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Jeffrey D. Kubik

*Syracuse University*, [jdkubik@syr.edu](mailto:jdkubik@syr.edu)

John R. Moran

*Syracuse University*

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**CAN POLICY CHANGES BE TREATED  
AS NATURAL EXPERIMENTS?  
EVIDENCE FROM STATE EXCISE TAXES**

**Jeffrey D. Kubik and John R. Moran**

**Center for Policy Research  
Maxwell School of Citizenship and Public Affairs  
Syracuse University  
426 Eggers Hall  
Syracuse, New York 13244-1020  
(315) 443-3114 | Fax (315) 443-1081  
e-mail: [ctrpol@syr.edu](mailto:ctrpol@syr.edu)**

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## **Abstract**

An important issue in public policy analysis is the potential endogeneity of the policies under study. If policy changes constitute responses on the part of political decision-makers to changes in a variable of interest, then standard analyses that treat policy changes as natural experiments may yield biased estimates of the impact of the policy (Besley and Case 2000). We examine the extent to which such political endogeneity biases conventional fixed effects estimates of behavioral parameters by identifying the elasticities of demand for cigarettes and beer using the timing of state legislative elections as an instrument for changes in state excise taxes. In both cases, we find sizable differences between these estimated demand elasticities and the fixed effect estimates cited in Evans, Ringel, and Stech (1999). We conclude that the use of fixed effects estimators in environments where policy interventions are endogenously determined may lead to large biases in the estimated effects of the policies.

# 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 (henceforth, labeled 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. 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.

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 example, 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.

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), 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.<sup>1</sup>

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.<sup>2</sup>

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 treating policy changes as natural experiments can lead to quantitatively important biases in the estimated effects of policy interventions. Here, we examine state tax policies toward cigarettes and beer and demonstrate that using plausibly exogenous variation in state excise taxes leads to large changes in the estimated price elasticities of demand for these products. In particular, we show that when state tax changes are instrumented using the timing of state legislative elections, we obtain substantially larger elasticities of demand than would be obtained using a standard fixed effects specification.

In addition to being of significant interest in their own right, taxes levied on tobacco and alcoholic beverages are 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, as well as the advantages of using instrumental variables methods in lieu of more common fixed effects and difference-in-difference approaches. While we do not claim that the models estimated in this

paper necessarily provide the best possible estimates of the parameters in question, we believe that our results do suggest caution in interpreting findings from models that fail to take into account the endogeneity of state policymaking.

The remainder of the paper is organized as follows. In Section 2, we focus on the demand for cigarettes, and demonstrate that failing to control for the endogeneity of state tax setting could potentially lead to a large bias in the estimated price elasticity of demand. Given this finding, we extend our analysis in Section 3 by examining the demand for beer, another product that has been the object of considerable scrutiny by policymakers in recent years. As in the case of cigarettes, we find a sizeable difference in the estimated impact of a change in taxes when the endogeneity of state tax policy is taken into account. In Section 4, we offer concluding remarks.

## **2. 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 policies (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 demand 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 consumption.<sup>3</sup> Below, we present some suggestive evidence on this point by showing that states that change their excise taxes on cigarettes are more likely to have experienced an increase in cigarette demand in the prior year, 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, Boyle, and Capilouto (1998), who instrument for state cigarette taxes using several state-level political and economic variables.<sup>4</sup>

In this section, 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 presenting evidence that states that change their excise taxes on cigarettes are more likely to have experienced an increase in cigarette consumption the previous year. 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 use 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. Finally, we compare these estimates to those obtained using the

election cycle as an instrument for price, finding evidence that failing to account for the endogeneity of state tax setting could result in a substantial underestimate of the likely impact of taxes on consumption.

## **2.1. Data**

The cigarette data used are a panel of the 50 United 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 (1998).<sup>5</sup> 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 about a dollar and a half on average (measured in 1997 dollars), and excise taxes (both state and federal) accounted for about 40 percent of the price on average.<sup>6</sup>

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 65 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 steadily rose until they peaked in the mid-1990s. Cigarette prices in 1997 were 170 percent 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 Consumption

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 consumption by estimating a regression of changes in state cigarette consumption on changes in state cigarette prices. The regression equation is:

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

where  $\text{Packs per person}_{i,t}$  is the number of packs of cigarettes per person per month consumed in state  $i$  in year  $t$ ;  $\text{Price per pack}_{i,t}$  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.<sup>7</sup>

The coefficient of interest is  $\beta$ , which measures the effect of changes in state cigarette prices on changes in state cigarette consumption. 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 that, 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  $\beta$  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 cigarette demand in any other way. A standard candidate for this instrument is a measure of changes in state cigarette excise taxes.<sup>8</sup> In Column (3) of Table 2, we present the regression estimates of Equation (1) using 2SLS with  $\Delta \ln \text{Excise tax}_{i,t}$  as an instrument. The coefficient on

$\Delta \ln \text{Price per pack}_{i,t}$  is negative and statistically different from zero. The implied demand elasticity of cigarette consumption is  $-0.51$ .<sup>9</sup> 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.<sup>10</sup>

An important assumption underlying these IV estimates is that state legislatures or voters are not influenced by changes in cigarette demand within the state when determining changes in cigarette excise taxes. If states are responding to cigarette demand changes when setting taxes, then demand elasticities estimated using tax changes as instruments can be biased.

There are several reasons why states might take demand changes into account when setting excise taxes. First, public health concerns about the dangers of smoking might cause states to increase excise taxes during periods of increasing demand. Under such a scenario, using excise taxes as an instrument would bias the estimated elasticity upward (towards zero). Warner (1981) and Chaloupka and Warner (2000) argue that there are several periods over the last 50 years when the United States and other countries have responded to public health concerns when setting excise taxes.

Also, state governments might take into account the revenue or political implications of changing cigarette demand when determining taxes. For example, if cigarette demand is growing, then states might be enticed to increase cigarette taxes to take advantage of the greater revenue that will be raised. 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.

If policymakers are responding to changes in cigarette consumption when setting taxes, then states that do not change their tax in a given year are not a good control group for states that

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; 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 states that change their taxes in a year (the treatment group) experienced the same change in cigarette consumption on average the previous year as other states (the control group). 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 regression model of changes in state cigarette consumption on an indicator for whether the state changed its cigarette tax the subsequent year:

$$\Delta \ln \text{Packs per person}_{i,t} = \alpha + \beta \text{Price Change}_{i,t+1} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (2)$$

where  $\text{Price Change}_{i,t+1}$  is an indicator that state  $i$  changed its cigarette tax in year  $t+1$ , and the other variables are defined as above.  $\beta$  measures whether changes in cigarette consumption in a state are different the year before a tax change than other years.

The OLS estimate of Equation (2) is presented in Column (1) of Table 3. The coefficient on the tax change indicator is positive and statistically different than zero, suggesting that cigarette consumption increases on average about 0.6 percent in a state the year before it increases its cigarette excise tax compared to other years. 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.<sup>11</sup>

These results indicate that states that change their cigarette taxes in a given year are different than other states for reasons other than the tax change, even after controlling for 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. We now use the state election cycle as an instrument for the state cigarette price to circumvent this endogeneity problem.

### **2.3. Election Cycles in State Cigarette Excise Tax Changes**

Because his focus was on city crime rates, Levitt (1997) 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 (and later on beer). 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.<sup>12</sup> 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 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:

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

where  $\text{Election previous year}_{i,t}$  is an indicator that the state held a legislative election the previous year and the other variables are defined as before. Here  $\beta$  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 Appendix Table 1. 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:

$$\Delta \ln \text{Packs per person}_{i,t} = \alpha + \beta \text{Election previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (4)$$

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 Effect of Price on Cigarette Consumption

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

The impact of cigarette prices on consumption 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 estimated elasticity using tax changes as the instrument instead of election cycles.

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) 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 demand elasticity is very similar to estimates obtained using only the election cycle as an instrument.

Our results suggest that traditional fixed effects 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 demand 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.<sup>13</sup>

The bottom of Table 6 reports the  $F$  statistic and the partial  $R^2$  of the election cycle instruments in the first-stage estimations. Except for Column (3), the instruments have a statistically significant effect on changes in cigarette prices; however, they explain much less of the variation in cigarette prices than can be explained using 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.

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 estimates, 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.<sup>14</sup>

In many circumstances, LIML performs better than 2SLS when there is finite-sample bias because of weak instruments (see Angrist, Imbens, and Krueger 1999). In Columns (5) and (6) of Table 6, we present the LIML estimates of the elasticity of demand for cigarettes using the region/election interactions as instruments. In Column (5), the price coefficient appears exaggeratedly 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, the instruments perform much better (as shown in Column (6)) 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 demand through non-price means such as clean indoor air laws or ad campaigns.<sup>15</sup> Using this smaller sample, we obtain qualitatively the same cigarette 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 cigarette 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.<sup>16</sup>

We also investigate whether there is direct 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.<sup>17</sup>

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 election indicator interacted with the census regions 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.<sup>18</sup>

As an additional specification check, we performed an overidentification test using 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 Columns (3) and (4) in Table 6 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  $\chi^2$  with degrees of freedom equal to the number of overidentifying restrictions. In both cases, the overidentifying restrictions could not be rejected (p-value = .26 when state effects were not included (Column (3)), and p-value = .82 when state effects were included (Column (4))).

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 percent level of confidence when the region/election interactions are used as instruments. However, we cannot reject this hypothesis when the election indicator is the sole instrument. In the case of the LIML models, the results are also mixed. In the specification that omits state effects, we are unable to reject the hypothesis that the true price elasticity is -0.40; however, we are able to reject this hypothesis using the elasticity estimate from the LIML model that incorporates state effects.

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 percent level of confidence the hypothesis that the two IV estimates are the same using the region/election interactions as instruments but, again, we cannot reject this hypothesis when only the election indicator is used.

### **3. Beer**

In this section, we investigate whether the large changes found in the estimated price elasticity of demand for cigarettes is peculiar to that example, or whether biases associated with policy endogeneity are likely to be a more general problem. To do so, we examine alternative estimates of the price elasticity of the demand for beer, another product that has attracted considerable attention from policymakers in recent decades. The literature on alcoholic beverage consumption shares with the tobacco literature a central focus on the role of taxation as a control policy. Although numerous other control policies have been analyzed (e.g., minimum legal drinking ages, advertising restrictions, etc.), the most robust finding to emerge is the inverse relationship between beverage prices (or taxes) and consumption (Cook and Moore 2000). In the case of beer, most estimates of the own-price elasticity of demand are clustered around a range of -0.20 to -0.40 (Duffy 1990; Johnson et al. 1992; Duffy 1995; Nelson and Moran 1995; Clements, Yang, and Zheng 1997; Salisu and Balasubramanyam 1997; Nelson 1999; Cook and Moore 2000). Although most of these estimates are derived from time series studies using aggregate data, it is worth noting that the OLS price elasticity estimates that we obtain using state panel data (-0.18 and -0.59) lie quite close to this range.

We perform a similar comparison to the one presented in the previous section for cigarettes. We begin by estimating a simple OLS model that treats state beer taxes as exogenous. Using the results from this model, we calculate the price elasticity estimates referenced above. Next, we instrument for beer taxes using the election cycle as our instrument, and compare the associated 2SLS estimates of the beer price elasticity to those derived from ordinary least

squares. As in the case of cigarettes, we find substantially larger price elasticities when the election cycle is used as an instrument, leading us to conclude that conventionally estimated price elasticities for beer are potentially biased by a failure to take into account the endogenous nature of state tax policy.

### **3.1. Data**

The beer data used are a panel of 49 states, with yearly observations running from 1970 to 1997.<sup>19</sup> Information on per capita beer consumption is taken from publications by the National Institute on Alcohol Abuse and Alcoholism (NIAAA). Excise taxes by state are from publications of the Distilled Spirits Council of the United States, and data on state per capita income over time are taken from BLS sources. Unfortunately, information on beer prices by state are not available for this time period.<sup>20</sup>

Summary statistics for this panel are presented in Table 7. On average, slightly fewer than 30 gallons of beer are consumed per capita over this time period, and excise taxes per gallon of beer averaged a little more than \$1 in 1997 dollars.

Figure 4 shows the time series (in logs) of per capita beer consumption in the sample. Consumption increased until the early 1980s, then has decreased steadily for the next two decades. The time series (also in logs) of beer taxes in the sample is presented in Figure 5. Except for a large increase in the federal excise tax in 1991, beer taxes have steadily fallen over the course of the sample period.

### **3.2. OLS Estimates of the Price Elasticity of Beer Consumption**

Unlike our cigarette application, Figures 4 and 5 show that there does not appear to be a strong relationship in the movements of beer taxes and consumption over time. However, we attempt to measure a causal effect of taxes on beer consumption in a more formal way by estimating a regression of state per capita beer consumption on state beer taxes. The regression equation is:

$$\Delta \ln \text{Gallons per person}_{i,t} = \alpha + \beta_1 \Delta \ln \text{Tax per gallon}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (5)$$

where  $\text{Gallons per person}_{i,t}$  is the number of gallons of beer consumed per person in state  $i$  in year  $t$ .  $\text{Tax per gallon}_{i,t}$  is the state excise tax on beer in state  $i$  and year  $t$ , and the other variables are defined the same as before.<sup>21</sup>

The coefficient of interest is  $\beta$ , which measures the elasticity of beer consumption with respect to changes in the beer tax. The OLS estimate of Equation (5) is presented in Column (1) of Table 8. The coefficient on state beer taxes is negative and statistically different from zero, indicating that a 10 percent increase in beer taxes is associated with about a 0.28 percent decrease in beer demand. In Column (2), we add state effects to the regression specification; the coefficient on beer taxes is almost identical to the previous estimate.

Because our benchmark model is estimated using OLS, we are concerned that estimating our model in differences might exacerbate the potential biases associated with measurement error. This was not an issue in the cigarette application because there the benchmark model was estimated using 2SLS, which produces consistent estimates even in the presence of measurement error. When we compare the estimated beer elasticity from this OLS regression to one based on our election cycle instrument, we would like to minimize the chances that any difference between the two is due to measurement error bias in the OLS estimate. Therefore, we also estimate the OLS benchmark in levels to lessen the potential bias due to measurement error.

The level estimates are presented in the last half of Table 8. Column (3) displays estimates of the regression of beer consumption measured in levels on the state excise tax, state per capita income, and state and year effects.<sup>22</sup> The estimated beer tax elasticity is almost three times larger in absolute value than the equivalent difference estimate in Column (1), suggesting that substantial measurement error is biasing the difference estimates toward zero. In Column (4), we add state linear trends to the level regression specification; the coefficient on the beer tax

falls substantially in absolute value. The beer tax elasticity in Column (4) is almost identical to the equivalent elasticity estimated in differences in Column (2).

For purposes of comparison with the existing literature, we converted these tax elasticities into price elasticities under the assumption that beer taxes are passed through to prices on a one-for-one basis.<sup>23</sup> Given this assumption, we computed a price elasticity for each tax elasticity estimate using averages for beer consumption (29.23 gallons per person per year) and state beer taxes (\$1.05 per gallon in 1997 dollars) over our sample period. To complete the conversions, we need information on the average beer price in effect over the sample period, which we estimate as \$7.74 per gallon (in 1997 dollars).<sup>24</sup> Calculated at these sample averages, the OLS price elasticity estimates (for the models run in levels) are -0.18 and -0.59 for the specifications with and without state-specific trends, respectively. These estimates are quite close to other price elasticity estimates for beer found in the literature (Cook and Moore 2000).

An important assumption underlying these OLS estimates is that changes in state beer excise taxes are not influenced by changes in beer consumption. If states are responding to beer demand when setting taxes, then demand elasticities identified using variation in state beer taxes over time can be biased. As with the cigarette example, state legislators might be motivated by either public health concerns or revenue needs when setting beer taxes; therefore, we use the state election cycle as an instrument for state beer taxes to avoid these potential political endogeneity problems.

### **3.3. Election Cycles in State Beer Excise Tax Changes**

We first measure whether there is a relationship between the election cycle and beer tax changes, examining whether states change beer taxes differently after election years compared to other years. The regression equation is:

$$\Delta \ln Tax \text{ per gallon}_{i,t} = \alpha + \beta Election \text{ previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (6)$$

where the variables are defined as above. Here  $\beta$  measures whether beer excise taxes change differently in years following state legislative elections than in other years.

The results of this regression are presented in Column (1) of Table 9. The coefficient on the state election indicator is positive and statistically significant from zero, indicating that beer taxes increase more after election years than after non-election years. The magnitude of the coefficient suggests that changes in beer taxes are about 2.4 percent higher after election years than in other years. Adding state effects to the regression specification in Column (2) does not change this conclusion.<sup>25</sup>

The propensity of states to increase beer taxes after election years also emerges when the data is analyzed on a year-by-year basis. Figure 6 displays a plot of the difference in the changes in excise taxes for states with and without elections the previous year. States with elections in the previous year increase their beer tax more, on average, than other states during 20 of the 27 years in our sample.

We can also examine the relationship between beer excise tax changes and elections by analyzing the data on a state-by-state basis. A full list of states, along with information on mean changes in beer taxes after election and non-election years is provided in Appendix Table 2. In 42 of the 49 states, the mean change in beer taxes is greater after election years compared to non-election years.

Interestingly, this result is the opposite of what we found for cigarette taxes, suggesting that a different political dynamic may exist in the case of alcoholic beverages. While explaining the political economy of state tax changes is beyond the scope of this paper, several possible reasons for this difference can be advanced. First, alcohol consumption, particularly drunk driving, may have been viewed as a more serious problem than smoking for most of our sample period, which could have made beer taxes more politically palatable than cigarette taxes. Second,

there appears to have been considerably more organized support for beer taxes from groups like Mothers Against Drunk Driving (MADD) than there was for cigarette taxes during our sample period.<sup>26</sup> As a result, state elected officials might have sought to appease these groups by passing tax increases during election years. Finally, it may be that the beer industry was simply less organized or less effective in lobbying against tax increases than was the tobacco industry.

Given that beer excise taxes are higher after state election years, then if taxes affect beer consumption, a reduced-form relationship between elections and beer consumption should emerge. The reduced-form specification is:

$$\Delta \ln \text{Gallons per person}_{i,t} = \alpha + \beta \text{Election year previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (7)$$

where the variables are defined as above. The estimates of Equation (7) are shown in Column (1) of Table 10. The coefficient on the election indicator is negative, suggesting that beer consumption falls about 0.59 percent more after election years than in other years. When state effects are added to the regression specification in Column (2), the negative relationship weakens slightly.

### **3.4. Using Election Cycles to Estimate the Effect of Price on Beer Consumption**

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

The impact of beer taxes on consumption is estimated using 2SLS. The results from estimating Equation (5) using the election cycle instrument are presented in Column (1) of Table 11. The effect of beer taxes on consumption is again negative; the tax elasticity implied by the coefficient is -0.25, which is about three times bigger in absolute value than the corresponding OLS elasticity estimate. Adding state effects to the regression specification does not greatly

change the elasticity estimate; however, the coefficients are not statistically different from zero in either specification.

We again expand the instrument set by allowing the effect of elections to vary across the nine census regions. Columns (3) and (4) present the 2SLS estimates using this larger instrument set. Again, the estimated tax elasticity is much larger than the corresponding OLS estimate, and the precision of the estimates is much greater; both tax elasticities are now statistically different from zero. Using the same procedure as was used for the OLS estimates, we converted the estimated tax elasticities into price elasticities, obtaining estimates of -1.08 and -1.59 for the specifications with and without state effects. These elasticity estimates are approximately six and three times larger than their respective OLS counterparts.

One concern with our estimates is that part of the difference between our OLS and 2SLS results might be because of measurement error in the OLS estimate. With classical measurement error, OLS coefficients are biased toward zero; instrumenting solves this problem. At this point, we do not know how much of the difference between our estimates is due to political endogeneity problems and how much is due to measurement error. But in either case, our estimated beer elasticities are much larger in absolute value than the usual estimates.

As in the cigarette application, there is the possibility that other policy interventions may be correlated with the timing of elections. In the case of beer, the most important policy changes occurring during our sample period (other than the tax changes that we analyze) were changes in state minimum legal drinking ages.<sup>27</sup> To examine whether changes in the minimum legal drinking age (MLDA) are confounding our results, we re-estimated our 2SLS models using only the period (1989 to 1997) during which the MLDA was uniform across states.<sup>28</sup> The tax elasticity estimates that we obtain are essentially the same as those obtained for the entire sample, suggesting that changes in the MLDA are not biasing our results.

Again, we need to worry about the potential problems of weak instruments. Because our 2SLS estimates are far away from the OLS estimates, if there is significant finite-sample bias, then this would suggest that the true elasticity is even farther away from the OLS result than our findings would indicate.<sup>29</sup> We also present LIML estimates of the tax elasticity in Columns (5) and (6) of Table 11. In both specifications, the estimated elasticity is close to our 2SLS estimates and much larger in absolute value than the OLS estimates. As with our cigarette application, we again perform the Hahn and Hausman (forthcoming) test to determine whether the conventional standard errors are accurate. Using the election indicator interacted with the census regions, we cannot reject the hypothesis that the forward and reverse 2SLS estimates are similar, suggesting that there is no problem with using the first-order asymptotics.

We also performed overidentification tests for the case where the region/election interactions were used as instruments. In the specifications with and without state effects, the overidentifying restrictions could not be rejected (p-value = .59 with state effects and p-value = .89 without state effects).

Finally, we present some evidence that the estimated tax elasticities we obtain using the election cycle as an instrument are statistically different from estimates that treat tax changes as natural experiments. As in the case of cigarettes, we examine this from two perspectives. First, we ask whether our estimates are statistically different from  $-0.30$ , the midpoint of the consensus range of price elasticity estimates cited by Cook and Moore (2000). For comparison with our estimates, which are *tax*, rather than *price*, elasticities, we convert the  $-0.30$  price elasticity estimate into a tax elasticity under the assumption of one-for-one pass through of taxes to prices. Evaluated at the mean values of taxes and prices in our sample (expressed in 1997 dollars), this leads to a consensus tax elasticity estimate of  $-0.04$ . Using our 2SLS models, we are only able to reject the hypothesis that the estimated tax elasticity is equal to  $-0.04$  for one of the four specifications; the other 2SLS estimates are too imprecise to reject this hypothesis.<sup>30</sup> However,

using the estimates from our LIML models, we are able to reject this hypothesis at a 95 percent level of confidence for the specification without state effects, and at a 90 percent level of confidence for the specification that includes state effects.

Second, we ask whether our election-instrumented elasticity estimates are statistically different from the OLS estimates we present in Table 8. To determine if these estimates are different, we perform a Hausman test using the OLS estimates from the first differences specification in Table 8. For the models that use only the election indicator as an instrument, we are unable to reject the null hypothesis of equal elasticities. However, when the region/election interactions are used, we are able to reject the equal elasticity hypothesis at a 95 percent level of confidence for the specification without state effects, and at a 90 percent level of confidence for the specification that includes state effects.

## 4. Conclusions

We have shown that in the case of two “sin” taxes, utilizing an instrumental variables approach that uses plausibly exogenous variation in state tax changes leads to substantially larger estimates of the price elasticities 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 or voters increase excise taxes on beer and cigarettes whenever the demand for these goods is high relative to their long-run trends. While we cannot draw any firm conclusions about the exact motives for this type of behavior, two possibilities spring to mind. First, policymakers may be concerned with the public health dimensions of alcohol and tobacco use, and may increase taxes in an effort to curtail consumption whenever consumption appears to be growing at an unusually high rate. Alternatively, it may be that policymakers are not concerned about the health consequences of alcohol and tobacco use, but instead look to products with growing demand as attractive targets for revenue-enhancing 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 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 variables is used suggests caution in interpreting results from models that fail to control for the endogeneity of state policymaking.

On a more positive note, this paper illustrates the potential usefulness of a simple approach for dealing with policy endogeneity first developed by Levitt to determine the causal effect of police on crime rates. The virtues of this approach are its simplicity, the readily

available nature of the data, and its potential generalizability; a priori it seems quite reasonable to believe that other policy variables may be subject to election cycles. A potential drawback is that relatively long panels of data may be needed to generate sufficient variation in the instruments and endogenous explanatory variables.

## Endnotes

- \* Kubik: Center for Policy Research; Syracuse University; 426 Eggers Hall; Syracuse, NY 13244-1020. Email: [jdkubik@maxwell.syr.edu](mailto:jdkubik@maxwell.syr.edu). Moran: Center for Policy Research; Syracuse University; 426 Eggers Hall; Syracuse, NY 13244-1020. Email: [jmoran@maxwell.syr.edu](mailto:jmoran@maxwell.syr.edu). We thank Dan Black; Gary Engelhardt; Jonathan Gruber; Doug Holtz-Eakin; Ted Joyce; Robert Kaestner; Don Kenkel; Tom Kniesner; Stuart Rosenthal; John Tauras; John Yinger; and seminar participants at Cornell University, Syracuse University, and the 12<sup>th</sup> Annual Health Economics Conference for helpful suggestions. We also thank Jonathan Gruber for generously providing us with data and Jim Williamson for assisting in the collection of data.
1. 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.
  2. While their analysis demonstrates a significant effect of female political participation on workers’ compensation benefits, it remains unclear whether this instrument can be treated as exogenous. For example, it may be the case that states with more rapidly growing economies, and therefore rising employment and wages, may also be more predisposed to elect female legislators. Although this does not appear to be a problem in the workers’ compensation example analyzed in their paper, as a general rule using political *outcomes* as instruments for policy choices would seem to be a potentially problematic identification strategy.
  3. 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).
  4. In particular, they use per capita 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 probability of smoking using their instrumental variables approach than when cigarette taxes are treated as exogenous.
  5. 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.

6. 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.
7. This empirical specification is widely used in the tobacco literature (see, for example, Evans and Huang 1998; Evans and Ringel 1999; Farrelly, Sfekas, and Hanchette 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, Sfekas, and Hanchette (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.
8. 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.
9. 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; Keeler et al. 1996), we find that on average increases in state excises taxes lead to more than 100 percent pass through of the tax to cigarette prices.
10. 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.
11. 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, we find that changes in cigarette consumption Granger cause changes in cigarette excise taxes. (We also find that taxes Granger cause consumption).
12. Like the United States 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.

13. 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 heard little from their constituents on the issue," (Jacobson, Wasserman, and Raube 1993, 812-813).
14. 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 percent. 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 percent. The same cannot be said for the specifications based on the region/election interactions. For those estimates, 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 fixed effects estimates understate the true price elasticity of demand.
15. 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).
16. 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 demand elasticity estimates.
17. Levitt (1997) also fails to find electoral cycles in welfare and education spending at the state and local levels.
18. We use this specification, rather than the one based on the election indicator alone, because the test requires that the system be overidentified.
19. Data from Hawaii are not used in our analysis because, for several years during our sample period, Hawaii imposed an *ad valorem* tax on beer. Without information on beer prices in Hawaii, we do not know the value of the tax.
20. Beer prices are collected quarterly across states by the American Chamber of Commerce Research Association (ACCRA); however, their price series begins in 1982. Also, the ACCRA price data are only for one brand of beer and there are significant gaps in the data for various states and years.

21. It is worth reiterating that by including year effects in the model we identify the tax elasticity using only legislatively enacted tax changes, and not variation in the real value of the tax due to inflation.
22. State fixed effects are automatically removed when differencing the data. Thus, the model presented in Column (3), which is estimated in levels, uses the same variation in the data as the model presented in Column (1), which is estimated in first differences.
23. In markets characterized by imperfect competition, taxes may be more than fully shifted to consumers (Besley 1989; Katz and Rosen 1985). In the case of retail sales taxes, Poterba (1996) found approximately one-for-one shifting, while Besley and Rosen (1999) find partial pass-through for some products and greater than 100 percent pass through for others. Young and Bielinska-Kwapisz (forthcoming) argue that results based on sales taxes may not generalize to excise taxes, and provide evidence that alcohol taxes may be significantly over-shifted. For beer, their preferred estimate implies that beer prices increase by \$1.71 for each \$1.00 increase in beer taxes. Using 1.71 instead of 1.00 in our price elasticity calculations leads to elasticities of -0.10 and -0.34 for the OLS models (in levels) with and without state trends, and -0.63 and -0.93 for the 2SLS models with and without state trends.
24. To calculate this average, we took the average nominal beer price for 1997 and used the beer CPI to construct nominal beer prices for each year in the sample. We then used the overall CPI to convert the nominal price for each year into 1997 dollars. Averaging these numbers resulted in an average beer price of \$7.74 per gallon (in 1997 dollars) for our sample period.
25. Recall that in our first-difference specifications, state effects capture linear state time trends in the data.
26. MADD was founded in 1980 and remains active in state politics to this day.
27. See Ruhm 1996; Dee 1999.
28. All but seven states had adopted a MLDA of 21 by 1989. The exceptions (Colorado, Idaho, Louisiana, Minnesota, Montana, Ohio, and Vermont) were dropped from the analysis.
29. As was the case in the cigarette application, the simulations conducted by Bound, Jaeger, and Baker (1995) do not indicate significant finite sample bias in the specifications that use the election indicator as the sole instrument.
30. This is the specification based on the nine region/election interactions without state effects.

**Table 1. Summary Statistics of Cigarette Consumption Data**

	Mean (1)	Minimum (2)	Maximum (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 all 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.

Source: Authors' calculations.

**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)
$\Delta \ln$ Price of Cigarettes	-.3966 (.0235)	-.3852 (.0234)	-.5076 (.0783)	-.4586 (.0795)
$\Delta \ln$ State Income per Capita	.1360 (.0376)	.1160 (.0378)	.1335 (.0379)	.1148 (.0379)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes
Instrument			$\Delta \ln$ Excise Tax on Cigarettes	$\Delta \ln$ Excise Tax on Cigarettes
<i>F</i> statistic of instrument in first stage			199.25	185.97
p-value of instrument in first stage			<0.001	<0.001
Partial $R^2$ of instrument in first stage			.0386	.0368

Notes: Dependent variable is  $\Delta \ln$  Cigarette Consumption per capita. The number of observations is 2036.

Source: Authors' calculations.

**Table 3. Changes in Cigarette Consumption the Year Before State Tax Changes**

	(1)	(2)
Indicator that State	.0064	.0079
Increased Tax Subsequent Year	(.0030)	(.0028)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is  $\Delta \ln$  Cigarette Consumption per capita. Number of observations is 2036.

Source: Authors' calculations.

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

	(1)	(2)
Indicator That State Held Legislative Election Previous Year	-.0073 (.0030)	-.0087 (.0031)
Year Effects	Yes	Yes
State Effects	No	Yes

*Notes:* Dependent variable is  $\Delta \ln$  Price of Cigarettes. Number of observations is 2036.

Source: Authors' calculations.

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

	(1)	(2)
Indicator That State Held Legislative Election Previous Year	.0073 (.0034)	.0092 (.0035)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is  $\Delta \ln$  Cigarette Consumption per capita. The number of observations is 2036.

Source: Authors' calculations.

**Table 6. Estimates of the Elasticity of Cigarette Consumption with Respect to Price Using the Election Cycle As an Instrument**

	2SLS				LIML	
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln$ Price of Cigarettes	-1.034 (.5083)	-1.066 (.4473)	-1.328 (.4642)	-0.9505 (.2616)	-3.118 (1.665)	-1.124 (.3222)
$\Delta \ln$ State Income per Capita	.1219 (.0455)	.1053 (.0458)	.1156 (.0512)	.1071 (.0432)	.0758 (.1111)	.1044 (.0467)
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes	No	Yes
Instrument	Election Indicator	Election Indicator	Region $\times$ Election Interactions			
$F$ statistic of instruments in first stage	5.78	7.66	1.01	2.26		
p-value of instruments in first stage	0.016	0.006	0.433	0.016		
Partial $R^2$ of instruments in first stage	0.0013	0.0016	.0020	.0044		

Notes: Dependent variable is  $\Delta \ln$  Cigarette Consumption per capita. The number of observations is 2036.

Source: Authors' calculations.

**Table 7. Summary Statistics of Beer Consumption Data**

	Mean (1)	Minimum (2)	Maximum (3)
Beer Consumption per Capita (gallons per person per year)	29.23 [5.68]	13.33	50.66
Excise Tax on Beer (per gallon: 1997 dollars)	1.05 [0.52]	0.38	4.38
State Income (per capita: 1997 dollars)	19925 [3768]	10809	35863

Notes: The sample is yearly data on all U.S. states between 1970 and 1997. Data on beer consumption are from the National Institute on Alcohol Abuse and Alcoholism (1999). Data on state beer taxes are from the Distilled Spirits Council of the United States (1999). Data on state per capita income are from the Bureau of Labor Statistics. Standard deviations are in brackets.

Source: Authors' calculations.

**Table 8. OLS Estimates of the Elasticity  
of Beer Consumption with Respect to Excise Taxes**

	Differences		Levels	
	(1)	(2)	(3)	(4)
Ln State Beer Tax	-.0275 (.0094)	-.0230 (.0095)	-.0796 (.0081)	-.0243 (.0074)
Ln State Income per Capita	.2062 (.0412)	.1956 (.0413)	.2497 (.0376)	.3227 (.0309)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	Yes	Yes
Linear State Trends	No	No	No	Yes

Notes: Dependent variable is ln Beer Consumption per Capita. The number of observations is 1323.  
Source: Authors' calculations.

**Table 9. The Election Cycle as a Predictor of Changes in Beer Taxes**

	(1)	(2)
Indicator That State Held Legislative Election Previous Year	.0243 (.0094)	.0247 (.0097)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is  $\Delta \ln$  Beer Tax. Number of observations is 1323.

Source: Authors' calculations.

**Table 10. The Election Cycle as a Predictor of Changes in Beer Consumption**

	(1)	(2)
Indicator That State Held Legislative Election Previous Year	-.0059 (.0032)	-.0053 (.0033)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is  $\Delta \ln$  Beer Consumption per Capita. The number of observations is 1323.  
Source: Authors' calculations.

**Table 11. Estimates of the Elasticity of Beer Consumption with Respect to Excise Taxes Using the Election Cycle as an Instrument**

	2SLS				LIML	
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln$ Beer Excise Tax	-.2506 (.1557)	-.2248 (.1527)	-.2153 (.0642)	-.1463 (.0719)	-.2398 (.0703)	-.2797 (.1221)
$\Delta \ln$ State Income per Capita	.1766 (.0535)	.1722 (.0514)	.1813 (.0479)	.1813 (.0448)	.1780 (.0496)	.1659 (.0539)
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes	No	Yes
Instrument	Election Indicator	Election Indicator	Region $\times$ Election Interactions			
<i>F</i> statistic of instruments in first stage	6.70	6.49	4.14	2.75		
p-value of instruments in first stage	0.010	0.011	<0.001	0.004		
Partial $R^2$ of instruments in first stage	.0050	.0048	.0272	.0161		

Notes: Dependent variable is  $\Delta \ln$  Beer Consumption per Capita. The number of observations is 1323.

Source: Authors' calculations.

**Appendix Table 1. 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.

Source: Authors' calculations.

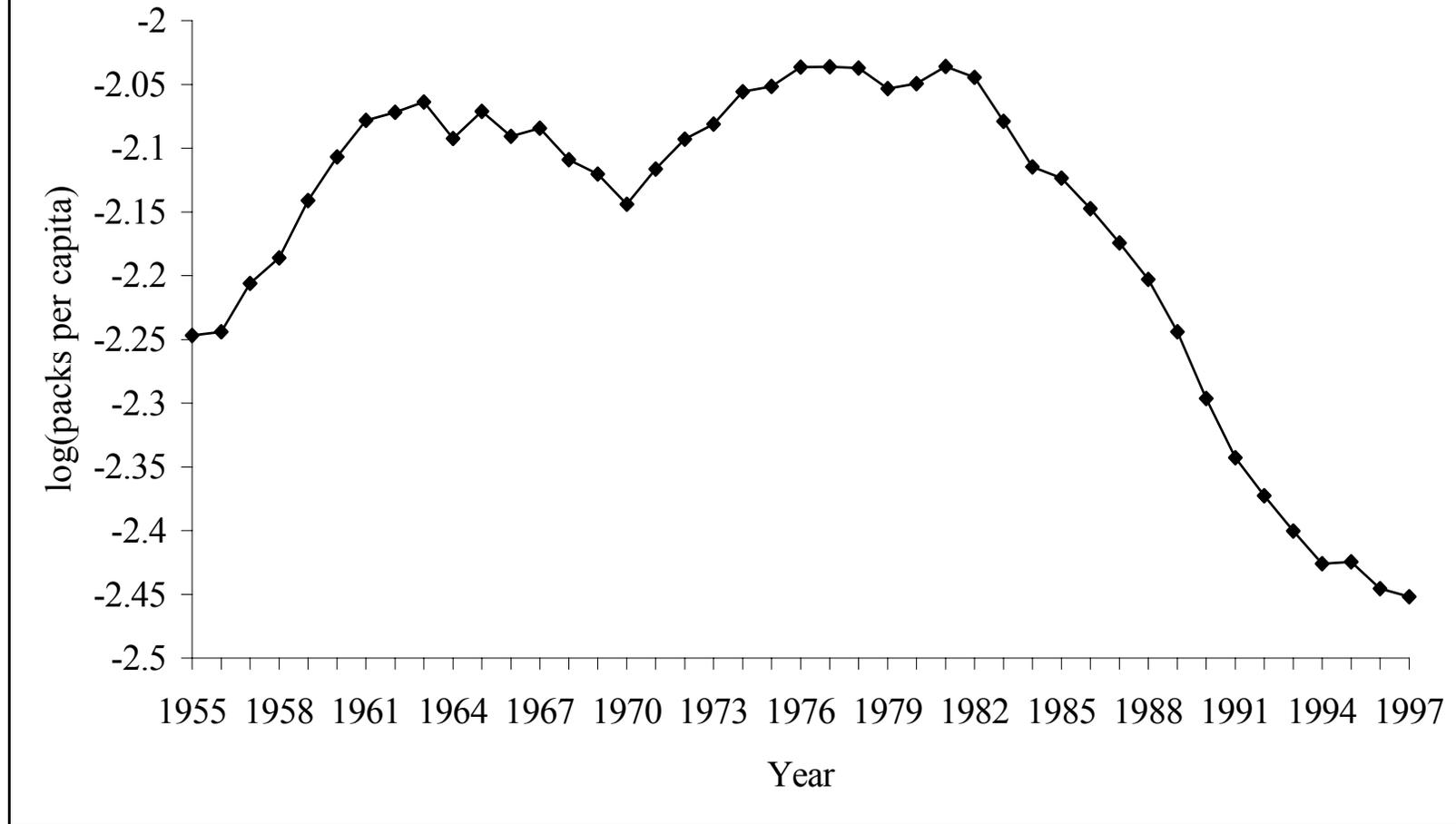
**Appendix Table 2. Average Changes in Real State Beer Excise Taxes**

	After Election Year	After Non-Election Year		After Election Year	After Non-Election Year
Alabama	-6.26	-6.16	Montana	-0.73	-1.19
Alaska	-1.85	-3.26	Nebraska	0.16	-0.94
Arizona	-1.07	-0.17	Nevada	-0.39	-0.80
Arkansas	-2.70	-2.93	New Hampshire	0.09	-1.61
California	0.80	-0.60	New Jersey	0.35	-0.45
Colorado	-0.81	-0.43	New Mexico	1.31	-0.80
Connecticut	0.05	-1.13	New York	-0.03	-0.15
Delaware	-0.81	0.02	North Carolina	-5.94	-6.46
Florida	-1.76	-4.59	North Dakota	-1.79	-1.94
Georgia	-5.36	-5.81	Ohio	-1.63	-1.95
Hawaii			Oklahoma	-3.77	-3.10
Idaho	-1.67	-1.82	Oregon	0.13	-0.82
Illinois	-0.78	-0.85	Pennsylvania	-0.89	-0.97
Indiana	-0.43	-1.20	Rhode Island	-0.45	-0.82
Iowa	-1.06	-1.21	South Carolina	-8.57	-9.30
Kansas	-0.39	-2.02	South Dakota	-2.92	-2.96
Kentucky	-0.96	-0.91	Tennessee	-1.04	-1.39
Louisiana	-3.63	-3.78	Texas	-1.20	-1.63
Maine	-2.79	-3.02	Utah	0.43	-1.84
Maryland	-0.89	0.14	Vermont	-2.64	-3.07
Massachusetts	-0.74	-1.22	Virginia	-1.69	-2.72
Michigan	-2.27	-2.46	Washington	1.12	-0.74
Minnesota	-0.51	-1.59	West Virginia	-1.98	-2.15
Mississippi	-4.47	-5.16	Wisconsin	-0.36	-0.39
Missouri	0.22	-0.73	Wyoming	-0.22	-0.24

Notes: Changes in beer excise taxes are measured per gallon of beer and are denominated in 1997 cents.

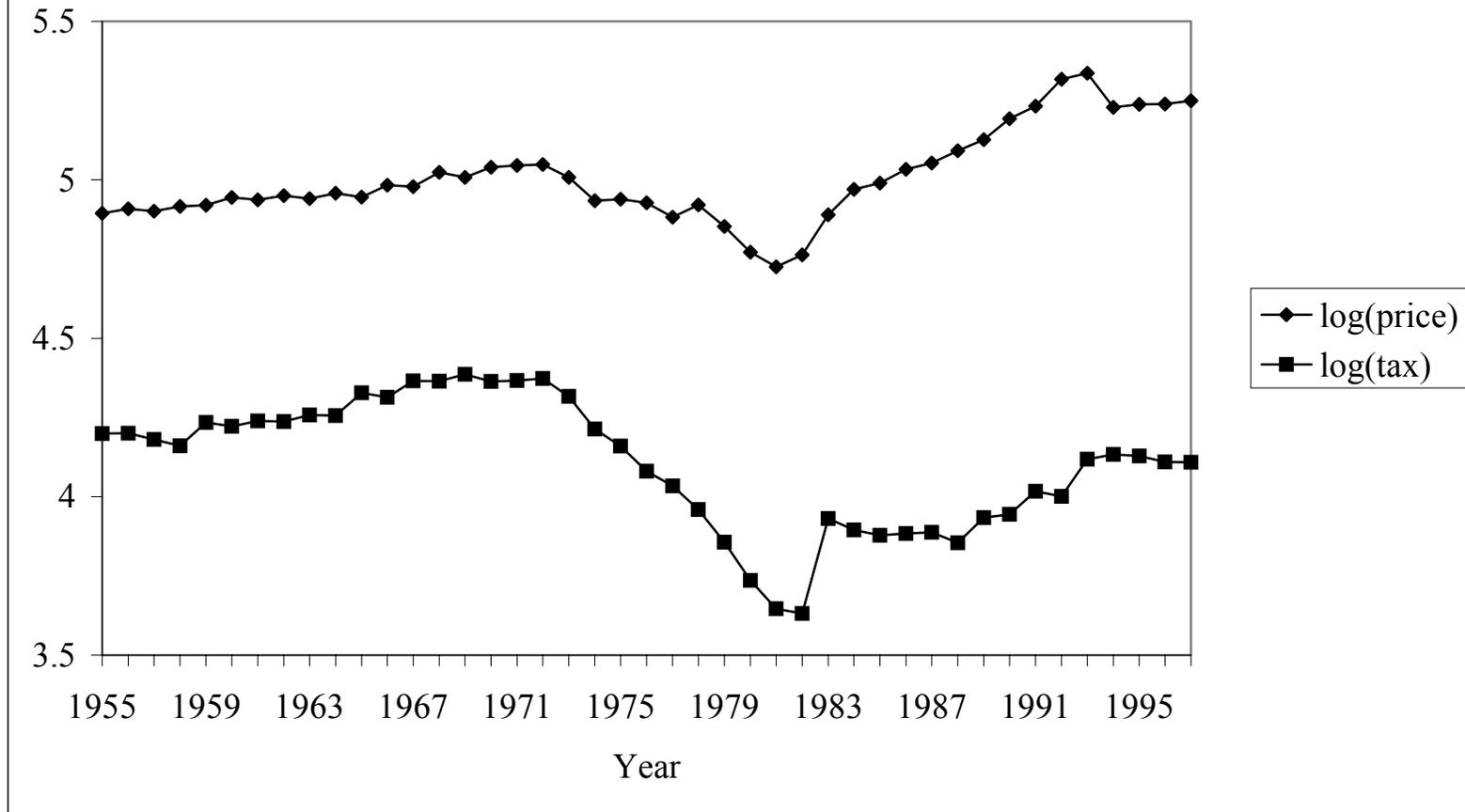
Source: Authors' calculations.

**Figure 1. Cigarette Consumption over Time,  
1955-1997**



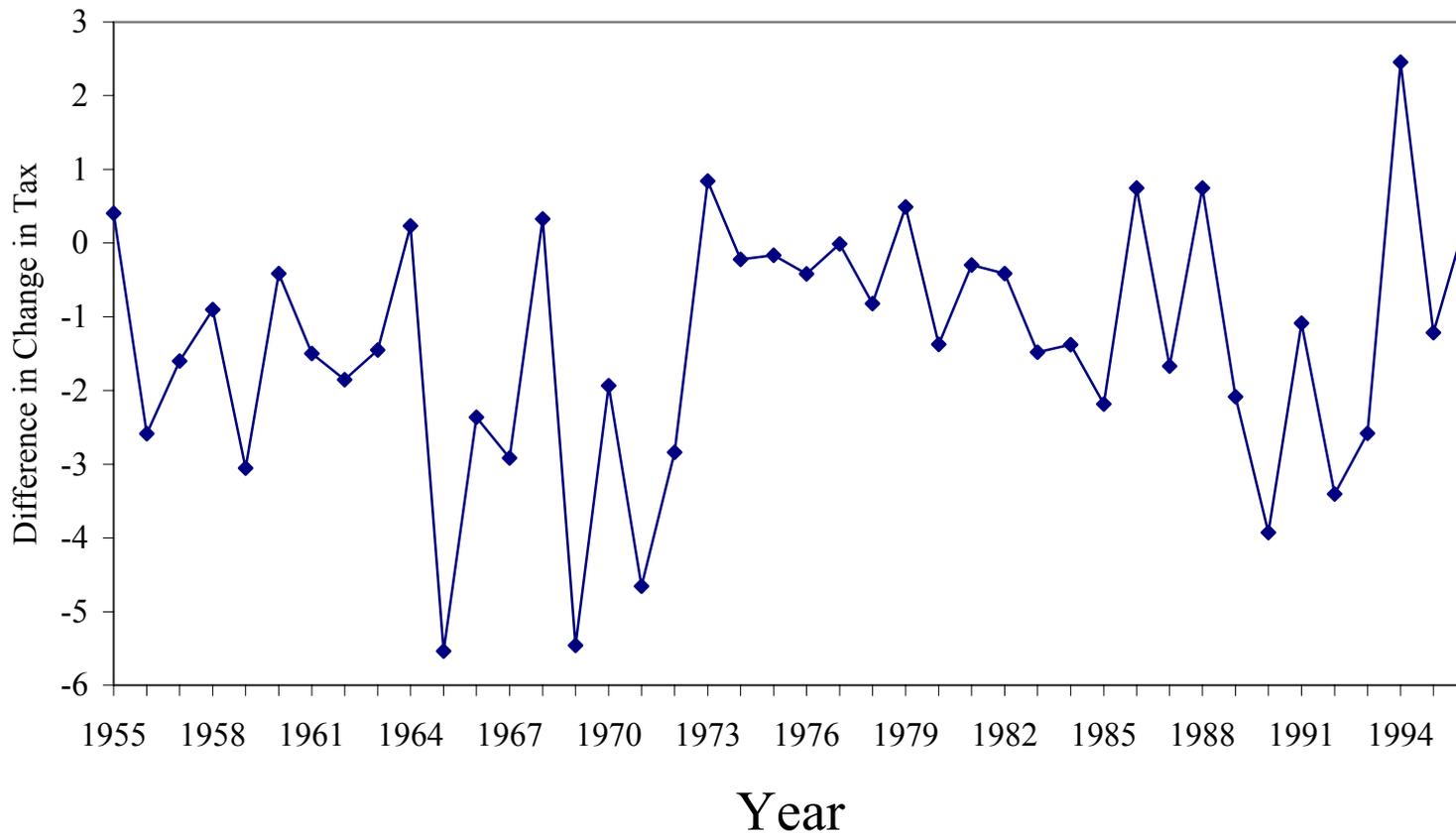
Source: Authors' calculations.

**Figure 2. Cigarette Prices and Taxes over Time, 1955-1997**



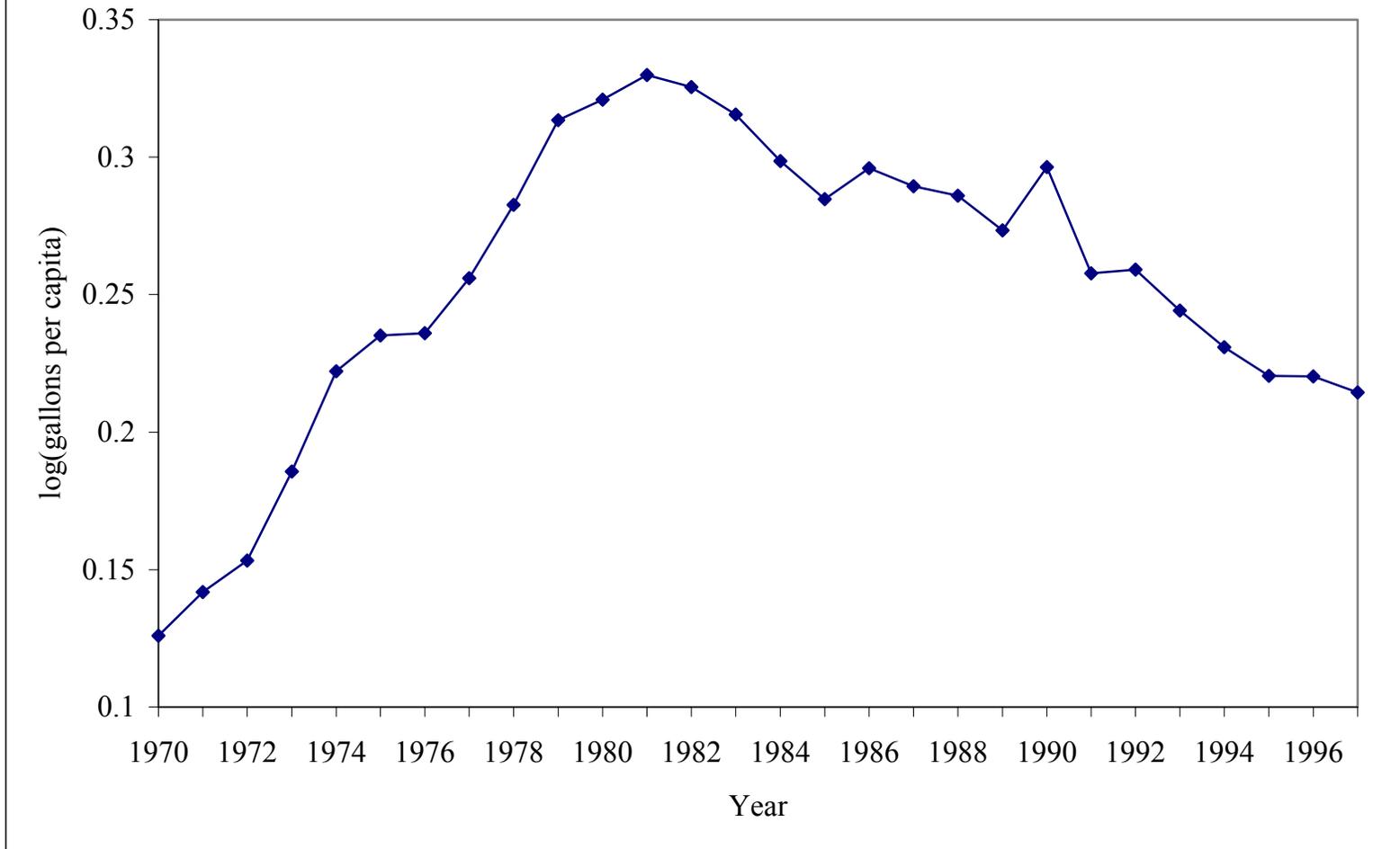
Source: Authors' calculations.

**Figure 3. Difference in Changes in Cigarette Taxes  
(States with Elections versus States with No Election)**



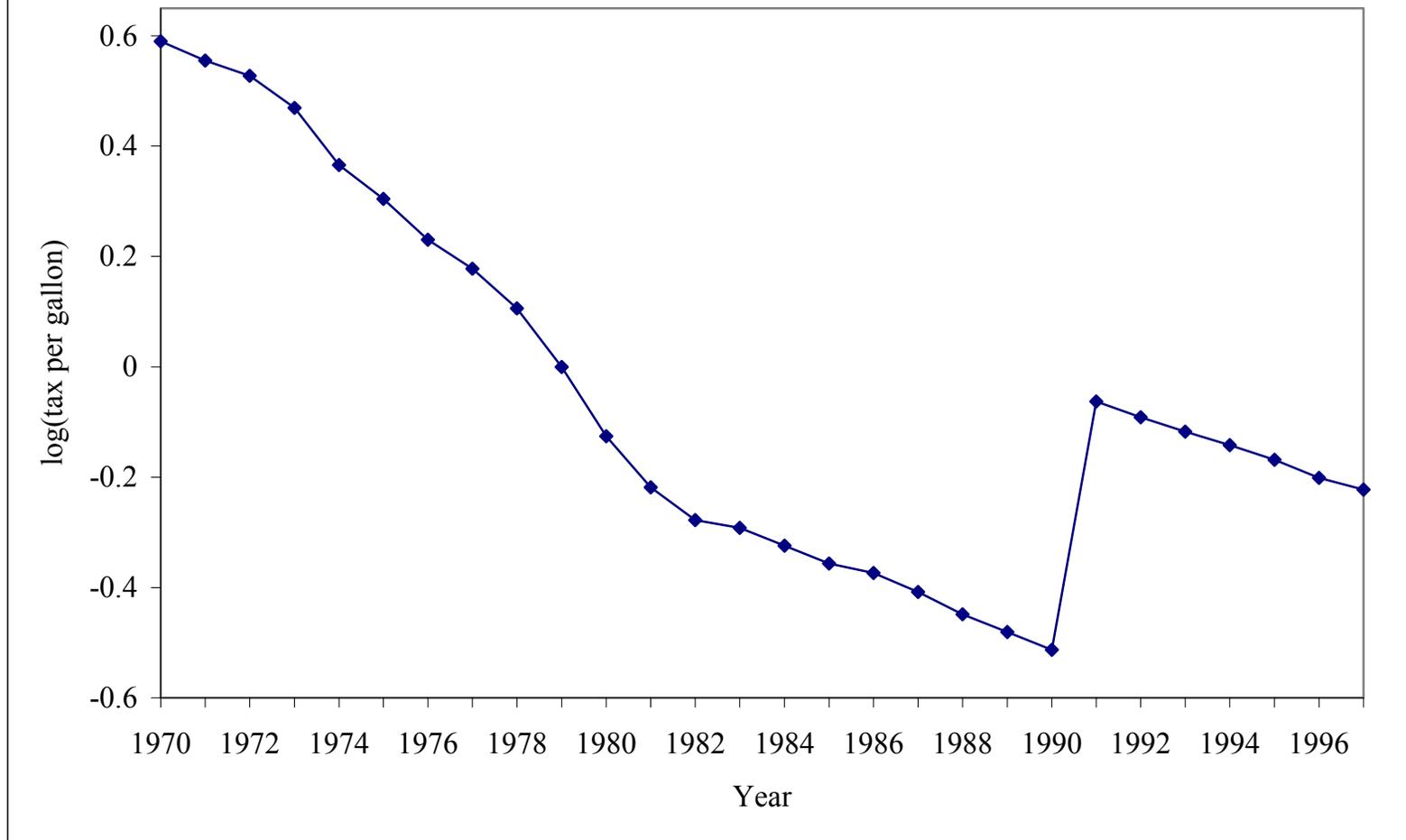
Source: Authors' calculations.

**Figure 4. Beer Consumption over Time: 1970-1997**



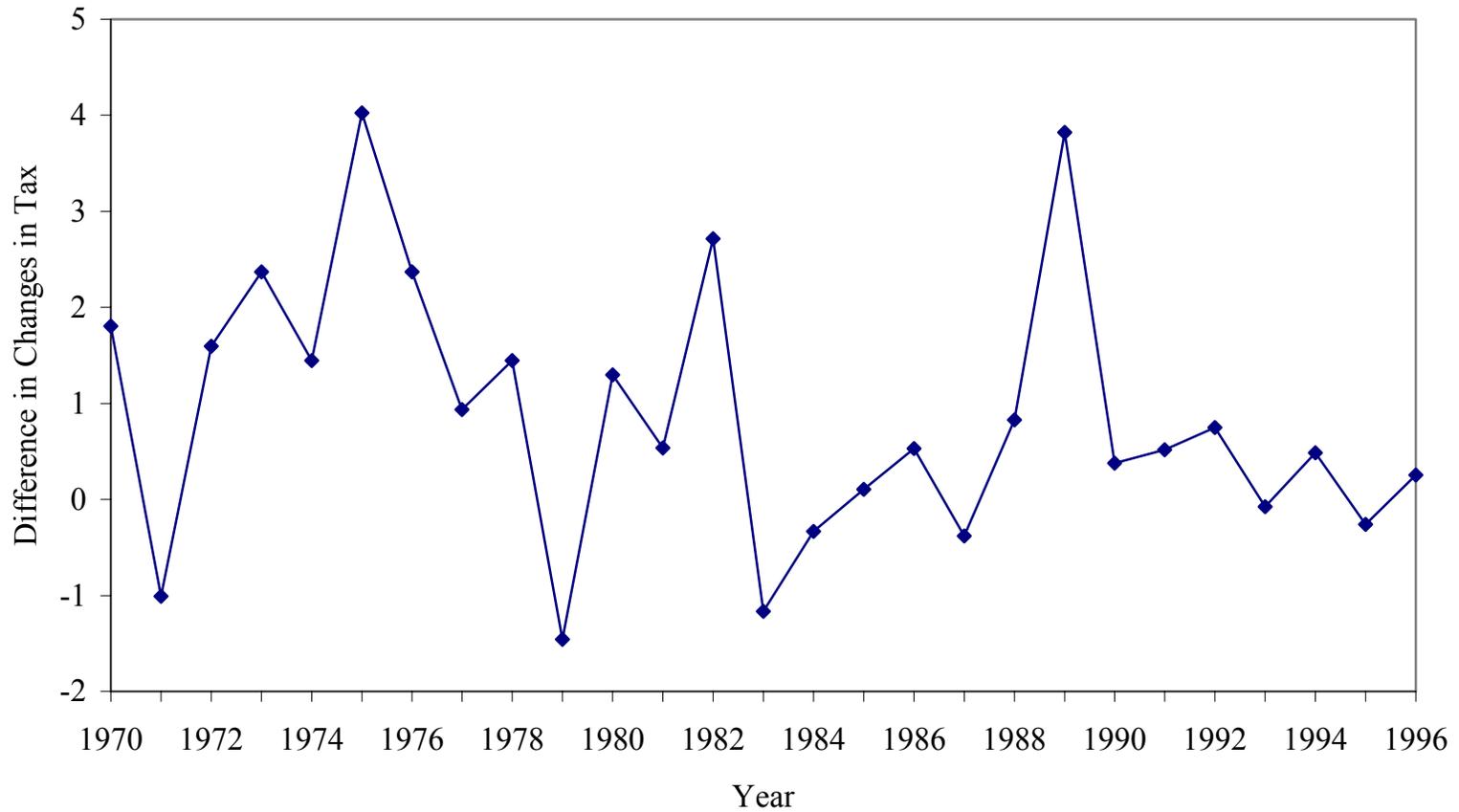
Source: Authors' calculations.

**Figure 5. Beer Taxes over Time, 1970-1997**



Source: Authors' calculations.

**Figure 6. Differences in Changes in Beer Taxes  
(States with Elections versus States with No Election)**



Source: Authors' calculations.

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