Economic Research Paper: 2012-04

CAN HIGHER CIGARETTE TAXES STILL IMPROVE BIRTH OUTCOMES? EVIDENCE FROM RECENT LARGE INCREASES

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MARCH 2012

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Can higher cigarette taxes still improve birth outcomes? Evidence from recent large increases

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March 2012

Abstract

Using U.S. natality data, we examine the impact of cigarette excise taxes on maternal smoking participation and low birth weight status. In particular, we implement three empirical strategies: First, we estimate standard two-way fixed effect models. Second, we examine the impact of large state-specific tax increases by matching large-increase states to non-increasing states with similar levels of measured "anti-smoking sentiment". Finally, we match these same large-increase states to non-increasing states with similar pre-increase trends in maternal smoking. Taken together, our findings imply that large cigarette taxes can still reduce maternal smoking and, in turn, improve birth outcomes, particularly for less-educated mothers.

We thank Michael Anderson, Kitt Carpenter, Carlos Dobkin, Mike Grossman, Ted Joyce, Bob Kaestner, Don Kenkel, Ellen Meara, Heather Royer, Anindya Sen and participants at meetings of the American Society of Health Economists and the Canadian Health Economics Study Group for very helpful comments. All errors remain our own.

1. Introduction

While research using data from the early and middle 1990s suggests that higher cigarette excise taxes reduce maternal smoking and lead to substantial improvements in birth outcomes, newer work implies a weaker connection. In particular, more recent work finds that the smoking behavior of pregnant women is relatively price-insensitive. Such behavior is consistent with the trend away from maternal smoking during pregnancy if those mothers who continue to smoke are those who most prefer or are most addicted to cigarettes. By implication, future cigarette tax or other price increases may be less effective in improving birth outcomes than previous ones.

Using data from the 1999-2003 Natality Detail Files (NDFs), a period that includes several historically-large state cigarette tax increases, we revisit the relationship between cigarette taxes and maternal smoking participation, and separately examine their impact on low birth weight status. Beyond their magnitude, the period in question provides useful variation in state cigarette taxes since the majority were prompted by revenue shortfalls in state budgets following the 2001 recession (Maag and Merriman, 2003). Indeed, while the set of large-increase states includes those well-known for strong anti-smoking sentiment, and thus high cigarette taxes, it also includes states that had not previously implemented such large increases.

We employ two general empirical strategies to identify the impact of state cigarette taxes on maternal smoking participation and a selected set of birth outcomes. First, we estimate standard two-way fixed effects models which, in addition to state and year indicators, include a limited set of individual and state-level covariates. As implemented, this strategy uses within-state variation in state cigarette taxes to identify their impact. If unobserved heterogeneity between states is time-invariant over the five years in question, this strategy will yield meaningful estimates. Second, we compare the impact of large cigarette tax increases in select states to the corresponding impact in non-increasing states over the relevant period. Potential comparison states are matched to large-increase states in two distinct ways. First, they are matched to large-increase states on the basis of an empirical measure of state-specific anti-smoking sentiment, as computed by DeCicca et al. (2008). This strategy assumes that states with similar measured anti-smoking sentiment have similar levels of unobserved heterogeneity and thus collectively provide a valid comparison group. Second, following Lien and Evans (2005), we match our large-increase states to non-increasing states with statistically indistinguishable pre-increase trends in maternal smoking participation on the assumption that the latter represent a reasonable counterfactual for the former.

While our smoking participation estimates from two-way fixed effect models are consistent with more recent evidence, corresponding estimates from our large increase strategies range in size from the larger-magnitude estimates of earlier studies to smaller estimates from more recent studies. With regard to the latter set of estimates, we find the greatest degree of price sensitivity among Connecticut and Indiana mothers and this is consistent across our two large-increase identification strategies. In particular, these estimates imply reductions in maternal smoking participation between roughly four and six percent. Consistent with this greater degree of price sensitivity, we find evidence of systematic reductions in the fraction of low birth weight babies in Connecticut. In general, these findings are more pronounced and more broadly seen for mothers with less than twelve years of formal education, whom we label "less educated" mothers.

In the following section, we review the relevant literature. In general, earlier studies find evidence of greater price-sensitivity among pregnant women than later ones. However, the literature is comprised of relatively few systematic studies of the impact of taxes or price, and none use the post-2001 recession-induced tax variation we exploit. Section 3 describes our data which consist of detailed birth records from the Natality Detail Files for the period 1999 to 2003. In addition, we describe our key dependent and independent variables as well as our analysis samples. Section 4 describes our three empirical strategies, which include a standard two-way fixed effect approach as well as two distinct strategies that isolate five large tax increases and compare each state's experience with respect to maternal smoking and birth outcomes to the corresponding experiences of similar non-increasing states. Section 5 presents our findings. First, we review our findings regarding the impact of cigarette taxes on maternal smoking participation. We then present our findings with respect to low birth weight. Section 6 concludes the paper.

2. Relevant Literature

Evans and Ringel (1999) were first to examine the impact of cigarette excise taxes on maternal smoking behavior during pregnancy and subsequent birth outcomes. Using data from the 1989-1992 Natality Detail Files, these authors find systematic effects of higher cigarette taxes on maternal smoking participation and birth weight. With respect to maternal smoking participation, their estimates imply a price participation elasticity of about -0.50 and with respect to birth weight they find that tax-induced cessation among pregnant women leads a newborn to gain about 400 grams on average. It is less clear, however, that such tax-induced effects are generated in the lower tail of the birth weight distribution as estimates from low and very low birth weight models do not consistently suggest a systematic relationship. Other studies that exploit data from time periods similar to and somewhat later than Evans and Ringel (1999) find similarly large effects on maternal smoking participation. For example, Ringel and Evans (2001) find an implied price participation elasticity of -0.70, with significant heterogeneity across certain demographic groups, while Gruber and Koszegi (2001) and Gruber and Zinman (2001) find price participation elasticities between -0.30 and -0.40 for pregnant women and pregnant teenagers, respectively. Like the original Evans and Ringel (1999) study, all use data from the Natality Detail Files. More specifically, Ringel and Evans use data from the years 1989-1995, Gruber and Koszegi (2001) use the years 1991-1997 and Gruber and Zinman (2001) use the years 1989-1996.[1] Bradford (2003), using data from the National Maternal and Infant Health Survey for the years 1988 and 1991, finds a nearly identical price participation elasticity of -0.34, but his estimates imply a similar elasticity for non-pregnant women of similar, childbearing ages, which suggests an age effect, rather than a pregnancy one. Colman, Grossman and Joyce (2003) cover a reasonably similar time period, but take a different approach to estimating the price sensitivity of maternal smoking behavior based on better detail regarding the timing of smoking by pregnant women. Using data from the 1993-1999 waves of the Pregnancy Risk Assessment Monitoring System (PRAMS), which contains information on smoking at multiple points in time before, during and after pregnancy, the authors find implied price elasticities of prenatal smoking cessation and postpartum relapse that are close to unity. Their findings suggest a strong response of price on maternal smoking behavior.

[1] In addition to the similarity of their data and its timing, these studies use similar methods to generate relevant estimates.

A common shortcoming of the studies described above is that they exploit relatively small cigarette tax increases. Two more recent studies rely on larger increases and one of them also uses a source of variation that is plausibly more exogenous than previous work. The first, Lien and Evans (2005), exploits four large increases in state cigarette taxes, ranging from fourteen to fifty cents per pack that occurred in 1993 and 1994. These authors estimate price participation elasticities ranging from -1.83 in Massachusetts to -0.10 in Illinois.[2] Corresponding elasticities in the other two states analyzed—Michigan and Arizona—are -0.22 and -0.33, respectively. So, if one places less weight on the very large Massachusetts elasticity, the authors seem to find relatively small tax impacts on maternal smoking participation. With respect to birth weight, the authors find that smoking cessation leads to a 189 gram, or roughly seven ounce, increase in birth weight when they pool data from three of their four large-increase states, a figure substantially less than the corresponding gain implied by Evans and Ringel (1999). That said, estimates from regressions that model the low birth weight threshold provide some evidence that these gains extend to the lower tail of the birth weight distribution. However, there is not a strong correspondence between price-sensitivity and improved birth outcomes. For example, despite the much greater price-sensitivity of maternal smoking participation by Massachusetts mothers, implied impacts on Massachusetts birth outcomes in reduced form and instrumental variables models do not differ much from the other three large-increase states and in no way could be interpreted as being substantially larger in magnitude. In an even more recent study, Levy and Meara (2006) exploit the per-pack price increase of roughly forty-five cents that followed the Master Settlement Agreement to examine the impact of higher cigarette prices on maternal smoking

[2] Corresponding elasticities based on probit marginal effects are slightly larger in magnitude.

behavior. Using data from the January 1996-February 2000 Natality Detail Files, these authors find evidence of much less price sensitivity on the part of pregnant women, relative to the other studies cited, though they find greater sensitivity among teen mothers.[3] In particular, their estimates imply price participation elasticities of -0.13 for all women and between -0.30 and -0.40 for teen mothers, consistent with Gruber and Zinman (2001). As the authors note, a key shortcoming is that all forty-six states that participated in the MSA experienced the same forty-five cent per pack increase in price. Such before-and-after type variation implies the lack of a well-defined control group. To address this, Levy and Meara (2006) also examine *relative* price changes induced by the flat forty-five cent increase, but find no evidence of price sensitivity in the smoking participation of pregnant women in these models. These authors speculate that recent reductions in smoking participation during pregnancy and the resulting increase in the fraction of "intransigent" smokers in the remaining pool of smokers may be partially responsible for their smaller estimates.

Our work is more closely related to the two most recent studies since it exploits large cigarette tax increases and implements two empirical strategies—one similar in spirit to Lien and Evans (2005) and one that implements their approach. In the next section we describe our data and analysis samples before explaining our empirical strategies in detail in Section 4.

3. Data

We use data from the 1999-2003 Natality Detail Files (NDFs) which consist of records for nearly all births in the United States. Two key features of the NDFs are their very

[3] These authors exclude data from March 1998 to November 1998 which corresponds to the implementation of the Master Settlement Agreement.

large sample sizes, which allow for credible estimation of heterogeneous responses to cigarette taxation, and their inclusion of maternal smoking information for all but a few states.[4] In addition to maternal smoking behavior, the NDFs contain extremely detailed birth-related information as well as less-detailed, but more standard, demographic information such as mother's age, race, education, and marital status. That said, they lack income measures and have only limited paternal information. In what follows, we describe our key dependent and independent variables as well as our analysis samples.

3.1. Maternal Smoking and Birth Outcomes

We examine maternal smoking participation, where the relevant dependent variable equals one if a mother reports smoking "during" pregnancy, and zero otherwise. While available, we do not examine the number of cigarettes smoked per day (i.e., conditional demand) for two reasons. First, previous work on maternal smoking during pregnancy suggests that higher taxes do indeed reduce the fraction of mothers who engage in such behavior. If so, the composition of those who continue to smoke may be affected. For example, if those who quit in response to higher taxes are the lightest smokers in terms of the number of cigarettes smoked per day, consistent with the findings of Colman, Grossman and Joyce (2003), then the pool of remaining smokers will be heavier smokers, on average, and this cessation-induced change in composition will bias the relevant tax estimates. Second, recent evidence finds that smokers who do not quit altogether in response to higher cigarette taxes gravitate towards longer cigarettes or those with higher tar and nicotine content (Evans and Farrelly, 1998). Even more recent evidence suggests that smokers tend to smoke cigarettes "harder" or longer as measured by the amount of

[4] California, which accounts for about one in six of all U.S. births, is the most notable state not to collect smoking information. This omission is common to all studies that use the Natality Detail Files.

cotinine, a by-product of nicotine ingestion, in continuing smokers' blood which rises when cigarette taxes are increased (Adda and Cornaglia, 2006). While neither study examines pregnant women specifically, both set of authors note that such behavioral responses may at least partially offset any health benefits of reduced cigarette consumption, and this logic extends to the health of newborn children.

In addition to maternal smoking, we examine the potential impact of cigarette tax increases on the newborn's low birth weight status. In particular, we model whether or not a newborn weighs less than 2500 grams, or roughly 5.5 pounds). We focus on this clinically-defined threshold because the distributional impacts of cigarette taxes likely matter more than average impacts, which are calculated across the entire support of the distribution of birth weight. For example, suppose that cigarette taxes are found to raise average birth weight. While meaningful, this provides no information regarding where in the distribution gains are generated. For example, do they accrue to children already at healthy birth weights or to those at low birth weights? Recent work that attempts to estimate the causal implications of low birth weight suggests that this is a meaningful distinction. For example, Oreopoulos et al. (2008), Currie and Moretti (2007), and Johnson and Schoeni (2007) find systematic relationships between low birth weight and high school completion. With respect to health, Oreopoulos et al. (2008) find a positive association between low birth weight and infant mortality in models that account for unobserved heterogeneity across families.[5]

3.2. Cigarette Excise Taxes, 1999 to 2003

[5] Like Almond et al. (2005), these authors find that the impact of low birth weight on infant mortality disappears when using twin-level variation. That said, they still find a relationship between very low birth weight (less than 1500 grams) and infant mortality.

We combine data on taxes and their enactment dates from Orzechowski and Walker (2005) to construct monthly state cigarette taxes. We merge this information to the NDFs which contain data on state of residence and month of birth.[6] Using monthly, rather than annual, variation allows for better identification of within-year changes in behavior, which may be especially important in the context of the large tax increases we exploit. In all models, we use real cigarette excise tax rates per pack of twenty cigarettes denominated in 1995 dollars.

As alluded to earlier, states enacted several plausibly large tax increases on cigarettes between 1999 and 2003. Table 1 presents all increases of at least forty cents per pack and their dates of enactment. As shown, all but one of these large increases occurred after 2001. Indeed, from January 2002 to December 2003 states implemented eighteen large increases in cigarette excise tax rates, with an average increase of just under fifty-nine cents per pack. While this list includes many "usual suspects", it contains others not known for high cigarette taxes or high levels of anti-smoking sentiment. Research in public finance suggests that the size and breadth of these increases were due to shortfalls in state budgets following the 2001 recesssion in the United States (Maag and Merriman, 2003).[7] To the extent that these increases were indeed driven by budgetary concerns, rather than state-specific anti-smoking sentiment, they represent more appropriate variation for estimating the causal effect of taxes.

From January 1999 to December 2003, the overall average state cigarette excise tax per pack increased from nearly thirty-nine to about seventy-three cents, nearly

[6] We postpone discussion of the issues surrounding the temporal assignment of cigarette taxes to birth records to the Empirical Strategy section.
[7] In separate work, these authors also show that relative to the 1991-92 U.S. recession, the 2001 recession had much deeper negative impacts on state tax revenues (Maag and Merriman, 2007).

doubling in nominal terms over this short timeframe. By contrast, the average state cigarette tax increased by less than a dime, from twenty-nine to thirty-eight cents, over the previous five year period. The dramatic increases underlying the large average gain over our period provide improved variation to causally identify the impact of taxes; this is especially true since these increases occurred over such a short period of time.

3.3. Analysis Samples

The 1999-2003 NDFs contain detailed information on 19,536,509 singleton births to mothers residing in one of the fifty states or the District of Columbia.[8] For our maternal smoking participation models, we further limit our sample to include records with valid maternal smoking participation and state of residence information which leaves 16,592,048 observations. For our low birth weight models, we limit our sample to include records with valid birth weight and state of residence information for the set of states that collect smoking information, which leaves 16,585,551 observations.[9] Given that we include indicators for missing covariates, these figures represent our primary analysis samples.[10] Of course, sample sizes naturally decrease when we investigate heterogeneity in the relationships of interest or when we implement our "large-increase" estimation strategies, which we describe in detail in the following section.

4. Empirical Strategies

We implement three empirical strategies to estimate the impact of cigarette excise taxes on maternal smoking participation and a limited set of birth outcomes. Each strategy attempts to produce credible estimates by reducing the influence of state-specific

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[8] Following many others, we limit our analysis to singleton births.
[9] We limit our low birth weight samples to include only those states that collect smoking information to make them more comparable to the samples used to estimate smoking participation models.
[10] We also estimate models that contain no covariates, other than relevant fixed effects (month, year and state), to check whether their inclusion meaningfully impacts our main estimates.

unobservables that are correlated with state-determined cigarette tax rates and simultaneously exert an independent influence on the outcome of interest. The first is a two-way fixed effects strategy which has become somewhat standard, while our second and third strategies focus more narrowly on the experience of specific states that implemented large tax increases. As part of these strategies, we match our large-increase states to 1) non-increasing states with similar measured "anti-smoking sentiment", and 2) non-increasing states with similar pre-increase trends in maternal smoking participation. In what follows, we describe each strategy in detail.

4.1. Two-way fixed effects

Including state and time fixed effects in cigarette demand models is standard way to reduce the influence of state-level unobserved heterogeneity. To the extent that the troublesome heterogeneity is time-invariant, such a strategy will generate an unbiased estimate of the impact of cigarette taxes.[11] To implement this strategy, we estimate equations of the form:

$$Y_{ijm} = \alpha + \partial T_{jm} + \beta X_{ijmt} + \sigma_j + \mu_m + \varepsilon_{ijm} \qquad (1)$$

Here, Y is the outcome of interest, T is state-level cigarette excise taxes, X is a limited set of individual and state-level covariates that includes mother's age, education, race, and marital status as well as child's gender, and birth characteristics such as day of week and whether the birth was the mother's first as well as state-specific monthly unemployment rates and indicators for whether the state banned smoking in restaurants and private workplaces in a given month-year pair, while σ and μ represent state and month-year of birth fixed effects, respectively. As implemented, this strategy uses within-state, rather

[11] While time-invariance is a strong requirement, intuitively it is likely more closely met the shorter the time series.

than across-state, variation in cigarette taxes to identify their possible impact on maternal smoking and birth outcomes.

An important issue is the temporal assignment of cigarette tax. As noted in the previous section, the Natality Detail Files include information about mothers' smoking behavior "during" pregnancy, rather than at a specific point or points during pregnancy.[12] Because of this lack of specificity, we estimate models that assign tax at three distinct times: three and six months *after* month of conception as well as contemporaneously with month of conception.[13] While we must estimate month of conception, such sensitivity analysis should reveal any meaningful differential responses based on temporal assignment of the cigarette tax variable.[14] Since there are no substantive differences in tax estimates in any model estimated, we present estimates from models that specify tax assigned contemporaneously with estimated month of conception in all models.

Given that the two-way fixed effects strategy uses within-state variation, another relevant issue is whether or not there is "enough" such variation. As discussed in the previous section, the period of analysis (1999-2003) includes many large tax increases that, relative to earlier periods, were motivated by budgetary concerns rather than state-specific anti-smoking sentiment, an unobserved factor that may directly influence smoking behavior and its implications (Maag and Merriman, 2003). To assess the extent of their within-state variation, we regress state cigarette taxes on a set of state and month-

[12] Starting in 2003, the final year of our sample, the Natality Detail Files contain information on maternal smoking before pregnancy and during the first, second and third trimesters separately.
[13] Alternatively, these can be thought of, respectively, as six months, three months, and nine months prior to month of birth. Note also that these different assignments imply subscripts of m-6, m-3 and m-9 on the tax variable (T).
[14] Since we lack exact date of birth, we define month of conception as nine months prior to reported birth month.

year fixed effects. One rule-of-thumb suggests that if the R-squared of this auxiliary regression is greater than 0.9 (i.e., F-statistic greater than 10), there is not enough independent variation to implement such a strategy (Kennedy, 1994). This regression yields an F-statistic of roughly 7.8, based on an R-squared of 0.87, suggesting that there is indeed sufficient, though not excessive, within-state variation in cigarette taxes available to implement the two-way fixed effect approach described above. Moreover, following Evans and Ringel (1999, p. 144) we confirm that we have enough observations to detect statistical differences at conventional levels of significance. We will provide additional details in future drafts, but the gist is that, for reasonable parameter values, the greater level of tax variation we exploit implies that much smaller samples are needed.

4.2. Large-increase strategies

4.2.1. Selecting large-increase and comparison states

In addition to two-way fixed effects, we compare the impact of large increases in selected states to states that did not experience a tax increase over the period in question and have similar levels of estimated "anti-smoking sentiment" and, separately, to those states with similar pre-increase trends in maternal smoking participation. With "treatment" and "control" states selected, we implement a difference-in-differences strategy to estimate the impact of each large increase. In what follows, we describe our criteria for selecting large-increase and corresponding comparison states.

In terms of selecting treatment states, we limit our analysis to those states that increased their cigarette excise tax by at least forty cents per pack between January 1999 and March 2003, had no increase in the prior three years, and implemented no other increases in this period.[15] Table 2 presents the five states that meet these criteria, the date of their large increase, its amount and the resulting tax, in cents per pack.[16] As can be seen, the large-increase states presented are somewhat geographically diverse and have reasonably different pre-large increase levels of excise taxation, consistent with the idea that the post-2001 recession-driven tax increases occurred more broadly than earlier increases. Moreover, the large increases in question occurred long enough after the Master Settlement Agreement so that their effects, if any, are not likely confounded by MSA-specific effects.[17]

More important than the selection of treatment (i.e., large-increase) states is the selection of valid comparison states, where the ideal is to identify states that represent what would have happened in a particular large-increase state had it *not* implemented a large tax increase. As noted earlier, the predominant concern in estimating the impact of state cigarette excise taxes is the existence of unobserved characteristics that are correlated with tax policy and also exert an influence on the outcome of interest. To address this we take two strategies that involve exploiting large cigarette tax increases.

First, we select as comparison states those states with similar values on an empirical measure of state anti-smoking sentiment (SASS) developed by DeCicca et al. (2008).[18] Using data from the Tobacco Use Supplements of the Current Population Surveys (TUS-CPS), these authors attempt to measure, in a systematic manner, otherwise

[15] Since we assign cigarette taxes in effect at the estimated month of conception, tax increases after March 2003 would, with few exceptions, affect births starting in January 2004 for which we do not have data.
[16] Pennsylvania increased it cigarette excise tax by 69 cents (from 31 to 100 cents per pack) in August 2002, but the 2003 wave of the Natality Detail Files does not include smoking information for Pennsylvania mothers.
[17] Note also that Trogdon and Sloan (2006) find that the impact of the MSA on state cigarette excise taxes was fairly small at roughly ten cents per pack.
[18] This measure is based on data which correspond closely to the first year of our sample. More precisely, data were gathered at three points in time: September 1998, January 1999 and May 1999. As such, they substantially precede the relevant (large) tax increases which, in principle, may affect measured SASS.

unobserved state-level attitudes towards tobacco and its regulation.[19] In particular, DeCicca et al. (2008) conduct a factor analysis which includes nine questions regarding respondent attitudes towards tobacco-related issues such as policies to restrict smoking in public places, the promotion and advertising of tobacco products and whether they allow smoking within their own homes. Results from the factor analysis imply that responses to the nine questions represent a common source which the authors term "anti-smoking sentiment".[20] To construct a state-level measure, they estimate the first factor for each respondent, which consists of an individual's responses to the nine relevant questions weighted by the relevant scoring coefficients, and they then compute state-specific averages for each of the TUS-CPS cycles (1992-93, 1995-96 and 1998-99). See DeCicca et al. (2008) for more detailed information.[21]

The empirical nature of these averages allows for a rank-ordering of states on the basis of SASS. As can be seen in our Table 3, which replicates the temporally relevant column of Table 3 in DeCicca et al. (2008), those states with the greatest and lowest levels of anti-smoking sentiment are consistent with reasonable prior expectations.[22] To assign comparison states, we selected the ten states, five with higher levels of SASS and five with lower levels, centered about each large-increase state.[23] Table 4A presents this

[19] The measure is intended to provide an alternative to state fixed effects in controlling for state-level heterogeneity in cigarette demand models and also to provide a method of doing so in data where inclusion of state fixed effects is not possible (e.g., cross-sectional data).

[20] More precisely, the pattern of the estimated eigenvalues implies a single factor representation of the nine items and the authors label this factor "anti-smoking sentiment".

[21] Recent work on cigarette demand has used this measure of state anti-smoking sentiment as an alternative to state fixed effects to check the robustness of tax or price estimates (c.f., Carpenter and Cook, 2008; Lovenheim, 2008).

[22] Note, however, that the two states with the greatest measured anti-smoking sentiment (California and Utah) and the state with the least (Kentucky) have scores that differ greatly from similarly-ranked states.

[23] While it meets the criteria we establish for a treatment (i.e., large-increase) state, we do not include Vermont because the five closest states with greater measured anti-smoking sentiment include California and Utah, which as mentioned appear to be very different from states just below them in the rank-ordering presented in Table 3.

set of ten states for each of the five large-increase states. Each set of ten states represents *potential* comparison states for each large-increase state. Because we want to compare the experience of the large-increase states to similar states that did not implement increases, we eliminate potential comparison states that experienced a tax increase within three years of their assigned large-increase states.[24] In addition, we eliminate states with tax increases after the relevant large increase, but before March 2003. The state names in bolded italics in Table 4A represent the actual comparison states for each large-increase state. As can be seen, each large-increase state has either six or seven comparison states assigned to it.

A few interesting details emerge from Table 4A. First, while we require that all comparison states implement no increase within three years of their assigned large-increase state, all but one experienced no increase going as far back as January 1997.[25] This is also true of the five treatment states themselves; prior to implementation of their large increases, none increased their cigarette tax between January 1997 and the date of their large increase. This similarity in tax policy is suggestive of similar attitudes towards tobacco and its regulation across treatment and comparison states prior to the relevant large increases. Second, treatment states vary considerably in terms of their estimated state anti-smoking sentiment. For example, only Michigan and Illinois belong to each other's initial set of ten potential comparison states. Of course, as treatment states they are each ruled out as comparison states since each experienced a (large) tax increase in the period of interest.

[24] Arkansas, which increased its cigarette excise tax by a mere 2.5 cents per pack in July 2001, is one exception to this rule. In effect, we treat Arkansas as though it experienced no increase.
[25] The exception is Alaska which increased its cigarette excise tax by 71 cents per pack (from 29 to 100 cents) in October 1997, roughly four and one-half years prior to Connecticut, the large-increase state to which it is matched.

While some of the assigned comparison states in Table 4A may differ ostensibly from their assigned large-increase state, this is also the case in Lien and Evans (2005) who also use the states of Illinois and Michigan as large-increase or treatment states. With respect to Illinois, Lien and Evans (2005) select as comparison states Alaska, Florida, Georgia, Kansas, Kentucky, Nevada, South Carolina, Tennessee, West Virginia and Wyoming, while our method selects South Carolina, Oklahoma, Mississippi, Delaware, Virginia and Nevada. With respect to Michigan, Lien and Evans (2005) use Delaware, Kentucky, Tennessee, West Virginia, and Wyoming as comparison states, while we use Georgia, South Carolina, Oklahoma, Mississippi, Arkansas, Delaware and Virginia. So, it seems that our method and the method of Lien and Evans (2005) produce reasonably similar sets of comparison states for the two treatment states we have in common.

In addition to comparing our "control" state selections to Lien and Evans (2005), we also implement their strategy directly. Consistent with our earlier strategy, we select *potential* comparison states for each large-increase state by eliminating those that increased their cigarette tax in the three years prior to the relevant large tax increase. Again, we also eliminate those states which implemented a tax increase after the relevant large increase, but before March 2003. From this set, we select *actual* comparison states by comparing pre-increase trends in maternal smoking participation, where "pre-increase" refers to the twenty-four months prior to the beginning of pregnancies that were underway when the tax was implemented.[26] Procedurally, we estimate regressions that include only observations from two states: the large-increase state in question and a

[26] For example, Connecticut increased its cigarette tax in April 2002 so we examine women who conceived between August 1999 and July 2001 to select its comparison states.

single potential comparison state, and we do this for each potential comparison state. In particular, we estimate the following model:

$$S_{ijm} = \lambda X_{ijm} + \sigma_j + \mu_m + (\mu_m * TAXSTATE_j) + \varepsilon_{ijm} \qquad (2)$$

Here, S represents maternal smoking participation, while X, σ and μ are defined as in equation (1). In order to test for similar pre-increase trends, we allow the month-year effects to vary across the two states included in each model via an interaction term where TAXSTATE is a dummy variable that represents the large-increase state. If we fail to reject the null hypothesis that the full set of coefficients on the interaction term, twenty-three altogether, are jointly zero this implies the large-increase state in question and the potential comparison state have statistically indistinguishable trends. If so, we label a potential comparison state an actual one.

Table 4B presents the set of potential and actual comparison states. As before, actual comparison states are in bolded italics. Comparing Tables 4A and 4B it is apparent that there is little correspondence between the sets of comparison states chosen for each large-increase state. Relative to a situation with substantial overlap, this implies that the two strategies have independent value in assessing the causal impact of cigarette taxation. Moreover, it is useful since the two sets of estimates can serve as sensitivity checks for each other. Finally, as seen in Table 4B, since New Jersey matches with only one state when we implement our second large-increase strategy, we refrain from estimating the corresponding models due to a lack of degrees of freedom.

4.2.2. Empirical implementation

The nature of the comparison (i.e., large-increase states versus similar states that did not implement an increase over the period in question) lends itself to a difference-in-differences strategy. More specifically, for each large-increase state we estimate models of the form:

$$Y_{ijm} = \gamma(TAXSTATE_j * POST_m) + \lambda X_{ijm} + \sigma_j + \mu_m + \varepsilon_{ijm} \quad (3)$$

Again, Y represents the outcome of interest, TAXSTATE is a dummy variable for the large-increase state and POST is a dummy variable that equals one for the period that begins nine months after a given large increase.[27] The vector X is a limited set of individual and state-level covariates as in equation (1), while σ represents state effects including the large-increase state and μ represents month-year effects. In this setting, γ is the coefficient of interest as it represents the impact of the large increase on the outcome of interest in the large-increase state relative to the relevant set of comparison states. Given that all of the large tax increases occur in 2002 and our data include only births until the end of 2003, γ should be interpreted strictly as a short-run response in smoking models.[28]

For each empirical strategy we also estimate relevant models for mothers who possess less than twelve years of formal education. We allow for such heterogeneity in the impact of cigarette taxes because such less-educated mothers exhibit relatively high levels of smoking during pregnancy and are more likely to give birth to low birth weight children. Moreover, if less-educated mothers tend to be from lower-income households, they may be more price-responsive due to larger income effects of higher cigarette taxes. Prior work fairly consistently suggests that teenage mothers are more price-sensitive in their smoking behavior than other groups (c.f., Ringel and Evans, 2001; Gruber and

[27] This definition of POST implies that (nearly) all births for which it equals one were conceived *after* the large tax increase in question.
[28] The short vs. long-run distinction is less meaningful with respect to the potential impact of cigarette taxes on birth outcomes, though any effects may, in principle, have long-run implications for the child.

Zinman, 2001; Levy and Meara, 2006). By focusing on less-educated mothers, we capture the majority of teenage mothers who have not completed high school, without excluding older women who exhibit higher smoking rates and a greater tendency to give birth to low birth weight babies.

5. Estimates

Tables 5-8 present the impact of state cigarette taxes on maternal smoking participation and, separately, the impact of these taxes on low birth weight status for two samples—all mothers and less-educated mothers. Each table is organized as follows: Panel A presents the estimated tax coefficient (δ) from our two-way fixed effect specification and involves only the first column. For convenience, we normalize this coefficient to represent the impact of a one-dollar increase, though we discuss estimates in terms of a hypothetical fifty-cent increase which is more consistent with the increases witnessed over this period. Panels B and C correspond to our large-increase strategies. In particular, Panel B corresponds to our strategy which uses measured "anti-smoking sentiment" to assign comparison states, while Panel C corresponds to our strategy that matches large-increase states to non-increasing ones on the basis of pre-increase trends in maternal smoking participation.[29] The five columns involved present estimates of γ from five distinct models, each of which corresponds to a large-increase state.[30] Recall that γ represents the impact of the large tax increase on the outcome of interest. As noted previously, the shortness of the post-tax period for each of our five large-increase states implies that we measure short-run tax effects.

[29] We label the first strategy as "Large Increase Strategy 1" and the second as "Large Increase Strategy 2" in the tables.
[30] For example, in Panel B the second column presents estimates from a model that implements equation (3) with Connecticut as the treatment state and, as seen in Table 4A, Minnesota, New Mexico, Colorado, Texas, Florida and Alaska as comparison states.

5.1. Maternal smoking participation

Table 5 presents estimates of the impact of state cigarette taxes on maternal smoking participation for a sample of all mothers. In Panel A, the two-way fixed effect model yields a tax coefficient that is statistically different from zero at the one percent level. Though precisely estimated, the implied effect is somewhat small in that it implies a fifty-cent tax increase would reduce maternal smoking participation by only two percent. The corresponding price participation elasticity is -0.14 which is considerably smaller than earlier estimates, but nearly identical to Levy and Meara (2006) who report a price participation elasticity of -0.13 for a similar sample of all mothers. Estimates from models that correspond to our five large-increase states are somewhat mixed, though all nine imply a negative impact of their respective large increase on maternal smoking participation.[31] In Panel B, our first large-increase strategy yields statistically precise estimates in two of five models reported (Connecticut and Indiana). In terms of magnitudes, these estimates imply price participation elasticities of -0.20 and -0.10, respectively, which are relatively more consistent with recent estimates of the impact of cigarette taxes on maternal smoking participation. Put differently, these estimates each imply reductions of nearly six percent and just over four percent, respectively.[32] Other relevant coefficient estimates reported in Panel B of Table 5 (Illinois, Michigan and New Jersey) imply reductions of less than two percent, though they are not precisely estimated. Despite considerable differences in state-specific sets of comparison states, Panel C

[31] Recall that we do not report estimates for New Jersey for our strategy that uses pre-increase trends in maternal smoking participation to match large-increase states to comparison states. Hence, there are nine relevant estimates, rather than ten.
[32] Given their sensitivity to participation rates, we prefer to report our estimates in terms of the implied reduction in smoking participation rather than via participation elasticities. That said, we continue to do so in order to be comparable to other studies.

depicts a very similar pattern to that seen in Panel B. In particular, estimates in Panel C imply systematic relationships for Connecticut and Indiana, though also for Michigan: the implied magnitude associated with the Michigan increase is similar across our two large increase strategies (i.e., 1.1% in Panel B versus 1.7% in Panel C).

Table 6 also presents estimates of the impact of state cigarette taxes on maternal smoking participation, but only for less-educated mothers whom we define as having less than twelve years of formal schooling. As noted earlier, we focus on this group because such mothers exhibit relatively high levels of smoking during pregnancy and are more likely to give birth to low birth weight children. Moreover, while other related work has focused on teenage mothers, by focusing on less-educated mothers we capture the majority of teenage mothers who have not completed high school, without excluding older women who, as noted, exhibit higher smoking rates and a greater tendency to give birth to low birth weight babies. Relative to Table 5, we see stronger evidence that higher cigarette taxes reduce maternal smoking participation. As reported in Panel A of Table 6, our two-way fixed effect estimate of the impact of cigarette taxes on maternal smoking participation is statistically precise and implies a price participation elasticity, -0.24, that is nearly twice as large as reported for the sample of all mothers in Table 5, implying a greater degree of price sensitivity among less-educated mothers. Consistent with this, we find that a hypothetical fifty-cent increase in the cigarette tax is associated with a 3.4 percent decrease in maternal smoking participation; again, this is somewhat larger in magnitude than the implied effect for the sample of all mothers. In terms of our large increase strategies, we again see the greatest price-sensitivity associated with the Connecticut and Indiana increases. In particular, estimates in Panels B and C imply between an eight and seventeen percent reduction associated with the Connecticut tax hike; for the Indiana increase our two large-increase strategies each imply a reduction of about five percent. Finally, note that our large increase strategy that uses pre-increase trends in maternal smoking participation to assign comparison states (i.e., Large-Increase Strategy 2) yields generally more precise estimates. For example, as seen in Panel C, this strategy yields a statistically precise estimate for the Michigan increase and a marginally significant impact for the Illinois increase, while corresponding estimates in Panel B imply no such systematic relationship.

5.2. Low birth weight status

Table 7 presents estimates of the impact of cigarette taxes on low birth weight status for our main sample of all mothers. As before, Panel A presents estimates from our two-way fixed effect approach. While the tax coefficient is statistically different from zero at the five percent level of significance, the implied magnitude is relatively small. In particular, the coefficient estimate implies that a hypothetical fifty-cent increase results in just over a one percent decrease in the fraction of low birth weight deliveries. Estimates of γ from large-increase state models in Panels B and C, which are all negative in sign, indicate that the largest percentage reductions in low birth weight status are generated by increases by Connecticut, Illinois and New Jersey, where each large increase is estimated to reduce the fraction of low birth weight babies by between two and three percent. Interestingly, no similar systematic effect is seen for the large increase implemented by Indiana, despite earlier evidence of a non-trivial reduction in maternal smoking participation. That said, comparing estimates in Panels B and C, there is a close correspondence in the implied magnitudes of the estimates generated by our two large-increase strategies.

Finally, Table 8 presents estimates of the impact of cigarette taxes on low birth weight status for our subsample of less-educated mothers. As seen in Panel A, our two-way fixed effect estimate implies that a hypothetical fifty-cent increase in cigarette tax is associated with a 1.7 percent reduction in the fraction of low birth weight children. This is somewhat larger than the estimate for all mothers, suggesting a larger proportional impact on this group of mothers who is relatively more likely to give birth to a low birth weight child. Once again, Panels B and C present estimates from our large-increase strategies. Each strategy suggests systematic reductions in the fraction of low birth weight children following large cigarette tax increases in Connecticut, Illinois and Indiana. In addition, the magnitudes implied by the two strategies are reasonably similar and range from roughly two to six percent. Moreover, they are also reasonably consistent with estimates in Table 6, where both large-increase strategies suggest systematic reductions in maternal smoking participation in Connecticut and Illinois.

6. Conclusions

While recent evidence suggests that maternal smoking has become less price-sensitive, we present evidence that suggests large cigarette tax increases may still reduce maternal smoking participation during pregnancy. In particular, though we find relatively small implied reductions in our two-way fixed effect models, estimates from two of five large-increase states (Connecticut and Indiana) imply substantial reductions in smoking participation. Moreover, in Connecticut we find consistent evidence of corresponding reductions in the fraction of low birth weight children. Estimated reductions in maternal smoking participation and low birth weight babies are more pronounced and are seen more broadly for less-educated women. This is interesting because these women are much more likely than average to smoke during pregnancy and are also substantially more likely to deliver low birth weight children. It is important to note, however, that there is substantial heterogeneity in estimated magnitudes across states, despite similar-sized tax increases. This geographic heterogeneity is similar in nature to Lien and Evans (2005) who implement a similar empirical strategy, but find an even greater range of price-sensitivity across their large-increase states.

Finally, our general finding that large cigarette tax increases may still improve birth outcomes is significant in the context of a steady movement away from maternal smoking participation during pregnancy. For example, over the 1990s, this fraction fell from roughly eighteen to twelve percent, a reduction of about one-third. To the extent that individuals with less attachment to smoking (e.g., lighter and/or less-addicted smokers) are the ones who have quit over time, the remaining pool of smokers is comprised of more "hard-core" smokers. Such a change in composition suggests, all else constant, that taxes may affect such individuals' smoking behavior to a smaller degree than in the past. Our findings imply that *large* cigarette tax increase can still reduce maternal smoking participation and, in turn, lead to improved birth outcomes, especially for less-educated women.

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Table 1. Cigarette tax increases of at least forty cents per pack, 1999-2003.

State	Increase	Resulting Tax	Effective Date
Arizona	58	118	November 2002
Connecticut	61	111	April 2002
Connecticut	40	151	March 2003
Illinois	40	98	July 2002
Indiana	40	55.5	July 2002
Kansas	46	70	July 2002
Massachusetts	75	151	July 2002
Michigan	50	125	August 2002
Montana	52	70	May 2003
Nevada	45	80	July 2003
New Jersey	70	150	July 2002
New Jersey	55	205	July 2003
New Mexico	70	91	July 2003
New York	55	111	March 2000
Oregon	60	128	November 2002
Pennsylvania	69	100	July 2002
Vermont	49	93	July 2002
Washington	60	142.5	January 2002
Wyoming	48	60	July 2003

Notes: Taxes and increases are denominated in nominal cents per pack. All regression models are estimated using real cigarette taxes denominated in real 1995 dollars.

Table 2. Large-increase states and related details.

Large-increase state	Date of Increase	Amount of Increase	Resulting Tax
Connecticut	April 2002	61	111
Illinois	July 2002	40	98
Indiana	July 2002	40	55.5
Michigan	August 2002	50	125
New Jersey	July 2002	70	150

Notes: Taxes and increases are denominated in nominal cents per pack. All regression models are estimated using cigarette taxes denominated in real 1995 dollars.

Table 3. Estimated SASS values from greatest to least measured anti-smoking sentiment.

State	SASS	State	SASS	State	SASS
Utah	0.488	Texas	0.167	Oklahoma	0.048
California	0.431	Florida	0.164	Pennsylvania	0.046
Hawaii	0.279	Alaska	0.156	Michigan	0.028
Idaho	0.276	New York	0.148	Mississippi	0.012
Oregon	0.274	Nebraska	0.140	Illinois	-0.002
Vermont	0.272	Iowa	0.131	Arkansas	-0.012
Massachusetts	0.271	North Dakota	0.128	Delaware	-0.013
Maine	0.266	South Dakota	0.123	Virginia	-0.016
Washington	0.252	Kansas	0.115	Louisiana	-0.017
New Hampshire	0.245	Montana	0.114	Nevada	-0.029
Rhode Island	0.243	Wash, D.C.	0.110	Missouri	-0.032
Arizona	0.209	Alabama	0.108	Indiana	-0.086
Maryland	0.205	New Jersey	0.098	Ohio	-0.112
Minnesota	0.201	Wyoming	0.098	Tennessee	-0.115
New Mexico	0.187	Georgia	0.080	West Virginia	-0.196
Connecticut	0.180	Wisconsin	0.061	North Carolina	-0.205
Colorado	0.175	South Carolina	0.057	Kentucky	-0.374

Notes: This information is taken from Table 3 in DeCicca et al. (2008) and was estimated from data gathered from the 1998-99 wave of the Tobacco Use Supplements of the Current Population Surveys (TUS-CPS). Values are rounded to three decimal places.

Table 4A. Potential and actual comparison states chosen on the basis of measured "anti-smoking sentiment"

Connecticut	Illinois	Indiana	Michigan	New Jersey
Rhode Island	***South Carolina***	***Delaware***	***Georgia***	***South Dakota***
Arizona	***Oklahoma***	***Virginia***	Wisconsin	Kansas
Maryland	Pennsylvania	Louisiana	***South Carolina***	***Montana***
Minnesota	Michigan	***Nevada***	***Oklahoma***	Washington, DC
New Mexico	***Mississippi***	***Missouri***	Pennsylvania	***Alabama***
Colorado	Arkansas	Ohio	***Mississippi***	***Wyoming***
Texas	***Delaware***	Tennessee	Illinois	***Georgia***
Florida	***Virginia***	***West Virginia***	***Arkansas***	Wisconsin
Alaska	Louisiana	***North Carolina***	***Delaware***	***South Carolina***
New York	***Nevada***	***Kentucky***	***Virginia***	***Oklahoma***

Note: Each column lists the ten potential comparison states as described in Section 4. Actual comparison states (i.e., those that meet the criteria outlined in Section 4) are listed in bolded italics.

Table 4B. Potential and actual comparison states chosen on the basis of pre-increase trends in maternal smoking participation.

Connecticut	Illinois	Indiana	Michigan	New Jersey
Alabama	Alabama	***Alabama***	***Alabama***	Alabama
Alaska	Alaska	***Alaska***	***Alaska***	Alaska
Colorado	Colorado	***Colorado***	***Colorado***	Colorado
Delaware	Delaware	***Delaware***	Delaware	Delaware
Florida	***Florida***	Florida	***Florida***	Florida
Georgia	Georgia	***Georgia***	***Georgia***	Georgia
Idaho	Idaho	***Idaho***	***Idaho***	Idaho
Iowa	Iowa	***Iowa***	Iowa	Iowa
Kentucky	Kentucky	***Kentucky***	***Kentucky***	Kentucky
Minnesota	Minnesota	***Minnesota***	***Minnesota***	Minnesota
Mississippi	***Mississippi***	***Mississippi***	***Mississippi***	Mississippi
Missouri	Missouri	***Missouri***	***Missouri***	Missouri
Montana	Montana	***Montana***	***Montana***	Montana
Nevada	***Nevada***	***Nevada***	Nevada	Nevada
			New Hampshire	
New Mexico	New Mexico	***New Mexico***	***New Mexico***	New Mexico
North Carolina	***North Carolina***	***North Carolina***	***North Carolina***	North Carolina
North Dakota	***North Dakota***	***North Dakota***	***North Dakota***	North Dakota
Oklahoma	Oklahoma	Oklahoma	Oklahoma	Oklahoma
South Carolina	South Carolina	South Carolina	South Carolina	South Carolina
South Dakota	South Dakota	South Dakota	South Dakota	South Dakota
Texas	Texas	Texas	Texas	Texas
Virginia	***Virginia***	***Virginia***	***Virginia***	Virginia
West Virginia	West Virginia	***West Virginia***	West Virginia	West Virginia
Wyoming	Wyoming	***Wyoming***	***Wyoming***	***Wyoming***

Note: Each column lists the potential comparison states as described in Section 4. Actual comparison states (i.e., those that meet the criteria outlined in Section 4) are listed in bolded italics.

Table 5. Estimated impact of state cigarette taxes on maternal smoking participation during pregnancy, 1999-2003.

A. Two-way Fixed Effects	Tax Coefficient	Connecticut	Illinois	Indiana	Michigan	New Jersey
Tax (δ)	-0.00465	----	----	----	----	----
	(0.00157)					
	[0.0047]					
Impact of a $0.50 increase	-2.0%	----	----	----	----	----
Price participation elasticity	-0.14	----	----	----	----	----
Proportion smokers	0.1187	----	----	----	----	----
N	16,592,048	----	----	----	----	----
B. Large Increase Strategy 1						
State*Post Tax Increase (γ)	----	-0.00464	-0.00168	-0.00686	-0.00139	-0.00120
		(0.00121)	(0.00288)	(0.00159)	(0.00351)	(0.00319)
		[0.0085]	[0.5787]	[0.0035]	[0.7042]	[0.7185]
Percent change in smoking	----	-5.7%	-1.4%	-4.3%	-1.1%	-1.1%
Price participation elasticity	----	-0.20	-0.08	-0.10	-0.06	-0.04
Proportion smokers	----	0.0808	0.1221	0.1590	0.1227	0.1136
N	----	3,784,902	2,583,517	2,388,483	2,642,973	2,067,419
C. Large Increase Strategy 2						
State*Post Tax Increase (γ)	----	-0.00442	-0.00079	-0.00648	-0.00206	----
		(0.00190)	(0.00132)	(0.00117)	(0.00095)	
		[0.0590]	[0.5710]	[0.0001]	[0.0453]	
Percent change in smoking	----	-3.5%	-0.8%	-4.7%	-1.7%	----
Price participation elasticity	----	-0.12	-0.04	-0.11	-0.09	----
Proportion Smokers	----	0.1261	0.1053	0.1364	0.1232	----
N	----	2,230,711	3,291,757	4,736,382	5,512,825	----

Notes: The first column represents an estimate of the impact of tax on the fraction of women who smoke during pregnancy using a standard two-way (state and month-year) fixed effects empirical strategy, while the remaining columns represent estimates from difference-in-differences strategies that exploit states which experienced a large tax increase over the period in question as explained in Section 4. All coefficient estimates presented are generated from linear probability models and, in the two-way fixed effect model, are normalized to represent a one-dollar tax increase. In two-way fixed effect models, tax elasticities are converted to price elasticities by multiplying them by the ratio of 2001 average price to 2001 average tax, while in large-increase models we use state-specific price and tax in the year prior to the relevant large-increase, in lieu of sample averages. Standard errors, adjusted for state-level clustering, are in parentheses and p-values from two-tailed tests are in brackets.

Table 6. Estimated impact of state cigarette taxes on maternal smoking participation during pregnancy, Less-educated mothers, 1999-2003.

A. Two-way Fixed Effects	Tax Coefficient	Connecticut	Illinois	Indiana	Michigan	New Jersey
Tax (δ)	-0.01411	----	----	----	----	----
	(0.00343)					
	[0.0002]					
Impact of a $0.50 increase	-3.4%	----	----	----	----	----
Price participation elasticity	-0.24	----	----	----	----	----
Proportion smokers	0.2064	----	----	----	----	----
N	3,394,925	----	----	----	----	----
B. Large Increase Strategy 1						
State*Post Tax Increase (γ)	----	-0.02044	-0.00599	-0.01448	-0.00440	-0.00334
		(0.00156)	(0.00502)	(0.00328)	(0.00696)	(0.00699)
		[0.0001]	[0.2718]	[0.0031]	[0.5472]	[0.6469]
Percent change in smoking	----	-17.2%	-2.7%	-4.9%	-1.9%	-1.6%
Price participation elasticity	----	-0.59	-0.15	-0.12	-0.10	-0.06
Proportion smokers	----	0.1186	0.2185	0.2963	0.2328	0.2150
N	----	952,086	540,359	484,289	539,673	417,192
C. Large Increase Strategy 2						
State*Post Tax Increase (γ)	----	-0.02005	-0.00460	-0.01323	-0.00508	----
		(0.00278)	(0.00240)	(0.00283)	(0.00199)	
		[0.0004]	[0.1040]	[0.0002]	[0.0207]	
Percent change in smoking	----	-8.3%	-2.5%	-5.3%	-2.2%	----
Price participation elasticity	----	-0.29	-0.14	-0.13	-0.12	----
Proportion Smokers	----	0.2427	0.1831	0.2514	0.2334	----
N	----	432,474	691,653	959,404	1,092,229	----

Notes: The first column represents an estimate of the impact of tax on the fraction of women who smoke during pregnancy using a standard two-way (state and month-year) fixed effects empirical strategy, while the remaining columns represent estimates from difference-in-differences strategies that exploit states which experienced a large tax increase over the period in question as explained in Section 4. All coefficient estimates presented are generated from linear probability models and, in the two-way fixed effect model, are normalized to represent a one-dollar tax increase. In two-way fixed effect models, tax elasticities are converted to price elasticities by multiplying them by the ratio of 2001 average price to 2001 average tax, while in large-increase models we use state-specific price and tax in the year prior to the relevant large-increase, in lieu of sample averages. Standard errors, adjusted for state-level clustering, are in parentheses and p-values from two-tailed tests are in brackets.

Table 7. Estimated impact of state cigarette taxes on low birth weight status, 1999-2003.

A. Two-way Fixed Effects	Tax Coefficient	Connecticut	Illinois	Indiana	Michigan	New Jersey
Tax (δ)	-0.00136	----	----	----	----	----
	(0.00054)					
	[0.0155]					
Impact of a $0.50 increase	-1.1%	----	----	----	----	----
Proportion low BWT	0.0627	----	----	----	----	----
N	16,585,551	----	----	----	----	----
B. Large Increase Strategy 1						
State*Post Tax Increase (γ)	----	-0.00177	-0.00162	-0.00037	-0.00093	-0.00136
		(0.00029)	(0.00071)	(0.00028)	(0.00096)	(0.00045)
		[0.0010]	[0.0567]	[0.2277]	[0.3637]	[0.0190]
Percent change in low BWT	----	-2.9%	-2.4%	-0.6%	-1.4%	-2.0%
Proportion low BWT	----	0.0616	0.0667	0.0654	0.0689	0.0683
N	----	3,831,438	2,609,463	2,401,044	2,676,948	2,104,203
C. Large Increase Strategy 2						
State*Post Tax Increase (γ)	----	-0.00163	-0.00127	-0.00051	-0.00091	----
		(0.00045)	(0.00065)	(0.00034)	(0.00040)	
		[0.0110]	[0.0980]	[0.1466]	[0.0369]	
Percent change in low BWT	----	-2.5%	-1.9%	-0.8%	-1.4%	----
Proportion low BWT	----	0.0649	0.0667	0.0654	0.0660	----
N	----	2,246,249	3,296,732	4,776,390	5,558,748	----

Notes: The first column represents an estimate of the impact of tax on the fraction of low birth weight babies using a standard two-way (state and month-year) fixed effects empirical strategy, while the remaining columns represent estimates from difference-in-differences strategies that exploit states which experienced a large tax increase over the period in question as explained in Section 4. All coefficient estimates presented are generated from linear probability models and, in the two-way fixed effect model, are normalized to represent a one-dollar tax increase. Standard errors, adjusted for state-level clustering, are in parentheses and p-values from two-tailed tests are in brackets.

Table 8. Estimated impact of state cigarette taxes on low birth weight status, Less-educated mothers, 1999-2003.

A. Two-way Fixed Effects	Tax Coefficient	Connecticut	Illinois	Indiana	Michigan	New Jersey
Tax (δ)	-0.00273	----	----	----	----	----
	(0.00117)					
	[0.0235]					
Impact of a $0.50 increase	-1.7%	----	----	----	----	----
Proportion low BWT	0.0821	----	----	----	----	----
N	3,393,780	----	----	----	----	----
B. Large Increase Strategy 1						
State*Post Tax Increase (γ)	----	-0.00311	-0.00424	-0.00377	-0.00034	-0.00103
		(0.00078)	(0.00160)	(0.00073)	(0.00166)	(0.00091)
		[0.0071]	[0.0326]	[0.0013]	[0.8459]	[0.2941]
Percent change in low BWT	----	-4.3%	-4.9%	-4.3%	-0.4%	-1.1%
Proportion low BWT	----	0.0728	0.0872	0.0879	0.0916	0.0898
N	----	959,927	546,003	486,977	545,669	424,330
C. Large Increase Strategy 2						
State*Post Tax Increase (γ)	----	-0.00161	-0.00511	-0.00316	-0.00124	----
		(0.00064)	(0.00116)	(0.00097)	(0.00077)	
		[0.0454]	[0.0459]	[0.0425]	[0.1273]	
Percent change in low BWT	----	-1.9%	-5.9%	-3.6%	-1.4%	----
Proportion low BWT	----	0.0870	0.0859	0.0877	0.0889	----
N	----	435,202	692,597	966,010	1,098,727	----

Notes: The first column represents an estimate of the impact of tax on the fraction of low birth weight babies using a standard two-way (state and month-year) fixed effects empirical strategy, while the remaining columns represent estimates from difference-in-differences strategies that exploit states which experienced a large tax increase over the period in question as explained in Section 4. All coefficient estimates presented are generated from linear probability models and, in the two-way fixed effect model, are normalized to represent a one-dollar tax increase. Standard errors, adjusted for state-level clustering, are in parentheses and p-values from two-tailed tests are in brackets.