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CIGARETTE TAXATION: RAISING REVENUES AND REDUCING CONSUMPTION

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This paper estimates a dynamic demand model for cigarettes based on panel data from 46 American states over the period 1963 to 1988. The first objective is to show some of the pitfalls of studies that rely on a time series regression of a specific state, or a cross-section regression for a given year. The second objective is to update the results of Baltagi and Levin (1986) from 1980 to 1988 and to study the sensitivity of these results to various ways of modelling the bootlegging effect as well as controlling for fixed or random state effects. This study finds a significant habit persistence effect, a small but significant 'border purchasing' effect, a significant but inelastic own price effect and a small but significant income effect.

1. INTRODUCTION

Two important developments in the last several years have made the option of increasing taxes on cigarettes more tempting for the US Congress and the respective states. The first development is the recent budget deficits across the US which prompted federal and state budget cuts and belt tightening, as well as a search for new ways of generating revenues. The Bush administration has promised no new Federal taxes. This left Congress and the respective states with the ever tempting option of increasing 'sin' taxes on cigarettes and alcohol.[3] The second important development pertains to the release of the new information about the ill effects of cigarettes especially with regard to the secondary effects of smoking. Consumers are continuously bombarded with anti-smoking messages as the Surgeon General warns of the health hazards of smoking and stronger warning labels are required on these products.[4] In addition, there is extensive medical evidence on the effect of smoking on pregnant women, children and even involuntary smokers, i.e. non-smokers who inhale someone else's cigarette smoke. This prompted many states to impose 'clean air laws' restricting smoking in the work place, health care facilities, schools, restaurants, public places and commercial flights within the US. These direct restrictions are designed to reduce

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[3] See Battison's (1985) paper in a special session on 'taxing sin' in the *National Tax Journal*. Some of the other options used in generating revenues by state and local governments include an increase in sales tax and gasoline tax, running a state lottery, and allowing dog or horse racing.

[4] See Harris (1979) and Ippolito *et al.* (1979). The 1989 Surgeon General's report (USDHHS, 1989) finds that in 1985, one of every six deaths in the US was the result of past and current smoking.

cigarette smoking and limit the exposure of non-smokers to cigarette smoke. Also, these 'secondary effects' establish cigarettes as a product whose use generates negative externalities thus providing another legitimation for its taxation. There are a number of factors which make cigarette taxation special and thus the main focus of this paper.

(i) It is already an important source of tax revenues. In fact, the Tobacco Institute reports that in 1988, excise taxes on tobacco products generated nearly \$9.8 billion in tax revenues for federal, state and local governments. The federal tax was doubled in 1983 from 8¢ per pack to 16¢ per pack and has remained at that level ever since. In contrast, states have taxed cigarettes differently and as of 1988 state excise taxes ranged from 2¢ per pack in North Carolina (the largest growing and producing tobacco state) to 38¢ per pack in Minnesota. These variations in state tax rates are responsible for the large variation in cigarette prices, especially among neighbouring states. This brings us to the second reason for studying cigarette taxation.

(ii) Cigarettes can be stored and are easy to transport. This fact accompanied with the price variation induced primarily by varying state tax rates result in 'casual' smuggling across neighbouring states. Simply put, higher taxes in State A will encourage consumers in that state to search for cheaper cigarettes in neighbouring states. For example, while North Carolina has a cigarette tax of 2¢ per pack, neighbouring Tennessee has a 13¢ tax per pack. New Hampshire has a 12¢ tax per pack whereas neighboring Massachusetts and Maine have a 26¢ and 28¢ tax per pack, respectively. The Advisory Commission on Intergovernmental Relations (ACIR 1977) found that disparities in tax rates among states cost the high-tax states \$391 million in 1975 with 17 states losing more than 9% of their cigarette revenue due to tax evasion or tax exemption. The official recognition of this problem was the enactment of the 1978 Federal Contraband Cigarette Act. This made interstate commerce in contraband cigarettes a federal criminal offence. In 1983, the ACIR (1985) estimated a loss of \$309 million with only eight states losing more than 9% of their tax revenues from tax exemption or tax evasion.

Finally, (iii) besides generating revenues, cigarette taxation can be used as a policy designed to combat smoking. Cigarette taxes raise the real price of cigarettes and the percent reduction in consumption depends upon the price elasticity of cigarette demand. Per capita consumption in the US reached a peak of 134 packs in 1978 and has been declining ever since to reach 113 packs in 1988. During that period, the average retail price of cigarettes in the US (in 1967 currency) rose from 27.8¢ in 1978 to 34.5¢ in 1988.

The questions we examine in this paper are the following. (i) To what extent does the increase in state taxes lead to an increase in revenues in that state and a reduction in consumption in that state? (ii) To what extent does this tax increase encourage casual smuggling across border states with lower taxes? For the state

[5] Barzel (1976), Johnson (1978), and Sumner and Ward (1981) evaluated the effect of an excise tax on cigarette demand. Also, Sumner and Wohlgenant (1985) studied the effects of an increase in the Federal excise tax on cigarettes on prices, quantities, quota lease rates, and producer economic rents in the tobacco industry.

contemplating an increase in tax these are important questions for policy prescriptions especially if the objective is to raise revenues.[5] The answers to these questions depend upon the price elasticity of cigarette sales in that state to its own price as well as to the bordering states' prices. The more difficult question of whether the increase in tax will induce less consumption of cigarettes in that state cannot be answered with the available data on sales, since consumers in State A might switch their purchases to neighbouring State B and the apparent reduction in sales in State A in this case is not necessarily due to a decline in this state's consumption.[6] The point is that reported sales in State A may understate or overstate consumption in this state depending on how much the 'casual smuggling' effect is important for this state.[7] Following Baltagi and Levin (1986), we proxy this bootlegging effect by including a neighbouring state price effect which acts as a substitute price. Specifically, we use the minimum real price of cigarettes in any neighbouring state. This proxy's for casual smuggling across bordering states, but does not take into account organized smuggling done over long distances by trucks. For example, from North Carolina to New York City, see Cook (1977) and the ACIR (1977, 1985). Also, it does not take into account that in large states, cross-border shopping may occur in different neighbouring states and not just the minimum-price neighbouring state. In Section 2, we study the sensitivity of this proxy for 'border-effect' purchases. Specifically, we replace the minimum real price by the maximum real price of cigarettes in any neighbouring state. This is another substitute price and an alternative proxy for border-effect purchases.

The problems of not being able to measure consumption in a specific state and that of the 'casual smuggling' effect would be trivial if the tax rates were uniform across states. However, even for the country as a whole, the use of cigarette taxation as a policy for reducing consumption and improving health is not an easy problem to quantify, see Harris (1980, 1987). Harris (1987) finds that as the real price of cigarettes rose 36% from 1981 to 1986, US per capita consumption of cigarettes declined by 15%. In fact, Harris (1987, p. 88) argues that:

> The decline in cigarette use reflected mostly a decrease in the number of cigarette smokers rather than in the amount smoked by continuing users.... The price increase does not induce smokers to cut down, but it induces existing smokers to quit or prevent potential smokers from starting [see Lewit *et al.* (1981), and Lewit and Coate (1982)].... Who cuts down on cigarettes, who quits, and who fails to start are critical questions in assessing the quantitative effect of a cigarette tax increase on the health of the population.

The effectiveness of cigarette taxation as a policy instrument depends upon a reliable estimate of the price elasticity of demand for cigarettes and on its stability

[6] 'Until 1965, when New York doubled its cigarette tax, New Yorkers consumed more cigarettes than the national average. Since then, they seem to be smoking much less—according to tax records. But nobody thinks they cut back that much, even allowing for health concerns. This differential between state and national consumption, both in New York and elsewhere, provides the best index anyone has of how widespread cigarette bootlegging may be'. See Cook (1977).

[7] Other studies indicating the importance of bootlegging include Wertz (1971), Manchester (1976), Doron (1979), Warner (1977, 1982), Sumner and Wohlgenant (1985), Baltagi and Levin (1986), and Baltagi and Goel (1987).

over time, see Laughhunn and Lyon (1971). One approach to estimating this price elasticity is through a quasi-experimental approach, see Simon (1966), Lyon and Simon (1968), and Baltagi and Goel (1987).[8] Lyon and Simon (1968) calculated these elasticities for 73 tax changes over the period 1951–64. They obtained a median price-elasticity of −0.511 with a 95% confidence interval of (−0.346, −0.713), whereas Baltagi and Goel (1987) found that this median price elasticity declined over time to −0.433 for the period 1965–71 and −0.173 for the period 1972–83. This drop in the price elasticity is statistically significant. However, if one controls for the bootlegging effect in the control states, this median price elasticity is much lower, −0.260 for 1956–65, −0.368 for 1965–71, and −0.190 for 1972–83, and the drop in elasticity over time is not statistically significant.[9]

The alternative approach to estimating this price elasticity involves the specification and estimation of a demand for cigarettes, see Schoenberg (1933), Maier (1955), Sackrin (1957), Laughhunn and Lyon (1971), Hamilton (1972), Doron (1979), and Baltagi and Levin (1986).

2. MODEL AND RESULTS

Our previous study (Baltagi and Levin, 1986) estimated a dynamic demand for cigarettes based on a panel data of 46 states over the period 1963–80 and found a significant price elasticity of −0.22, a significant neighbouring price elasticity of 0.08, a very strong and significant lagged consumption effect 0.93, and a negligible and insignificant income elasticity −0.002.[10] In this study, we update the data eight more years from 1980 to 1988 so that the panel covers 46 states over the period 1963–88 and we estimate the following dynamic demand equation

$$\ln C_{it} = \alpha + \beta_1 \ln C_{i,t-1} + \beta_2 \ln P_{i,t} + \beta_3 \ln Y_{it} + \beta_4 \ln Pn_{it} + u_{it} \qquad (1)$$

where the subscript i denotes the ith state ($i = 1, \ldots, 46$), and the subscript t denotes the tth year ($t = 1, \ldots, 26$). C_{it} is real per capita sales of cigarettes by persons of smoking age (14 years and older). This is measured in packs of cigarettes per head. P_{it} is the average retail price of a pack of cigarettes measured in real terms. Y_{it} is real per capita disposable income. Pn_{it} denotes the minimum real price of cigarettes in any neighbouring state.[11] The disturbance term is specified as a two-way error component model:

$$u_{it} = \mu_i + \lambda_t + \nu_{it} \qquad i = 1, \ldots, 46; \quad t = 1, \ldots, 26 \qquad (2)$$

where μ_i denotes a state-specific effect and λ_t denotes a year-specific effect; see

[8] This method computes the percentage quantity change for a specific state before and after a tax change while controlling for the percentage quantity change of comparison states where no tax change occurred. No specification and estimation of the functional form of demand are needed and the elasticity computation is tailored around the tax change, so that the period over which the elasticity is computed is well defined.

[9] This elasticity is in line with the price elasticity obtained by Baltagi and Levin (1986).

[10] Baltagi and Levin (1986) also examined the importance of advertising, the Fairness Doctrine Act, and the Congressional ban of broadcast advertising on the demand for cigarettes.

[11] For data sources, as well as details on the construction of the neighbouring price, see Baltagi and Levin (1986).

Hsiao (1986) for the popularity of this specification in panel data studies. We assume that the time-period effects (the λ_t's) are fixed parameters to be estimated as coefficients of time dummies for each year in the sample. This can be justified given the numerous policy interventions as well as health warnings and Surgeon General's reports which previous studies accounted for using time dummy variables (see Hamilton, 1972; Baltagi and Levin, 1986). Major policy interventions include: (i) the imposition of warning labels by the Federal Trade Commission effective January 1965, (ii) the application of the Fairness Doctrine Act to cigarette advertising in June 1967, which subsidized anti-smoking messages from 1968 to 1970, and (iii) the Congressional ban of broadcast advertising of cigarettes effective January 1971.[12] Note that equation (1) does not include an advertising variable. Advertising is not available at the state level and only an aggregate measure of advertising for the US can be included. Since this variable is invariant across states it will be accounted for by the time dummies.[13] One of the advantages of a panel is its ability to control for all time-invariant variables or state-invariant variables, whose omission could bias the estimates in a typical cross-section study or a time-series study.[14] The μ_i's are state-specific effects which can represent any state-specific characteristic including the following. (i) Indian reservations sell tax-exempt cigarettes. States with Indian reservations like Montana, New Mexico, and Arizona are among the biggest losers in tax revenues from non-Indians purchasing tax-free cigarettes from the reservations (see ACIR, 1985). (ii) Florida, Texas, Washington, and Georgia are among the biggest losers of revenues due to the purchasing of cigarettes from tax exempt military bases in these states (see ACIR, 1985). (iii) Utah, which has a high percentage of Mormon population (a religion which forbids smoking), has a per capita sales of cigarettes in 1988 of 55 packs, a little less than half the national average of 113 packs. (iv) Nevada, which is a highly touristic state, has a per capita sales of cigarettes of 142 packs in 1988, 29 more packs than the national average. These state-specific effects may be assumed fixed, in which case one includes state dummy variables in equation (1). The resulting estimator is the 'Within' estimator reported in Table 1.[15] Alternatively, one can assume the μ_i's are random IID$(0, \sigma_\mu^2)$. In fact, if these state effects are hopelessly correlated with all the regressors, i.e. $E(\mu_i \mid X_{it}) \neq 0$, then the within estimator is the appropriate estimator. On the other hand, if $E(\mu_i \mid X_{it}) = 0$ and the state effects are uncorrelated with the regressors then one can apply the Anderson and Hsiao (1981) method to estimate the dynamic demand for cigarettes given in equation (1).[16] Specifically, we first difference (1) and use $\ln C_{i,t-2}$ as an instrumental

[12] See Doron (1979, appendix A), for a chronology of Federal Commission's interventions in the market practices of the cigarette industry.

[13] Baltagi and Levin (1986) assumed that the λ_t's are random errors and estimated the advertising effect on cigarette demand using the Hausman and Taylor (1981) method. The advertising effect was found to be small and insignificant. The importance of advertising on cigarette demand has mixed reviews in the literature, Schmalensee (1972) finds it insignificant while Hamilton (1972) finds it to be significant.

[14] See Hsiao (1985) for other benefits and limitations of panel data.

[15] See Mundlak (1978) for arguments in favour of this estimator.

[16] A specification test for the hypothesis $E(\mu_i \mid X_{it}) = 0$ is given by Hausman (1978).

TABLE 1. *Pooled Estimation Results*[1,2] *of Cigarette Demand Equation* 1963–88

	$\ln C_{i,t-1}$	$\ln P_{it}$	$\ln Y_{it}$	$\ln Pn_{it}$	$\ln Pm_{it}$	*Constant*	R^2
(A) Minimum neighbouring price							
OLS	0.953	−0.142	0.0005	0.048	—	0.121	0.97
	(145)	(8.6)	(0.06)	(3.5)		(3.2)	
Within	0.792	−0.350	0.144	0.064	—	0.189	0.97
	(59)	(14)	(5.7)	(2.3)		(1.9)	
Anderson–Hsiao	0.602	−0.476	0.134	0.085	—	—	0.77
	(2.7)	(13)	(2.5)	(1.8)			
(B) Maximum neighbouring price							
OLS	0.953	−0.127	0.0002	—	0.048	0.126	0.97
	(145)	(8.3)	(0.03)		(2.7)	(3.2)	
Within	0.791	−0.350	0.144	—	0.050	0.172	0.97
	(59)	(14)	(5.7)		(1.8)	(1.7)	
Anderson–Hsiao	0.602	−0.473	0.134	—	0.006	—	0.81
	(2.6)	(12.7)	(2.5)		(0.3)		
(C) Both minimum and maximum neighbouring prices							
OLS	0.952	−0.147	0.002	0.043	0.039	0.146	0.97
	(145)	(8.9)	(0.3)	(3.1)	(2.2)	(3.7)	
Within	0.793	−0.353	0.147	0.056	0.038	0.199	0.97
	(59)	(14)	(5.8)	(1.9)	(1.3)	(2.0)	
Anderson–Hsiao	0.599	−0.475	0.135	0.084	0.003	—	0.78
	(2.6)	(12.8)	(2.5)	(1.8)	(0.2)		

[1] Numbers in parentheses are *t*-statistics.
[2] All regressions include time dummies which are not reported here, but are available upon request from the authors.

variable for $(\ln C_{i,t-1} - \ln C_{i,t-2})$, see Anderson and Hsiao (1981) and Arellano (1989). The result is reported as the Anderson–Hsiao estimator in Table 1. All coefficients are statistically significant with the lagged coefficient of consumption estimated at 0.60 for the Anderson–Hsiao estimator indicating a significant habit-persistence effect for this addictive commodity.[17] The price elasticity is inelastic and ranges from −0.48 for the Anderson–Hsiao estimator to −0.14 for the ordinary least squares (OLS) estimator. The income elasticity estimate is inelastic and ranges from an insignificant 0.0005 for the OLS estimator to a significant 0.144 for the Within estimator.[18] The neighbouring price effect ranges from 0.048 for OLS to 0.085 for the Anderson–Hsiao estimator indicating a small but significant 'casual smuggling' effect across states, even after accounting for state-specific and time-specific effects. It is important to emphasize the differences between this study and that of Baltagi and Levin (1986). (i) The data covers the longer period 1961–88 rather than 1961–80 for 46 states. (ii) Time effects are

[17] This emphasizes the importance of dynamics for cigarette demand. Studies that specify a static demand equation risk omission bias of the type discussed in Simon and Aigner (1970) and Baltagi and Griffin (1984).

[18] This warns about the bias from the OLS estimator not accounting for state-specific effects. In fact, if random state effects are present, the standard errors of OLS will also be biased, see Moulton (1986).

assumed fixed throughout this study, whereas Baltagi and Levin (1986) assumed these effects are random for the Hausman–Taylor estimator and fixed only for the Within estimator. (iii) For this study, the individual state effects are assumed fixed for the Within estimator and random for the Anderson–Hsiao estimator. Baltagi and Levin (1986) did not control for state effects. These differences are responsible for the lower lagged coefficient estimate on consumption (0.60 compared with 0.93); a higher price elasticity in absolute terms (−0.47 compared with −0.22), roughly the same neighbouring price elasticity (0.08) and a small but significant income elasticity (0.13) compared with an insignificant negative income elasticity of 0.002. One possible reason for the difference in income elasticity estimates is the important effect of 'education' which is highly correlated with 'income'. The effect of education on smoking is negative, while a 'pure income effect' controlling for education can be positive.[19] Education levels across different states are mostly time-invariant and would be controlled for by the state dummies. This means that the Within estimator is estimating a 'pure income effect' to the extent that it is controlling for state education levels. On the other extreme, the OLS estimator of the income elasticity does not account for state effects and captures, among other things, the omission bias of differing education levels across these states.

With the specification given in (1), the long run estimated elasticities can be obtained from the short run estimated elasticities by multiplying the latter by $1/(1-\hat{\beta}_1)$, where $\hat{\beta}_1$ is the coefficient estimate of lagged consumption. The Anderson–Hsiao estimate of this long run multiplier is $1/(1-0.60)=2.5$. Therefore, the long run price, income and neighbouring price elasticities are −1.196, 0.337, and 0.214, respectively. These are more plausible than those implied by Baltagi and Levin (1986).

Panel B of Table 1 studies the sensitivity of our results to replacing the minimum neighbouring price by the maximum neighbouring price. Maximum neighbouring price is also a 'substitute price' and another proxy for neighbouring state purchases.[20] As is clear from the comparison of panel A (minimum neighbouring price) and panel B (maximum neighbouring price), our results are robust to the inclusion of either substitute price. Panel C also shows that our results are robust to the inclusion of both substitute prices.[21]

[19] See Wasserman *et al.* (1991) who estimated cigarette demand using data from the National Health Interview Survey. With this micro level data, one can control, among other things, for the effects of education and income on smoking.

[20] The simple correlation coefficient between $\ln P$ and $\ln Pn$ is 0.735, between $\ln P$ and $\ln Pm$ is 0.687, and between $\ln Pn$ and $\ln Pm$ is 0.684.

[21] Note also that one could account for long haul smuggling from North Carolina to New York City, by using the following proxy: include the logarithm of the real North Carolina price multiplied by a distance measure from North Carolina to the suspected long-haul smuggling state and an index of transportation costs which would depend on the real price of fuel. Note that the distance measure is time-invariant and is spanned already by the state-dummies. Similarly, the North Carolina price and the index of transportation costs are invariant across states and are spanned by the time dummies. This means that the log of this variable is completely spanned by the state and time dummies, which in turn means that the Within estimator controls for variables like this and guards the other regression coefficients against omission bias.

Having presented the pooled regression results, we now turn to some diagnostics and specification tests performed on this data set. We also contrast the dynamic pooled results with cross-section regressions for each year and time-series regressions for each state. The purpose of this last comparison is to show some of the pitfalls of studies that usually rely on a time-series study of a specific state or a specific cross-section of these states for a given year.

2.1. *Diagnostics*

The specific state and time dummies were tested for their significance jointly as well as separately. The observed F-statistic for the significance of the time dummies is 12.3 which has a p-value of 0.0001 under the null distribution of $F(24,1121)$. The observed F-statistic for the significance of state dummies is 3.60 which has a p-value of 0.0001 under the null distribution of $F(45,1098)$. Both state and time dummies were significant with an observed F-statistic of 8.79 and a p-value of 0.0001 under the null distribution of $F(69,1076)$. These tests emphasize the importance of individual-state effects in the cigarette demand equation.[22] A Chow-test for the stability of this regression across time yielded an F-value of 0.41 which has a p-value of 0.001 under the null distribution of $F(96,1025)$. Finally, a Hausman (1978) specification test based on the Within and Anderson–Hsiao estimator was performed. This yielded a χ^2 value of 0.03 which has a p-value of 0.99 under the null distribution of χ^2_4. This diagnostic indicates no correlation between the explanatory variables and the individual-state effects, and the Anderson–Hsiao estimator is appropriate.

2.2. *Dynamic Cross-section Regressions for each Year*

Table 2 gives the results of the dynamic cross-section regressions for the years 1964 to 1988. White's (1980) test shows that we reject homoscedasticity in eight out of the 25 dynamic cross-section regressions considered. Not surprisingly, the lagged coefficient on cigarette consumption is close to one and statistically significant for all years. The price elasticity is negative and statistically significant in 11 out of the 25 years, and ranges in value between −0.36 and −0.13 for those years. Income elasticity is statistically insignificant in 17 out of 25 years and negative in seven out of the eight years in which it is significant. This income elasticity never exceeds 0.17 in absolute value. The neighbouring price is significant in only four of the 25 years and never exceeds 0.17 in absolute value. The long-run elasticities implied by these results are huge and implausible.

In summary, cross-section regressions focus on a particular year and one does not have to worry about different year effects, but this cross-section regression cannot effectively control for state specific effects, like those of Montana, New Mexico, and Arizona with tax-free Indian reservations, and Florida, Texas, Washington, and Georgia with tax exempt military bases, or states like Utah with a high Mormon population, or Nevada, a state that depends heavily on tourism.

[22] See Moulton and Randolph (1989) for evidence in favour of the application of the ANOVA F-test in the random effect models.

TABLE 2. *Dynamic Cross-section Regressions*[1] *for each Year* (1964–88)

Year	$\ln C_{i,t-1}$	$\ln P_{i,t}$	$\ln Y_{it}$	$\ln Pn_{it}$	*Constant*	$\bar{R}^2$	*SE*	*W*[2]
1964	1.06*	−0.15*	−0.096*	0.17*	−0.02	0.98	0.034	9.1
	(32)	(2.2)	(2.9)	(1.8)	(0.2)			
1965	0.95*	−0.10	0.04	0.04	0.05	0.98	0.032	12.8
	(31)	(1.2)	(1.4)	(0.6)	(0.4)			
1966	1.09*	−0.36*	−0.104*	0.07	−0.52*	0.96	0.050	28.2*
	(17)	(2.8)	(1.8)	(0.7)	(2.2)			
1967	0.92*	−0.07	0.007	0.07	0.41*	0.98	0.037	34.7*
	(17)	(1.1)	(0.1)	(1.0)	(2.1)			
1968	0.93*	−0.14*	0.03	0.04	0.19	0.97	0.036	29.9*
	(23)	(1.7)	(0.6)	(0.7)	(1.0)			
1969	0.88*	−0.35*	0.05	0.14	0.19	0.96	0.042	22.4
	(23)	(3.8)	(1.2)	(2.4)	(0.9)			
1970	0.95*	−0.26*	−0.02	0.15*	0.16	0.95	0.049	15.6
	(18)	(2.9)	(0.3)	(1.6)	(0.7)			
1971	1.00*	−0.13*	−0.11*	0.07	0.31*	0.97	0.037	6.8
	(57)	(2.4)	(2.5)	(1.1)	(1.8)			
1972	0.99*	−0.32*	−0.08*	0.07	0.09	0.96	0.045	5.7
	(51)	(4.4)	(2.6)	(1.3)	(0.8)			
1973	0.94*	−0.147*	−0.03	0.08	0.31*	0.98	0.032	27.2*
	(34)	(3.3)	(0.9)	(2.0)	(2.2)			
1974	0.92*	−0.05	−0.04	0.008	0.51*	0.98	0.026	39.4*
	(35)	(1.5)	(0.9)	(0.2)	(1.8)			
1975	0.95*	−0.17*	0.011	0.08*	0.12	0.98	0.027	10.6
	(46)	(2.7)	(0.3)	(2.0)	(0.8)			
1976	0.99*	−0.08	−0.105*	0.04	0.38*	0.98	0.029	14.8
	(33)	(1.4)	(3.2)	(0.9)	(2.5)			
1977	0.94*	−0.036	−0.09*	0.02	0.63*	0.97	0.032	31.7*
	(33)	(0.5)	(1.8)	(0.3)	(2.1)			
1978	0.91*	−0.115	−0.02	0.056	0.45*	0.98	0.029	29.3*
	(38)	(1.3)	(0.7)	(0.9)	(2.2)			
1979	0.95*	0.017	0.033	0.06	0.21	0.98	0.025	34.4*
	(69)	(0.2)	(0.8)	(1.3)	(0.9)			
1980	1.01*	0.12*	−0.16*	−0.7	0.56*	0.96	0.035	22.8
	(31)	(1.7)	(3.1)	(1.2)	(3.9)			
1981	0.86*	−0.23*	0.17*	0.095*	−0.06	0.95	0.035	12.0
	(25)	(3.5)	(4.4)	(1.7)	(0.4)			
1982	1.02*	0.016	−0.02	−0.05	−0.07	0.98	0.027	3.6
	(44)	(0.3)	(1.2)	(1.4)	(0.6)			
1983	0.97*	−0.017	−0.002	−0.06	0.046	0.98	0.023	14.9
	(37)	(0.3)	(0.1)	(1.3)	(0.3)			
1984	0.93*	0.008	0.03	−0.05	0.158	0.97	0.029	20.8
	(25)	(0.1)	(1.3)	(0.7)	(1.1)			
1985	0.99*	0.09	−0.006	−0.046	0.065	0.97	0.026	20.1
	(26)	(1.1)	(0.3)	(1.1)	(0.5)			
1986	0.98*	−0.15*	0.036	−0.06	−0.28*	0.97	0.026	13.6
	(39)	(2.2)	(1.2)	(1.0)	(2.3)			
1987	0.99*	0.06	0.016	−0.07	−0.03	0.94	0.039	11.4
	(19)	(0.7)	(0.7)	(0.8)	(0.1)			
1988	1.07*	−0.13	0.03	0.002	−0.613*	0.93	0.048	20.7
	(16)	(1.2)	(0.8)	(0.02)	(1.8)			

[1] Numbers in parentheses are *t*-statistics based on White's (1980) heteroscedasticity consistent standard errors.

[2] *W* denotes White's (1980) nR^2 test for homoscedasticity which is distributed as χ^2_{14} under the null. Note that $\chi^2_{14,0.05} = 23.7$.

2.3. *Dynamic Time-series Regressions for each State*

Table 3 gives the results of the time-series regressions for 46 states over the period 1963–88. Serial correlation is a problem in seven out of the 46 dynamic regressions as shown by the Breusch (1978) and Godfrey (1979) test for serial correlation. Therefore, the Wallis (1967) Two-Stage (WTS) estimation procedure which accounts for serial correlation and the presence of the lagged dependent variable is performed for each state.[23] We include advertising as well as a dummy variable *FD* accounting for the effect of the Fairness Doctrine Act in the time-series regression. Briefly, the Fairness Doctrine Act tied the number of anti-smoking messages to the number of cigarette commercials. Hence it effectively subsidized anti-smoking messages over 1968–70. The value of these anti-smoking messages were estimated at $75 million for 1970 (see Harris, 1979). This is roughly one-third the industry's advertising expenditures on TV and radio for that year. *FD* takes the value of per capita index of advertising in television and radio for the years 1968–70 and zero for the remaining years.

For the Dynamic state by state results, the lagged consumption effect is significant for 44 states with a mean (or median) of 0.9. The price elasticity is significant for 19 states with a positive significant sign for only one of these states. The mean price elasticity is −0.3 while its median is −0.2. Income elasticity is significant for only 10 states and has a negative significant sign for only two of these states. The mean income elasticity is 0.09 and its median is 0.03. Neighbouring price elasticity is significant for nine states with two of the states having a significant negative sign. The mean neighbouring price effect is 0.3 and its median is 0.17. Advertising effect is significant for 15 states but has a negative significant sign for all of these states. The mean (or median) advertising effect is −0.03. Finally, the 'Fairness Doctrine' effect is significant for eight states and has a positive significant sign for four of these states. The mean (or median) 'Fairness Doctrine' effect is −0.01. The long run price elasticities are again huge and implausible.

In summary, individual state regressions focus on a particular state and one does not have to worry about different state effects, but this regression cannot effectively control for specific time effects, like the Surgeon General report, other health warnings, the Fairness Doctrine Act, and the Congressional ban on broadcast advertising. In contrast, a pooled cross-section, time-series regression allows for a richer data set that accounts for the specific state effects as well as time effects. One can also test for these effects (see Section 2.1) and estimate this model allowing for correlation between these effects and some of the explanatory variables (see Table 1).

3. DISCUSSION AND CONCLUSION

Our pooled results show that the bootlegging effect is alive and well across bordering states, and individual states contemplating an increase in their cigarette

[23] The WTS estimator was modified to take into account the initial observation, see Fomby and Guilkey (1983).

TABLE 3. *Dynamic Time-series Regressions for each State*[1] (1963–88)

State	$\ln C_{i,t-1}$	$\ln P_{i,t}$	$\ln Y_{i,t}$	$\ln Pn_{it}$	ADV_t	FD_t	$\bar{R}^2$	*SE*	BG^2
1. Alabama	0.97	−0.17	0.034	0.22	−0.04	−0.03	0.93	0.031	0.215
	(13)	(0.9)	(0.5)	(1.0)	(2.2)	(1.9)			
2. Arizona	0.94	−0.23	0.06	0.14	−0.04	0.013	0.97	0.047	0.001
	(15)	(1.2)	(0.5)	(0.6)	(1.2)	(1.2)			
3. Arkansas	0.70	−0.38	0.11	0.21	−0.02	−0.02	0.84	0.027	0.158
	(3)	(2.0)	(2.1)	(1.1)	(0.9)	(1.6)			
4. California	0.92	−0.21	−0.17	0.07	−0.03	0.01	0.95	0.040	6.03*
	(15)	(1.0)	(0.7)	(0.4)	(0.9)	(0.4)			
5. Connecticut	0.84	−0.57	0.11	0.26	−0.04	−0.01	0.99	0.047	0.016
	(12)	(2.7)	(0.8)	(1.3)	(1.0)	(0.6)			
6. Delaware	0.98	−0.97	−0.007	0.04	0.004	0.03	0.99	0.018	4.20*
	(25)	(1.2)	(0.1)	(0.5)	(0.3)	(3.7)			
7. DC	0.92	−0.47	0.30	0.36	0.05	−0.05	0.97	0.075	1.48
	(9)	(1.0)	(1.6)	(1.1)	(0.9)	(1.0)			
8. Florida	0.91	−0.32	0.06	0.20	−0.03	−0.01	0.99	0.029	1.77
	(20)	(2.2)	(0.7)	(1.4)	(1.4)	(0.6)			
9. Georgia	0.99	−0.3	0.07	−0.03	−0.02	0.004	0.99	0.022	0.002
	(30)	(0.3)	(1.2)	(0.2)	(1.6)	(0.3)			
10. Idaho	0.99	−0.15	−0.04	0.14	−0.05	0.002	0.96	0.043	1.58
	(10)	(0.5)	(0.2)	(0.4)	(1.7)	(0.1)			
11. Illinois	0.94	−0.21	0.01	0.09	−0.01	−0.005	0.99	0.039	10.6*
	(8)	(0.8)	(0.1)	(0.3)	(0.3)	(0.2)			
12. Indiana	0.96	−0.26	0.02	0.22	−0.02	0.01	0.96	0.032	0.935
	(16)	(1.1)	(0.2)	(1.1)	(1.1)	(0.7)			
13. Iowa	0.95	−0.30	0.02	0.16	−0.03	0.01	0.98	0.042	0.762
	(11)	(1.2)	(0.1)	(0.6)	(1.2)	(0.6)			
14. Kansas	0.90	−0.44	0.05	0.33	−0.02	0.02	0.94	0.033	1.91
	(13)	(2.0)	(0.5)	(1.3)	(1.7)	(1.0)			
15. Kentucky	1.14	0.36	−0.09	0.60	−0.09	−0.002	0.97	0.027	4.66*
	(13)	(1.3)	(0.9)	(2.0)	(4.8)	(0.2)			
16. Louisiana	1.01	0.14	−0.097	−0.242	−0.007	−0.01	0.996	0.024	0.606
	(19)	(1.1)	(0.9)	(1.7)	(0.3)	(0.9)			
17. Maine	1.01	−0.17	−0.4	0.20	−0.02	−0.007	0.99	0.029	0.138
	(21)	(1.0)	(0.5)	(1.0)	(0.8)	(0.4)			
18. Maryland	1.01	−0.20	−0.02	0.17	−0.03	0.009	0.99	0.019	0.454
	(39)	(1.4)	(0.4)	(1.5)	(2.1)	(1.0)			
19. Massachusetts	0.96	−0.40	0.04	0.30	−0.03	0.009	0.99	0.024	0.747
	(29)	(2.6)	(0.6)	(1.9)	(1.4)	(0.6)			
20. Michigan	0.87	−0.395	0.19	0.27	−0.04	0.009	0.99	0.036	0.577
	(10)	(1.6)	(1.4)	(0.9)	(1.2)	(0.4)			
21. Minnesota	0.22	−1.14	−0.16	0.82	−0.06	−0.004	0.78	0.034	3.52
	(1)	(4.9)	(1.6)	(3.5)	(3.0)	(0.2)			
22. Mississippi	0.55	−0.32	0.21	0.13	−0.04	−0.004	0.97	0.023	1.55
	(4)	(2.1)	(2.5)	(0.8)	(2.4)	(0.4)			
23. Missouri	0.84	−0.70	0.21	0.55	−0.05	−0.008	0.99	0.034	2.13
	(9.5)	(3.4)	(1.4)	(2.7)	(1.6)	(0.4)			
24. Montana	0.999	0.12	−0.11	−0.28	−0.02	0.009	0.99	0.039	7.55*
	(13)	(0.6)	(0.8)	(1.2)	(0.8)	(0.3)			
25. Nebraska	0.95	−0.18	−0.05	0.05	−0.02	0.006	0.99	0.029	0.037
	(22)	(1.2)	(0.6)	(0.3)	(0.9)	(0.4)			
26. Nevada	0.86	−0.13	0.33	0.02	−0.8	0.05	0.98	0.053	1.50
	(7.9)	(0.4)	(1.9)	(0.04)	(1.8)	(1.2)			
27. New	1.07	−0.67	−0.37	0.75	0.07	−0.03	0.98	0.053	1.46
Hampshire	(13)	(1.8)	(2.4)	(2.5)	(1.2)	(1.0)			
28. New Jersey	1.09	−0.24	0.02	0.17	0.002	−0.003	0.88	0.045	1.24
	(12)	(1.1)	(0.3)	(0.8)	(0.1)	(0.2)			

TABLE 3—(*continued*)

State	$\ln C_{i,t-1}$	$\ln P_{i,t}$	$\ln Y_{i,t}$	$\ln Pn_{it}$	ADV_t	FD_t	$\bar{R}^2$	SE	BG^2
29. New Mexico	0.95	−0.12	0.02	−0.01	−0.04	0.02	0.98	0.049	2.20
	(11)	(0.4)	(0.2)	(0.1)	(0.9)	(0.7)			
30. New York	0.91	−0.15	0.07	−0.005	−0.04	0.02	0.99	0.023	2.91
	(26)	(1.6)	(2.1)	(0.1)	(3.3)	(2.2)			
31. North Dakota	0.98	−0.21	−0.02	0.05	−0.05	−0.02	0.98	0.036	2.88
	(21)	(0.9)	(0.3)	(0.2)	(2.5)	(1.0)			
32. Ohio	0.84	−0.75	0.20	0.48	−0.08	−0.002	0.99	0.022	0.208
	(14)	(4.3)	(1.9)	(3.6)	(3.2)	(0.2)			
33. Oklahoma	1.04	−0.22	−0.11	0.12	−0.04	−0.03	0.99	0.023	0.017
	(22)	(1.7)	(1.3)	(0.9)	(2.4)	(2.2)			
34. Pennsylvania	0.96	−0.39	−0.11	0.36	−0.05	−0.01	0.97	0.036	0.439
	(19)	(1.9)	(1.1)	(1.3)	(1.4)	(0.6)			
35. Rhode Island	1.18	−0.98	−0.32	1.21	0.08	0.07	0.98	0.051	0.059
	(7)	(4.3)	(1.2)	(3.8)	(1.4)	(2.3)			
36. South Carolina	1.02	−0.07	0.03	0.09	−0.03	0.007	0.95	0.035	3.40
	(13)	(0.3)	(0.3)	(0.4)	(1.4)	(0.4)			
37. South Dakota	0.99	−0.86	−0.02	0.85	−0.05	−0.05	0.99	0.033	1.16
	(19)	(4.0)	(0.2)	(3.4)	(2.2)	(2.2)			
38. Tennessee	0.94	−0.31	0.0005	0.22	−0.05	−0.03	0.98	0.024	0.340
	(28)	(1.9)	(0.01)	(1.5)	(1.9)	(2.0)			
39. Texas	0.90	−0.50	−0.07	0.27	−0.06	−0.0005	0.93	0.039	0.316
	(14)	(1.7)	(0.8)	(0.8)	(1.9)	(0.02)			
40. Utah	0.05	−0.31	0.16	−0.24	−0.04	0.02	0.73	0.043	0.044
	(0.1)	(1.1)	(0.9)	(0.7)	(1.5)	(0.7)			
41. Vermont	0.85	0.04	0.19	−0.29	−0.05	0.007	0.97	0.045	0.616
	(6.1)	(0.2)	(0.8)	(0.9)	(1.6)	(0.3)			
42. Virginia	0.98	0.71	0.02	−0.65	−0.07	−0.003	0.96	0.031	4.99*
	(19)	(2.2)	(0.3)	(1.9)	(3.2)	(0.2)			
43. Washington	0.86	−0.34	0.19	0.26	−0.03	0.04	0.98	0.043	2.18
	(13)	(2.0)	(2.1)	(1.3)	(1.4)	(2.5)			
44. West Virginia	0.90	−0.33	0.18	0.22	−0.04	0.02	0.98	0.039	1.22
	(12)	(1.3)	(1.3)	(1.0)	(1.2)	(0.9)			
45. Wisconsin	0.92	−0.41	0.05	0.22	−0.0001	0.009	0.99	0.031	6.69*
	(12)	(1.8)	(0.4)	(1.0)	(0.004)	(0.5)			
46. Wyoming	0.79	−0.27	0.35	0.06	−0.03	0.04	0.99	0.05	0.226
	(7)	(0.8)	(1.7)	(0.2)	(0.8)	(1.3)			

[1] Numbers in parentheses are t-statistics.

[2] BG denotes the Breusch–Godfrey test for serial correlation. This statistic is distributed as χ_1^2 under the null. Note that $\chi^2_{1,0.05} = 3.84$.

state tax ought to worry about losses in revenue to adjoining states. The short-run estimate for this border-effect is 0.08 while its long-run estimate is 0.21. Denote the absolute value of the elasticity of cigarette consumption (C) or cigarette tax revenue (R) with respect to the tax rate (t) by η_{Ct} and η_{Rt}, respectively. These can be written as $\eta_{Ct} = \eta_{CP}\eta_{Pt}$ and $\eta_{Rt} = 1 - \eta_{CP}\eta_{Pt}$.[24] Note that we have estimated cigarette price elasticity η_{CP} but not the relationship between tax rates and prices. It is quite plausible that an increase in a state tax increases cigarette prices in that state by the same magnitude. This may be due to the fact that

[24] $R(t) = t \cdot C(P(t))$ and $R'(t) = C(P(t)) + t \cdot C'(P(t)) \cdot P'(t)$. Thus, $\eta_{Rt} = R'(t) \cdot t/R(t) = 1 - \eta_{CP}\eta_{Pt}$, and η_{Ct} is established in the same way.

cigarette companies are reluctant to change their price strategy in response to a change in one market out of so many. In such a case, $P'(t) = 1$, so that $\eta_{Rt} = 1 - \eta_{CP} \cdot (t/P)$ and $\eta_{Ct} = \eta_{CP} \cdot (t/P)$. Our estimate of η_{CP} is 0.476 in the short-run and 1.196 in the long-run, with t/P rates in 1988 ranging from 5% for North Carolina to 31% in Minnesota. Therefore, $\eta_{Rt} > 0$ both in the short run and the long run. Thus individual states can raise revenues by increasing their state tax on cigarettes.

However, it is entirely likely that $P'(t) = 1$ does not hold with respect to a change in the Federal tax rate. This is due to the oligopolistic structure of the cigarette market (see Schmalensee, 1972; Harris, 1987). In such markets, even horizontal marginal cost curves do not imply that a change in the tax rate will induce the same change in the price. In fact, Harris (1987) argues that there were episodes where a given increase in the Federal tax rate increased cigarette prices by more than 100%. Thus, in order to estimate the impact on consumption and Federal tax revenue from a given increase in the Federal tax rate, one needs to estimate the effect of such a change on market prices first before one can apply estimates of price elasticities.

In conclusion, it is important to emphasize that the increase in revenue for a specific state will depend upon the importance of border-effect smuggling for that state and whether the adjoining states follow suit in raising their state taxes. Also, the long-run own-price elasticity estimate is elastic indicating that in the long-run taxation may be an effective tool for reducing consumption. However, this conclusion should be tempered by Harris's (1987) arguments given in the introduction. Briefly, Harris (1987) argues that the price increase will reduce the consumption of cigarettes mostly by reducing the number of cigarette smokers. This is achieved by preventing potential smokers from starting or inducing existing smokers to quit.[25] The reduction in the number of smokers can be better studied using micro level data, see Lewit *et al.* (1981), and more recently Wasserman *et al.* (1991).

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[25] For example, Warner (1986) argues that over 85% of smokers begin smoking before their 20th birthday and Lewit *et al.* (1981) argue that teenage smokers are sensitive to higher prices.

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