medicaid eligibility is the same as constructed and used by Hoynes and Luttmer (2011).

leave policies are changing at this time but are limited in scope (only five states and Washington, DC, have these laws) and evidence suggests that these policies have little to no impact on child health (Currie and Rossin-Slater 2015). For the cohorts in my sample, universal preschool programs are starting to be offered, but could harm health because children grouped together are exposed to more sickness (Baker, Gruber, and Milligan 2008). Not controlling for universal pre-K would bias my results downward.

### D. *Event Study Methodology*

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. The case $j = 0$ indicates that a cohort is in the third trimester when the tax hike occurs. These event time dummies replace the treatment variable in the regression model. Otherwise the event studies include the same demographic and policy controls as in my preferred regression model. For my initial specification, I define event time in quarters. Only states that experience an event are included in the event study sample. Following Almond, Hoynes, and Schanzenbach (2011), I also balance the event study such that events are only included if there are two full years in both the pre-period and post-period. Without balancing, the graph of the event study could pick up demographic changes from states entering and exiting the event window. In some specifications, I aggregate event time into six-month bins because using these more aggregated event dummies reduces noise and makes the pattern of the coefficients smoother.

Since the x-axis of the event study tracks cohorts in their third trimester relative to the tax hike, the pre-trends are a combination of cohort specific trends in child health and the effect of secondhand 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 are 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. Therefore, an improvement in health for this cohort could be due to the tax reducing secondhand smoke exposure at the time of birth. Alternatively, improving health in the pre-period could reflect a cohort specific trend. In spite of not separately identifying these factors, 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 an improvement in child health from in utero exposure to the tax relative to postnatal exposure.[14] 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 state trends driving my results.

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

My excise tax variation does not fit neatly into the standard event study approach. Differing magnitudes of taxes means that the policy cannot be simply characterized as a dichotomous treatment. The majority of the states in my sample had two to three tax hikes, making it difficult to separately analyze a single event. Finally, event studies tend to be most effective at identifying differential trends when many events are occurring in the same general time period. To address these issues, I take all tax hikes and assign them percentiles (unweighted) based on the amount of the hike. I define a discrete tax event as any tax hike greater than or equal to the eighty-fifth percentile of all hikes (\$0.72 in 2009 dollars) and placing the events in the "high frequency" tax period of 1997 to 2001. This cutoff has the advantage of being the lowest cutoff such that there is only one discrete event per state.

As a robustness check, I show that the event studies follow similar patterns when I define an event as a tax hike at or above the fiftieth, twenty-fifth, or zeroth percentile of all hikes. For maternal smoking, I also show that a flat cutoff of 25 cents or more gives similar results. The 25 cents cutoff follows from Lien and Evans (2005), whose analysis showed that there were detectable effects of taxes on maternal smoking behavior for hikes at this level or higher. At the lower cutoffs, the event studies need to be modified to deal with multiple potential events per state. I handle this by using the first qualifying event in each state, though this decision is robust to using all hikes per state and a reweighting scheme as described in the online Appendix. Perhaps because event studies are traditionally cleanest when there is only one event per state, at the lower cutoffs the event studies are significantly noisier, and I address this by using six month rather than quarter bins for these graphs. In the online Data Appendix I discuss more details on how I constructed the event studies.

## V. Results

### A. *Impact of Taxes on Smoking*

I begin by showing how taxes impact smoking during pregnancy using the US Vital Statistics. The first row of Table 2 shows this relationship for children from the sick days from school cohorts in the NHIS (1988–2005). A dollar increase in the tax reduces smoking by 0.92 percentage points, or 6.2 percent of the mean. The second row of Table 2 shows results for births matched to the doctor visits cohorts (1988–2009), with qualitatively similar results. Columns 2–4 show that the coefficient estimates are robust to adding the state and policy controls described above.

To check the validity of the pre-trends in maternal smoking, I show an event study on smoking during pregnancy in Figure 2. In panel A of Figure 2, an event is defined as an increase of 25 cents or more (following the lowest tax hike amount shown to affect behavior in Lien and Evans 2005). Panel B of Figure 2 mirrors my second stage event studies described above and defines an event as any hike at or above the eighty-fifth percentile of all hikes. In both cases the pre-trends in smoking are relatively flat (and not statistically different from zero), followed by a steep decline in smoking during pregnancy around the time of the tax hike. Defining events at other cutoffs produces similar looking event studies.

Table 2—Impact of Taxes on Smoking during Pregnancy

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A. Cohorts 1989–2005 (sick day cohorts)* | | | | |
| Excise tax (dollars) | −0.92 | −0.96 | −1.00 | −1.01 |
| | (0.25) | (0.27) | (0.34) | (0.35) |
| *F*-test on tax coefficient | 13.54 | 12.64 | 8.65 | 8.32 |
| Number of cells | 8,298,053 | 8,298,053 | 8,298,053 | 8,298,053 |
| *Panel B. Cohorts 1989–2009 (doctor visits cohorts)* | | | | |
| Excise tax (dollars) | −0.94 | −0.96 | −0.94 | −0.98 |
| | (0.29) | (0.30) | (0.32) | (0.33) |
| *F*-test on tax coefficient | 10.51 | 10.24 | 8.63 | 8.82 |
| Number of cells | 10,765,099 | 10,765,099 | 10,765,099 | 10,765,099 |
| Mean smoking during pregnancy | 14.79% | | | |
| Policy controls | | X | X | X |
| Unemployment rate | | | X | X |
| Indoor air laws | | | | X |

*Notes:* The dependent variable is an indicator for smoking during her pregnancy. Vital statistics data is collapsed into cells based on state-time-demographic group. Models include fixed effects, state-time linear trends, demographic controls, and their interactions. Policy controls are the state's Medicaid eligibility threshold and an indicator for welfare reform. The data are balanced by dropping states with more than 50 percent missing smoking across several years. I weight the vital statistics by the cell size and to be representative of the cohorts in the NHIS. Tax multiplied by 100 for ease of reading.

I show the *F*-statistic for instrument strength below the point estimates in Table 2. *F*-statistics range from 13.54 to 8.32, making them either above or close to the standard cutoff of 10. The earlier literature found a stronger effect of taxes on smoking during pregnancy, with *F*-statistics above 100 and larger estimated tax coefficients (Evans and Ringel 1999; Ringel and Evans 2001). Online Appendix Table A-2 resolves the differences between my estimates and the earlier literature. When restricting the sample to 1989–1995 (using the years and basic model in Ringel and Evans 2001), the decline in *F*-statistics is shown to be driven by clustering on state. The coefficients decline in magnitude when extending the years of the sample to include the full range of cohorts that match the NHIS sample (1989–2009); in spite of the smaller coefficients, the *F*-statistics almost always remains above 10 going as high as 114 when clustering on state year-month.

While the analysis of the birth certificate data is encouraging, the *F*-statistic of the tax coefficient falls below ten in some specifications. To address this concern, and to further check the validity of the first stage, I turn to using supplementary data on cigarette consumption, which does not suffer from the same measurement error issues as self-reported smoking during pregnancy. While cigarette consumption data does not exclusively focus on pregnant women, Table 2 implies elasticities of smoking for pregnant women of −0.4, which is at the midpoint in the range of consensus smoking elasticities for the overall population.[15] Therefore the pregnant women in my sample respond similarly to taxes as other adults.

[15] The reported mean smoking for this sample was around 15 percent and average prices were $6.50 in 2001 (Orzechowski and Walker 2011). From these prices and smoking rates, I calculate an elasticity of smoking of −0.40.

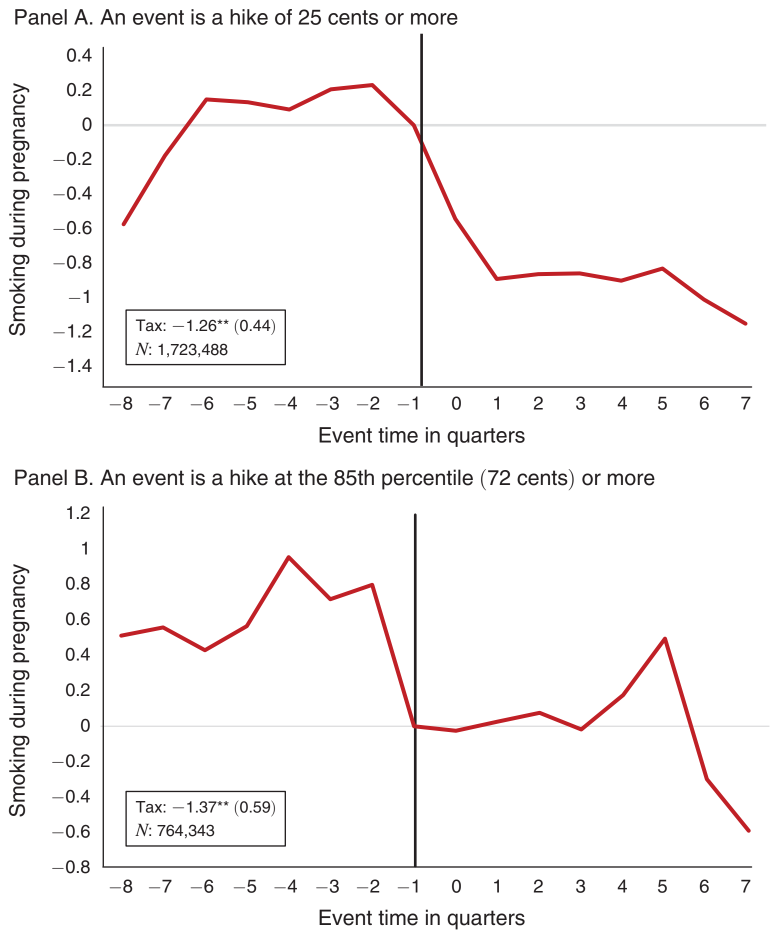


FIGURE 2. EVENT TIME ESTIMATES OF IN UTERO EXPOSURE TO A TAX HIKE ON SMOKING

*Notes:* All models are estimated with a linear probability model. The event study specification includes demographic controls, policy controls, and state linear trends, as in my preferred vital statistics smoking during pregnancy regressions. Vital statistics data are balanced by dropping states with more than 50 percent of the data missing across several years of the sample due to not reporting smoking on the birth certificate. I weight the cohorts in the vital statistics to be representative of the cohorts in the NHIS. Event time tracks the number of quarters before or after a tax hike during which a cohort of pregnant mothers reported smoking.

Table 3 shows the coefficients from regressing state per capita consumption on the excise tax using different policy and state controls. Across the specifications, a dollar tax reduces per capita smoking by about a pack per month. The *F*-statistics range from 19 to 60, making them well above 10 across all specifications. More generally, the results of taxes on smoking across Tables 2, A-2, and 3 are qualitatively similar regardless of the exact *F*-statistic, with clustering on state being the most conservative specification.

The consensus smoking participation elasticities of the overall population range from −0.3 to −0.5 (Chaloupka and Warner 2000).

Table 3—Impact of Taxes on State Per Capita Cigarette Consumption

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A. Cohorts 1989–2005 (sick day cohorts)* | | | | |
| Excise tax (dollars) | −0.87 | −0.86 | −0.93 | −1.01 |
| | (0.20) | (0.20) | (0.15) | (0.16) |
| *F*-test of tax coefficient | 19.00 | 19.28 | 37.25 | 41.41 |
| State-year-month cells | 10,404 | 10,404 | 10,404 | 10,404 |
| *Panel B. Cohorts 1989–2008 (doctor visit cohorts)* | | | | |
| Excise tax (dollars) | −0.91 | −0.91 | −0.93 | −1.03 |
| | (0.16) | (0.16) | (0.13) | (0.13) |
| *F*-test of tax coefficient | 33.80 | 33.89 | 53.4 | 60.93 |
| State-year-month cells | 12,852 | 12,852 | 12,852 | 12,852 |
| Policy controls | | X | X | X |
| Unemployment rate | | | X | X |
| Indoor air laws | | | | X |

*Notes:* Data comes from Orzechowski and Walker (2011). The dependent variable is a state's annual per capita sales of cigarette packs divided by 12. I merged this data onto monthly state excise tax data. The excise tax is in 2009 dollars. All models include fixed effects for state and time, state linear trends, the state's Medicaid eligibility threshold as a percent of the poverty line, ImpacTeen ratings, and a welfare reform dummy. I weight the years in the data to be representative of the cohorts in the NHIS.

## B. *Impact of Taxes on Child Health*

For child health outcomes, I focus on two of the higher prevalence outcomes in the NHIS: sick days from school and two or more doctor visits. The majority of children had multiple sick days in the past 12 months and on average 61.88 percent 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 4 shows results for sick days from school. My initial specification includes demographic controls and state and time fixed effects.[16] A 1 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. I begin with a core set of state policy controls: the state's income threshold for pregnant women to qualify for Medicaid and SCHIP and an indicator for the state having implemented welfare reform. As shown in column 2 of Table 4, adding these controls has little effect on my results. I next add the state unemployment rate at the third trimester. There is virtually no change to the coefficient, implying that my results are not driven by changes in state economic conditions.

In column 4 of Table 4, I control for the state's ImpacTeen rating for smoke-free indoor air laws in bars and private work places. Neither the ImpacTeen controls nor

[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.

Table 4—The Impact of Cigarette Taxes on Sick Days from School

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Excise tax (dollars) | −0.32 | −0.31 | −0.31 | −0.34 | −0.38 | −0.54 |
| | (0.16) | (0.15) | (0.15) | (0.17) | (0.18) | (0.25) |
| Average increase in excise tax, 1980–2007 | \$0.80 | | | | | |
| Mean sick days | 3.43 | | | | | |
| Observations | 85,117 | | | | | |
| State policy controls | | X | X | X | X | X |
| Unemployment rate | | | X | X | X | X |
| Indoor smoking law rating | | | | X | X | X |
| Current tax | | | | | X | X |
| State linear time trends | | | | | | X |

*Notes:* The 1997–2010 NHIS is the main dataset used in this table. The dependent variable is the number of sick days from school in the past 12 months measured for children ages 5 to 17. The excise tax is the tax faced in the child's third trimester in 2009 dollars. Regressions are weighted using NHIS sample child weights. All models include fixed effects for state, time, demographic controls, and their interactions. Policy controls includes the state's Medicaid and SCHIP eligibility threshold as a percent of the poverty line, and an indicator for welfare reform. Indoor air laws are the ImpacTeen indoor air smoking rating in bars and restaurants. Standard errors clustered on state are in parentheses.

the current cigarette tax significantly change my estimates. My preferred specification is column 5, which includes all of the previous controls and additionally adds the current cigarette tax. Column 5 shows that a dollar tax decreases sick days from school by 0.38 of a day, a similar magnitude effect found by other studies that have looked at the impact of policy shocks on child health and educational outcomes (Nilsson 2008; Currie, Decker, and Lin 2008; and Fowler, Davenport, and Garg 1992). The robustness of my results across these different controls suggests that the coefficients are not driven by unobserved state level changes correlated with the tax hikes.

How should the magnitude of a 0.38 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. In other words, a dollar increase in the tax causes a 10 percent decrease in sick days relative to the mean. Given that between 1980 and 2009 state cigarette taxes increased by \$0.80 on average, taxes over this period caused a decline in sick days of 8.0 percent relative to the mean.

Column 6 of Table 4 shows that the tax coefficient retains the same 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 −0.54. However, my event study analysis sheds light on why this is the case.

Figure 3 shows the event study for sick days at the eighty-fifth percentile cut off. Ideally, an event study shows a flat pre-period. Here, if anything, there is a slight upward trend, though none of the pre-trend event time parameter estimates are statistically different from zero. The pre-period coefficient on an event time dummy reflects a combination of cohort specific trends in health and the impact of secondhand smoke exposure. Since sheltering children from secondhand smoke

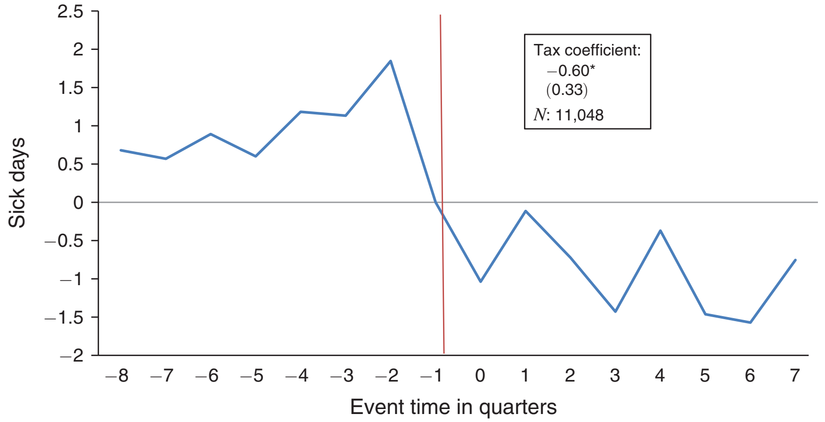


Figure 3. Event Time Estimates of In Utero Exposure to a Large Cigarette Tax Hike on Sick Days from School

*Notes:* An event is defined as any cigarette tax increase equal or above the eighty-fifth percentile (72 cents) in 1997 or later. The sample includes children ages 5 to 17. NHIS child weights are used. All models include fixed effects for state, time, demographic controls and their interactions, state policies, the state unemployment rate, the ImpacTeen rating for bars and private work places, and the current cigarette tax. The reported tax coefficient is the coefficient on the excise tax from running my regression model on the 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. The tax increase occurs for cohorts in their third trimester at event time 0. When event time is −1, this corresponds to a cohort being in their third trimester the quarter before a tax hike. Thus, event time −1 corresponds to children born around the time of the hike. Therefore, the pre-trends in the event study captures both state trends in child health before a tax increase and the effect of a change in secondhand smoke exposure after birth.

exposure should improve health, this suggests (if anything) a worsening pre-trend. This upward trend is consistent with state linear trends increasing the magnitude of the tax coefficient in the regression results in Table 4. The event study clearly shows a discrete decrease in sick days beginning around period −1, which roughly corresponds to the time of birth. Seeing the decline at this time means there is a differential effect of exposure to smoke in early life and in utero relative to exposure in the years after birth.

In online Appendix Figure A-1, I show that the event study follows similar patterns when using a number of alternate cutoffs other than the eighty-fifth percentile and when aggregating event time into six-month bins. Across the zeroth, twenty-fifth, fiftieth, and eighty-fifth percentiles, the more aggregated event studies show no evidence of a downward pre-trend and also reveal a relative improvement in child health beginning between birth and the third trimester. While in some cases there is an upward spike in sick days before the hike, none of these parameter estimates are statistically significant.

Since the sample changes when doing an event study, I re-ran my baseline regression models limiting the observations to only those that are also in the event study sample. These regression results are shown as the "tax coefficient" in the box in the event study figures. Across the different event studies the "tax coefficient" is roughly the same as the coefficient on the excise tax in Table 4. This means that the change in the sample is not driving the event study result, and confirms that my results hold

Table 5—Taxes on Sick Days with Controls for the Tax in the Years before and after Birth

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Third trimester 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) |
| Observations | 78,348 | 46,328 | 85,107 | 84,946 | 46,167 |

*Notes:* The 1997–2010 NHIS is the main dataset used in this table. The dependent variable is sick days from school in the past 12 months measured for children ages 5 to 17. The excise tax is in 2009 dollars. The sample size falls slightly when I add in the taxes faced at later and earlier ages, because some observations have missing values for these taxes due to limiting the tax to values through 1988–2009. This change in sample does not change my baseline results. Standard errors clustered on state are in parentheses.

when limiting my control group to only including states that eventually pass a large tax hike.

Table 5 presents results using my preferred specification from Table 4, but adding controls for the tax level faced at different times before or after a child's birth. Column 1 of Table 5 controls for a 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 tax levels faced before birth, all are of mixed signs, and only one is significant. Similarly, columns 3 and 4 include the tax faced when the child was one- to five-years-old. The fact that the taxes faced at later ages do not change the coefficient on the in utero tax supports my hypothesis that the results are not driven by smoke exposure at later ages. Furthermore, the coefficients on the taxes faced at later ages are not consistently negative, and none of them are statistically significant. 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. Looking across the columns of Table 5 shows that the coefficient on the in utero

Table 6—The Impact of Cigarette Taxes on the Likelihood of Two or More Doctor Visits

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Excise tax (dollars) | −3.15 | −3.14 | −3.13 | −3.27 | −2.92 | −2.34 |
| | (0.94) | (0.89) | (0.90) | (0.89) | (0.90) | (1.00) |
| Average increase in excise tax, 1980–2007 | $0.80 | | | | | |
| Percent of children with 2 or more doctor visits | 61.88 | | | | | |
| Observations | 113,719 | | | | | |
| State policy controls | | X | X | X | X | X |
| Unemployment rate | | | X | X | X | X |
| Indoor smoking law rate | | | | X | X | X |
| Current tax | | | | | X | X |
| State time trends | | | | | | X |

*Notes:* The 1997–2010 NHIS is the main dataset used. The dependent variable is an indicator for 2 or more doctor visits in the past 12 months for children ages 2 to 17. The tax coefficient is multiplied by 100 for ease of reading. The excise tax is in 2009 dollars. All models include fixed effects for state, time, demographic controls, and their interactions. Regressions are weighted using NHIS sample child weights. Policy controls includes the state's Medicaid and SCHIP eligibility threshold as a percent of the poverty line, and an indicator for welfare reform. Indoor air laws are the ImpacTeen indoor air smoking rating in bars and restaurants. Standard errors clustered on state are in parentheses.

excise tax stays negative, statistically significant, and is generally stable in magnitude regardless of how I control for earlier or later taxes. The sample size falls when I add in the taxes faced at different ages because some observations have missing values for these taxes due to limiting the tax variation to be between 1988–2009. I re-ran my preferred specification from Table 4 on the smaller sample and the change in sample doesn't affect my baseline results.

Results for two or more doctor visits are shown in Table 6. My preferred coefficient estimates (in column 5) show 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 2.92 percentage points. The ITT coefficient represents a 4.7 percent 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 tax remains negative and significant.[17]

Table 7 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

[17] I also run my results using alternative methods of constructing the doctor visits variable. I use one or more doctor visits and four or more visits as alternate cutoffs and get qualitatively similar results. In addition, I also ran an event study on doctor visits. Unfortunately, many of the children of ages 2 to 5 are dropped when balancing the event study due to being at the edge of the event 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.

Table 7—Taxes on Doctor Visits with Controls for the Tax in the Years before and after Birth

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Third trimester 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) |
| Observations | 111,996 | 78,959 | 118,826 | 113,509 | 73,642 |

*Notes:* The dependent variable is an indicator for 2 or more doctor visits in the past 12 months measured for children ages 2 to 17. All regressions use NHIS child weights and a full set of controls. The sample size falls after adding in the taxes faced at later/earlier ages due to limiting the tax variation through 1988–2009. This change in sample does not change my results.

later ages are statistically significant (though not always negative). This could possibly be evidence of an independent effect of secondhand smoke. However, because the tax faced at later ages does not change the in utero tax coefficient, secondhand smoke exposure should not be driving my main results. Table 7 also confirms that a tax before the child is born has no impact on visits.

One way to get an estimate for the effect of smoking during pregnancy on child health 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 mothers accurately report smoking during pregnancy, and if there is no effect from secondhand smoke exposure on infants, then this will be the true TOT. 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). Assuming accurate reporting and no exposure in infancy are overly restrictive assumptions. Still, I calculate the TOT as a statistic of interest, but with the caveat that it is an upper bound. Dividing the coefficient on sick days by the change in maternal smoking of 0.92 percentage points gives a TOT estimate of roughly 0.41 of a day sick from school or around 12 percent of the mean. Similarly, for two or more doctor visits dividing by the maternal smoking coefficient gives a TOT estimate of around 3.17 percentage points or approximately 5.13 percent of the mean.

### C. *Results by Subgroup*

I now jointly use both the NHIS and Vital Statistics to compare the pattern of tax coefficients across various demographic subgroups for health outcomes in early life relative to later life. I begin by graphing a scatterplot with the tax impact on the probability of a low birth weight birth by subgroup on the x-axis and the tax impact on sick days by subgroup on the y-axis. I include estimates for the entire sample as one of the points on the graph. The size of the points on the graph reflects relative subgroup size (using NHIS weights). I also include estimates by mother's education, child gender, cohort time period (before or after 2000), race, mother's age, and marital status. Panel A of Figure 4 reveals a strong correlation between being in a subgroup that gained birth weight from a tax as an infant and having fewer sick days later in life.[18] Panel B of Figure 4 does the same exercise but for doctor visits: the pattern of results is strikingly similar. I take Figure 4 as strong evidence that the gains to child health from cigarette taxes 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 the form of improved birth weight and later show up 2 to 17 years down the line in childhood.[19]

In Figure 5, I perform the same analysis but replacing low birth weight with "smoking during pregnancy" as the dependent variable on the x-axis. Here I include state linear trends in all models to make the smoking coefficient estimates from Table 2 directly comparable to the NHIS child health estimates. The general pattern is that subgroups with a larger decrease in maternal smoking have a larger decrease in sick days and doctor visits. However, the results are noisier than those in Figure 5. The additional noise could come from the underreporting of maternal smoking, or because smoking data are only included in 46 states in the vital statistics during this time. That being said, the pattern is still one of larger magnitude smoking elasticities correlated with greater childhood health gains.

One important pattern reflected in Figure 5 are differences in the impact of taxes in the earlier (pre-2000 cohorts) relative to later (post-1999 cohorts). My results become small, positive, and insignificant when looking at the post-1999 cohorts. This is similar to the finding of Levy and Meara (2006), who showed that past 1996, pregnant women were less responsive to a large national increase in cigarette prices. As discussed in the data section above, cohorts born past 1999 represent a disproportionately small portion (only roughly 80 percent) of my NHIS sample. I show these results by time period in table form in online Appendix Table A-3. I

[18] 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.

[19] 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.

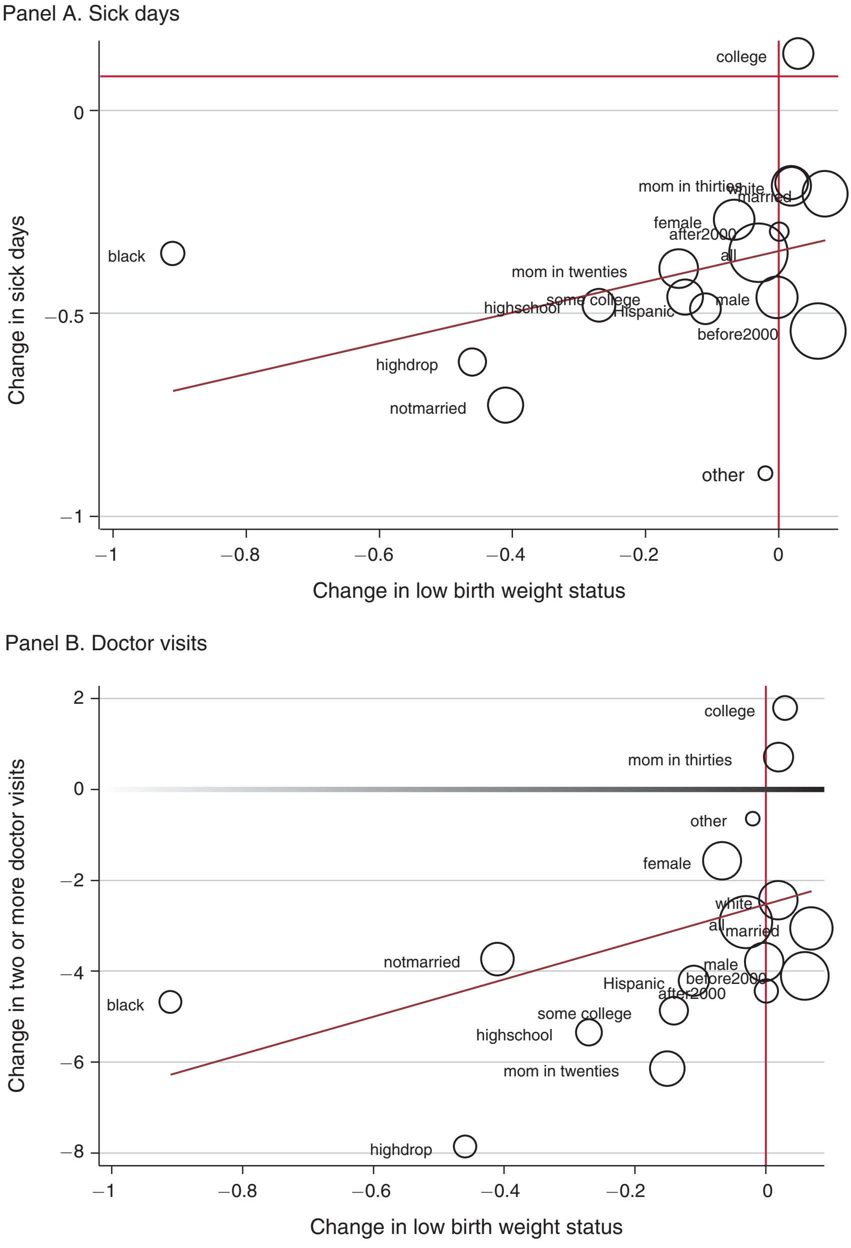


Figure 4. Subgroup Estimates of Cigarette Taxes on Child Health

*Notes:* The points on the graph represent estimates for different demographic subgroups. The x-axis plots the coefficients from a regression of cigarette taxes on low birth weight status. The y-axis on panel A plots the coefficients from a regression of cigarette taxes on number of sick days from school in the last 12 months. The y-axis on panel B 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.

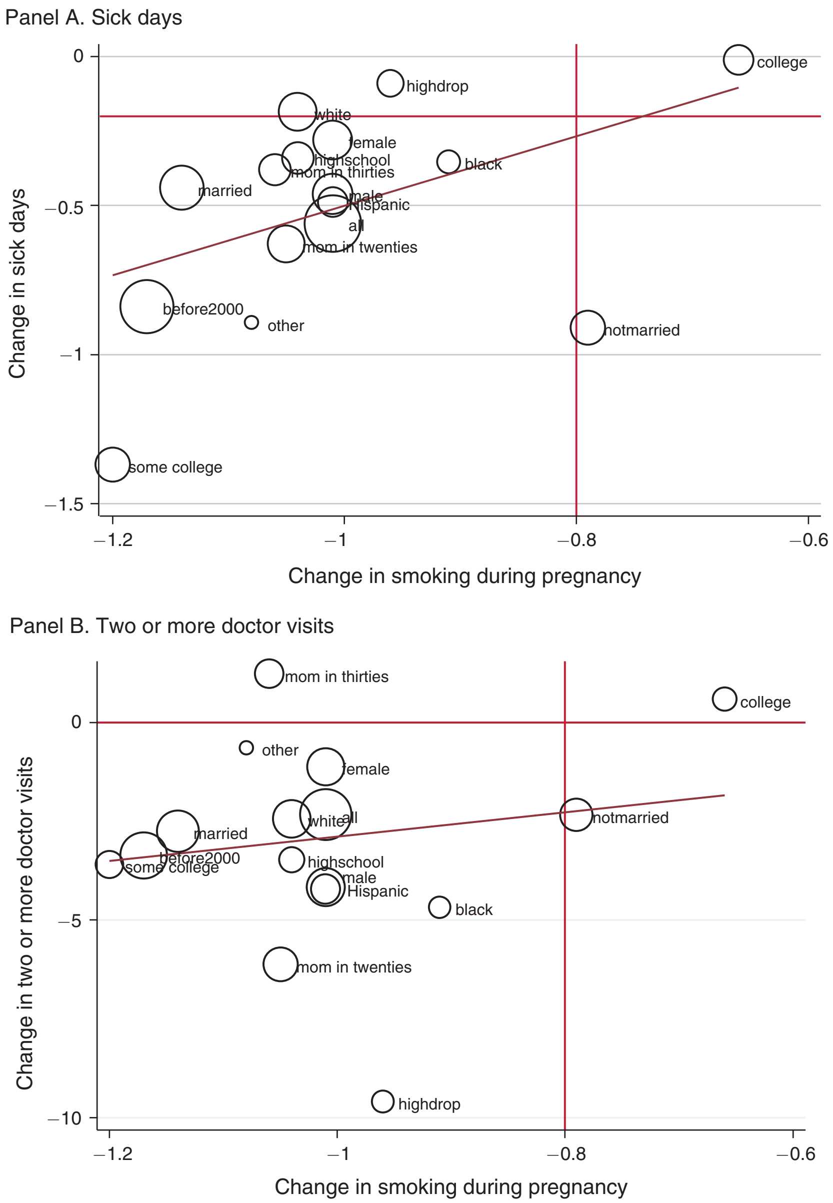


Figure 5. Subgroup Estimates of Cigarette Taxes on Maternal Smoking

*Notes:* The points on the graph represent estimates for different 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 on panel A plots the coefficients from a regression of cigarette taxes on number of sick days from school in the last 12 months. The y-axis on panel B 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 8—The Impact of Cigarette Taxes on Other Child Outcomes

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| *Panel A. Asthma attack in 12 months* | | | | | | |
| Excise tax (dollars) | −0.95 | −1.01 | −0.99 | −0.97 | −0.97 | 0.12 |
| | (0.50) | (0.50) | (0.51) | (0.52) | (0.52) | (0.54) |
| Mean | 5.78 | | | | | |
| Observations | 120,169 | | | | | |
| *Panel B. 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) |
| Mean | 2.29 | | | | | |
| Observations | 23,632 | | | | | |
| *Panel C. Emergency room visit in 12 months* | | | | | | |
| Excise tax (dollars) | −1.75 | −1.76 | −1.80 | −1.74 | −1.74 | −0.19 |
| | (1.24) | (1.22) | (1.22) | (1.36) | (1.36) | (1.36) |
| Mean | 20.41 | | | | | |
| Observations | 119,747 | | | | | |
| 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:* The dependent variables are listed above the coefficient estimates and measured for all children ages 2 to 17. All regressions use NHIS child weights and a full set of controls. Tax coefficients are multiplied by 100.

additionally show subgroup results in table form stratified by both mother's education in online Appendix Table A-4 and by gender in online Appendix Table A-5.[20] After stratifying on mother's education for the sick day's outcome, none of the coefficients are statistically significant, and these results need to be interpreted in the context of their large standard errors.

### D. *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 or of lower incidence. In spite of this, I find some evidence of effects. The results are shown in Table 8 for emergency room visits, overnight hospitalizations, and having an asthma attack.[21] For the first specification (column 1) of Table 8, the coefficients are negative across all outcomes. A dollar tax hike causes a −0.95 percentage point change in the likelihood of having an asthma attack in the

[20] It is interesting to note that across all outcomes the improvements in health from exposure to the cigarette tax are greater for boys relative to girls, which 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.

[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.

Table 9—Impact of Taxes on Total Fertility and the Composition of Births

| | log (births) | Female | Black | Teen mother | Married |
|---|---|---|---|---|---|
| Excise tax (dollars) | −0.11 | −0.01 | 0.64 | 0.81 | 0.92 |
| | (0.07) | (0.02) | (0.46) | (0.18) | (0.01) |
| Mean | 1.83 | 49.00 | 16.00 | 12.00 | 67.00 |
| Observations | 2,269,504 | 2,269,504 | 2,269,504 | 2,269,504 | 2,269,504 |

*Notes:* Each column is a separate model with the dependent variable either being the log number of births in a given state year-month cell, or the fraction of births to a specific demographic group in that cell. The vital statistics (1989–2009) 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. Standard errors clustered on state are in parentheses.

past 12 months, an ITT of 16 percent of the mean. A dollar tax hike also causes a −0.35 percentage point change in having an overnight hospitalization (an ITT of 15 percent of the mean). For emergency room visits, the coefficient on the tax is negative though not significant. Looking across the columns of Table 8, these results are also quite robust. Taxes have a strong and negative effect on asthma attacks and hospitalizations in most specifications. However, these results are not statistically significant across all specifications.

## VI. Robustness Checks

One concern is that cigarette taxes may be correlated with changes in the demographic composition of a state and these changes could be driving improvements in health. I use demographic variables in the vital statistics to test this. Table 9 shows that the cigarette tax does not predict total log births, the fraction of 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; so if teen pregnancies are increasing with the tax, this will bias my results downwards.

I test to see if cigarette tax revenue spent on health programs is biasing my results by controlling for transfer payments to individuals. Annual data on state transfer payments come from the Regional Economic Information System (REIS) database, which tracks transfer receipts from personal income accounts.[22] I show these robustness tests in Table 10. Across the columns of Table 10, I directly control for either medical benefits or for public medical assistance,[23] depending on the specification. I

[22] 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.

[23] Medical benefits includes all spending on public medical programs, including veteran insurance benefits. Public medical assistance is more specific and captures spending on all state level means tested insurance programs such as Medicaid and SCHIP.

Table 10—Robustness of Results to Controlling for State Transfers to Individuals

| | *Panel A. Sick days* | | | *Panel B. Two or more doctor visits* | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (1) | (2) | (3) |
| Excise Tax (dollars) | −0.34 | −0.39 | −0.35 | −2.64 | −2.45 | −2.11 |
| | (0.17) | (0.15) | (0.13) | (0.76) | (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 the Regional Economic Information System (REIS) database. The dependent variables are either sick days from school (for children ages 5–17) or an indicator for 2 or more doctor visits (for children ages 2 to 17). Medical benefits includes all spending on public medical programs as well as veteran insurance benefits. Public medical assistance includes all means tested insurance programs in the state such as Medicaid and SCHIP. Education and training controls for dollar spending on education and job training programs. 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. Standard errors clustered on state are in parentheses.

also control for other programs that could potentially improve welfare: food stamps, family income support, supplemental security income (SSI) payments, and education. The coefficient and significance on the excise tax for both sick days and doctor visits does not change regardless of how I control for the different categories of transfers.

Additionally, I look at the impact of taxes on outcomes unrelated to smoke exposure as a placebo test. Specifically, I use chicken pox, anemia, chronic headaches,[24] injuries, food allergies, and a pooled index of the low incidence placebos.[25] Online Appendix Table A-6 shows that for these placebo tests the tax coefficients are all small in magnitude and not significant. In online Appendix Table A-7, I show the robustness of my results to controlling for different types of state trends. In addition to state linear trends, I allow for state trends with different slopes over the two different periods in the sample (1989 to 1999 and 2000 to 2009), and state quadratic trends. Even with these very flexible trends, my results hold. Finally, my results are fully robust to adding controls for the state beer tax and an index of antismoking sentiment (results available upon request).

A remaining concern is that the decision to quit smoking during pregnancy is persistent, and my results are driven by accumulated secondhand exposure throughout childhood. To address this, I show that relative to the current tax, the in utero tax has a much smaller and not significant effect on current smoking (see online Appendix

[24] This is a valid placebo variable since the vast majority of chronic headaches in children are migraines (Abu-Arefeh and Russell 1994), and genetic factors play a leading role in determining the incidence of migraines (Russell, Iselius, and Olesen 1996).

[25] My construction of the placebo index follows the process used in Kling, Liebman, and Katz (2007). Headaches, anemia, food allergies, and injuries are all relatively low incidence. I normalize each of these outcome variables 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.

Table A-8).[26] I also directly test for secondhand exposure by estimating the effect of a tax increase on my outcomes when a child is six-years-old. For both sick days from school and doctor visits, the coefficient on the tax faced at age six are small and not significant (results are available upon request).

Small changes in the timing of when I assign the tax coefficient will not change how the tax is classified for the majority of observations. Regardless, I test the timing assumptions in online Appendix Table A-9. I find that assigning treatment to different trimesters makes little qualitative difference to my results. Further, when I include the excise tax in each trimester in the same regression model, the third trimester of the tax stays negative and significant and has the strongest impact. Online Appendix Table A-10 tests the sensitivity of my results to the assumptions I made when constructing the sample. I run my results dropping all children missing a mother identifier, dropping all children missing an exact month or year of birth, and merging in the tax based on the state of birth data (with a smaller sample due to missing data). My baseline results are fully robust to these changes.

## VII. Economic Significance

To get a sense of the monetary value of my findings, I perform some back-of-the-envelope calculations in online Appendix Table A-11.[27] Row 1 of Table A-11 shows the cost related to each of the outcomes I find effects for. For doctor visits, I use the average cost of a pediatric office visit which comes to \$606. For asthma attacks, I use the average yearly expenditures for a child's medical services related to asthma of \$1,359 (Agency for Health Care Research and Quality 2009).

Quantifying the cost of a sick day is more complicated, and my approach draws on the education literature to estimate foregone wages from missed days of schooling. Card and Krueger (1992) directly estimate the correlation between primary school term length and later life wages and find that an additional day of school has a wage return 0.04 percent. Alternatively, using a returns to education of 7 percent a year (Harmon, Oosterbeek, and Walker 2003) and dividing this by a 180-day school year gives a similar estimate of 0.03 percent. Researchers have also used natural experiments to estimate that a day of instruction increases test scores by 0.01 to 0.03 of a standard deviation (Hansen 2011, Lavy 2015), and Chetty et al. (2011) found that a 1 standard deviation increase in test scores leads to an 18 percent increase in wages. Jointly this implies a return to a day of 0.18 percent. Given this range of estimates, it is conservative but reasonable to assume a return to a day of schooling of 0.03 percent. Using a return of 0.03 percent, the 2009 median earnings from the American Community Survey (Office of the Chief Actuary 2010) of \$26,000,[28] and a 40-year work life, I estimate the lifetime value of a missed day of school by multiplying these numbers together. This results in a day of education being worth \$312.[29]

[26] Unfortunately, retrospective data on smoking during pregnancy is not available in the NHIS data.

[27] These calculations are meant only to be a rough quantification of the benefits I estimate. Performing a full cost/benefit analysis associated with a cigarette tax increase is beyond the scope of this work.

[28] The 2009 median earnings reported by the Social Security Administration came to \$26,000.

[29] I make the overly strict assumption that there are no sheepskin effects. However, this estimate is conservative in the sense that it ignores the foregone earnings of parents who take time off from work to care for sick children.

In online Appendix Table A-11, I multiply the treatment effect from my preferred specifications 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 4, I multiply this monetary benefit by the number of years of estimated treatment effects to get the full health benefits over the course of childhood.[30] Doctor visits likely includes some asthma treatment, so I do not add these values together and instead use the benefits associated with asthma since it is the smaller of the two. Summing over sick days and asthma treatment, the health benefits of a dollar tax comes to \$1,626 per child.

One way to think about these benefits is to compare them to the value of reducing low birth weight births. Using Almond, Chay, and Lee (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 dollar tax hike is $-0.004$ percentage points. Therefore, the value of a dollar tax hike in terms of reducing birth weight costs comes to \$100 per child born. In this context, the long-term costs of early life smoke exposure dwarf the infant health costs.

## VIII. 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 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 caused by this exposure.

Based on my estimates, what can be said about the economic benefits of increases in cigarette taxes in the United States? Between 1980 and 2007, state cigarette taxes increased by \$0.80 on average in 2009 dollars.[31] For an average-sized cohort of four million children, an \$0.80 tax increase amounted to a \$5.20 billion improvement in welfare for the affected children.[32] Thus, my work demonstrates that policies designed to shield infants from cigarette smoke can potentially have large societal returns. One novelty of this paper is that it examines the medium-term outcomes of an early life shock. As the cohorts affected by the tax hikes of the 1990s mature, future research can look at how early life smoke exposure influences labor market outcomes and health in adulthood.

[30] Ages 6 to 17 reflects 12 years for sick days from school, and 24 months to 17 years reflects around 15 years for the other outcomes. 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.

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

[32] Average cohort size derived from the 1989–2004 vital statistics data. The health benefits per child of \$1,626 are taken from online Appendix Table A-11 multiplied by 0.8 for an 80 cent tax increase.

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