IAAEU Discussion Paper Series in Economics
No. 03/2017

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May 2017

Institute for Labour Law and Industrial Relations in the European Union (IAAEU)
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The effect of cigarette taxes during pregnancy on educational outcomes of the next generation*

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May 18, 2017

Abstract

Smoking during pregnancy is most common among women with a low socioeconomic status and is negatively associated with important infant health measures such as birth weight. Cigarette taxes decrease smoking amongst pregnant women, thereby leading to improved birth outcomes. In this paper we investigate whether increasing cigarette taxes can reduce the intergenerational transmission of a low socioeconomic status by reducing smoking rates among pregnant women with low educational attainment. In a first step, we exploit variation in cigarette taxes across U.S. states over time to show that increasing cigarette taxes leads to improvements in the health of newborns which are larger for babies of low educated mothers. In a second step, we look at subsequent educational success of 16-year-olds measured by grade retention and school enrollment in a large sample of adolescents. We find that increasing cigarette taxes improves the outcomes of children from a low socioeconomic background, but find no effects among children from a higher socioeconomic background. Our findings therefore suggest that cigarette taxes can be an effective policy instrument for mitigating the propagation of a low socioeconomic status from one generation to the next.

JEL Classification: I12, I14, I24.

Keywords: Early Life, Tobacco Taxes, Socioeconomic Inequalities.

*We would like to thank Eric Bettinger, Nadia Campaniello, Diego Cortes Huber, Owen O'Donnell, Christopher Roth, Emilia Soldani, Johannes Wohlfart, seminar participants at the Universities of Frankfurt, Mainz and Karlsruhe as well as participants at the 1st joint seminar by the GSEFM and the IPP and at the IAAEU Workshop on Labor Economics for helpful comments. We also thank Sergej Samoilov for excellent research assistance.

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1 Introduction

Important life outcomes such as health, education and income are highly correlated across generations. Consequently, socioeconomic inequalities are persistent (Currie and Hyson, 1999; Currie, 2011; Case et al., 2002; Currie, 2009; Aizer and Currie, 2014). Already by the time of birth, socioeconomic gradients are sizeable. This can be measured by the birth weight of infants, which is the best available proxy for newborn health and a powerful predictor of later life outcomes such as education, later life health (Case et al., 2005; Currie, 2009), life expectancy (Van den Berg et al., 2006; Oreopoulos et al., 2008) and labor market outcomes (Black et al., 2007; Currie and Hyson, 1999; Behrman and Rosenzweig, 2004). Reasons for such early gradients are access to and use of medical care and family planning, environmental factors such as pollution, the mother's health and her nutrition during pregnancy as well as health behaviors such as smoking (Aizer and Currie, 2014; Currie, 2009). Mothers from less advantaged socioeconomic backgrounds on average do worse on all these factors (Cutler and Lleras-Muney, 2010; Phares et al., 2004; Aizer and Currie, 2014).

Public health professionals have identified smoking during pregnancy as the largest risk factor for low birth weight that can be modified by maternal behavior (Kramer, 1987; Shiono and Behrman, 1995). Moreover, socioeconomic differences in this health behavior are strong: Smoking rates in the U.S. are around seven times higher for the lowest income group than for the highest (CDC, 2016a).[1] Smoking is therefore a potential channel for the propagation of socioeconomic inequalities from one generation to the next. Cigarette taxes, in turn, have been shown to reduce smoking during pregnancy, leading to improved birth outcomes (Evans and Ringel, 1999). Reactions to tax increases can be expected to be stronger amongst women with a low socioeconomic status due to a higher incidence of smoking and because poorer women are likely to be more price sensitive than others.

In this paper we study the potential of cigarette taxes in reducing the propagation of socioeconomic inequalities across generations. We do this by analyzing whether cigarette taxes during pregnancy have heterogenous effects across socioeconomic groups at different points in the lifecycle of the offspring. Namely we look at effects on smoking during pregnancy, on infant health and on later life educational outcomes. If cigarette taxes are most effective on the least privileged mothers they will have a diminishing effect on the socioeconomic gradient in smoking behavior. If tax effects translate into improved birth outcomes and later life educational outcomes, then corresponding differences in these outcomes between socioeconomic groups will be reduced as well. One advantage of tobacco taxes as a means of improving relative outcomes of

[1] The lowest income group corresponds to less than 10.000 USD income per year (22.3% of mothers smoked during pregnancy), the highest income group is 50.000 USD or more (2.7% of mothers smoked during pregnancy.) These numbers are from 2011.

underprivileged groups is their political implementability. Inequalities can be reduced without a direct redistribution from rich to poor being necessary.

We exploit variation in cigarette excise taxes across U.S. states and over time and use data from two large cross-sectional datasets: The Pregnancy Risk Assessment Monitoring System (PRAMS), providing data on maternal smoking behavior and birth outcomes of live births and the American Community Survey (ACS), providing data on educational outcomes of children and adolescents. In both data sets, information on mothers' educational attainment allows us to analyze heterogenous cigarette tax effects across three broad maternal education groups as proxies for socioeconomic status: i) below high school (less than 12 years education), ii) high school (12 years education), iii) some college or more (more than 12 years education).

We find that cigarette taxes have a strong and significant effect on mothers from the lowest socioeconomic group, i.e. these mothers smoke less in response to increased cigarette taxes. Consequently, their babies benefit in terms of better health at birth. Specifically, we find that a typical tax increase of 10 cents leads to an average weight gain of 2 grams for babies from the lowest socioeconomic group.[2]

Importantly, there are significant effects at the lower end of the weight distribution where weight gains matter most for infant health and for later life outcomes. For each 10 cents tax increase a baby from our lowest socioeconomic group is 0.07 percentage points less likely to weigh below 2500 grams, the common threshold for a low birth weight. This effect is sizeable, corresponding to more than 10 percent of the difference in the outcome variable between the lowest and the intermediate maternal education group.

In line with the positive cigarette tax effects on birth weight, we find suggestive evidence for corresponding effects on prematurity and on a measure of growth retardation.[3] Effect sizes correspond to 5 and 10 percent of the gap to the intermediate socioeconomic group. Especially the latter measure is highly important given that it has been shown to drive the correlation between birth weight and later life health outcomes (Godfrey and Barker, 2001). All effect sizes decrease with maternal education and are economically and statistically insignificant for the highest socioeconomic group. We find no evidence that cigarette taxes affect the probability of the baby surviving the first weeks of its life.

Our results also show that improved infant health translates into better educational outcomes

[2]In our sample period for the PRAMS sample the average cigarette tax increase amounted to 35 cents in real 1998-USD.

[3]Growth retardation stands for a low weight relative to the gestational age of the baby.

at age 16. Several proxies for human capital accumulation at age 16 are available all of which are reported by the head of the household, i.e. in most cases the father or mother of the adolescent.

For each tax increase of 10 cents we find a positive effect of around one percentage point on whether a 16-year-old from the lowest socioeconomic group has completed the ninth grade by the time of being surveyed.[4] Similarly, there are positive effects on a measure of school enrollment and decreasing effects on the probability of grade retention at any time before being surveyed. Lastly, we also provide evidence that the observed tax effects on educational outcomes are at least partially driven by observed cognitive or non-cognitive skills of the adolescent as reported by the head of the household. The effect size of a typical cigarette tax increase of 10 cents corresponds to a reduction in the gap between the lowest and the intermediate socioeconomic group by around 10 percent across all outcomes.

Looking at a restricted set of educational outcomes available for younger children we also show that heterogenous cigarette tax effects are already measurable at age 8 to 15. Taken together, our results suggest that increased cigarette taxes have positive effects on the human capital accumulation of less privileged adolescents, thereby mitigating the transmission of socioeconomic differences to the next generation.

Our results are robust to controlling for a large number of family characteristics as well as state level policies that have been shown to affect infant health and later life educational outcomes. Regarding birth outcomes we show robustness of our results to the quality of prenatal care and other individual-level controls that could systematically differ at the birth cohort level. For the estimated effects on human capital at age 16 we make sure that our results are not spuriously driven by a changing demographic composition of the states' population over time. Moreover, we show robustness to a large range of different specifications. We control for the family's income and mother's and father's employment to make sure that these measures are not driving our results. Furthermore, we control for a large range of state level policies during pregnancy (such as eligibility thresholds for medicaid, smoking bans and the applicable beer tax) and after pregnancy (such as school finance reforms and current cigarette taxes). Even though we allow for the effects of all additional controls to differ by mother's education our estimated cigarette tax effect remains economically and statistically significant.

Previous research gave evidence for a positive effect of cigarette taxes during pregnancy on birth outcomes (Evans and Ringel, 1999; Lien and Evans, 2005) and also showed effects on physical health during childhood and adolescence (Simon, 2016). Our paper is, to the best of our

[4] In our sample period for the ACS sample the average cigarette tax increase amounted to 12 cents in real 1998-USD.

knowledge, the first to examine the effects of cigarette taxes during pregnancy on the educational success of the next generation and in particular to study their potential in mitigating the propagation of socioeconomic differences from one generation to the next.[5]

In the study most closely related to ours, Simon (2016) uses two cross-sectional datasets in order to analyze the effect of cigarette taxes on birth weight and on the physical health of 2- to 17-year-olds. He finds that a one USD higher cigarette tax during the last term of pregnancy leads to a sizeable decrease in sick days from school and in the probability of having two ore more doctor visits per year. We extend his finding by showing that cigarette taxes also have the potential to improve educational outcomes of the next generation.[6]

At a more general level, our paper also adds to the literature examining the overall effect of cigarette taxes on smoking behavior during pregnancy and on infant health (Evans and Ringel, 1999; Bradford, 2003; Gruber and Köszegi, 2001; Colman et al., 2003; Adams et al., 2012; Lien and Evans, 2005; Philip DeCiccca, 2012).[7] Some studies have examined heterogenous cigarette tax effects on pregnant women from different socioeconomic groups. They found mixed evidence, with one pointing to higher price elasticities of more highly educated mothers (Ringel and Evans, 2001) and a majority pointing to stronger effects for lower educated mothers (Hawkins and Baum, 2014; Markowitz et al., 2013; Simon, 2016). We complement this literature by looking at a larger number of infant health measures than existing studies. Our results are compelling because all our estimated effects together provide consistent evidence for a positive effect of cigarette taxes on infant health that is stronger amongst lower socioeconomic groups.

We also contribute to a growing literature on the effect of the early life environment on later life educational and other human capital outcomes.[8] Existing studies exploit variation from natural and policy experiments such as legislation on alcohol availability (Nilsson, forthcoming), the introduction of food stamps (Hoynes et al., 2016), the 1918 influenza pandemic (Almond, 2006), fasting during ramadan (Almond et al., 2015), radioactive pollution due to the 1986 Chernobyl meltdown (Almond et al., 2009) or the Clean Air Act of 1970 (Isen et al., forthcoming). They all point to the importance of the early life environment on measures of human capital across the lifecycle. We study the effect of a specific policy intervention that is easy to influence and

[5]Education is a strong predictor of later life income and other proxies of socioeconomic status (Card, 1999; Waldfogel et al., 2005; Muennig, 2005).

[6]The results of Simon (2016) are highly relevant to our study. Especially the tax effect on days of sick leave from school suggests itself as one potential channel through which educational outcomes may be affected by prenatal cigarette tax exposure.

[7]We also contribute to the literature on the causal effects of smoking during pregnancy on infant health that uses identification strategies ranging from controlled experiments (Sexton and Hebel, 1984) to sibling studies (Yan, 2013; Tominey, 2007), minimum ages for cigarette purchase (Yan, 2014) and the 1998 Master Settlement Agreement (Levy and Meara, 2006).

[8]For an overview on the early life origins of human capital development, see also Currie and Almond (2011) and for the early life origins of general life-cycle well-being see Currie and Rossin-Slater (2015).

therefore especially interesting for policy makers.

Another related strand of literature links birth weight, as a proxy for the prenatal environment, to later life educational outcomes. Existing studies use data on twins and siblings (Black et al., 2007; Royer, 2009; Oreopoulos et al., 2008; Currie and Moretti, 2007; Behrman and Rosenzweig, 2004). Effect sizes could however be underestimated due to compensatory prenatal investment or overestimated due to larger perinatal investments into higher birth weight children. Omitted variables could further bias results. Our study adds to this literature by credibly identifying causal effects of cigarette taxes as one important aspect of the early life environment on outcomes at different stages in the life cycle.[9]

Lastly, our paper contributes to analyzing the mechanisms behind the widely observed propagation of socioeconomic status from one generation to the next. The role of the family and socioeconomic background for an individual's human capital development is recognized but not fully understood (Coleman, 1966; Case et al., 2002; Currie and Hyson, 1999). Aizer and Currie (2014) and Currie (2009) propose different environmental and behavioral mechanisms that differ systematically across socioeconomic groups and that are likely to be drivers of the propagation of inequalities acting prior to birth. Our study sheds light on one such mechanism by making use of the arguably exogenous variation in cigarette taxes and analyzing short- and medium-term effects on the next generation.

The rest of the paper is structured as follows: Section 2 provides background information on potential effects of cigarette taxes on the development of the exposed fetus, Section 3 describes the data and section 4 explains the empirical specification. Results are presented in section 5, followed by robustness checks in section 6. Section 7 interprets the magnitude of the estimated effects and Section 8 concludes.

[9]In order to find out what effect prenatal influences have on birth outcomes and how early consequences translate into later life outcomes, a panel data set would be ideal, following individuals exposed to and not exposed to a given influence over the life cycle. Unfortunately, such data are scarce and sample sizes are often too low to detect effects such as those from cigarette taxes. One solution that is possible with existing data is to look at repeated cross sections drawn from the same population, which is our approach. There are few studies to date looking at the effect of the same prenatal influence at more than one point in life. One notable exception is the above-described paper by Simon (2016). Another exception, with data on infant health and later life outcomes drawn from different populations, is the analysis of the effects of in-utero exposure to Ramadan fasting. Almond and Mazumder (2011) find effects of being in utero during Ramadan on birth outcomes using data from the U.S. and on later life health outcomes using data from Uganda and Iraq. Almond et al. (2015) add evidence on later life educational outcomes, using data from England.

2 Background

Cigarette taxes might have a positive effect on educational outcomes via a number of channels. First, smoking during pregnancy could affect the educational outcomes of the offspring by affecting the brain development before birth, and hence directly restricting the offspring's to acquire cognitive and non-cognitive skills during childhood. Second, it may indirectly affect educational outcomes by damaging the baby's physical health prior to birth and thereby impeding cognitive and non-cognitive development later on.

Starting with potential direct effects on the brain development of the fetus, the most relevant factor are the more than 4000 chemicals that are inhaled when smoking a cigarette (Thielen et al., 2008). Some of these chemicals are believed to potentially cause cellular damage through changes in cell structure and hormone levels (Dempsey and Benowitz 2001) and could therefore directly alter the brain development of the fetus. Nicotine, for instance, crosses the placenta and the concentration of nicotine in the embryo can be 15% higher than in the body of the mother (Lambers and Clark, 1996). Nicotine receptors are present very early in the brain of the fetus (Hellström-Lindahl and Nordberg, 2002) and have an important role in brain development (Navarro et al., 1989). Therefore, nicotine could potentially lead to brain damage (Shea and Steiner, 2008). Correlational evidence suggests effects of prenatal tobacco exposure on educational outcomes through delayed or inhibited mental development (Kiechl-Kohlendorfer et al., 2010; Kable et al., 2009) and behavioral outcomes such as physical aggression during early childhood (Huijbregts et al., 2007, 2008) amongst others. Moreover, animal experiments suggest that the cognitive development of animals exposed to nicotine in early life is influenced, even when nicotine levels are too small to have an effect on birth weight (Slotkin, 1998).

Regarding indirect effects, there are several channels through which smoking could have an impact on later life human capital accumulation. First, smoking has an anorexigenic effect, i.e. the mother has less appetite, she might end up eating less and her baby gets less nutrients (Bergen, 2006). Second, smoking leads to a narrowing of blood vessels. Consequently, the blood flow to the placenta is restricted and again, the baby gets less oxygen and nutrients (Lindblad et al., 1988). Third, the inhaled carbon monoxide occupies the hemoglobin in the red blood cells, thereby inhibiting the transportation of oxygen to the fetus (Bureau et al., 1983). All these factors can lead to consequences that will potentially be reflected in birth weight as a proxy for nutritional intake of the fetus. A low birth weight will, on expectation, have consequences over the life-cycle of the offspring (Currie and Hyson, 1999). Lastly, nicotine also hinders the movement of the embryo, which could retard the development of the child's nervous system (CDC, 2010).

3 Data

We use two large cross-sectional data sets covering the different stages of the life cycle we are interested in. First, we use data from the Pregnancy Risk Assessment Monitoring System (PRAMS) on smoking behavior during pregnancy and on birth outcomes reported on a monthly level. Second, we make use of the American Community Survey (ACS) for the analysis of educational outcomes of adolescents. We look at the years 1988-2013 in the PRAMS sample and at 16-year-olds born in 1989-1998 in the ACS.[10][11]

Our main independent variable are state level cigarette taxes. Nominal cigarette taxes reported on a daily level come from Orzechowski and Walker (2014).[12] We deflate them using the seasonally adjusted monthly CPI provided by the U.S. Bureau of Economic Analysis (2016).[13]

3.1 PRAMS Data

The Pregnancy Risk Assessment Monitoring System (PRAMS), is a comprehensive data set collected by the CDC (2016b) in cooperation with the health departments of participating states. It contains information on birth outcomes and on maternal experiences before, during and shortly after pregnancy for a sample of live births. Mothers are selected based on a stratified sampling design with an oversampling of women at risk for adverse pregnancy outcomes (i.e. low birth weight births, minority races). States are allowed to choose their own stratification criteria but data collection has to follow standardized guidelines developed by the CDC. Probability weights are provided in order to ensure that the sample is representative for the underlying population of recent mothers in the U.S. states that are part of the sample. Sample sizes per state and month generally range between 100 and 250 observations.

The PRAMS data contain information from birth certificates as well information reported by mothers via questionnaires shortly after birth. Data from both sources are combined and only states that reach response rates to questionnaires of at least 70 percent in a given year are included in the PRAMS data set. All measures of infant health we use in our analysis are sourced from birth certificates[14] and are therefore reported with high precision and completeness. The

[10] Unfortunately we cannot restrict the PRAMS sample to include the same cohorts as the ACS sample for robustness checks, the reason being that the PRAMS dataset includes too few states in the earlier years. Despite this non-perfect overlap of survey years the PRAMS and the ACS dataset together provide a coherent set of results.

[11] We provide additional evidence by looking at 8-15 year olds and at 17-year olds. The respective cohorts were born in 1990-2006 and 1988-1997.

[12] For New York City, which appears as a separate entity in the PRAMS data, we manually added the city tobacco tax to the state-level tax of New York State.

[13] The base period was set to June 1998.

[14] The only exception is the variable "Baby Alive", which strictly speaking is not a birth outcome variable but a measure for whether the baby was alive at the time of being sampled for the PRAMS dataset.

indicator of smoking behavior is self-reported and will be discussed below.

A total of 38 PRAMS sites granted us access to at least one year of data available during the period 1988 to 2013. We dropped observations where birth weights were below 500 grams because systematic reporting according to the WHO guideline starts at 500 grams. Observations with birth weights above 6000 grams were also dropped, just like twins and other multiple births. We analyze tax effects on the following outcome variables:

- **Smoker:** A dummy taking on the value one when the mother smoked during pregnancy
- **Birth Weight:** The birth weight of the newborn measured in grams and grouped in 250g-brackets
- **LBW:** A dummy for whether the baby's birth weight was below <2500 grams[15]
- **Prematurity:** A dummy for prematurity of the baby measured as gestational age below 37 weeks[16]
- **SGA 10th pctl.:** A dummy for whether the baby was small for its gestational age based on the 10th percentile in its gestational age group[17]
- **Baby Alive:** A dummy whether the baby was still alive at the time of sampling for the PRAMS dataset.[18]

Regarding the smoking dummy we use as our measure of maternal smoking behavior, there are two potential problems. First, there are observations where the smoking dummy is missing (2.4% of the sample). This is not surprising given that such behavior is a sensitive topic. Under our highly restrictive specification, however, there is no significant correlation between the cigarette tax and the probability of the smoking dummy to be missing. From this we can conclude that the missing observations themselves are unlikely to cause bias. Second, non-missing but falsely reported smoking behavior might be an issue. Studies comparing self-reported smoking with results from blood tests report that up to 20-30% of smoking mothers claim they did not smoke during pregnancy (Brachet, 2008; Dietz et al., 2011; Klebanoff et al., 2001). Even though the PRAMS data set was shown to have higher reported smoking rates than other available data sets (Colman et al., 2003), the estimated cigarette tax effect on smoking behavior should be

[15] 2500 grams is the common cutoff below which a newborn is considered to be at risk for severe health and developmental difficulties. (Almond et al., 2005)

[16] 37 weeks is the cut-off for prematurity as defined by the WHO

[17] Growth retardation, as measured by a low birth weight relative to gestational age, has not been analyzed in any previous study establishing causality in the context of prenatal smoking exposure/cigarette taxes. It is highly relevant, however, because existing research linking birth weight to later life health outcomes such as coronary heart disease, stroke, hypertension and diabetes has found that associations "depend on lower birth weight in relation to gestation rather than the effects of premature birth" (Godfrey and Barker, 2001)

[18] The time at which it is recorded in the dataset whether the baby is alive is a bit unsystematic. The variable can be yes based upon information from the birth certificate (at the time of sampling, limited to 2-6 months after birth, but usually 2-4 months), or when state PRAMS personnel compare the birth certificate number to the infant death registry at the time of mailing (usually within a week or two of the sample).

interpreted with care.[19] We will therefore consider tax effects on birth outcomes as our primary evidence.[20]

Summary statistics for the outcome variables and the main regressors are given in Table 1. Summary statistics for the large range of other covariates can be found in Table A.1 in the appendix. Figure 2 illustrates the social gradient in smoking during pregnancy and in infant health.

[Insert Table 1 and Figure 2]

3.2 ACS Data

The American Community Survey is conducted once per year by the Census Bureau, providing a representative sample of the U.S. population (Ruggles et al., 2015). The ACS is a repeated cross-sectional dataset covering more than 3 million households and group quarters[21] per year. Household heads from selected households are asked to provide information on all members of the household. Responses to the ACS are required by law and response rates are exceptionally high[22].

We look at the ACS samples for the years 2005 until 2014 because the quarter of birth of each respondent is first reported in 2005. This variable allows us a matching of prenatal cigarette taxes to respondents on a quarterly basis. We first drop all individuals born outside the U.S. to include only those that were exposed to cigarette taxes in a U.S. state. Next, we confine the sample to 16-year-olds born during the first half of the year that live in the same household as their mothers. In Table 2 we provide evidence that our sample of 16-year-olds cannot be distinguished from the full population of 16-year-olds born in the U.S., based on observable characteristics. In Table A.2 of the appendix we show in addition that there is no sample selection based on cigarette taxes.

We include only individuals living with their mother because we want to proxy socioeconomic status by mother's education similar to the PRAMS sample. Maternal characteristics can only be observed for individuals who live in the same household as their mothers. We therefore drop

[19] If misreporting is unrelated to cigarette tax increases it should simply add noise to the data and thereby lead to a downward bias of the estimated tax coefficient.

[20] The PRAMS dataset also includes self-reporeted information on the number of cigarettes smoked per day. We do not use this information in our regressions due to low reporting quality. Namely, information on the daily quantity of cigarettes is missing for more smokers than non-smokers and in some cases it is entirely censored at the state-level. However, we do use reported information on numbers of cigarettes in order to cross-validate the reported smoking dummy. Specifically, we exclude those observations from the smoking regressions where mothers indicated that they smoked during pregnancy but reported zero cigarettes as well as those where mothers indicating they did not smoke yet reported a strictly positive number of cigarettes per day. These we assume to be reporting errors. They make up less than 0.06 percent of the total sample.

[21] Group quarters include college/university student housing, residential treatment centers, skilled nursing facilities, group homes, military barracks, correctional facilities, workers' group living quarters and Job Corps centers, and emergency and transitional shelters.

[22] Usually around 98% of the selected households are included in the final dataset. The year 2013 is an exception: Due to the government shutdown, the ACS did not have a second mailing, a telephone followup, or a person followup for the October 2013 housing unit panel. Therefore, 2013 response rates amount to 90%.

individuals living in group quarters (0.9% of all 16-year olds) and individuals who live in a household but whose mother was not identified in the same household. Educational attainment of the mother is reported for more than 99% of our remaining sample.[23]

We focus on 16-year-olds instead of older individuals, first, because school enrollment is obligatory in all states at least until age 16 and has been so throughout the survey years we consider. Consequently, state- and cohort-specific differences in the respective legislation cannot potentially bias our results. Second, at age 16, 88 percent of our remaining sample of 16-year-olds still live in the same household as their mother, which is barely less than at age 10 (90 percent). This share starts to shrink at age 17 and drops to 80 percent by age 18. The decision to move out from home around age 18 may potentially be an outcome affected by cigarette taxes during pregnancy.

We focus at 16-year-olds instead of younger children because in the ACS, educational attainment is only reported in detail starting from grade 9. Also, some of the main educational outcomes reported in the ACS, especially grade retention, get more common as children get older. Nevertheless, in Section 5.2 we also provide evidence on the effect of cigarette taxes on school enrollment and cognitive difficulties among younger age groups.

Lastly, we confine our sample to individuals born in the first and second quarter of the year. Cutoff dates for kindergarten enrollment usually fall between August and December, with a majority in September. For cohorts born in the third and fourth quarter, therefore, changes in these cutoff dates could make a difference in terms of having completed a certain grade at a given age. For children born in the first and second quarter such policy changes should make no difference.[24]

We look at several outcome variables to measure educational attainment of 16-year-olds:

- **Grade 9:** A dummy taking on the value one if Grade 9 has been completed by the individual. It takes on the value zero either because the individual is still going to school but has not yet completed grade 9 (i.e. grade retention) or because the individual dropped out of high school before having completed grade 9. Grade 9 completion by the age of 16 is a rather conservative benchmark. However, we believe that a tax effect should be most pronounced at the bottom of the educational achievement distribution.

[23] Identified mothers include confirmed mothers as indicated on survey questionnaires (>96 Percent of our sample), but also cases where in the absence of information the census bureau classified female household heads or other adult women living in the same household as most likely mothers (<3 Percent of our sample) and cases where mothers are classified as stepmothers based on marriage to the father (<0.05 Percent). Our results remain unchanged if we include only confirmed mothers.

[24] It has been shown that relative age within a school class affects academic performance of pupils (Bedard and Dhuey, 2006). Changes in cutoff-dates would slightly change the relative age even of individuals born in January to June. However, there is no reason why cigarette tax increases in utero and changes in cutoff-dates for school enrollment five years later should be systematically correlated across states. Even if there was some spurious correlation, it would be surprising that we find similar effects for different cohorts, looking at 8-11-year-olds, 12-15-year-olds, 16- and 17-year olds.

- **Enrolled:** A dummy taking on the value one if the individual was enrolled in a regular school[25] at any time in the three months before the ACS interview. For individuals that were not enrolled but had already finished Grade 10, the dummy is still set to one. This variable measures the extensive margin for not having completed Grade 9.

- **Grade 9|school:** A dummy taking on the value one if Grade 9 has been completed by the individual and zero if not but the individual is still enrolled at school. The dummy is set to missing if the individual is not enrolled in any regular school. This variable measures the intensive margin for not having completed Grade 9, i.e. grade retention.

- **Cogn. Diff.:** A dummy taking on the value one if the individual was reported by the respective household head to have any type of cognitive difficulty. The corresponding questionnaire item reads as follows: "The person has cognitive difficulties (such as learning, remembering, concentrating, or making decisions) because of a physical, mental, or emotional condition."

Summary statistics for our sample of 16-year-olds are shown in Table 3. Figure 3 illustrates the social gradient in educational outcomes at ages 12 and 16. All variables for this figure are coded such that a higher bar stands for a more negative outcome. The bars labelled "retention" stand for the share of 12 (16) year-olds that have not yet completed grade 5 (grade 9) at the time of the survey. Not surprisingly, retention rates increase from age 12 to age 16 because underlying causes are accumulating over time and result in the observed event at some point in time. However, the socioeconomic gap in retention rates is already visible by the age of 12. In contrast, the share of children and adolescents that are reported to not be enrolled in a school is similar across socioeconomic groups at age 12. This suggests that at younger ages, determinants of enrollment are less related to family background. Over the years the socioeconomic gap increases for this outcome as well and by the age of 16, a larger fraction of children from the lowest socioeconomic group have left school than in the other two groups. Cognitive difficulties as perceived by the head of household, however, seem to be visible by age 12 already.

[Insert Table 2, Table 3 and Figure 3]

3.3 State cigarette taxes

In our sample period between 1988 and 2013 there were 206 tax increases by U.S. states, amounting to 35 cents (1998 USD) on average and leading to an increase in real average state level cigarette taxes of more than 700 percent.[26] For a detailed description of the tax decision process

[25] Regular schools encompass all schools leading toward a high school diploma or college degree. Other types of schooling count only if a regular school or college would have accepted it for credit.

[26] The average tax per package increased from 0,19 to 1.63 (1998-USD) over this period. Regarding the shorter sample period of the ACS sample between 1988 and 1998, there were 84 tax increases in the U.S., with an average size of increases of 14 cents (1998 USD).

we refer to Simon (2016) who convincingly argues that cigarette taxes are unlikely to be correlated with sudden shifts in public attitudes towards smoking.[27]

In order to map the applicable cigarette tax rate to each observation we calculate the date of conception with as much precision as allowed by the data: In the PRAMS-sample, we observe births in different month-year-periods, in the ACS-sample we can estimate quarter-year-periods in which individuals were born. For each individual we compute the average tax rate in the nine months following the estimated date of conception.[28]

3.3.1 Applicable tax rates for the PRAMS sample

Figure 1 gives an overview of the cigarette tax variation over time for the states (plus New York City) covered by PRAMS data.[29]

[Insert Figure 1]

In the PRAMS sample, the month and year of birth of the baby are available, as well as a categorical variable on the length of gestation. We assigned each of the five available categories an approximate duration in months, as described in Section B in the appendix and calculate the approximate date of conception, assuming that all babies were conceived in the middle of the respective month. We then used the mean applicable cigarette tax in the state of birth in the nine months after the estimated month of conception.[30] Thereby we derive the following cigarette tax rate, applicable for individual i born in state s an conceived at time period t as follows:

$$Tax_{ist} = \frac{1}{9}\sum_{m=t}^{t+8} Tax_{sm}$$

Tax_{sm} stands for the average tax rate between the 15th of month m and the 15th of month $m+1$.

[27] We refer specifically to the online appendix B of Simon (2016) where the legal processes behind the tax decision process are described and the use of tax funds is outlined.

[28] Some existing studies exploit tax variation during the last three months of pregnancy because this period was shown to be most important for birth weight effects (United States Department of Health and Human Services, 2001). For the development of cognitive and non-cognitive capacities leading to later life school success, it is unclear, however, which are the sensitive periods during fetal development. The brain and central nervous system of a fetus start to develop already 16 days after conception when the neural plate forms which then evolves into the baby's brain in week six or seven of pregnancy. Several studies suggest that especially smoking early in pregnancy is associated with cognitive damage (Falk et al., 2005; Roza et al., 2007). Similarly, (Almond et al., 2015) show that exposure to Ramadan during the first months of pregnancy has larger negative effects on children's educational performance than exposure during later months.

[29] New Hampshire and Iowa joined the PRAMS in 2013 (which is the last year covered by the data) and are therefore not included in the tax graph. Nevertheless, due to the monthly tax variation we exploit, the two states are still included in the respective regressions.

[30] We decided against using the mean tax during the actual individual months of pregnancy because this could lead to an upward-bias in our estimated tax effect on a range of birth outcomes. For instance a baby that was born prematurely would be assigned a lower cigarette tax and at the same time would have a lower birth weight and gestational age. This correlation would be captured by the tax coefficient.

3.3.2 Applicable tax rates for the ACS sample

The ACS reports the age of each individual at the moment of being surveyed and also their quarter of birth. However, no information on year of birth is given and the exact date of interview is unknown. We know, however, that survey dates are approximately uniformly distributed over the months January to December. This allows us to construct weighted averages of the applicable cigarette tax rates in the two possible years of birth of each individual, with the weights corresponding to the probability of the individual being born in each of the two consecutive years in question.[31]

We know that for seven out of eight individuals born in the first quarter of the year we have $BirthYear = SurveyYear - Age$ because these individuals had their birthday in the survey year before the time of being surveyed. For the remaining one out of eight, the true year of birth is $BirthYear = SurveyYear - Age - 1$. For those individuals born between April and June, the probability of having one's birthday after the interview date is higher: For them, three out of eight individuals get interviewed before their birthday and five after their birthday. To minimize deviations from true birth dates we assume that all individuals born in Q1 were born on the 15th of February and all individuals born in Q2 were born on the 15th of May (treating all months are equally long). Also, given that we have no information about the length of gestation of each individual, we assume that all individuals were in utero for nine months. Based on these considerations we calculate the applicable tax rate for individual i born in state s in period t as follows:

$$Tax_{ist} = \begin{cases} \frac{7}{8}(\frac{1}{9} \sum_{m=May(y-1)}^{Feb(y)} Tax_{sm}) + \frac{1}{8}(\frac{1}{9} \sum_{m=May(y-2)}^{Feb(y-1)} Tax_{sm}), & \text{if Q=1} \\ \\ \frac{5}{8}(\frac{1}{9} \sum_{m=Aug(y-1)}^{May(y)} Tax_{sm}) + \frac{3}{8}(\frac{1}{9} \sum_{m=Aug(y-2)}^{May(y-1)} Tax_{sm}), & \text{if Q=2} \end{cases}$$

where t stands for the time period defined by quarter Q and year y in which the individual was conceived in case the ACS interview took place after the individual's birthday in the survey year. Tax_{sm} stands for the average tax rate between the 15th of month m and the 15th of month $m+1$. The described way of mapping taxes obviously introduces measurement error to the data. This does not affect the internal validity of our findings, however, given that if anything the estimated cigarette tax coefficient will be biased towards zero.

[31] One simplyfing assumption we make is that all months have equal length and that birth dates are uniformly distributed over time.

4 Empirical Specification

4.1 General Set-up

We want to analyze how cigarette taxes affect smoking behavior of pregnant women, birth outcomes and educational outcomes of 16-year-olds. Our estimating equation is similar across outcome variables.

For average effects (regardless of socioeconomic background), we estimate the following specification:

$$Y_{ist} = \alpha \text{Tax}_{st} + \Pi^T \mathbf{X}_{ist} + \beta_t + \rho_s + \gamma_s t + u_{ist} \tag{1}$$

for individual i, conceived at time t and born in state s. Tax_{ist} is the applicable tax during pregnancy, β_t are time of conception dummies, ρ_s are state dummies and $\gamma_s t$ are state-specific time trends. Time units correspond to month-year-periods in the PRAMS-sample and to quarter-year-periods in the ACS sample. The identifying variation in Tax_{ist} stems from tax changes within states over time that deviate from a linear trend. $\mathbf{X}_{ist}$ is a vector of individual characteristics including mother's age at giving birth, her education at the moment of being surveyed and the individual child's gender. The additional set of controls in $\mathbf{X}_{ist}$ differs depending on the sample. When using the PRAMS data our main specification controls for a large number of dummies covering the following information: Mother's race, her marriage status, a measure of self-reported stressors she experienced during pregnancy, her pregnancy intention[32], the Kessner-index for prenatal care as assessed by a doctor[33], the questionnaire phase[34]. In all regressions using the ACS, respectively, $\mathbf{X}_{ist}$ includes dummies for individual *i's* race. We do not control for any further characteristics in the ACS-regressions because other available variables were determined after birth and could therefore potentially be affected by cigarette taxes.

Our main interest lies in heterogeneous tax effects across socioeconomic groups as proxied by mothers' educational attainment. For the analysis of these heterogeneous effects we estimate the following specification:

$$Y_{ist} = \alpha_1 \text{Tax}_{ist} + \alpha_2 (\text{mom HS})\text{Tax}_{ist} + \alpha_3 (\text{mom >HS})\text{Tax}_{ist} + \Pi^T \mathbf{X}_{ist} + \beta_t + \rho_s + \gamma_s t + u_{ist} \tag{2}$$

where (mom HS) is a dummy taking on the value 1 for all mothers from the intermediate ed-

[32] i.e. whether she wanted to become pregnant now, later, sooner, never

[33] The Kessner index is reported on the birth certificate on a four-item scale comprising the levels "adequate", "intermediate", "inadequate", "unknown".

[34] The PRAMS questionnaire was reviewed 6 times after its first version in 1988.

ucation group[35] and (mom >HS) stands for the highest education group[36] respectively. Consequently the coefficient α_1 measures the cigarette tax effect on the lowest education group[37]. The vector $\mathbf{X}_{ist}$ includes the variables described above. All regressions take account of sampling weights. Standard errors are clustered on state-level.

5 Results

5.1 The effect of cigarette taxes on smoking during pregnancy and birth outcomes

Tables 4 shows the main results for mother's smoking during pregnancy and subsequent birth outcomes, using PRAMS data. Panel A shows that on aggregate, cigarette taxes decrease smoking during pregnancy and thereby improve a range of birth outcomes. For instance the effect in Column (3) suggests that a 10 cent increase in the applicable cigarette tax rate during pregnancy decreases the probability that a baby weighs less than 2500 grams at birth by 0.04 percentage points. Surprisingly, the tax effect on mother's smoking in column (1) is insignificant, while the effect on the probability of having a low birth weight baby in Column (3) is significant. This might be due to noise in the data caused by misreported as described in section 3.1 above.

Table 4 also includes the price elasticities of smoking participation based on our regression results in squared brackets.[38] The overall price elasticity of participation in our sample is -0.42. This suggests that a one percent increase in the price of cigarettes reduces the likelihood of an individual woman to smoke during pregnancy by 0.42 percent. This number lies within the range of estimates in the existing literature.

Panel B reports heterogeneous tax effects. The coefficients in the first row represent the effect which cigarette taxes (measured in terms of 10 cent increases) during pregnancy have in the lowest socioeconomic group. The coefficients in the second and third row stand for the difference in the tax effect when the mother has a high school degree and at least some college. Adding up the first and the second coefficient, for instance, yields the cigarette tax effect in the intermediate socioeconomic group.

The estimated coefficients in Columns (1) suggest that mothers from the lowest socioeconomic group are 0.4 percentage points less likely to smoke when the applicable cigarette tax

[35] 12 years of education
[36] more than 12 years of education
[37] less than 12 years of education
[38] Details on the calculation of price elasticities can be found in Section C in the appendix.

reate inceases by 10 cents. The effect on mothers from the intermediate socioeconomic group are significantly smaller and those for the highest socioeconomic group are close to zero.

The differences are not only driven by a higher share of smokers in the lower socioeconomic groups. Price elasticities reported in squared brackets show that mothers from the lowest socioeconomic group are more than twice as price sensitive as mothers from the intermediate or from the highest socioeconomic group.

Column (2) shows that for each 10 cent increase in cigarette taxes, babies born to the lowest educated mothers gain around two grams on average. Correspondingly, the tax coefficient in Column (3) illustrates that the probability of a baby in the lowest socioeconomic group to weigh less than 2500grams at birth is reduced by 0.07 percentage points. This is quite a substantial effect size, corresponding to more than one tenth of the effect of the mother having a high school degree as compared to being a high school dropout.[39]

Columns (4) and (5) give further insights into reasons for weight gains. A higher birth weight can be driven by a longer gestation or by a higher birth weight relative to gestational age. Results in Column (4) provide suggestive evidence that babies stay in utero beyond the 37th week of gestation as a consequence of cigarette taxes and thereby have more time to gain important weight whereas Column (5) provides evidence for cigarette taxes mitigating growth retardation in the lowest socioeconomic group.

We find no evidence for a tax effect on whether the baby was still alive at the time of being sampled by the PRAMS, i.e. 2-6 months after birth (Column 6). The coefficients in Column (6) suggest that we do not need to worry about sample selection in our ACS-sample due to selective survival of the strongest babies after birth.[40]

The orders of magnitude of the tax coefficient on all measures of infant health are substantial. Depending on the outcome variable, a tax increase of 10 cents (in real 1998 USD) reduces the socioeconomic health gap between the lowest and the intermediate maternal education group by around 5 to 10 percent.

[Insert Table 4]

[39] In the sampling period of the PRAMS dataset the average tax increase was around 35 cents. One in five tax increases even exceeded 50 cents in terms of real-1998-USD. Assuming a linear tax effect, a 35 cents tax increase would reduce the difference between the lowest and the intermediate socioeconomic group in the probability of a low birth weight by more than one third.

[40] Unfortunately we cannot say anything about the effect of cigarette taxes on spontaneous abortions due to the PRAMS sample being restricted to life births. If there was a selection of the strongest infants based on smoking during pregnancy, however, this should lead to a downward bias of our estimated tax coefficient.

5.2 The effect of cigarette taxes on later life educational outcomes

Table 5 displays the results on the effect of prenatal cigarette tax exposure on educational attainment at age 16. All aggregate coefficients in Panel A point into the expected directions, indicating overall higher educational attainment of 16-year-olds due to cigarette taxes and less cognitive difficulties as assessed by the respective head of household. Similar to the infant health outcomes described before, the average effects are insignificant.

Panel B again illustrates heterogeneous tax effects. Column (1), for instance, suggests that for the lowest socioeconomic group a 10 cent tax increase during pregnancy increases the likelihood that the offspring has completed grade 9 by the age of 16 by around one percentage point. This effect size corresponds to around 10 percent of the educational difference to the intermediate socioeconomic group. As expected, estimated tax effects are significantly weaker for the intermediate socioeconomic group and there are no effects on the educational outcomes of the most privileged group.

Figure 4 illustrates this finding, looking at grade 9 completion rates. It is based on a probit regression with the same restrictive specification as the regression results in Table 5. The x-axis spans the 10th to 90th percentile in terms of real cigarette taxes in 1998-USD applicable for our sample of 16-year-olds. The Figure illustrates how the educational achievement gap at age 16 is significantly reduced when cigarette taxes increase.

Results in Columns (2) and (3) of Table 5 shed further light on how exactly cigarette taxes affect the measure of grade 9 completion. Namely, a 10 cents increase in the prenatally experienced cigarette tax leads to a 0.88 percentage points increase in the likelihood of being enrolled in school at the age of 16. Also, it reduces the likelihood of having experienced at least one grade retention prior to being surveyed by 0.65 percentage points. The first of these two effects is very large, especially when considering that school enrollment is obligatory at least until age 16 and that correspondingly, enrollment rates in this age group amount to more than 96 percent, even in the lowest socioeconomic group. The effect size in Column (2) corresponds to a reduction in the difference to the intermedicate socioeconomic group by almost 15 percent, that in Column (3) by a little less than 10 percent.

Finally, Column (4) provides evidence for the role of cognitive difficulties of the individual as reported by the father or mother in shaping the observed effects on educational outcomes. The measure in Column (4) does not distinguish between cognitive aspects such as intelligence and acquired knowledge and non-cognitive aspects such as self-motivation and emotional stability, to name just a few. Nevertheless, it provides credibility to estimated effects in Columns (1) - (3).

[Insert Table 5 and Figure 4]

It is important that the magnitudes of coefficients should not be compared directly between birth outcomes on the one hand (Table 4) and educational outcomes at age 16 on the other hand (Table 5). This is mainly for two reasons:

First, the two measures of socioeconomic status are not directly comparable. In Table 4, mother's educational attainment is measured around the time of giving birth, in Table 5 it is measured 16 years later, at the moment of being surveyed for the ACS. Within these 16 years, a lot of mothers, especially younger ones, complete high school and/or go to college. In the PRAMS sample, around 20 percent of mothers have less than a high school degree, compared to only 10 percent in the ACS. We can therefore expect any cigarette tax effect to be more pronounced in the lowest socioeconomic group of the ACS-sample given that the measure of low socioeconomic status is stricter.

Second, Tables 4 and 5 cover slightly different time periods. Whereas the ACS-sample is largely from the 1990s, the majority of data points in the PRAMS sample come from the years after 2000. One has to consider that average cigarette prices as well as price increases before the year 2000 were relatively low whereas after 2000 a number of large tax hikes was implemented[41].

Figure 5 illustrates the development of cigarette taxes between 1988 and 2013. There is a notable increase in the average cigarette tax as well as in the variation across U.S. states. We can expect that the moderate cigarette tax increases in the 1990s influenced the most price sensitive mothers and that the effect of cigarette taxes was smaller in the 2000s. This argument is supported by empirical findings (Simon, 2016; Levy and Meara, 2006). Nevertheless, we believe that our measured tax effects in both the early life stage and at age 16 give a consistent impression of the potential of cigarette taxes in lowering the propagation of socioeconomic inequalities over generations.

[Insert Figure 5]

We also examine the effect of cigarette taxes on educational and cognitive outcomes at a younger age and for 17-year-olds. Grade completion is not reported on a yearly basis below grade 9. Therefore, in Table 6, we look at cognitive difficulties (Columns (1)-(3)) and school enrollment rates (Columns (4)-(6)) of 8- to 15-year-olds.

[41] These differences result in different standard deviations in cigarette taxes across our two samples. Looking at Tables 1 and 3 we see that a standard deviation in cigarette taxes in our earlier ACS sample corresponds to 34 cents whereas in the later PRAMS sample a standard deviation corresponds to 82 cents in real 1998 Dollars.

Whereas the overall tax effect in Panel A is small and insignificant, heterogenous tax effects reported in Panel B show a similar pattern as for the sample of 16-year-olds. Looking at Columns (4) to (6), it is interesting is that both the effect of mother's education and the cigarette tax effect are larger for the slightly older age group of 12- to 15-year olds than for the youngest children. It seems that family background has an increasingly large effect with increasing age and that prenatal smoking is driving some of this effect.

In Table 7 we replicate our main findings for 17-year-olds. The estimated tax effect is similar to that observed among 16-year-olds. If anything, it seems to be a little stronger for the 17-year-olds. The heterogenous tax effect on cognitive difficulties in Column (4) appears insignificant due to large standard errors. The size of the coefficients, however, is comparable to that for 16-year-olds.

[Insert Table 6 and Table 7]

6 Robustness

In this section, we address concerns regarding the causality of our estimated tax effects. There could be omitted variables driving our results, such as policies that vary at the cohort level and influence our outcomes of interest.

6.1 Robustness of the tax effect on smoking and birth outcomes

For the robustness of the early life stage one potential confound could be that revenue generated from cigarette taxes is being used for the financing of the health sector. If this was the case, the observed tax effect would not be caused by changes in prenatal smoke exposure but by an improved quality of state-financed prenatal care, especially for mothers from a relatively low socioeconomic background. For this concern we refer to Simon (2016) who illustrates in detail that most of the cigarette tax revenue in the U.S. goes into the states' general fund or is earmarked for spending unrelated to health care.

We complement his argumentation in Table A.3 where we control for the individually experienced prenatal care of the mother with two different measures. Panel A repeats the baseline specification whereas in Panel B we add a dummy for whether the mother received state-financed supplementary nutrition (WIC) during her pregnancy.[42] Panel C controls for the Kessner index which is a measure for the quality of prenatal care as assessed by a doctor on the birth certificate.

[42] WIC is a program supplementing families at risk of poor pregnancy outcomes with additional healthy nutrition during pregnancy.

We allow all additional controls to vary by mother's education.[43]

Another potential confound could be that tax hikes might increase the general awareness in the population regarding the dangers of smoking during pregnancy. If this was the case, not the price of cigarettes but the public opinion towards smoking would be driving our results. In Panel D of Table A.3 we show evidence on the robustness of our results to whether the mother remembers being informed by her doctor about the dangers of smoking to her baby.

Our results remain economically and statistically significant in Panels B to D, showing that neither health care financing nor public awareness seem to be a concern for our specification.

Lastly, other tobacco policy changes enacted at the same time as cigarette tax changes could lead to an upward-bias in our estimated tax coefficients. One example for such policy changes are clean indoor air laws that are decided on a state level. Several existing studies have controlled for cigarette taxes and smoking bans at the same time, showing that tax effects on smoking during pregnancy and related birth outcomes are driven by cigarette taxes rather than clean indoor air laws (Simon, 2016; Evans and Ringel, 1999; Gruber and Köszegi, 2001; Markowitz et al., 2013; Hawkins and Baum, 2014; Adams et al., 2012).

[Insert Table A.3]

6.2 Robustness of the tax effect on educational outcomes

6.2.1 Robustness to changes in the demographic composition of the sample

Regarding the estimated tax effect on educational outcomes at age 16 we first make sure that our results are not driven by systematic changes in the sample composition that are correlated with cigarette taxes. Table 8 shows how cigarette taxes are correlated with the demographic composition of our sample across the three categories of maternal education. On average, girls have better school outcomes, just like Whites and Asians.

The tax coefficients in Columns (1) to (3) show that, if anything, the described changes in the sample composition should mitigate the heterogenous tax effect we find. However, most of the tax coefficients are insignificant.

[Insert Table 8]

[43] The Kessner Index is already included in our baseline specification. Now we allow its effect to be different according to maternal education group.

6.2.2 Robustness to split samples

In our baseline specification, state-specific time trends do not capture differential trends in education across socioeconomic groups. If, for instance, states that raised cigarette taxes also introduced state policy changes helping less privileged children to succeed in education, these trends could be captured by our tax variable which is allowed to differ by socioeconomic background. In Table 9 we show that our results are robust to splitting our sample by mother's education. In this alternative specification, the state-specific time trends as well as all other control variables are allowed to have different effects across maternal education groups. Still, the cigarette tax effect for the lowest socioeconomic group remains significant whereas for the intermediate and for the highest socioeconomic groups there is no effect.

[Insert Table 9]

6.2.3 Robustness to current economic situation of the family

In Table A.4 we show that our results are robust to the family's current economic situation. A family's economic situation could arguably influence the school success of children. The ACS dataset includes a range of interesting measures at the family level which we again interact with mother's education to allow for differential effects according to the family background.[44]

Panel A shows the baseline results, in Panel B we add the log of the family's real total income, controlling for the number of children and the household size. In Panel C we use a measure of relative income generated by the Census Bureau: It expresses the family's total income as a percentage of the poverty thresholds established by the Social Security Administration. This measure takes into account inflation and depends not only on the total family income but also on the size of the family, the number of people in the family who are children, and the age of the head of household.

In Panels D and E, we add the current employment status of the mother and the father. Our results remain robust, showing that the effect of taxes experienced in utero acts independently of the economic situation of the family. In Panel E, Column (3) effect sizes decrease slightly compared to the baseline results in Panel A, but the general pattern remains the same.

[Insert Table A.4]

[44]For instance, an increase in family income for a family with a low socioeconomic status could make a larger difference to the offspring's educational success than the same increase for a family with a high socioeconomic status.

6.2.4 Robustness to other health policies, the business cycle and transfer payments

Table 10 compares our baseline estimates (Panel A) to specifications that control for smoking bans in private worksites, bars and daycare centers (Panel B)[45], for the relevant beer tax in the in-utero period[46] (Panel C) and for the current cigarette tax the observed 16-year-olds are exposed to (Panel D) in their state of residence.

In Panel B and C we create averages of the control variables over the 9 months of in-utero period for each individual observation. We then interact each of the newly created variables with the three maternal education groups. Therefore, in Panel B we control for nine and in Panel C for three additional covariates. According to the estimated coefficients in Panel B, our results in Columns (1) and (2) are robust to jointly including all three measures of smoking bans. In Colums (3) and (4) coefficients miss the conventional levels of significance due to large standard errors. Results in Panel C are similar to those in Panel A, suggesting that changes in the beer tax during the in-utero period are not driving our results.

Current cigarette taxes could play a role as confounds because health behavior during youth, specifically the consumption of tobacco, may negatively impact educational attainment via absence from school (Zhao et al., 2012), impaired cognitive development (Jacobsen et al., 2005; Trauth et al., 2000) or physical health (Ding et al., 2009). In Panel D we show that our estimated effect of prenatal cigarette tax exposure is not driven by current cigarette taxes.

[Insert Table 10]

Table A.5 compares our baseline results (Panel A) to the estimated cigarette tax effect when controlling for state-level Medicaid eligibility (Panel B), state welfare reforms (Panel C) and the business cycle (Panel D). Again, we interact all additional controls with maternal education.

States are entitled to determine eligibility criteria for Medicaid (The Kaiser Commission on Medicaid and the Uninsured, 2015). During our sample period, changes in eligibility thresholds were implemented.[47] These thresholds take the form of income levels relative to the poverty line and there are specific values for pregnant women. We use values provided by Hoynes and Luttmer (2011). Panel A shows that our estimated tax effect remains unaffected by including

[45]Our measures of smoking bans come from ImpacTeen, a research organization on youth health, which rates the stringency of indoor air laws by state on a yearly basis.

[46]Smoking and drinking could be complementary behaviors, i.e. when beer is cheaper people drink more and consequently also smoke more. Alternatively, one could think of smoking and drinking as substitutes.

[47]Until 1986 families receiving cash assistance were automatically eligible for Medicaid. Beginning in 1987 Medicaid expanded eligibility for children and pregnant women. States were given considerable flexibility to modify their program rules (Mann, 1999) and many states expanded Medicaid beyond the federally mandated levels (Gruber, 2003).

medicaid eligibility thresholds.

During our sample period there were also considerable changes in state welfare systems.[48] These welfare reforms were found to have negative consequences for the health and health care, especially of unmarried women (Bitler et al., 2005) which could potentially carry over to infant health. We therefore control for a dummy, taking on the value one for completed welfare reform on a state-year level, as defined by Bitler et al. (2005).[49] Again, our results remain largely unchanged with results in Column (3) being a little more susceptible to the inclusion of additional controls than the rest of the coefficients.

[Insert Table A.5]

Lastly, we also check robustness of our results to controlling for state-level transfer payments in the form of several types of social benefits. Following Simon (2016), we use data from the Regional Economic Information System (REIS), a database which tracks transfer receipts from personal income accounts. We look at per capita expenditure on food stamps[50], income maintenance[51] and public medical assistance[52]. We construct one measure of average expenditure for each of them for the period of pregnancy up to age five and one measure for age six up to age 16.

In Table A.6 we jointly control for the early childhood and for the later childhood transfer measures. Again, as before, we interact all variables with maternal education, allowing for effects to differ by socioeconomic background. Our main results (Panel A) remain unaffected when controlling for any measure of state-level transfers (Panel B - Panel D).

[Insert Table A.6]

[48] In the period 1993-1996, the Clinton administration granted waivers to 43 states, allowing states to experiment with their welfare systems. Waivers included, among other things, provisions which may require work and/or training, sanctions for those not complying with requirements and limits on the duration of benefit receipt. In addition, in 1996 a law was passed, leading to the replacement of the AFDC program by the TANF program. The primary motivation of all these changes was to decrease the caseload, to help families move quickly from welfare to work, and thus toward self-sufficiency. In the described time period, the welfare caseload dropped dramatically throughout the country (Dion and Pavetti, 2000).

[49] Bitler et al. (2005) define a dummy that takes on the value one whenever a state has either implemented a major waiver, implying a significant deviation from the state's AFDC program or whenever a state has replaced its AFDC program by TANF in a given year. Between 1992 and 1997 all states subsequently implemented welfare reform, according to this definition, with a majority in 1996/97.

[50] Exposure to food stamps from the in-utero period up to age five, for instance, were shown to have positive effects on adult health and economic outcomes (Hoynes et al., 2016). Therefore, we control separately for this category, even though it is also included in the category "income maintenance".

[51] Income maintenance includes supplemental security income (SSI) benefits, Earned Income Tax Credit (EITC), the Supplemental Nutrition Assistance Program (SNAP), family assistance and other income maintenance benefits

[52] By controlling for public medical assistance our aim is to control for influences on the health of children, especially from less privileged backgrounds where access to health care may be severely limited. Therefore we chose to include Medicaid expenditures as well as other medical care benefits but excluded, for instance, Medicare benefits and military medical insurance benefits.

6.2.5 Robustness to changes in the education system

Lastly, we also want to make sure that our results on educational outcomes are robust to changes in the education system. In Panel B of Table 11 we therefore control for state level per pupil expenditures on public elementary and secondary schooling.[53] Specifically, we create measures of average expenditures for each individual's school career, i.e. covering the year in which the observed individual reached the school-entry age of five, up to one year before the survey. The estimated cigarette tax effect remains robust even though we allow the education expenditures to have differential effects according to the individual's socioeconomic background.

Moreover, we demonstrate robustness to an exogenous changes in state level education expenditures. Namely, during our sampling period a number of court-mandated changes in school financing principles were exogenously imposed, improving the educational outcomes of children living in poor school districts via better financing (Jackson et al., 2016).[54] As a consequence of changing state-level financing principles, our cohorts of 16-year-olds were exposed to different education expenditures depending on their state and year of birth.[55] In Panel D of Table 11 we control for the share of school years each individual spent under the different financing principles. Each of these additional variables is interacted with mother's education, i.e. we include 15 additional variables in this specification.

Panel C in Table 11 shows the baseline results excluding states that are not included in the sample of Jackson et al. (2016).[56] Panel D shows the actual robustness test. Given the large number of additional control variables it is not surprising that some of the variation in the data is absorbed, resulting in slightly smaller coefficients. The overall picture is, however, that the estimated cigarette tax effect is robust to the described exogenous changes in school financing.

[Insert Table 11]

7 Magnitude of the effect

Our findings indicate that cigarette taxes during the 1990s had a significantly negative effect on socioeconomic difference in human capital accumulation measured at age 16. In this section

[53] These data come from the National Public Education Financial Survey (NPEFS), provided by the National Center for Education Statistics.

[54] Traditionally the U.S. public education system relied heavily on the local property tax base for school district funding. This started changing in the early 1970s, when a range of major school finance reforms (SFRs) was implemented. Reforms accelerated in the 1980s causing some of the most dramatic changes in the structure of K–12 education spending in U.S. history. Up to the 2000s there were court-mandated financing reforms. Jackson et al. (2016) encoded the court mandated changes into five different principles that are more or less equality-oriented and found significant effects on educational outcomes, especially in poor districts.

[55] The assumption that everybody in our sample spends their school years in their state of birth is a simplification. It is true for slightly above 80 percent of our sample of 16-year-olds.

[56] South Dakota and the District of Columbia.

we discuss the magnitude of the estimated effect as well as implications for individuals and society.

Most of the 84 tax increases in the U.S. over the years 1988 to 1998 amounted to only a couple of cents, with an average size of increases of 14 cents. Only three tax hikes in the whole sampling period were of 50 cents or larger. Therefore, for the interpretation of magnitudes we choose to consider the effect of a typical increase of ten cents. Our main regression results suggest that a ten cents tax increase during pregnancy leads to a one percentage point increase in the likelihood of a 16-year-old from the lowest socioeconomic group to have completed grade 9 by the age of 16. This corresponds to roughly ten percent of the difference in this likelihood between 16-year-olds from the lowest socioeconomic group and the intermediate socioeconomic group.

What are the implications and expected long-term benefits for these adolescents? Grade retention is a proxy for underlying skills accumulated over the years. In addition, the event of being retained was shown to have direct negative effects on school success, reinforcing existing disadvantages of low-performing students[57]. We provide some back-of-the-envelope calculations on the personal returns from cigarette taxes per exposed child from the lowest socioeconomic group. (Details of the calculations can be found in the Appendix.)

Each 10 cents tax increase leads to a 1.1 percentage point higher chance of having completed grade 9 by the age of 16. Based on representative statistics, having completed grade 9 by the age of 16 is associated with a roughly 15 percent higher chance of completing high school instead of dropping out (Barro and Kolstad, 1987). We can now calculate the intention-to-treat effect of a 10 cents tax increase on high school completion: The corresponding estimated effect size is 0.165 percentage points for somebody from the lowest socioeconomic group. How does this translate into later life outcomes for the affected person?

Considering expected additional income as well as a lower chance of being unemployed, having a high school degree as compared to having no high school degree is associated with an increase in income of 18,000 USD for men and 14,700 USD for women, again based on representative statistics (Snyder et al., 2016). The corresponding income effect from a 10 cents cigarette tax increase are an annual 30 USD for men and 24 USD for women from the lowest socioeconomic group. These are intention-to-treat effects over the full population of offspring from low educated mothers. The effect on an individual whose mother decided not to smoke based on cigarette taxes

[57] Consider high school completion. In the U.S., the lawful minimum age for leaving school ranges between 17 and 19, depending on the state (Education Commission of the States , ECS). Therefore, a person who had to repeat a grade at some point and then stays in school for as long as legally required will leave school with one completed grade less. A scarce literature establishing causality finds negative effects of grade retention on high school completion in case the retention happens at older ages (Jacob and Lefgren, 2009). Suggested channels are low self-esteem, poor social adjustment, negative attitudes towards school, and problem behaviors (Jimerson et al., 2000; Jimerson and Ferguson, 2007; Nagin et al., 2003).

would be higher.

There are some obvious limitations to these calculations. One we would like to point out is that measures of school success are just proxies for future life success. School success driven by underlying characteristics arguably matters more for life outcomes than educational attainment that was "forced upon" students by external incentives such as schooling laws (Eckstein and Wolpin, 1999). Cigarette taxes during pregnancy exert their effect prior to birth and therefore they must necessarily affect underlying characteristics of children. The numbers we used for our back-of-the-envelope calculations represent plain correlations between schooling and later life outcomes and are therefore likely to understate the positive effects cigarette taxes have on later life outcomes.

Lastly, we want to emphasize that the above calculations consider only personal benefits of the affected cohorts. However, additional benefits for society can be expected from the long-term human capital effects of cigarette taxes. Apart from higher productivity and higher tax payments as a result of higher qualifications, research has shown that higher education is associated with positive attitudes and civic behaviors of individuals (Dee, 2004) including voting and volunteering in their communities (Junn, 2005). Also, higher educated persons are less likely to engage in criminal activity (Raphael, 2006), less likely to rely on public services such as food stamps or housing assistance (Waldfogel et al., 2005), less likely to need government health care (Muennig, 2005) and lastly, also more likely to raise healthier, better-educated children (Wolfe and Haveman, 2002).

8 Conclusion

We investigate whether cigarette taxes are an effective instrument in lowering the propagation of socioeconomic inequalities from one generation to the next. The causal channel we examine runs from maternal smoking behavior during pregnancy via infant health to human capital accumulation by the age of 16, as proxied by school outcomes.

In a first step, we analyze the effect of cigarette taxes experienced during pregnancy on maternal smoking behavior during pregnancy and on birth outcomes. Not surprisingly, cigarette taxes have the strongest effect on the smoking behavior of pregnant women with less than a high school degree. Consequently their babies gain most in terms of infant health. Effect sizes decrease with increasing maternal education.

In a second step, we examine whether the effects we observe on birth outcomes are also reflected in educational attainment of young adults in the United States. Indeed, we observe that prenatal exposure to higher cigarette taxes leads to higher educational attainment of those whose mothers have less than a high school degree. These improvements are driven by lower dropout rates and by a lower incidence of grade retention. Moreover, we find that for the lowest socioeconomic group cigarette taxes lead to better outcomes in terms of cognitive and non-cognitive skills assessed by the parents.

Our findings demonstrate that smoking during pregnancy is one channel through which a low socioeconomic status is passed on from one generation to the next. This finding is relevant for policy makers who want to create equal starting conditions for children, regardless of their socioeconomic background. Our results suggest that by lowering smoking rates amongst pregnant women, for instance through increases in cigarette taxes, the persistence of socioeconomic inequalities can be mitigated effectively.

Existing research suggests physical health as one potential channel leading to our observed effects (Simon, 2016). Future research with more detailed data could disentangle different channels behind our estimated tax effect, i.e. cognitive aspects such as intelligence, non-cognitive aspects such as personality traits and physical health. Moreover, it will be interesting to directly study long term cigarette tax effects among adults, for instance by looking at outcomes such as employment, income and adult health.

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Main Tables

Table 1: Summary statistics for PRAMS sample (1988-2013): main variables

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Cig. Tax	0.82	0.72	0.04	4.32	683828
Smoker	11.62	32.05	0	100	668578
Birth Weight	3332.86	567.44	625	5875	679882
LBW (<2500g)	6.17	24.06	0	100	679882
Prematurity (<37 wks.)	7.96	27.07	0	100	675604
Rel. LBW (<10th pctl.)	9.90	29.88	0	100	662030
Baby Alive	99.74	5.12	0	100	614491
0-11 yrs educ (<HS)	18.22	38.60	0	100	673128
12 yrs educ (HS)	29.94	45.80	0	100	673128
13+ yrs educ (>HS)	51.84	49.97	0	100	673128

Means and standard deviations of cigarette taxes are reported in USD. Birth weight is reported in grams. For all other variables, means and standard deviations are reported in percent of the sample.

Table 2: Summary statistics for full ACS and our subsample (means and st. dev. in percent)

	Age 16					
	Full ACS Sample			Subsample Living with Mother		
Variable	Mean	Std. Dev.	N	Mean	Std. Dev.	N
Grade 9	94.38	23.02	391,731	95.02	21.75	338,012
Enrolled	98.80	10.91	391,731	98.98	10.04	338,012
Grade 9|school	94.83	22.14	381,192	95.41	20.93	330,503
Cogn. Diff.	4.694	21.15	391,731	4.246	20.16	338,012
Born Q1 (Jan-Mar)	24.34	42.91	391,731	24.33	42.91	338,012
Born Q2 (Apr-Jun)	24.97	43.29	391,731	25.05	43.33	338,012
Born Q3 (Jul-Sep)	25.99	43.86	391,731	25.98	43.85	338,012
Born Q4 (Oct-Dec)	24.70	43.13	391,731	24.64	43.09	338,012
Female	48.76	49.98	391,731	49.29	50.00	338,012
Race: Amer. Indian/ Alaska Native	2.069	14.23	391,731	1.890	13.62	338,012
Race: Asian	3.908	19.38	391,731	4.070	19.76	338,012
Race: Black or African American	17.19	37.73	391,731	16.00	36.66	338,012
Race: Pacific Islander	0.386	6.201	391,731	0.377	6.131	338,012
Race: White	74.80	43.41	391,731	76.06	42.67	338,012
Race: Other Race	5.945	23.65	391,731	5.854	23.48	338,012

Table 3: Summary statistics for ACS sample (1988-1998): main variables

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Cig. Tax	0.339	0.156	0.0253	0.860	391,731
mom >HS	55.13	49.74	0	100	335,239
mom <HS	9.883	29.84	0	100	335,239
mom HS	34.99	47.69	0	100	335,239
Grade 9	94.38	23.02	0	100	391,731
Enrolled	98.80	10.91	0	100	391,731
Grade 9|school	94.83	22.14	0	100	381,192
Cogn. Diff.	4.694	21.15	0	100	391,731

Means and standard deviations of cigarette taxes are reported in USD. For all other variables, means and standard deviations are reported in percent of the sample. The numbers in this table refer to the full sample of 16-year-olds born between 88/89 and 97/98.

Table 4: Tobacco tax effect on smoking during pregnancy and on birth outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Smoker	Birth Weight	LBW	Prematurity	SGA 10th pctl.	Baby Alive
Panel A: Aggregate Tax Effect						
Cig. Tax	-0.119	0.503	-0.043***	-0.014	-0.021	-0.006
	(0.086)	(0.501)	(0.012)	(0.030)	(0.027)	(0.012)
	[-0.42]					
mom HS	-5.154***	29.306***	-0.441***	-0.303***	-1.126***	0.037*
	(1.019)	(4.284)	(0.115)	(0.093)	(0.257)	(0.019)
mom >HS	-13.145***	63.274***	-1.454***	-0.973***	-2.678***	0.128***
	(1.486)	(5.284)	(0.133)	(0.126)	(0.337)	(0.022)
Mean of dep. var.	11.52	3335	6.103	7.901	9.811	99.75
Panel B: Heterogeneous Tax Effect						
Cig. Tax	-0.39***	1.96***	-0.07***	-0.05	-0.08*	-0.00
	(0.08)	(0.61)	(0.02)	(0.03)	(0.04)	(0.01)
Price elasticity (mom <HS)	[-0.76]					
Tax x (mom HS)	0.27***	-0.80*	0.02	0.04*	0.02	0.00
	(0.06)	(0.45)	(0.02)	(0.02)	(0.03)	(0.00)
Price elasticity (mom HS)	[-0.29]					
Tax x (mom >HS)	0.36***	-2.18***	0.04**	0.04*	0.09**	-0.00
	(0.12)	(0.47)	(0.02)	(0.02)	(0.04)	(0.00)
Price elasticity (mom >HS)	[-0.23]					
mom HS	-7.23***	35.63***	-0.60***	-0.60***	-1.31***	0.03
	(1.18)	(5.83)	(0.16)	(0.19)	(0.38)	(0.02)
mom >HS	-16.05***	81.00***	-1.78***	-1.31***	-3.41***	0.15***
	(1.81)	(6.76)	(0.17)	(0.19)	(0.47)	(0.04)
Mean outc. <HS	20.93	3236	8.118	9.438	12.89	99.64
Mean outc. HS	16.73	3302	6.964	8.521	10.88	99.70
Mean outc. >HS	5.391	3386	4.932	7.031	8.165	99.82
Observations	615,794	624,387	624,387	620,828	621,519	561,563
State FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the month of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. Columns 1 and 3-6 look at different dummy outcome variables. Dummies are coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. Column 1 in addition includes price elasticities of smoking participation per maternal education group in squared brackets. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends (state dummy*time). All specifications control for a large set of controls: baby's gender, mother's age (7 age groups), pregnancy intention (5 groups), number of stressors experienced during pregnancy (4 groups), the Kessner index for quality of prenatal care (4 groups), mother's marriage status (ever married), mother's race (11 groups), questionnaire phases (7 phases). Standard errors are clustered by state*year. Panel A shows aggregate results. Panel B shows results with interaction terms between applicable cigarette taxes and dummies for mother's education group. Coefficients for all other variables are still aggregate. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Tobacco tax effect on educational outcomes at age 16

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Aggregate Tax Effect				
Cig. Tax	0.11	0.04	0.13	-0.10
	(0.13)	(0.06)	(0.12)	(0.14)
mom HS	7.54***	2.78***	6.10***	-1.66***
	(1.28)	(0.95)	(0.87)	(0.61)
mom >HS	9.92***	3.42***	8.25***	-2.66***
	(1.38)	(1.00)	(0.99)	(0.70)
Mean outc.	95.16	99.07	95.51	4.219
Panel B: Heterogeneous Tax Effect				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
mom HS	10.98***	5.91***	7.74***	-3.43***
	(2.13)	(1.72)	(1.30)	(0.85)
mom >HS	14.04***	6.77***	10.55***	-4.64***
	(2.05)	(1.74)	(1.34)	(0.85)
Mean outc. <HS	86.55	96.37	88.12	6.067
Mean outc. HS	94.27	98.93	94.58	4.748
Mean outc. >HS	97.21	99.62	97.32	3.568
Observations	165,260	165,260	161,697	165,260
Cohorts	89-98	89-98	89-98	89-98
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects, state-specific time trends, gender, race and mother's age at giving birth. Panel A shows aggregate results. Panel B shows results with interaction terms between applicable cigarette taxes and dummies for mother's education group. Coefficients for all other variables are still aggregate. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Tobacco tax effect on educational outcomes of 8- to 15-year-olds

	(1)	(2)	(3)	(4)	(5)	(6)
	Cogn. Diff.			Enrolled		
Age Group	8-15	8-11	12-15	8-15	8-11	12-15
Panel A: Aggregate Tax Effect						
Cig. Tax	-0.03	-0.04	-0.10	-0.01	0.01	0.00
	(0.02)	(0.04)	(0.08)	(0.02)	(0.02)	(0.03)
mom HS	-1.19**	-1.03**	-1.34**	0.33	-0.01	0.69*
	(0.46)	(0.44)	(0.51)	(0.22)	(0.11)	(0.36)
mom >HS	-2.30***	-2.30***	-2.29***	0.70***	0.31**	1.10***
	(0.57)	(0.59)	(0.57)	(0.22)	(0.12)	(0.36)
Mean outc.	4.624	4.696	4.552	98.06	98.26	97.85
Panel B: Heterogeneous Tax Effect						
Cig. Tax	-0.19*	-0.18**	-0.32	0.09*	0.03	0.21**
	(0.10)	(0.08)	(0.20)	(0.05)	(0.03)	(0.10)
Tax x (mom HS)	0.15	0.11	0.21	-0.10**	-0.02	-0.25**
	(0.10)	(0.10)	(0.14)	(0.05)	(0.03)	(0.11)
Tax x (mom >HS)	0.19	0.17	0.27	-0.11**	-0.03	-0.24**
	(0.12)	(0.11)	(0.17)	(0.05)	(0.02)	(0.11)
mom HS	-1.83***	-1.57**	-2.16***	0.77**	0.07	1.63**
	(0.50)	(0.61)	(0.54)	(0.35)	(0.18)	(0.64)
mom >HS	-3.12***	-3.10***	-3.30***	1.18***	0.46***	2.01***
	(0.54)	(0.64)	(0.58)	(0.34)	(0.17)	(0.64)
Mean outc. <HS	6.159	6.220	6.097	98.06	98.26	97.85
Mean outc. HS	5.346	5.562	5.142	98.30	98.17	98.42
Mean outc. >HS	3.940	3.950	3.931	98.69	98.52	98.86
Cohorts	1990-2006	1994-2006	1990-2002	1990-2006	1994-2006	1990-2002
State FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Age FE	✓	✓	✓	✓	✓	✓
Observations	1,302,957	643,641	659,316	1,302,957	643,641	659,316

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. Robust standard errors clustered on state-level are in parenthesis. All specifications control for state of birth fixed effects, time fixed effects, state-specific time trends, gender, race and age dummies as well as mother's age at giving birth. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Tobacco tax effect on educational outcomes at age 17

	(1)	(2)	(3)	(4)
VARIABLES	Grade 10	enrolled	Grade10|school	Cogn. Diff.
Panel A: Aggregate Tax Effect				
Cig. Tax	0.25*	0.16**	0.11	0.00
	(0.15)	(0.08)	(0.15)	(0.12)
mom HS	9.34***	3.66***	6.89***	-1.36*
	(1.82)	(1.06)	(1.27)	(0.68)
mom >HS	12.00***	4.48***	8.92***	-2.30***
	(1.98)	(1.10)	(1.41)	(0.79)
Mean outc.	94.50	98.90	95.41	4.221
Panel B: Heterogeneous Tax Effect				
Cig. Tax	1.80***	1.09***	1.10**	-0.47
	(0.52)	(0.33)	(0.42)	(0.36)
Tax x (mom HS)	-1.60***	-0.95***	-1.04**	0.64*
	(0.53)	(0.33)	(0.44)	(0.34)
Tax x (mom >HS)	-1.79***	-1.08***	-1.12**	0.46
	(0.64)	(0.33)	(0.55)	(0.36)
mom HS	14.70***	6.85***	10.44***	-3.49**
	(2.36)	(1.71)	(1.78)	(1.36)
mom >HS	17.99***	8.11***	12.73***	-3.85***
	(2.64)	(1.70)	(2.06)	(1.35)
Mean outc. <HS	84.01	95.26	87.32	5.745
Mean outc. HS	93.59	98.72	94.59	4.710
Mean outc. >HS	96.85	99.62	97.16	3.652
Cohorts	88-97	88-97	88-97	88-97
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Observations	161,781	161,781	155,541	161,781

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Panel A shows aggregate results. Panel B shows results with interaction terms between the applicable cigarette tax and dummies for mother's education group. Coefficients for all other variables are still aggregate. Robust standard errors clustered on state-level are reported in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Demographic composition of the sample of 16-year-olds

VARIABLES	(1) Rel. family income	(2) Female	(3) White	(4) Asian
Cig. Tax	-2.59*	-0.23	-0.62	-0.62
	(1.53)	(0.35)	(1.05)	(0.40)
Tax x (mom HS)	2.43	0.12	1.00	0.37
	(1.48)	(0.43)	(0.92)	(0.29)
Tax x (mom >HS)	4.40***	0.15	0.97	0.73*
	(1.50)	(0.37)	(1.06)	(0.40)
mom HS	79.96***	0.15	6.61***	-0.49
	(5.19)	(1.72)	(2.29)	(0.72)
mom >HS	153.26***	-0.01	9.95***	-0.25
	(5.47)	(1.42)	(2.64)	(0.94)
Observations	165,260	165,260	165,260	165,260
State FE	✓	✓	✓	✓
Year of birth FE	✓	✓	✓	✓
Quarter of Birth FE	✓	✓	✓	✓
Assessment Year FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Gender	✓	✗	✓	✓
Race	✓	✓	✗	✗
Mother's Age at giving birth	✓	✓	✓	✓

The outcome variables in columns (2) to (5) are dummy variables, the outcome variable in column (1) is a continuous variable expressing total family income in percent above the poverty threshold. All regressions are based on our standard specification, controlling for state fixed effects, time fixed effects, state-specific time trends (state dummy*time), mother's education and her age at giving birth. Standard errors are clustered on state level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Split samples: Cigarette tax effect on educational outcomes at age 16

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Split Sample <HS				
Cig. Tax	1.31**	0.66*	1.27**	-0.46
	(0.57)	(0.38)	(0.54)	(0.80)
Observations	13,962	13,962	13,066	13,962
Panel C: Split Sample HS				
Cig. Tax	-0.13	-0.05	-0.09	0.04
	(0.22)	(0.10)	(0.20)	(0.17)
Observations	57,480	57,480	56,086	57,480
Panel D: Split Sample >HS				
Cig. Tax	0.02	-0.01	0.06	-0.10
	(0.12)	(0.04)	(0.12)	(0.12)
Observations	93,818	93,818	92,545	93,818
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Panel A shows the baseline results. Panels B, C and D show results for separate regressions by maternal education group. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Robustness of tax effect on educational outcomes to health policies

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Robustness to smoking bans				
Cig. Tax	1.07**	0.87***	0.60	-0.58
	(0.44)	(0.31)	(0.36)	(0.36)
Tax x (mom HS)	-0.97*	-0.93***	-0.42	0.58**
	(0.49)	(0.34)	(0.37)	(0.27)
Tax x (mom >HS)	-1.10**	-0.97***	-0.55	0.60**
	(0.47)	(0.34)	(0.38)	(0.29)
Bans in private worksites x Mother's Educ	✓	✓	✓	✓
Bans in restaurants x Mother's Educ	✓	✓	✓	✓
Bans in daycare centers x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel C: Robustness to beer tax				
Cig. Tax	1.06**	1.07***	0.44	-0.56*
	(0.46)	(0.36)	(0.34)	(0.33)
Tax x (mom HS)	-1.04*	-1.11***	-0.33	0.49*
	(0.52)	(0.39)	(0.37)	(0.25)
Tax x (mom >HS)	-1.14**	-1.17***	-0.43	0.55**
	(0.52)	(0.39)	(0.39)	(0.27)
Beer Tax x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel D: Robustness to current cigarette tax				
Cig. Tax	1.63***	1.24***	0.88*	-0.67*
	(0.58)	(0.43)	(0.45)	(0.40)
Tax x (mom HS)	-1.61**	-1.30***	-0.76	0.60*
	(0.65)	(0.47)	(0.49)	(0.31)
Tax x (mom >HS)	-1.74**	-1.36***	-0.89	0.66*
	(0.68)	(0.47)	(0.55)	(0.34)
Current cigarette tax x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Cohorts	89-98	89-98	89-98	89-98
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Panel A shows the baseline results. The remaining panels control for additional state-level health policies prevailing in the state of birth during the in-utero period (Panel B and Panel C) and in the state of residence in the year of being surveyed (Panel D). All additional controls are interacted with dummies for mother's education group. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: Robustness of tax effect on educational outcomes to changes in the educ. system

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Robustness to Education Expenditures				
Cig. Tax	0.86***	0.67***	0.48**	-0.44**
	(0.30)	(0.22)	(0.23)	(0.20)
Tax x (mom HS)	-0.85**	-0.70***	-0.42*	0.41**
	(0.34)	(0.25)	(0.25)	(0.16)
Tax x (mom >HS)	-0.92***	-0.73***	-0.49*	0.45**
	(0.34)	(0.25)	(0.26)	(0.17)
Education exp. x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel C: Baseline, sample with SFR data				
Cig. Tax	1.08**	0.85**	0.63*	-0.61*
	(0.45)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.97*	-0.87**	-0.45	0.52**
	(0.51)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.95***	-0.65	0.58**
	(0.50)	(0.35)	(0.39)	(0.27)
School Financing Principles x Mother's Educ	✗	✗	✗	✗
Observations	164,062	164,062	160,518	164,062
Panel D: Robustness to school finance reforms				
Cig. Tax	0.74*	0.76*	0.31	-0.40
	(0.42)	(0.38)	(0.27)	(0.31)
Tax x (mom HS)	-0.63	-0.80*	-0.14	0.26
	(0.50)	(0.43)	(0.30)	(0.24)
Tax x (mom >HS)	-0.73	-0.87**	-0.22	0.34
	(0.47)	(0.42)	(0.32)	(0.25)
School Financing Principles x Mother's Educ	✓	✓	✓	✓
Observations	164,062	164,062	160,518	164,062
Cohorts	89-98	89-98	89-98	89-98
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Main Figures

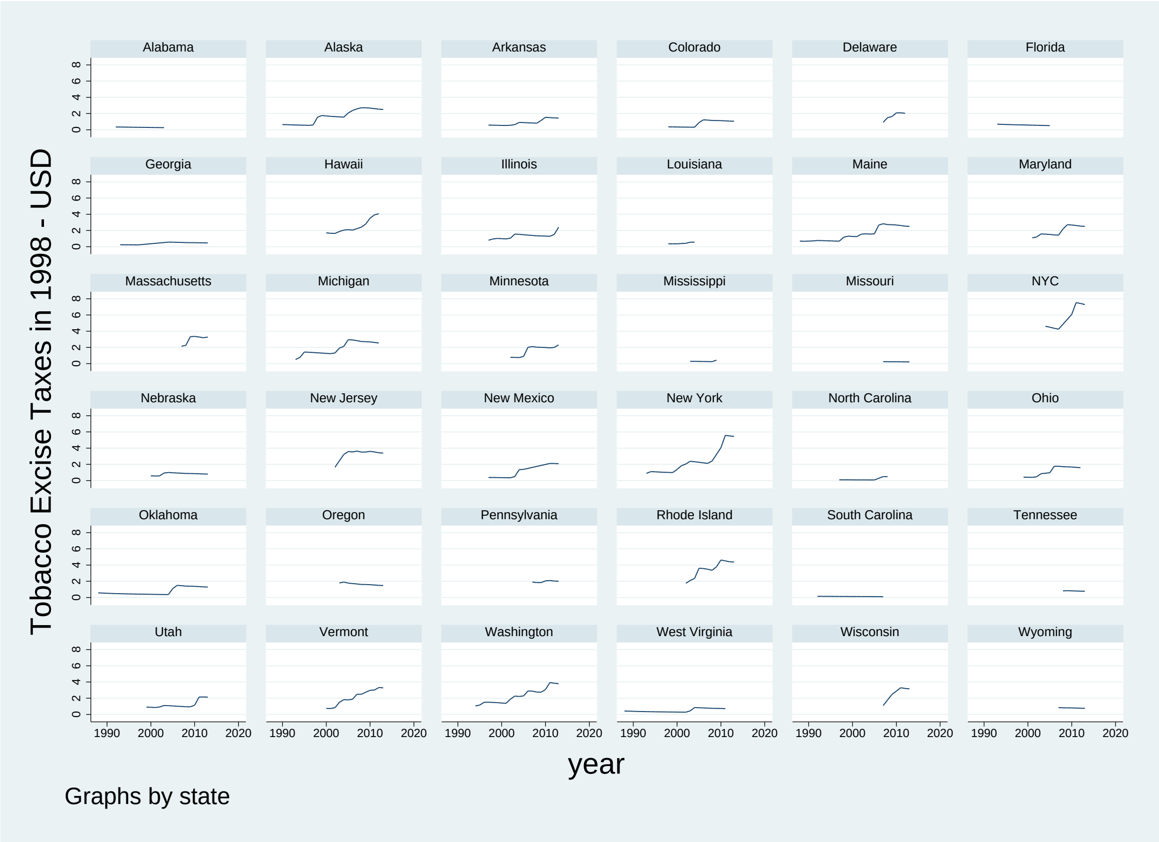

Figure 1: The evolution of tobacco taxes by state (in real 1998-Dollars, corresponding to PRAMS sample)

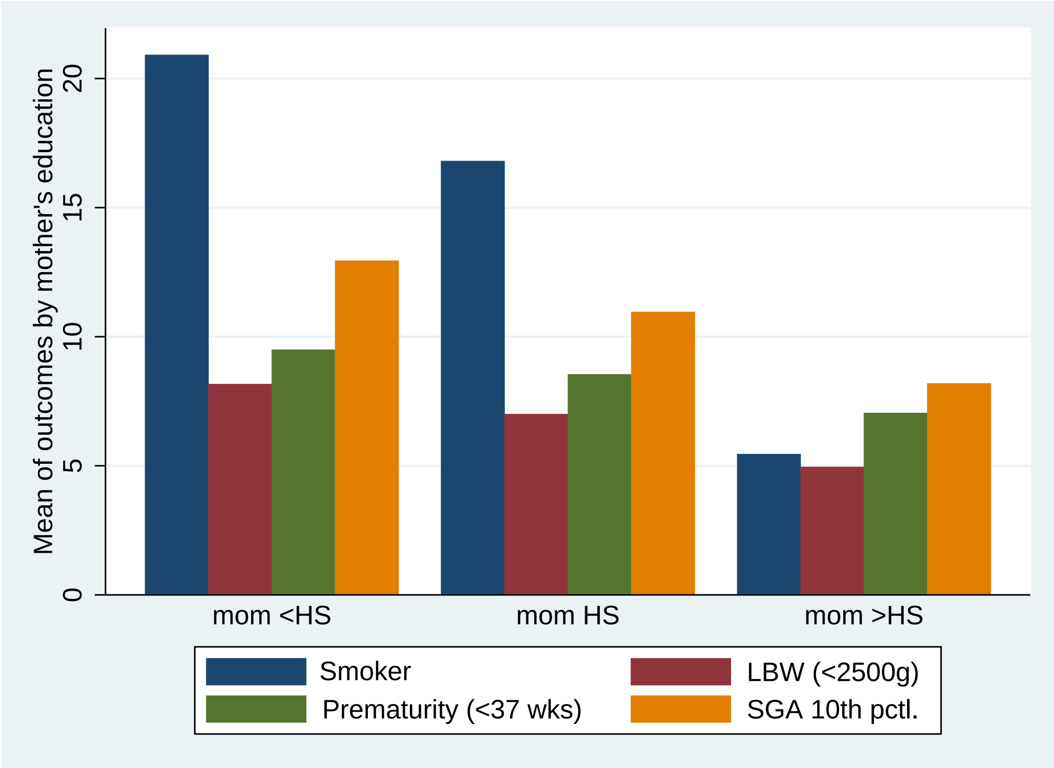

Figure 2: Socioeconomic differences in prenatal tobacco exposure and infant health

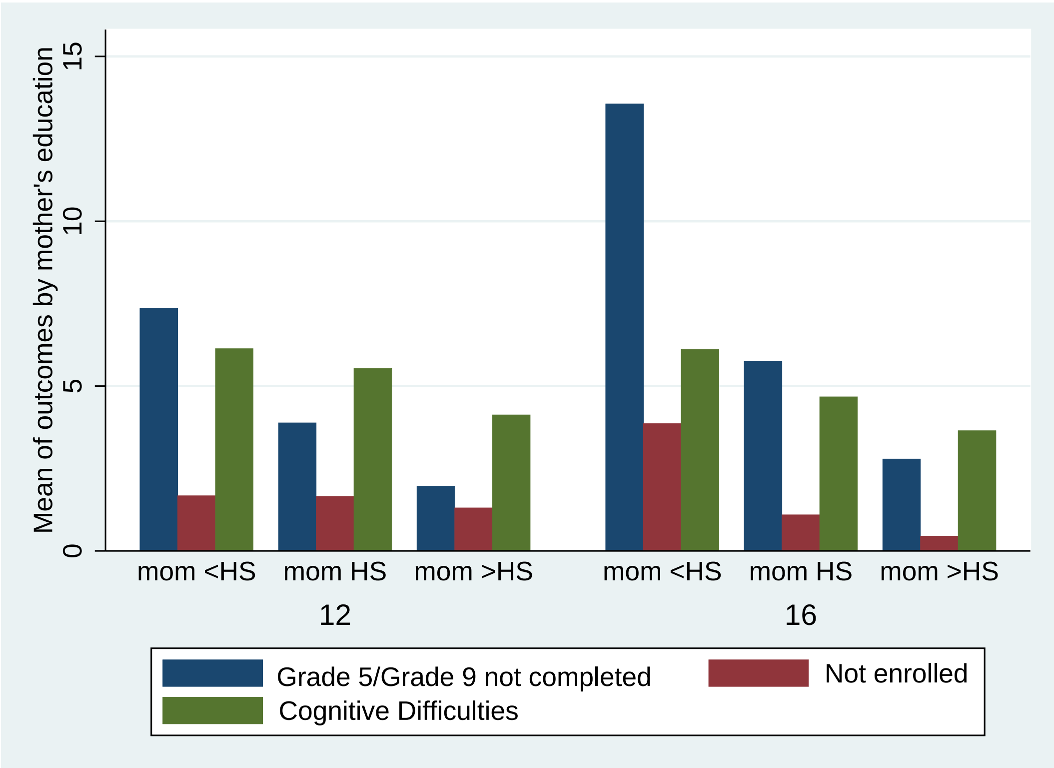

Figure 3: Socioeconomic differences in education at age 12 and 16

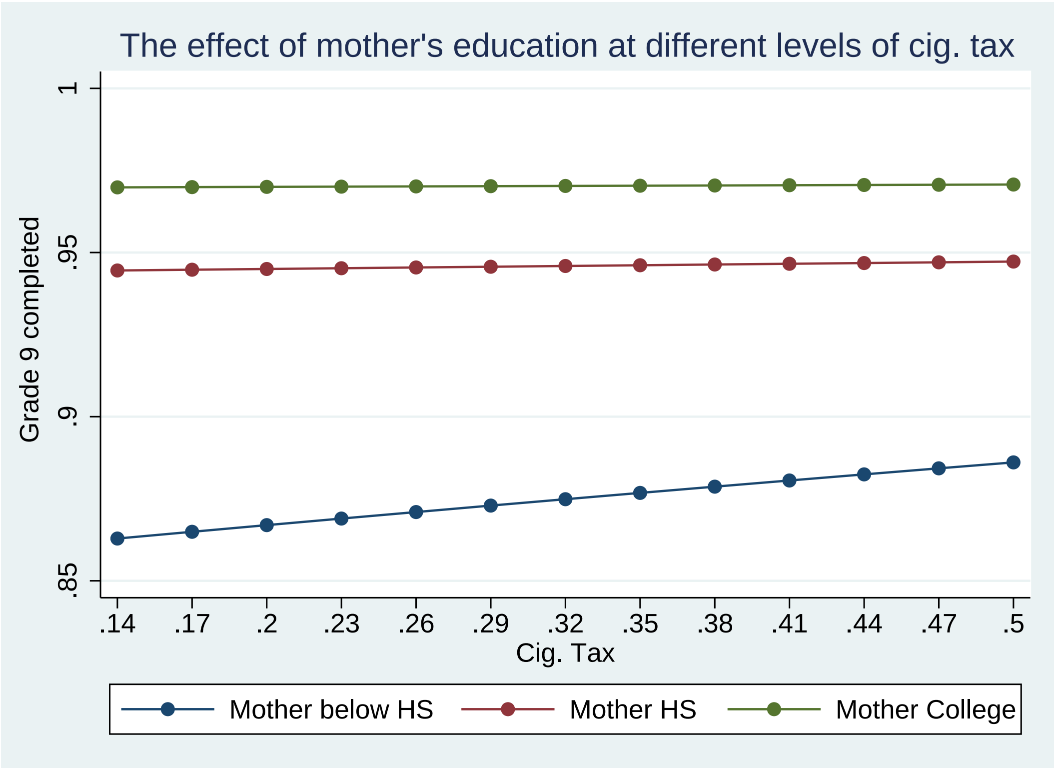

Figure 4: The heterogeneous effect of cigarette taxes on Grade 9 completion by age 16

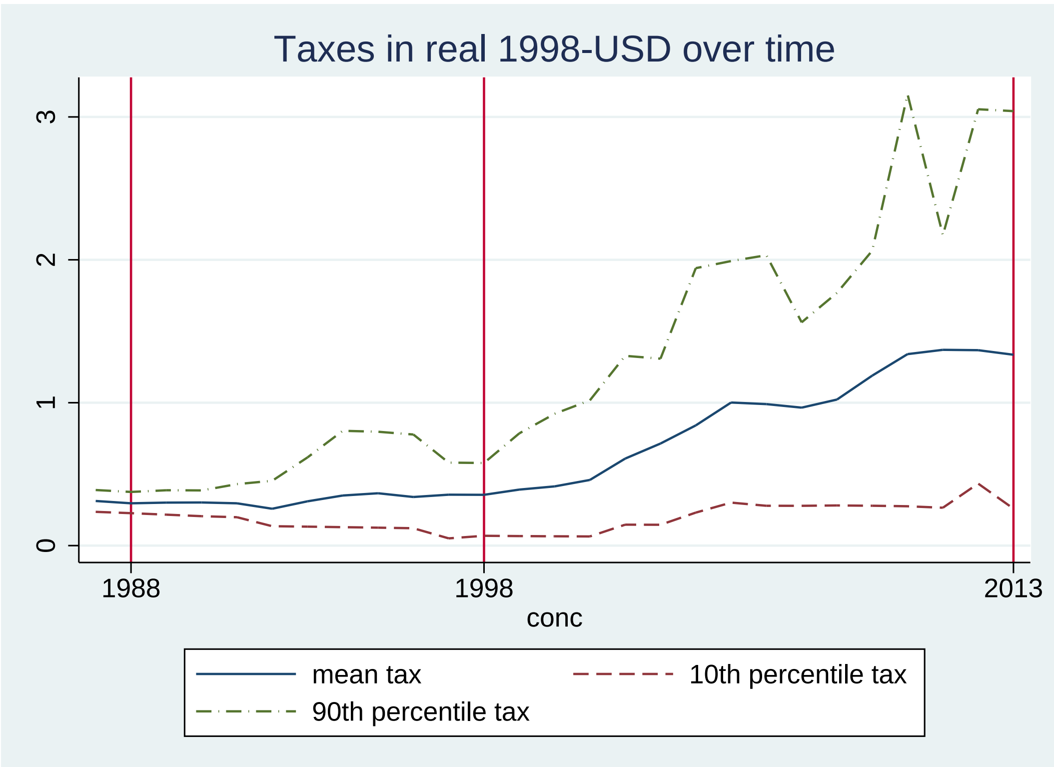

Figure 5: The development of cigarette taxes in real 1998 - USD

A Additional Tables

Table A.1: Summary statistics on additional controls: PRAMS sample

Variable	Mean	Std. Dev.	Min.	Max.	N
Baby is female	0.489	0.5	0	1	684007
Age < =17	0.036	0.186	0	1	683247
Age 18-19	0.069	0.253	0	1	683247
Age 20-24	0.241	0.428	0	1	683247
Age 25-29	0.282	0.45	0	1	683247
Age 30-34	0.238	0.426	0	1	683247
Age 35-39	0.11	0.313	0	1	683247
Age 40+	0.024	0.152	0	1	683247
Kessner Index: Adequate PNC	0.698	0.459	0	1	684026
Kessner Index: Intermediate PNC	0.196	0.397	0	1	684026
Kessner Index: Inadequate PNC	0.049	0.215	0	1	684026
Kessner Index: Unknown PNC	0.057	0.232	0	1	684026
Preg. Intention: later	0.178	0.382	0	1	661743
Preg. Intention: sooner	0.301	0.459	0	1	661743
Preg. Intention: now	0.406	0.491	0	1	661743
Preg. Intention: never	0.1	0.3	0	1	661743
Preg. Intention: not sure	0.015	0.121	0	1	661743
Mother has been or is married	0.64	0.48	0	1	683196
0 stressors	0.283	0.45	0	1	677646
1 stressor	0.236	0.424	0	1	677646
2-4 stressors	0.368	0.482	0	1	677646
5-9 stressors	0.11	0.313	0	1	677646
10-14 stressors	0.004	0.062	0	1	677646
Other Asian	0.025	0.157	0	1	664653
White	0.745	0.436	0	1	664653
Black	0.165	0.371	0	1	664653
American Indian	0.01	0.101	0	1	664653
Chinese	0.006	0.08	0	1	664653
Japanese	0.002	0.045	0	1	664653
Filipino	0.006	0.079	0	1	664653
Hawaiian	0.002	0.05	0	1	664653
Other Nonwhite	0.026	0.158	0	1	664653
Alaska Native	0.002	0.044	0	1	664653
Mixed Race	0.009	0.095	0	1	664653

Table A.2: Sample selection

Outcome Variable Mother in HH	(1) age 8	(2) age 12	(3) age 16	(4) age 17	(5) age 18	(6) age 19	(7) age 22	(8) age 24	(9) age 26
Cig. Tax	0.08	0.17	-0.14	-0.02	0.85**	0.78	0.34	-0.80**	-0.04
	(0.06)	(0.23)	(0.20)	(0.22)	(0.34)	(2.54)	(0.36)	(0.34)	(0.81)
Observations	179,155	184,876	192,852	192,313	196,341	179,692	149,283	144,235	141,100
Cohorts	97-06	93-02	89-98	88-97	87-96	86-95	83-92	81-90	79-88
State FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
State spec. cohort trends	✓	✓	✓	✓	✓	✓	✓	✓	✓
Assessment year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Race and Gender	✓	✓	✓	✓	✓	✓	✓	✓	✓

The outcome variable in all columns is a dummy taking on the value 100 if the observed individual lives in a household with his/her mother (as compared to a group quarter or a household without mother). The cigarette tax coefficient is expressed in real 1998-USD and scaled such that coefficients represent tax changes of ten cents. All regressions are based on our standard specification, controlling for state fixed effects, time fixed effects as well as state-specific time trends. Maternal characteristics are not being controlled for. Standard errors are clustered on state level.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3: Robustness of tax effect on birth outcomes to inclusion of additional control variables

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Smoker	Birth Weight	LBW	Prematurity	SGA 10th pctl.	Baby Alive
Panel A: Baseline Results						
Cig. Tax	-0.39***	1.96***	-0.07***	-0.05	-0.08*	-0.00
	(0.08)	(0.61)	(0.02)	(0.03)	(0.04)	(0.01)
Tax x (mom HS)	0.27***	-0.80*	0.02	0.04*	0.02	0.00
	(0.06)	(0.45)	(0.02)	(0.02)	(0.03)	(0.00)
Tax x (mom >HS)	0.36***	-2.18***	0.04**	0.04*	0.09**	-0.00
	(0.12)	(0.47)	(0.02)	(0.02)	(0.04)	(0.00)
Observations	615,794	624,387	624,387	620,828	621,519	561,563
Panel B: Robustness to WIC						
Cig. Tax	-0.40***	2.06***	-0.07***	-0.05	-0.08**	-0.01
	(0.08)	(0.63)	(0.02)	(0.04)	(0.04)	(0.01)
Tax x (mom HS)	0.27***	-0.82*	0.02	0.04	0.02	0.00
	(0.06)	(0.47)	(0.02)	(0.02)	(0.03)	(0.00)
Tax x (mom >HS)	0.36***	-2.22***	0.04*	0.04*	0.09**	-0.00
	(0.11)	(0.48)	(0.02)	(0.02)	(0.04)	(0.00)
WIC x Mother's Educ	✓	✓	✓	✓	✓	✓
Observations	610,527	619,053	619,053	615,529	616,218	556,711
Panel C: Robustness to Kessner Index						
Cig. Tax	-0.39***	1.95***	-0.07***	-0.05	-0.08*	-0.00
	(0.08)	(0.60)	(0.02)	(0.03)	(0.04)	(0.01)
Tax x (mom HS)	0.26***	-0.76*	0.02	0.04*	0.02	0.00
	(0.06)	(0.43)	(0.02)	(0.02)	(0.03)	(0.00)
Tax x (mom >HS)	0.36***	-2.18***	0.04**	0.04*	0.09**	-0.00
	(0.12)	(0.47)	(0.02)	(0.02)	(0.04)	(0.00)
Kessner Index x Mother's Educ	✓	✓	✓	✓	✓	✓
Observations	615,794	624,387	624,387	620,828	621,519	561,563
Panel D: Robustness to Information						
Cig. Tax	-0.31***	1.83**	-0.06***	-0.05	-0.07*	-0.00
	(0.07)	(0.67)	(0.02)	(0.04)	(0.04)	(0.01)
Tax x (mom HS)	0.23***	-0.61	0.02	0.03	0.02	0.00
	(0.06)	(0.44)	(0.01)	(0.02)	(0.03)	(0.00)
Tax x (mom >HS)	0.31**	-1.87***	0.04**	0.03*	0.08**	-0.00
	(0.13)	(0.49)	(0.02)	(0.02)	(0.04)	(0.00)
Doctor Info x Mother's Educ	✓	✓	✓	✓	✓	✓
Observations	538,351	541,538	541,538	539,899	540,308	479,227
State FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. Columns 3-6 look at different dummy outcome variables. Dummies are coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. The baseline specification is described below Table 4. Additional controls in this table are: i) a dummy for whether the mother received supplementary nutrition during her pregnancy ("WIC"), ii) dummies for whether the mother's prenatal care was rated as adequate, intermediate, inadequate or "unknown" on the birth certificate, iii) A dummy for whether the woman recalls being informed by her doctor about the dangers of smoking. All controls are interacted with three levels of maternal education. Panel A repeats the baseline results from Table 4. Panels B-D show results for the baseline specification plus different control variables. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Robustness of tax effect on educational outcomes to family's economic situation

VARIABLES	(1) Grade 9	(2) Dropout	(3) Grade9|school	(4) Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Robustness to Family Income and Size				
Cig. Tax	1.11**	0.90***	0.63*	-0.54*
	(0.43)	(0.29)	(0.33)	(0.31)
Tax x (mom HS)	-1.02**	-0.92***	-0.47	0.48*
	(0.48)	(0.32)	(0.35)	(0.24)
Tax x (mom >HS)	-1.25**	-0.99***	-0.68*	0.54**
	(0.47)	(0.32)	(0.37)	(0.25)
(Family Income, Size) x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel C: Robustness to Family's rel. Income				
Cig. Tax	1.12**	0.89***	0.66**	-0.61*
	(0.44)	(0.32)	(0.33)	(0.32)
Tax x (mom HS)	-1.02**	-0.90**	-0.48	0.53**
	(0.50)	(0.35)	(0.35)	(0.25)
Tax x (mom >HS)	-1.22**	-0.97***	-0.68*	0.61**
	(0.49)	(0.35)	(0.37)	(0.26)
Family's rel. Income x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel D: Robustness mother's employment				
Cig. Tax	1.04**	0.84***	0.62*	-0.54
	(0.44)	(0.31)	(0.34)	(0.33)
Tax x (mom HS)	-0.92*	-0.86**	-0.43	0.46*
	(0.50)	(0.33)	(0.37)	(0.24)
Tax x (mom >HS)	-1.12**	-0.92***	-0.62	0.52**
	(0.49)	(0.34)	(0.39)	(0.26)
Mother's Employment x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel E: Robustness father's employment				
Cig. Tax	1.02	1.10*	0.26	-0.76***
	(0.62)	(0.58)	(0.38)	(0.26)
Tax x (mom HS)	-1.01	-1.15*	-0.23	0.67**
	(0.70)	(0.62)	(0.39)	(0.25)
Tax x (mom >HS)	-1.15*	-1.24*	-0.33	0.72**
	(0.67)	(0.63)	(0.39)	(0.28)
Father's Employment x Mother's Educ	✓	✓	✓	✓
Observations	122,024	122,024	119,731	122,024
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race, mother's education and her age at giving birth. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: Robustness of tax effect on educational outcomes to state-level policy changes and to the business cycle

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Robustness to Medicaid eligibility				
Cig. Tax	0.83**	0.72**	0.46	-0.54*
	(0.39)	(0.31)	(0.29)	(0.30)
Tax x (mom HS)	-0.71	-0.76**	-0.27	0.45*
	(0.43)	(0.33)	(0.29)	(0.25)
Tax x (mom >HS)	-0.87**	-0.78**	-0.44	0.53**
	(0.40)	(0.32)	(0.30)	(0.25)
Medicaid elig. thresh. x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel C: Robustness to welfare reform				
Cig. Tax	0.94**	0.83**	0.50	-0.56*
	(0.42)	(0.33)	(0.31)	(0.32)
Tax x (mom HS)	-0.83*	-0.84**	-0.32	0.48*
	(0.48)	(0.35)	(0.33)	(0.25)
Tax x (mom >HS)	-0.96**	-0.89**	-0.45	0.53**
	(0.45)	(0.35)	(0.33)	(0.25)
Welfare Reform x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel D: Robustness to the business cycle				
Cig. Tax	0.99**	0.70**	0.66*	-0.54*
	(0.43)	(0.28)	(0.36)	(0.30)
Tax x (mom HS)	-0.85*	-0.71**	-0.46	0.46*
	(0.46)	(0.30)	(0.36)	(0.24)
Tax x (mom >HS)	-1.07**	-0.78**	-0.67*	0.52**
	(0.45)	(0.30)	(0.37)	(0.25)
Unemployment rate x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Cohorts	89-98	89-98	89-98	89-98
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Race and Gender	✓	✓	✓	✓
Mother's age at giving birth	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Robustness of tax effect on educational outcomes to state transfers

	(1)	(2)	(3)	(4)
VARIABLES	Grade 9	enrolled	Grade9|school	Cogn. Diff.
Panel A: Baseline Results				
Cig. Tax	1.10**	0.88***	0.65*	-0.58*
	(0.44)	(0.32)	(0.35)	(0.33)
Tax x (mom HS)	-0.98*	-0.89**	-0.46	0.51**
	(0.50)	(0.35)	(0.37)	(0.25)
Tax x (mom >HS)	-1.18**	-0.96***	-0.66*	0.57**
	(0.49)	(0.35)	(0.38)	(0.27)
Observations	165,260	165,260	161,697	165,260
Panel B: Robustness to Food Stamps				
Cig. Tax	1.17**	0.88**	0.73**	-0.63*
	(0.45)	(0.33)	(0.31)	(0.33)
Tax x (mom HS)	-1.04**	-0.92**	-0.51	0.53**
	(0.51)	(0.36)	(0.34)	(0.24)
Tax x (mom >HS)	-1.20**	-0.97***	-0.67*	0.60**
	(0.49)	(0.36)	(0.34)	(0.25)
Food stamp exp. x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel C: Robustness to Income Maintenance				
Cig. Tax	1.01**	0.68**	0.68*	-0.44
	(0.43)	(0.33)	(0.36)	(0.35)
Tax x (mom HS)	-0.76*	-0.67*	-0.38	0.42
	(0.45)	(0.34)	(0.33)	(0.26)
Tax x (mom >HS)	-0.96**	-0.72**	-0.59	0.48*
	(0.44)	(0.34)	(0.36)	(0.26)
Income maintenance exp. x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Panel D: Robustness to Pub. Medical Ass.				
Cig. Tax	1.43***	1.06***	0.86**	-0.74*
	(0.49)	(0.36)	(0.35)	(0.38)
Tax x (mom HS)	-1.31**	-1.09***	-0.66*	0.64**
	(0.56)	(0.40)	(0.39)	(0.30)
Tax x (mom >HS)	-1.48***	-1.14***	-0.84**	0.73**
	(0.55)	(0.39)	(0.41)	(0.31)
Pub. med. ass. exp. x Mother's Educ	✓	✓	✓	✓
Observations	165,260	165,260	161,697	165,260
Cohorts	89-98	89-98	89-98	89-98
State FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
State spec. Cohort Trends	✓	✓	✓	✓
Race and Gender	✓	✓	✓	✓
Mother's age at giving birth	✓	✓	✓	✓

The cigarette tax variable corresponds to the mean of the cigarette tax rate in the 9 months following the estimated date of conception. It is deflated to real mid-1998 USD using a monthly consumer price index and scaled such that coefficients correspond to the effect of 10-cent cigarette tax increases. All outcome variables are dummies, coded as 0-100 for better readability so that coefficients can be interpreted as percentage points. All specifications control for state fixed effects, time fixed effects as well as state-specific time trends. In addition, all specifications control for gender, race and mother's age at giving birth. Robust standard errors clustered on state-level are in parenthesis. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Calculation of duration of pregnancy in months

In the PRAMS data set, length of gestation is reported in five categories each of which is spanning several weeks. In order to assign a cigarette tax rate to each pregnancy, as described in section 3.3.1, we map a gestational length measured in months to each of the five categories. This is illustrated in Table B.1 which at the same time gives an overview of the distribution of pregnancies over the five categories. The gestational age categories reported on birth certificates are calculated starting from the beginning of the menstrual cycle during which the woman got pregnant. They therefore include the approximately two weeks before the pregnancy started. When converting gestational age categories into months we take this into account. When information on the length of gestation was missing, we assumed a standard length of nine months.

Table B.1: Mapping of gestational age categories to monthly duration of pregnancy

Gestational age category	Share of pregnancies in %	sd	assigned duration in months
<27 weeks	0.466	6.808	6 months
28-33 weeks	1.626	12.65	7 months
34-36 weeks	5.866	23.50	8 months
37-42 weeks	91.77	27.48	9 months
43+ weeks	0.268	5.173	10 months
gestational age missing	0.821	9.024	9 months

C Calculation of price elasticities

Price elasticities of smoking participation based on Evans and Ringel (1999) are calculated as follows: $e_p = \frac{\frac{\partial S}{\partial T}}{\frac{\partial P}{\partial T}} \frac{\bar{P}}{\bar{S}}$. In this equation, $\frac{\partial S}{\partial T}$ is the point estimate of the tax impact on smoking participation, i.e. the estimated tax coefficient. $\frac{\partial P}{\partial T}$ is the pass-through rate of cigarette taxes on retail prices. Based on the existing literature we use the value 1.15 (Evans and Ringel, 1999) which is supported by a large number of other sources as well (Sumner, 1981; Sullivan, 1985; Keeler et al., 1996; Federal Trade Commission, 1997; Evans et al., 1999; Gruber and Köszegi, 2001). $\bar{P}$ is the average price of cigarettes. We use data provided by Orzechowski and Walker (2014) on yearly average cigarette prices per state and adjust these to the same base period as our tax data (1998). We then calculate the average price experienced by each mother during her pregnancy and average across all individuals in our sample. $\bar{S}$ is the average rate of smokers in the sampling period. The calculation of price elasticities are displayed in Table C.1.

Table C.1: Calculation of price elasticities of smoking participation

	Full sample	mom <HS	mom HS	mom >HS
Estimated coefficient $\frac{\partial S}{\partial T}$	-0.12	-0.39	- 0.12	-0.03
Pass-through rate $\frac{\partial P}{\partial T}$	1.15	1.15	1.15	1.15
Average price $\bar{P}$	4.67	4.67	4.67	4.67
Average smoking $\bar{S}$	11.62	20.91	16.80	5.45
Resulting price elasticity	-0.42	-0.76	-0.29	-0.23

D Calculation of labor market effects from cigarette tax increases

This section illustrates the calculation of the estimated long-term labor market effect of cigarette taxes which are mentioned in Section 7. Our regression results suggest that a 10 cent tax increase leads to a 1.1 percentage points higher chance of having completed grade 9 by the age of 16 for somebody from the lowest socioeconomic group. A student who has completed grade 9 by the age of 16 has a roughly 15 percent higher chance of completing high school instead of dropping out, based on representative statistics from the U.S. (Barro and Kolstad, 1987).[58] Based on these numbers, we can calculate the intention-to-treat effect of a 10 cent tax increase on high school completion:

$$\text{ITT(High School)} = 0.011 * 0.15 = 0.00165$$

A 10 cent tax increase leads to a 0.17 percentage point increase in the probability of completing high school of the exposed cohorts from the lowest socioeconomic group. How does this translate into later life outcomes? In terms of expected income, a high school degree compared to no high school degree has been estimated to lead to gains in median annual earnings of 10,000 USD in the year 2013 for men and 8,500 USD for women (Snyder et al., 2016). In addition, using labor market statistics from 2013, the chance of being employed for somebody with a high school degree as compared to a high school dropout was 20 percentage points higher (51 percent vs. 31 percent) (Snyder et al., 2016). Both components lead to expected increases in annual earnings associated with a high school degree as follows:

[58]This number is based on statistics from the High School and Beyond - Survey (HS&B) sophomore cohort of 1980. The HS&B data set is representative for pupils enrolled in public schools in the United States. According to these numbers, students who were around 14 at the moment of starting grade 9 had a roughly 15 percent smaller chance of dropping out of high school than students who were one year older when starting grade 9 (24 percent vs. 9 percent according to Barro and Kolstad (1987), p.53). An alternative measure based on the same data is the difference in dropping out from high school of those that have ever been retained (27.2 percent) compared to those that have never been retained (12.4 percent). The resulting difference is around 15 percentage points as well.

$$\text{Income gain} = \begin{cases} 0.2 * \text{median earning men} + \text{HS mark-up} = 0.2 * 40,000 + 10,000 = 18,000 \text{ for men} \\ 0.2 * \text{median earning women} + \text{HS mark-up} = 0.2 * 31,000 + 8,500 = 14,700 \text{ for women} \end{cases}$$

The corresponding ITT effect from a 10 cent cigarette tax in terms of additional earnings in the lowest socioeconomic group is as follows:

$$\text{ITT (Earnings)} = \begin{cases} 0.00165 * 18,000 = 29.70 \text{ for men} \\ 0.00165 * 14,700 = 24.26 \text{ for women.} \end{cases}$$

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