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Can Policy Changes Be Treated as Natural Experiments? Evidence from Cigarette Excise Taxes

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Abstract

An important issue in public policy analysis is the potential endogeneity of the policies under study. We examine the extent to which such political endogeneity biases estimates of behavioral parameters by identifying the elasticity of demand for cigarettes using the timing of state legislative elections as an instrument for changes in cigarette excise taxes. We find sizable differences between our estimates and those cited in Chaloupka and Warner (2000), which treat cigarette taxes as exogenous. Our results add to a growing body of evidence that policy changes may be codetermined with the outcomes they are thought to influence.

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1. Introduction

A critical, but rarely addressed, issue affecting the empirical analysis of public policies is the potential endogeneity of the policies under study. If, as seems plausible, many policy changes constitute responses on the part of political decision-makers to changes in a variable of interest (the "outcome" variable), then standard analyses that treat policy changes as exogenous may yield biased estimates of the impact of the policy. For example, as noted by Levitt (1997), a puzzling feature of the literature on criminal deterrence was the consistent failure by researchers to uncover a negative relationship between the size of city police forces and crime rates.[1] In his paper, Levitt argued that the existing studies, although differing in important ways, shared a common bias in that none adequately controlled for the endogeneity of police hiring decisions. For instance, if cities respond to rising crime rates by hiring more police officers, a positive relationship between the number of officers and crime will emerge, even if the true causal effect of police on crime is negative. Levitt addressed this problem by instrumenting for the number of police hired using the timing of mayoral and gubernatorial elections as instruments. Using the exogenous variation in police hiring induced by state and local election cycles, Levitt found that hiring additional police officers does in fact reduce crime.[2]

Although recognized for some time, policy endogeneity has only recently begun to attract systematic attention in the literature. As discussed in a recent paper by Besley and Case (2000),

[1] In a review of the literature, Cameron (1988) found that 18 out of 22 studies on the subject found either no relationship, or a positive relationship, between these two variables.

[2] In a recent article, McCrary (2002) illustrates how a programming error resulted in understated standard errors for several of Levitt's elasticity estimates, leading two previously-significant coefficients to become insignificant at conventional levels. In a reply to McCrary, Levitt (2002) demonstrates that a negative and statistically significant effect of police on crime rates can be obtained by using the per capita number of firefighters in each city as an instrument for the size of the city's police force.

a majority of studies that analyze the impact of public policies treat variation in state-level policy variables as exogenous. Most of these studies employ either fixed effects or difference-in-difference estimators that rely on within-state variation in the policy and outcome variables to identify the effect of the policy change. However, as noted by Besley and Case, neither approach addresses the fundamental problem associated with endogenously-determined policies, which is the response of policymakers to within-state changes in either the outcome variable itself, or to an unobserved factor, like voter sentiment, that independently influences the outcome variable.[3]

Besley and Case illustrate this point by using an instrumental variables approach to estimate the effect of workers' compensation benefit generosity on employment and earnings. Using the gender composition of state legislatures to instrument for the actuarial cost of state workers' compensation benefits, they demonstrate that one would reach different conclusions regarding the effect of workers' compensation benefits on employment and earnings if an IV estimator were used in place of a standard fixed effects specification that treats benefit generosity as exogenous.

Although the endogenous nature of policymaking represents a potential source of bias in a wide variety of settings, it remains an open question whether such biases are empirically important. In this paper, we add to the evidence generated by Levitt and Besley and Case that

[3] Although fixed effects specifications remove any endogeneity stemming from time-invariant differences in state characteristics, including differences in the fixed proclivities of state policymakers, problems remain if policy changes are prompted by changes over time in either the outcome variable itself or the attitudes of policymakers toward the outcome variable. The use of state-specific trends as a control variable may mitigate these effects, but need not eliminate them if policymakers respond to shocks which move the outcome variable away from its trend. For example, legislators may be content to remain passive in the face of a slowly evolving trend in a variable like youth smoking, but may be driven to take action following a sudden "spike" in the variable.

treating policy changes as natural experiments can potentially lead to biased estimates of the effects of policy interventions. Here, we examine state tax policy toward tobacco and demonstrate that when the timing of state legislative elections is used as an instrument for cigarette price changes, an approach that utilizes plausibly exogenous variation in state excise taxes, we obtain substantially larger price elasticity estimates than are obtained when the tax changes themselves are used as an instrument for price. Moreover, many of the specifications are estimated with sufficient precision that we are able to reject the hypothesis that the tax and election-instrumented elasticity estimates are the same.

Although we do not claim that the models estimated in this paper necessarily provide the best possible estimates of the parameters in question, the large changes in parameter estimates that occur when exogenous variation in cigarette taxes is used raises the possibility that previous estimates of the cigarette price elasticity could be biased. In addition to being of significant interest in its own right,[4] tobacco taxation is substantively quite different from the criminal justice and social insurance policies analyzed in the aforementioned papers. As a result, the findings presented here highlight the potentially widespread nature of this problem in applied policy research.

The paper proceeds as follows. In Section 2, we present alternative estimates of the price elasticity of demand for cigarettes. The first set uses state excise taxes as an instrument for price, a common methodology that treats state tax changes as exogenous. After conducting a preliminary test of this assumption, we report a second set of estimates using only the variation in cigarette prices (taxes) attributable to the presence of a legislative election in the state. These estimates are larger across several model specifications and estimators, and, in many cases, are statistically different from the estimates that treat tax changes as exogenous. We also present a robustness check in which the party affiliation of the state's governor is used as an instrument for price. Concluding remarks are offered in Section 3.

2. The Price Elasticity of Demand for Cigarettes

The large and burgeoning literature on tobacco consumption and control has had as its main focus the price elasticity of demand for cigarettes (Chaloupka and Warner (2000)). This emphasis reflects both the perceived importance of price as a policy tool and the greater ease with which the influence of price can be evaluated relative to other control polices (e.g. advertising restrictions, counter advertising, and clean indoor air laws). This literature is also one that has struggled with fundamental identification issues, and one that has embraced several of the empirical methodologies discussed earlier in the paper.

Many studies have attempted to identify the impact of price on demand using variation in state excise taxes (see Becker, Grossman, and Murphy (1994); Evans and Huang (1998); Evans, Ringel, and Stech (1999); Gruber (2000); and Gruber and Koszegi (forthcoming)). These studies have improved upon earlier work by using panel data and including state and year fixed effects in their models, and, in some cases, state-specific trends (Evans and Huang (1998); Evans, Ringel, and Stech (1999)). As discussed in the Introduction, however, these methodological advances will not always be sufficient to protect the analysis from the biases associated with policy endogeneity. For example, if states that experience unusually rapid growth in cigarette consumption are more likely to increase excise taxes, then standard analyses that treat changes in state cigarette excise taxes as exogenous will understate the (negative) impact of price on

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[4] Public policy toward tobacco products is discussed at length in Evans, Ringel and Stech (1999) and Chaloupka and Warner (2000).

demand.[5] Below, we present some suggestive evidence on this point by showing that states that experienced an increase in cigarette consumption in the prior year are more likely to change their excise taxes on cigarettes, even after controlling for within-state trends in cigarette consumption.

Although many cigarette demand studies produce elasticity estimates that fall into the consensus range cited by Chaloupka and Warner (2000), all use variation in taxes (or prices) that potentially reflects the responses of politicians or voters to changes in cigarette consumption. Thus, although these studies may differ in terms of their data and methods employed, they potentially share a common bias, akin to the bias present in the criminal deterrence literature, based on the nature of the variation used for identification. This problem has been largely overlooked in the tobacco literature; a recent exception being Ohsfeldt et al. (1998), who instrument for state cigarette taxes using several state-level political and economic variables.[6]

In this paper, we document the existence of an electoral cycle in state cigarette excise taxes and use the election cycle as an instrument to examine whether conventional estimates of the price elasticity of cigarette demand are potentially biased. We begin by using state-level panel data to estimate a standard demand equation that treats cigarette prices as exogenous. We then instrument for price using state excise taxes, a widely-used methodology that relies on the assumption that state cigarette taxes are exogenously determined. We conduct a "pre-test" of this assumption using a method popularized by Ashenfelter and Card (1985) and Heckman and Hotz (1989) for evaluating the quality of a control group: in our case, the states that did not

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[5] Similar arguments apply to a variety of "natural experiments" that have been considered in the literature, such as tax changes prompted by voter referenda (see Hu et al., (1994)).

[6] In particular, they use per capita state spending, per capita tobacco production, a measure of political ideology within the state, and an index of competition among political parties as instruments for state cigarette taxes. Although one may question the exogeneity of several of these variables, it is interesting to note that the authors find a larger impact of taxes on the

change their cigarette taxes in a given year. We find that states that experienced an increase in cigarette consumption the previous year are more likely to change their excise taxes on cigarettes. This is consistent with a simple policy endogeneity story in which states increase their taxes on cigarettes whenever demand is high relative to its long-run trend. Next, we compare the tax-instrumented elasticity estimates to those obtained using the election cycle as an instrument, finding evidence that failing to account for the endogeneity of state tax policy could result in a substantial underestimate of the impact of taxes on consumption. As a robustness check, we also explore the sensitivity of our estimates to the use of an alternative instrument: the party affiliation of the state's governor.

2.1. Data

The cigarette data used are a panel of the 50 U.S. states, with yearly observations running from 1955 to 1997. Information on per capita cigarette consumption, cigarette prices and excise taxes by state is taken from the publication *Tax Burden on Tobacco* published by the Tobacco Institute.[7] Demographic data on state per capita income over time are taken from Bureau of Labor Statistics (BLS) sources.

Summary statistics of this panel are presented in Table 1. Slightly fewer than 12 packs of cigarettes are smoked per person per month over this time period. A pack of cigarettes costs

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probability of smoking using their instrumental variables approach than when cigarette taxes are treated as exogenous.

[7] Data on cigarette consumption is not available for all states for the early years of the panel. The following is a list of the states that do not have consumption data starting in 1955 and when the consumption data for that state begins: Alaska 1959, California 1960, Colorado 1965, Hawaii 1960, Maryland 1959, Missouri 1956, North Carolina 1970, Oregon 1967 and Virginia 1961. If we conduct our analysis using only the years for which complete data are available for all states (1970 and onward), we obtain similar results to those presented below. Also, because it does not have the same election system as the 50 states, we do not include data for the District of Columbia. Therefore, we have 2086 complete state/year observations in the panel.

about a dollar and a half on average (measured in 1997 dollars), and excise taxes (both state and federal) accounted for about forty percent of the price on average.[8]

Figure 1 shows the time series (in logs) of per capita cigarette consumption in the sample. Consumption increased until the early 1960s, then remained approximately constant for two decades. Since the early 1980s, there has been a precipitous decline in cigarette consumption. Per capita cigarette consumption in 1997 was only sixty-five percent of consumption in 1981.

The time series (also in logs) of cigarette prices and excise taxes in the sample is presented in Figure 2. There is a high positive correlation in the movement of cigarette prices and excise taxes. Cigarette prices fell in the 1970s as the real value of excise taxes eroded with inflation. Since the early 1980s, real cigarette prices rose steadily until they peaked in the mid-1990s. Cigarette prices in 1997 were 170% higher than the 1981 price. State and federal taxes increased substantially over this same time period.

2.2. Using Tax Changes to Measure the Price Elasticity of Cigarette Demand

Figures 1 and 2 show that cigarette consumption decreased substantially starting in the 1980s as cigarette prices were increasing to their highest levels, suggesting that there might be an important effect of cigarette prices on cigarette demand. We attempt to measure the causal effect of prices on cigarette demand by estimating a regression of changes in state cigarette consumption on changes in state cigarette prices. The regression equation is:

$$\Delta \ln \mathit{Packs\ per\ person}_{i,t} = \alpha + \beta \Delta \ln \text{Price}\ \mathit{per\ pack}_{i,t} + \Delta \ln X_{i,t}\delta + \varphi_t + \varepsilon_{i,t} \tag{1}$$

[8] We measure the excise tax of a state in June of the year. This month is chosen because we want yearly changes in a state's excise tax to be measured at the same time as the changes in cigarette prices. Our results are similar if we use other months in the year.

where $Packs\ per\ person_{i,t}$ is the number of packs of cigarettes per person per month consumed in state i in year t; $Price\ per\ pack_{i,t}$ is the average price of a pack of cigarettes in state i in year t. Both variables are log-differenced in the regression specification. $X_{i,t}$ is a measure of state per capita income. $\varphi_{i,t}$ is a set of year dummies, and $\varepsilon_{i,t}$ is the error term.[9, 10]

The coefficient of interest is β, which measures the effect of changes in state cigarette prices on changes in state cigarette demand. The OLS estimate of equation (1) is presented in column (1) of Table 2. The coefficient on changes in state cigarette prices suggests that a 10 percent increase in cigarette prices is associated with about a 4 percent decrease in cigarette consumption. In column (2), we add state effects to the regression specification which, because the data are differenced, control for linear state time trends in cigarette consumption. The correlation between changes in cigarette prices and cigarette consumption is almost identical to the estimate in column (1).

To give β a causal interpretation as a demand elasticity, we need to instrument for changes in state cigarette prices with a variable that affects cigarette prices but does not affect

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[9] This empirical specification is widely used in the tobacco literature (see, for example, Evans and Huang (1998); Evans and Ringel (1999); Farrelly et al. (2000); or Gruber and Koszegi (forthcoming)). One potential problem is that we do not control for either cross-state cigarette smuggling or state smoking regulations. The issue of smuggling from low to high-tax states has received much attention in the literature. However, a recent paper by Farrelly et al. (2000), which contains the richest set of controls to date for smuggling activity, finds that estimated price elasticities are not appreciably affected by omitting controls for cross-border sales. A similar conclusion is reached by Evans and Ringel (1999), who also demonstrate that excluding controls for state smoking regulations does not affect the estimated impact of taxes on smoking.

[10] Recent work indicates that it is important to take into account serial correlation in the error term when calculating the standard errors in difference-in-difference models, especially in applications such as this one where both the dependent variable and the independent variable of interest are serially correlated. Bertrand, Duflo and Mullainathan (2001) show that in settings such as ours a satisfactory correction for this problem can be obtained by clustering the standard errors at the state level, a procedure that we utilize in all of our regression models.

cigarette demand in any other way. A standard candidate for this instrument is a measure of changes in state cigarette excise taxes.[11] In column (3) of Table 2, we present the regression estimates of equation (1) using 2SLS with $\Delta \ln Excise tax_{i,t}$ as an instrument. The coefficient on $\Delta \ln Price\ per\ pack_{i,t}$ is negative and statistically different from zero. The implied demand elasticity of cigarette consumption is –0.51.[12] In column (4), we again add state effects to the regression specification. The estimated demand elasticity decreases slightly in absolute value to -0.46 compared to our previous estimate in column (3). These estimates are consistent with the range of –0.30 to –0.50 cited by Chaloupka and Warner (2000) as encompassing the majority of recent price elasticity estimates for cigarettes.[13]

An important assumption underlying these IV estimates is that state legislatures or voters are not influenced by changes in cigarette consumption within the state when determining

[11] Note that because the data have been log differenced, the inclusion of year dummies removes any variation in real tax rates or prices that is attributable to inflation. As a result, the variation used to identify the price elasticity of demand comes solely from legislated tax changes and not from inflation-induced changes in the real value of the tax.

[12] Changes in state excise taxes are highly correlated with changes in cigarette prices in the first-stage estimation. As shown at the bottom of Table 2, the *F* statistic of the instrument in the first-stage regression is very high. As with many other studies (for example Harris (1987) and Keeler *et al.* (1996)), we find that on average increases in state excises taxes lead to more than 100% pass through of the tax to cigarette prices.

[13] It should be noted that this range applies primarily to estimates of the total price elasticity of demand. The total elasticity is a measure of the responsiveness of the total number of cigarettes purchased (by all consumers) to a change in price. Studies based on microdata often decompose the total elasticity into a participation elasticity, which measures the sensitivity of the probability of smoking to price, and a conditional demand elasticity, which measures the price responsiveness of current smokers. The total elasticity incorporates both of these effects. Although it would be desirable to examine participation and conditional demand elasticities as well, to do so using our election cycle instrument would require a relatively long panel of individual-level data that could be matched to state-level data on cigarette excise taxes. To the best of our knowledge, the only data set that meets all of these requirements is the Monitoring the Future Survey, which tracks high school students over time. Unfortunately, the public use version of this data set does not contain state identifiers.

changes in cigarette excise taxes. If states are responding to changes in cigarette purchases when setting taxes, then demand elasticities estimated using tax changes as instruments can be biased.

There are several reasons why states might take consumption changes into account when setting excise taxes. First, state governments might consider the revenue or political implications of changing cigarette consumption when determining taxes. For example, if cigarette sales are growing, then states might be enticed to increase cigarette taxes to take advantage of the greater revenue that will be raised. Under such a scenario, using excise taxes as an instrument would bias the estimated elasticity upward (towards zero). On the other hand, higher cigarette demand might mean that more voters would be upset by a tax increase, lessening the chance that state legislatures will vote for such increases. Alternatively, public health concerns about the dangers of smoking might cause states to increase excise taxes during periods of increasing demand. Warner (1981) and Chaloupka and Warner (2000) argue that there have been several periods over the last fifty years when U.S. states and other countries have responded to public health concerns when setting excise taxes.

If policymakers are responding to changes in cigarette consumption when setting taxes, then states that did not change their tax in a given year are not a good control group for states that did change their tax because, besides the effect of the tax changes, the two groups did not on average experience the same changes in cigarette consumption. The program evaluation literature has developed several methods to evaluate the quality of a control group. One popular methodology is to examine the characteristics of the treatment and control group before the treatment (see Ashenfelter and Card (1985) and Heckman and Hotz (1989)). A necessary condition for a good control group is that it has similar pre-treatment characteristics to the treatment group. We implement this type of pre-test by examining whether changes in cigarette consumption in a state the previous year affect whether a state changes its cigarette tax. If politicians or voters are not responding to changes in cigarette consumption when setting taxes, there should be no association between previous consumption changes and tax changes. We estimate a probit model in which an indicator variable for whether a state changed its cigarette tax is regressed on changes in the state's cigarette consumption the previous year:

$$\mathit{Indicator\ for\ tax\ change}_{i,t} = \alpha + \beta \Delta \ln \mathit{Packs\ per\ person}_{i,t-1} + \Delta \ln X_{i,t}\delta + \varphi_t + \varepsilon_{i,t} \tag{2}$$

where $\mathit{Indicator\ for\ tax\ change}_{i,t}$ is a dummy that state *i* changed its cigarette tax in year *t*, and the other variables are defined as above. β measures whether the likelihood that a state changes its cigarette tax is influenced by changes in cigarette consumption the previous year.

The estimation of equation (2) is presented in column (1) of Table 3.[14] The coefficient on the tax change indicator is positive and statistically different than zero at the seven percent level. The estimated marginal effect suggests that a ten percent increase in cigarette consumption the previous year increases the likelihood that a state implements a cigarette tax change by about 3 percentage points, which corresponds to a 21% increase over the baseline probability. When state effects are added to the regression specification in column (2), the coefficient on the tax change indicator is even larger and still statistically different than zero.[15]

These results indicate that states that change their cigarette taxes in a given year are different from states that do not for reasons other than the tax change, even after controlling for

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[14] Virtually identical results are obtained when equation (2) is estimated as a linear probability model.

[15] Given our relatively long panel, we also performed Granger causality tests (Granger (1969)) in which current state cigarette tax changes were regressed on lagged changes in state taxes and consumption, plus state and year fixed effects. Across a variety of specifications and lag lengths,

state fixed effects and state-specific time trends. This suggests that using state excise tax changes to identify the effect of price on cigarette demand might not measure the true causal effect of price on demand.

2.3. Election Cycles in State Cigarette Excise Tax Changes

Because his focus was on city crime rates, Levitt used mayoral and gubernatorial election cycles to instrument for the number of police hired at the local level. Here, we focus on state-level policies and use the timing of state legislative elections to instrument for changes in state excise taxes on cigarettes. Legislative election cycles vary across states for a couple of reasons. First, some states have statewide legislative elections every two years while other states only have elections every four years.[16] Also, most states schedule their elections on even calendar years, but there is a significant minority of states that hold elections in odd years. Note that in all but a handful of cases, state legislative elections occur in the same years as gubernatorial elections; thus the legislative election cycle subsumes the gubernatorial election cycle in most states.

There are several reasons why one might expect to observe a link between election timing and the timing of cigarette excise tax changes. First, taxes are often a critical political issue in elections, so legislators might be reluctant to vote on and pass tax increases during an election year. The tax increase might alienate voters in general or smokers in particular. Such sentiments are often expressed in media reports and are frequently cited as one reason for "gridlock" in election years. On the other hand, if smokers are an unimportant voting minority, then

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we find that changes in cigarette consumption Granger cause changes in cigarette excise taxes. (We also find that taxes Granger cause consumption).

[16] Like the U.S. Congress, a typical state has an upper and lower house. Usually, the entire lower house is up for election each election cycle and part of the upper house is up for election. Nebraska is the only state with a unicameral legislature.

legislators might want to increase cigarette taxes during election years if additional revenue is needed by the state to avoid raising taxes on more politically important constituencies. In either case, it seems intuitive that legislators will seek to capture any political advantage that can be gained by strategically timing the passage of legislation. Second, because legislators are spending time campaigning and raising funds during election years, they might devote less time to passing laws. If so, excise tax changes should be less common in election years than in other years.

Underscoring the existence of a relationship between the timing of elections and the timing of public policy changes is evidence from a number of previous studies documenting the existence of election cycles. In addition to Levitt's finding for police staffing, Berry and Berry (1992) find that proximity to a gubernatorial election is the strongest and most consistent factor influencing the probability that a new tax will be adopted. Similarly, Poterba (1994) finds that both tax increases and spending reductions are smaller in gubernatorial election years than in other years.

Empirically, changes in state excise taxes, and therefore changes in state cigarette prices, do tend to be affected by election cycles. We measure whether changes in state cigarette prices are different following election years than other years. The regression equation is:

$$\Delta \ln \mathit{Price\ per\ pack}_{i,t} = \alpha + \beta \mathit{Election\ previous\ year}_{i,t} + \Delta \ln X_{i,t}\delta + \varphi_t + \varepsilon_{i,t} \qquad (3)$$

where $\mathit{Election\ previous\ year}_{i,t}$ is an indicator that the state held a legislative election the previous year and the other variables are defined as before. Data on the timing of state legislative elections is taken from the *Book of the States*. Here β measures whether cigarette prices change differently after election years in a state compared to other years.

The results of this regression are presented in column (1) of Table 4. The coefficient on the indicator of a state having a legislative election is negative and statistically different from zero, suggesting that cigarette prices increase less after election years compared to other years. This result might suggest that legislators are hesitant to vote for a cigarette tax increase during an election year that takes effect the next year; conversely, it might suggest that legislators want tax increases to take effect *during* election years to please anti-smoking advocates. The magnitude of the coefficient indicates that changes in cigarette prices are slightly less than 1 percent lower after election years than after non-election years. In column (2), state effects are also included in the regression specification; the effect of elections on changes in cigarette prices is almost identical to the estimate in column (1).

This relationship between the timing of elections and changes in cigarette prices is caused by the fact that states change their excise tax less after election years than in other years. The propensity of states not to increase cigarette excise taxes after election years also emerges when the data is analyzed on a year-by-year basis. Figure 3 displays a plot of the difference in the changes in excise taxes for states with and without elections the previous year. While there is substantial year-to-year variability in the average change in excise taxes, states with elections the previous year exhibit lower tax changes in 33 of the 43 years of our sample.

Another way of examining the robustness of the relationship between cigarette excise tax changes and elections is to analyze the data on a state-by-state basis. A full list of states, along with information on mean changes in cigarette excise taxes after election and non-election years is provided in the Appendix Table. In 41 of the 50 states, the mean change in excise taxes after an election year is lower than after a non-election year.

Given that cigarette price changes are lower after election years, then if price changes affect cigarette consumption, a reduced-form relationship between elections and cigarette consumption should emerge. The reduced-form specification is:

$$\Delta \ln Packs\ per\ person_{i,t} = \alpha + \beta Election\ previous\ year_{i,t} + \Delta \ln X_{i,t}\delta + \varphi_t + \varepsilon_{i,t} \tag{4}$$

where the variables are defined as above. The estimates of equation (4) are shown in column (1) of Table 5. The coefficient on the election indicator is positive and statistically different from zero, indicating that cigarette consumption grows in states the year after an election. The estimates imply that cigarette consumption grows about 0.73 percent more after election years than after non-election years. When state effects are added to the regression specification, the estimates suggest that consumption grows slightly less than 1 percent more after an election year compared to other years.

2.4. Using Election Cycles to Estimate the Effect of Price on Cigarette Demand

The preceding section demonstrates a negative correlation between elections and changes in cigarette prices, as well as a positive correlation between changes in cigarette demand and elections. Together, those results suggest a direct relationship between cigarette prices and demand that is examined in this section using election timing as an instrument for changes in cigarettes prices.

The impact of cigarette prices on demand is estimated using 2SLS, treating cigarette prices as endogenous and the other right-hand-side variables as exogenous. The particular form of the equation to be estimated is equation (1) defined above. The results from estimating this equation using the election instrument are presented in column (1) of Table 6. The effect of cigarette prices on consumption is again negative and statistically different from zero. The elasticity implied by the coefficient is –1.03 and is bigger in absolute value than the estimate using tax changes as an instrument. In column (2), the regression specification also includes state effects. The elasticity increases slightly in absolute value to –1.07. Both estimates are about twice as large in absolute value as the elasticity estimated using tax changes as an instrument instead of election cycles.

Table 7 shows the results when we expand the instrument set by allowing the effect of elections to vary across time and across regions of the U.S. First, we break up our sample period into two time periods: 1955-1975 and 1976-1996. Using this set of time period/election interactions as instruments in columns (1) and (2) of Table 7, the estimated price elasticity is slightly smaller in absolute value than the just-identified estimates, but they are still much larger in absolute value than the estimates obtained using tax changes as instruments.

We next expand the instrument set by allowing the effect of elections to vary across census regions of the U.S. Columns (3) and (4) of Table 7 present 2SLS estimates of the effect of cigarette prices on consumption using a set of nine region/election interactions as instruments. In both specifications, the estimated price elasticity is similar to the estimates obtained using only the election cycle as an instrument.

Our results suggest that traditional models that treat state tax changes as natural experiments produce price elasticity estimates that are biased towards zero. However, it is difficult to "work backwards" from this result to pinpoint the exact source of the bias. It must be remembered that the bias we identify is relative to a benchmark specification that controls for both state and year fixed effects and state-specific trends. Thus, even after controlling for trends in consumption and smoking attitudes at both the state and national levels, we still observe an endogenous response on the part of state policymakers. This implies that any bias in conventionally estimated price elasticities is not due to a failure to control for long-run trends in either consumption or anti-smoking sentiment, but rather from year-to-year responses on the part of policymakers to changes in these variables about their trends.

In this regard, our findings are consistent with an environment in which legislators increase taxes whenever consumption is high relative to its long-run trend. However, the exact rationale for this behavior, e.g. whether it is driven by revenue or health concerns, remains an open question. It also remains unclear what role changes in anti-smoking attitudes play since, given our specification, the confounding effects of attitudinal changes must arise in year-to-year fluctuations around a long-run trend.[17]

At the bottom of Tables 6 and 7, we report the *F* statistics and p-values of the election cycle instruments in the first-stage regressions. Except for column (3) of Table 7 (where the instrument set is the nine region/election interactions and the model does not contain state effects), the instruments have a statistically significant effect on changes in cigarette prices; however, they exhibit a much weaker correlation with cigarette prices than do changes in state excise taxes. Bound, Jaeger and Baker (1995) and others have shown that there are potential problems with instrumental variables estimates when there is a low correlation between the instrument and the endogenous explanatory variable.

[17] Further complicating our ability to draw inferences about the role played by changes in anti-smoking sentiment in biasing earlier price elasticity estimates is the possibility that public attitudes towards smoking may have less influence on state policies than has previously been assumed. In their case study of anti-smoking legislation in six states, Jacobson, Wasserman, and Raube (1993) found that states' willingness to adopt anti-smoking regulations was largely the result of an "insiders" game between anti-smoking groups and the tobacco lobby, with public opinion exerting surprisingly little influence. To quote from their study, "… it seems clear that strong public opinion by itself is an insufficient legislative motivator to overcome opposition from an important constituency like the tobacco industry. One reason is that the intensity of the public's antismoking views remains in some doubt, as many legislators reported that they have

First, with a "weak instrument", the finite-sample bias of 2SLS might be severe, with the IV estimates biased toward the OLS results. However, in our case, instrumenting with the election cycle indicator moves the price elasticity estimate farther away from the OLS estimate than using the change in the excise tax as an instrument. Therefore, if finite-sample bias is a problem with our estimates, this would suggest that the true price elasticity is even farther from the conventionally-estimated elasticity than our estimates would indicate, implying that policy endogeneity is an even greater problem than it would appear based on our results.[18]

In many circumstances, LIML performs better than 2SLS when there is finite-sample bias because of weak instruments (see Angrist, Imbens and Krueger (1999) and Stock, Wright and Yogo (2002)). In addition, under conventional assumptions, the finite-sample bias of the LIML estimator does not increase with the number of instruments, as is the case with 2SLS (Rothenberg (1984)).[19] In Table 8, we present the LIML estimates of the price elasticity of demand for cigarettes using the time period/election interactions in columns (1) and (2) and the

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heard little from their constituents on the issue," (Jacobson, Wasserman, and Raube, (1993), pp. 812-813).

[18] For the specifications in which the election indicator is the only instrument, simulations conducted by Bound, Jaeger, and Baker (1995) suggest that our estimates are unlikely to be subject to finite-sample bias. For the case that most closely approximates ours, one with two instruments and a partial F-statistic of 4, the simulations presented in Table A.1 of their paper imply a finite-sample bias in our IV estimates (relative to the OLS bias) of only 2%. Because the bias attributable to weak instruments is generally negatively related to the partial F-statistic from the first-stage regression, but positively related to the number of instruments used, the bias in our estimates should be less than 2%. For the time period/election interactions, we have two instruments and partial F-statistics of approximately 3. This would translate into a bias of between 2% and 14%. In the case of the region/election interactions, which utilize nine instruments and yield partial F-statistics of 1.27 and 2.57, substantial finite-sample bias could exist; however, as discussed in the text, the effect of any such bias would be to underestimate the degree to which the tax-instrumented estimates understate the true price elasticity of demand.

[19] Indeed, the numerical results reported in Stock, Wright and Yogo (2002) suggest that the strength of the correlation between the instruments and the endogenous explanatory variable needed to eliminate finite-sample bias actually declines as the number of instruments increases from 1 to 20.

region/election interactions in columns (3) and (4) as instruments. These LIML estimates are very similar to the 2SLS estimates in all but one specification. In column (3) (using the region/election interactions as instruments with no state effects in the specification), the price coefficient is substantially negative and the standard error blows up; this is probably not surprising given the sensitivity of LIML to the particular choice of specification. When state effects are added to the LIML specification in column (4), the instruments perform much better (as shown at the bottom of Table 7) and the price elasticity estimate is very similar to the 2SLS estimates.

Second, even a weak correlation between the instrument and the error term of the second-stage regression can lead to large inconsistencies in IV estimates if the instrument is weak. If the state election cycle is correlated with changes in cigarette consumption for reasons other than changes in cigarette prices, then demand elasticities estimated using the election indicator as an instrument might be flawed.

Probably the best story supporting the existence of a correlation between the second-stage error and the election instrument is one that involves a relationship between state election timing and the adoption of other state anti-smoking policies. For example, if states are more likely to enact restrictive public indoor smoking laws or produce anti-smoking ad campaigns before elections, and these interventions affect cigarette demand, then our IV strategy might attribute the effect of these policies to price changes. To determine if this is a problem with our empirical strategy, we re-estimated our cigarette demand regressions using only the first twenty years of our sample (1955-1974). During this period, there were few attempts by states to control smoking through non-price means such as clean indoor air laws or ad campaigns.[20] Using this smaller sample, we obtain qualitatively similar demand elasticities as with the full sample. Therefore, it does not appear that our results are being driven by an election cycle in other state smoking policies.

Another story is one that involves state elections leading to an electoral cycle in state fiscal policy, as described by Poterba (1994). If there are political business cycles at the state level, and if changes in cigarette demand are related to state economic conditions, then changes in cigarette demand might be correlated with election timing. If this were the case, and we were unable to control for state economic conditions in our regression specification, then our estimated demand elasticities might be biased. Although it seems unlikely that political business cycles at the state level would have a quantitatively important impact on cigarette consumption, we nonetheless include state per capita income as a control variable in all of our models.[21]

We also investigate whether there is evidence of electoral cycles in state spending programs, especially programs targeted for low-income people who tend to spend more on products like cigarettes. Using the same methodology as we used to identify an election cycle in cigarette excise taxes, we did not find any evidence of an electoral cycle in state welfare benefit payments, state Unemployment Insurance benefit payments, or state minimum wages.[22]

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[20] The primary non-price control policies used by state governments have been counter advertising, clean indoor air laws, and youth access laws. State counter advertising campaigns didn't begin until the late 1980s and the first state clean indoor air law wasn't enacted until 1973 (in Arizona). Youth access laws were viewed as largely ineffective until the 1992 Synar Amendment provided incentives for heightened enforcement activity by the states. For a detailed history of each of these policy initiatives, see Chaloupka and Warner (2000).

[21] We have examined how our results change if we include lags and leads of changes in per capita income in the regression specification. These additional state economic controls do not greatly change our elasticity estimates.

[22] Levitt also fails to find electoral cycles in welfare and education spending at the state and local levels.

Third, several studies have shown that conventional standard errors can be inaccurate when there are weak instruments (see, for example, Staiger and Stock (1997)). Hahn and Hausman (forthcoming) have developed a new specification test to determine if the conventional IV asymptotics are reliable in a given situation. Their test involves comparing the 2SLS coefficient of the endogenous regressor to the reciprocal of the 2SLS regression where the endogenous regressor and the left-hand-side variable are switched. Under the null hypothesis that the conventional first order asymptotics are accurate, the two estimates are similar. Using the time period/election interactions and the region/election interactions as our set of instruments, we cannot reject the hypothesis that the forward and reverse 2SLS regressions produce similar estimates, suggesting that our standard errors are reliable.[23]

As an additional specification check, we performed an overidentification test using the time period/election interactions and the region/election interactions as instruments. To implement the test, we took the residuals from the second-stage regressions of the 2SLS estimates shown in Table 7 and regressed them on the instruments and all of the exogenous variables in the model. The test statistic of the validity of the overidentifying restrictions is computed as $N \times R^2$, where N is the number of observations and R^2 is the unadjusted R^2 from the regression of the residuals on the exogenous variables and the instruments. The test statistic is distributed χ^2 with degrees of freedom equal to the number of overidentifying restrictions. Using both sets of instruments, the overidentifying restrictions could not be rejected.[24]

[23] We use these specifications, rather than the one based on the election indicator alone, because the test requires that the system be overidentified.

[24] With the time period/election interactions as instruments, the p-values were 0.71 when state effects were omitted from the model and 0.80 when state effects were included. With the region/election interactions as instruments, the p-values were 0.22 without state effects and 0.74 with state effects included.

Finally, we present some evidence that the cigarette demand elasticity estimates that we obtain using the election instruments are statistically different from estimates that treat tax changes as exogenous. We investigate this from two perspectives. First, we ask whether our elasticity estimates are statistically different than -0.40, the midpoint of the consensus range of price elasticity estimates cited by Chaloupka and Warner (2000). For our 2SLS models, we can reject the hypothesis that the estimated price elasticity is equal to -0.40 with a 95% level of confidence for all specifications considered. In the case of the LIML models, the results are more mixed. In the specification that includes state effects and uses the region/election instrument set, we are able to reject the hypothesis that the true price elasticity is –0.40; however, we are unable to reject this hypothesis using the other specifications.

Second, we ask whether our election-instrumented elasticity estimates are statistically different from the estimates that we obtain using changes in excise taxes as instruments. Because these alternative IV estimates are not nested, we test if they are different by performing an overidentification test that includes both sets of instruments; if the overidentification test fails, then that suggests that the estimates are statistically different. We can reject with a 95% level of confidence the hypothesis that the two IV estimates are the same using the time period/election interactions and the region/election interactions as instruments, but we cannot reject this hypothesis when only the election indicator is used.

2.4 Party Affiliation of the Governor as an Alternative Instrument

Being cyclical by nature, the election instrument could be problematic if there were other cyclical phenomena related to cigarette consumption that had the same periodicity as the election cycle. A priori, we believe that this is unlikely because we use legislative election cycles, which arise at two-year intervals in most states. Also, as discussed previously, the timing of elections varies across states; while most states have two-year election cycles, some have four-year cycles, and while most states hold elections in even calendar years, some hold them in odd years.

Nonetheless, we investigate this possibility by re-estimating our benchmark model from Table 6 using the party affiliation of the state's governor as an instrument for cigarette price changes. Although perhaps "less exogenous" than our election instrument, we believe that this variable provides a useful sensitivity check on our main results for a couple of reasons. First, by comparing differences in cigarette prices (taxes) across Republican and Democratic administrations within states, we are exploiting variation in cigarette prices attributable to the differing attitudes of the two political parties regarding the use of cigarette taxes as a revenue raising device, relative to other taxes. Like the election cycle, this captures the political basis for tax changes. Second, it is not related to an inherently cyclical phenomenon; unlike the election year indicator, which turns on and off with a fixed periodicity (two years in most cases), the party affiliation indicator is switched on for all of the years in which a particular party holds the governorship in a given state.

We begin by estimating the first-stage relationship between the governor's party affiliation and cigarette price changes. Data on party affiliation comes from the *Book of the States*; we classify all governors in our sample period as Republicans, Democrats or Independents. We measure whether cigarette prices change differently during Republican or Independent administrations relative to Democratic regimes. The regression equation is:

$$\Delta \ln Price\ per\ pack_{i,t} = \alpha + \beta_1 GOP_{i,t} + \beta_2 Independent_{i,t} + \Delta \ln X_{i,t}\delta + \varphi_t + \varepsilon_{i,t} \qquad (5)$$

where $GOP_{i,t}$ is an indicator that the governor in state *i* in year *t* is a Republican, $Independent_{i,t}$ is an indicator that the governor is an Independent, and the other variables are defined as before.

The results of this regression are presented in column (1) of Table 9. The coefficient on the Republican governor indicator is positive and statistically different from zero, indicating that cigarette price increases are larger during Republican administrations relative to Democrats. This suggests that Democrats might be more hesitant to increase cigarette taxes, which disproportionately affect lower-income voters. The coefficient on the indicator for an Independent governor is negative but imprecisely estimated. In column (2), state effects are also included in the regression specification; the effect of party affiliation is largely unchanged in this specification.

Next, we use this variation in cigarette price changes to identify the cigarette demand elasticity by instrumenting for price changes with an indicator for the party affiliation of the governor. Results for this 2SLS estimation are shown in Table 10. Focusing first on column (1), the effect of cigarette prices on consumption is negative and statistically different from zero. The estimated elasticity is -1.16, which is substantially larger in absolute value than the estimates which use tax changes as an instrument, but is very similar to the estimates obtained using legislative elections as an instrument. When state effects are added in column (2), the estimated price elasticity declines in absolute value, but it is still substantially larger than the consensus estimates.

Although the estimates using the governor's party affiliation are less precise than those based on election timing, they yield demand elasticities that are quite similar to the elasticities obtained using legislative election cycles. This provides some evidence that our earlier results are not an artifact of some unobserved cyclical phenomena that independently influences cigarette demand. The fact that we obtain similar, and substantially larger, estimates of the price elasticity of demand using a second instrument lends additional support to the view that methodologies which treat cigarette taxes as exogenous (e.g. by using tax changes as an instrument for price), may be subject to bias.

3. Conclusions

We have shown that in the case of excise taxes on cigarettes, utilizing an instrumental variables approach that uses exogenous variation in state tax changes leads to substantially larger estimates of the price elasticity of demand than those derived from other methodologies that treat tax changes as exogenous. These findings are consistent with a policy environment in which state legislators increase excise taxes on cigarettes whenever the demand for cigarettes is high relative to its long-run trend. While we cannot draw any firm conclusions about the exact motives for this type of behavior, two possibilities spring to mind. First, policymakers may look to products with growing demand as attractive targets for revenue-enhancing tax increases. Alternatively, it may be that policymakers are concerned with the public health dimensions of tobacco use, and increase taxes in an effort to curtail consumption whenever demand appears to be growing at an unusually high rate. The former rationale would appear to be more relevant for the bulk of the time period that we study, although health concerns probably have played a role in some tax increases.

Regardless of which motive is at work, our findings indicate that it may be problematic to treat state-level policy changes as having been exogenously determined for purposes of public policy analysis. Instead, policy interventions are best viewed as purposive responses on the part of policymakers to changes in the outcome variable being studied, or perhaps to some third factor that simultaneously influences both the policy and outcome variables. While we do not claim that the models estimated in this paper necessarily provide the best possible estimates of the parameters of interest, the large changes in parameter estimates that occur when plausibly exogenous variation in the policy variable is used adds to a growing body of evidence that policy changes may be jointly determined with the outcomes they are hypothesized to influence (Levitt, 1997; Besley and Case, 2000).

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Table 1: Summary Statistics of Cigarette Consumption Data

	Mean	Minimum	Maximum
	(1)	(2)	(3)
Cigarette Consumption per capita (packs per person per month)	11.70 [2.95]	4.27	29.14
Price of Cigarettes (per pack: 1997 dollars)	1.50 [0.26]	0.87	2.65
Excise Tax on Cigarettes (per pack: 1997 dollars)	0.60 [0.17]	0.17	1.11
State Income (per capita: 1997 dollars)	17703 [4966]	6123	35863

Notes: The sample is yearly data on U.S. states between 1955 and 1997. Data on cigarette consumption, prices and taxes are from the Tobacco Institute. Data on state per capita income are from the Bureau of Labor Statistics. Standard deviations are in brackets.

Table 2: Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using Changes in Cigarette Excise Taxes as an Instrument

	OLS		2SLS	
	(1)	(2)	(3)	(4)
Δ ln Price of Cigarettes	-.3966 (.0415)	-.3852 (.0421)	-.5076 (.1356)	-.4586 (.1417)
Δ ln State Income per capita	.1360 (.0567)	.1160 (.0546)	.1335 (.0570)	.1148 (.0547)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes
Instrument			Δ ln Excise Tax on Cigarettes	Δ ln Excise Tax on Cigarettes
F statistic of instrument in first stage			46.77	43.03
p-value of instrument in first stage			<0.001	<0.001

Notes: Dependent variable is Δ ln Cigarette Consumption per capita. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 3: Lagged Change in Cigarette Consumption as a Predictor of Cigarette Tax Changes

	(1)	(2)
Lagged Δ ln Cigarette Consumption per capita	1.500 (.8207) [.3396]	2.347 (.8512) [.4903]
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is an indicator that the state changed its cigarette excise tax. Estimates are from a probit model. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. Marginal effects are in brackets. The number of observations is 1986.

Table 4: The Election Cycle as a Predictor of Changes in Cigarette Prices

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	-.0073 (.0037)	-.0087 (.0037)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is Δ ln Price of Cigarettes. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 5: The Election Cycle as a Predictor of Changes in Cigarette Consumption

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	.0073 (.0025)	.0092 (.0024)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is Δ ln Cigarette Consumption per capita. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 6: 2SLS Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using the Election Cycle as an Instrument

	(1)	(2)
Δ ln Price of Cigarettes	-1.034	-1.066
	(.3138)	(.3058)
Δ ln State Income per capita	.1219	.1053
	(.0610)	(.0594)
Year Effects	Yes	Yes
State Effects	No	Yes
Instrument	Election Indicator	Election Indicator
F statistic of instruments in first stage	3.97	5.45
p-value of instruments in first stage	0.052	0.024

Notes: Dependent variable is Δ ln Cigarette Consumption per capita. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 7: 2SLS Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using the Election Cycle as an Instrument

	(1)	(2)	(3)	(4)
Δ ln Price of Cigarettes	-.8191	-.8876	-1.318	-.9505
	(.1984)	(.1981)	(.4702)	(.2574)
Δ ln State Income per capita	.1266	.1081	.1156	.1071
	(.0591)	(.0575)	(.0657)	(.0579)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes
Instruments	Time Period $\times$ Election Interactions	Time Period $\times$ Election Interactions	Region $\times$ Election Interactions	Region $\times$ Election Interactions
F statistic of instruments in first stage	2.94	3.28	1.27	2.57
p-value of instruments in first stage	0.062	0.046	0.278	0.017

Notes: Dependent variable is Δ ln Cigarette Consumption per capita. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 8: LIML Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using the Election Cycle as an Instrument

	(1)	(2)	(3)	(4)
Δ ln Price of Cigarettes	-.8573 (.4137)	-.9298 (.3845)	-3.118 (1.665)	-1.124 (.3222)
Δ ln State Income per capita	.1258 (.0421)	.1074 (.0431)	.0758 (.1111)	.1044 (.0467)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes
Instruments	Time Period × Election Interactions	Time Period × Election Interactions	Region × Election Interactions	Region × Election Interactions

Notes: Dependent variable is Δ ln Cigarette Consumption per capita. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 9: The Party Affiliation of the Governor of a State as a Predictor of Changes in Cigarette Prices

	(1)	(2)
Indicator that Governor is Republican	.0030	.0026
	(.0012)	(.0013)
Indicator that Governor is Independent	-.0071	-.0111
	(.0097)	(.0103)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is $\Delta \ln$ Price of Cigarettes. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Table 10: Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using the Party Affiliation of the Governor as an Instrument

	(1)	(2)
Δ ln Price of Cigarettes	-1.156 (.5085)	-.8361 (.5159)
Δ ln State Income per capita	.1192 (.0681)	.1089 (.0610)
Year Effects	Yes	Yes
State Effects	No	Yes
Instruments	Party Affiliation of Governor	Party Affiliation of Governor
F statistic of instruments in first stage	3.56	2.92
p-value of instruments in first stage	0.036	0.063

Notes: Dependent variable is Δ ln Price of Cigarettes. Standard errors are shown in parentheses and are adjusted to take into account the correlation of observations within states. The number of observations is 2036.

Appendix Table: Average Changes in Real State Cigarette Excise Taxes

	After Election Year	After Non-Election Year		After Election Year	After Non-Election Year
Alabama	-0.69	0.23	Montana	-1.15	0.77
Alaska	-1.10	1.26	Nebraska	-0.69	1.45
Arizona	2.35	-0.17	Nevada	-1.18	1.99
Arkansas	-1.52	1.32	New Hampshire	-0.62	0.95
California	0.52	0.61	New Jersey	-1.27	2.32
Colorado	-1.46	1.75	New Mexico	-0.17	0.03
Connecticut	-1.78	3.32	New York	-0.29	2.10
Delaware	-0.70	0.99	North Carolina	-0.24	0.01
Florida	0.48	-0.30	North Dakota	-0.77	1.15
Georgia	-0.38	0.10	Ohio	-0.71	1.28
Hawaii	0.22	1.95	Oklahoma	-0.37	0.04
Idaho	-0.10	0.58	Oregon	0.26	1.00
Illinois	-1.57	2.81	Pennsylvania	-1.72	2.05
Indiana	-0.84	0.72	Rhode Island	1.89	0.16
Iowa	-0.93	1.78	South Carolina	-0.66	0.14
Kansas	-0.13	0.42	South Dakota	-1.30	2.02
Kentucky	-0.35	-0.36	Tennessee	-1.22	0.41
Louisiana	-1.28	-0.47	Texas	-0.91	1.72
Maine	-1.19	1.81	Utah	-0.89	1.01
Maryland	-1.02	0.99	Vermont	-1.25	2.21
Massachusetts	1.91	0.28	Virginia	-0.32	-0.43
Michigan	3.54	-0.83	Washington	-0.82	3.60
Minnesota	-1.52	2.67	West Virginia	0.76	-1.09
Mississippi	-0.61	0.05	Wisconsin	-0.87	2.12
Missouri	-0.56	0.85	Wyoming	-0.80	0.80

Notes: Changes in cigarette excise taxes are denominated in 1997 cents.

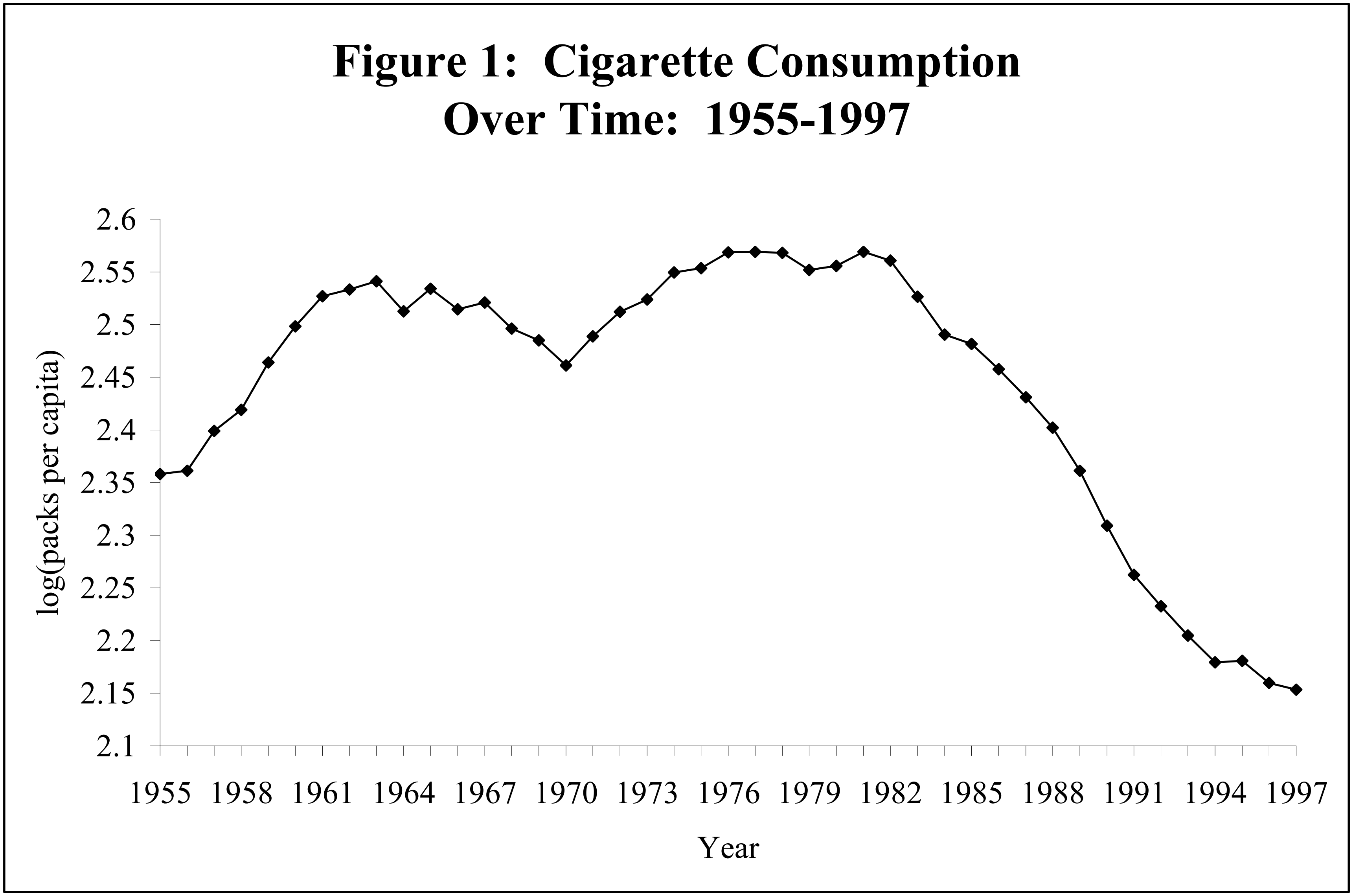
Figure 1: Cigarette Consumption
Over Time: 1955-1997
2.6
2.55
2.5
2.45
2.4
2.35
2.3
2.25
2.2
2.15
2.1
log(packs per capita)
1955 1958 1961 1964 1967 1970 1973 1976 1979 1982 1985 1988 1991 1994 1997
Year

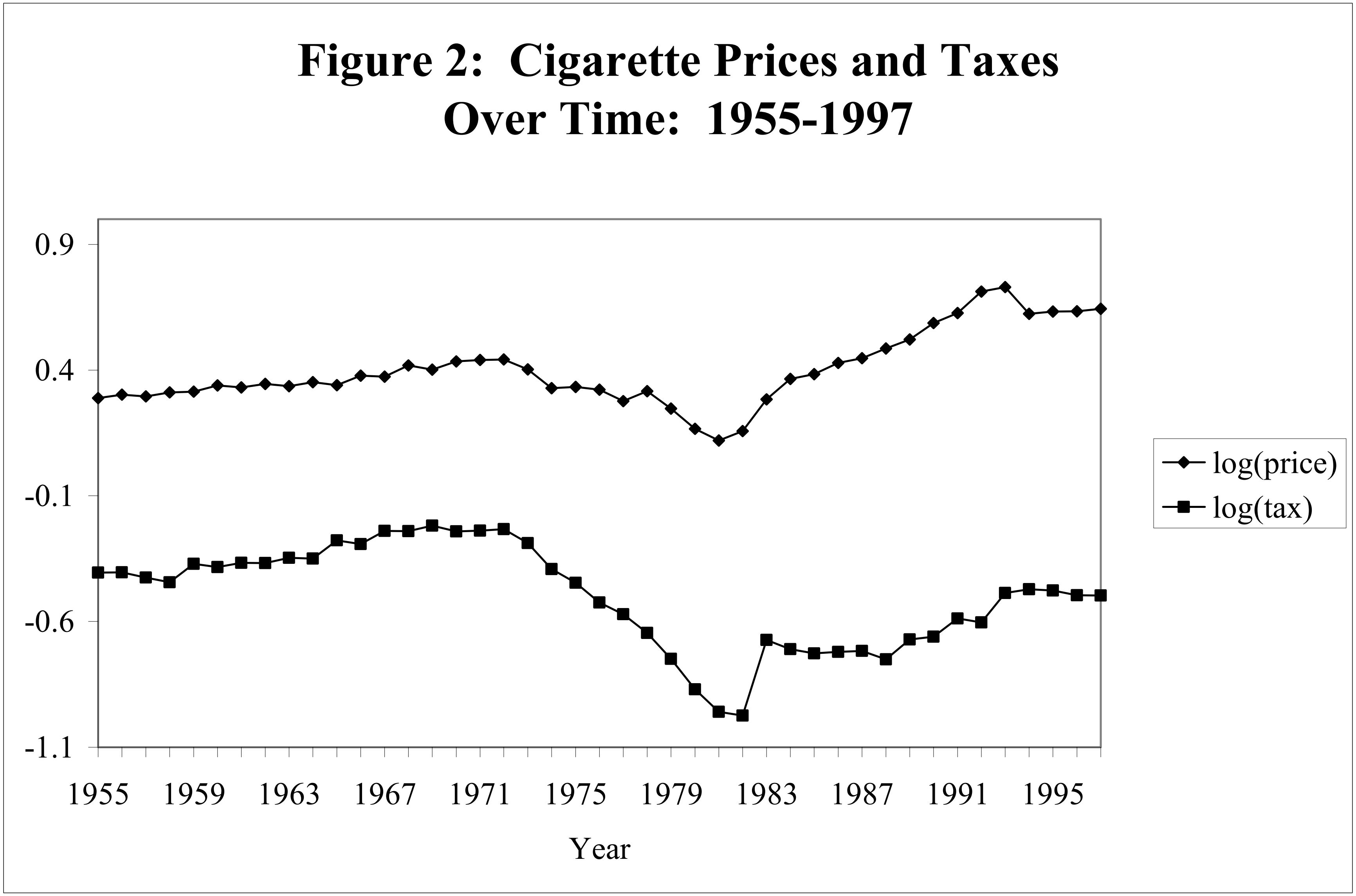
Figure 2: Cigarette Prices and Taxes
Over Time: 1955-1997
0.9
0.4
-0.1
-0.6
-1.1
1955
1959
1963
1967
1971
1975
1979
1983
1987
1991
1995
Year
log(price)
log(tax)

Figure 3: Difference in Changes in Cigarette Taxes (States with Elections Versus States with no Election)