New State-Level Estimates of Dynamic Cigarette Demand

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ABSTRACT

Objectives: Declines in cigarette sales appear to have stabilized in recent years. This calls into question whether older and newer sales data should be pooled when estimating cigarette demand. In this research, we sought to estimate the short- and long-run price elasticity of cigarette demand using recent time series data and a novel dynamic model specification.

Methods: Dynamic models for cigarette sales were fit using panel data for the 48 contiguous United States and District of Columbia over the period 1989–2001. Demand was modeled as a second-order autoregressive process. Ordinary least squares (OLS) and fixed effects (FE) estimates were compared with those derived from generalized method of moments (GMM) estimators.

Results: Cigarette demand was found to be highly persistent over time, with estimated short- and long-run price elasticities of -0.30 ($p < 0.01$) and -3.51 ($p = 0.02$), respectively. Our estimate of the long-run elasticity of demand is greater than many previous estimates and indicates that, in the long run, the demand for cigarettes is price elastic. Educational attainment was negatively associated with demand, with an estimated elasticity of -0.06 ($p = 0.07$).

Conclusions: The OLS and FE estimators perform poorly when applied to dynamic models, as do overidentified GMM estimators. Future investigations of cigarette demand should account for the persistency of cigarette sales using dynamic models with more complex lag structures. Our results suggest that policy efforts to reduce tobacco use will be strengthened by placing higher federal and state taxes on tobacco products.

INTRODUCTION

Tobacco use remains the leading cause of preventable death in the United States. A recent American Cancer Society (ACS) report[1] found that over 168,000 people die each year from cancers caused by tobacco use, making it by far the leading cause of cancer death. Fortunately, progress has been made since the publication of the 1964 Surgeon General's Report, which identified tobacco use as a causal factor for disease. According to the ACS[1], the consumption of tobacco is lower now than at any point since World War II.

Despite this progress, the global picture is less promising. Tobacco companies market heavily around the world, and the World Health Organization[2] projects that tobacco-caused deaths will increase from 5 million this year to 10 million by 2020. In order to better understand and address this global epidemic, the National Cancer Institute is funding the Initiative on the Study and Implementation of Systems (ISIS) to better understand the inter-related systems that sustain tobacco use. This initiative is designed to bring together experts on systems modeling, social network analysis, knowledge management, and informatics so that these unique but related areas of expertise can be used to better address tobacco control. It is anticipated that a better understanding of these systems will yield more efficient and effective interventions designed to reduce tobacco use.

It is particularly important to understand the relationship between cigarette prices and demand. Cigarette prices are influenced to a large degree by state and federal excise taxes, and tax increases have been a staple of tobacco control policies since the 1960s[3]. During the 1970s, state-level tax increases slowed in response to the casual and organized smuggling that resulted from large differentials in cigarette prices between low- and high-tax states. However, in recent years states have begun again to use tax increases to discourage smoking. This is most evident in states like California, Massachusetts, Michigan, and Arizona, where large tax increases were approved in the 1990s.

A number of studies[4-10] have investigated the relationship between cigarette prices and demand using pooled time-series data for states. A common feature of these studies has been the presumption that current cigarette consumption depends on cigarette consumption at some previous point in time, i.e., that consumption is dynamic. Consider the dynamic model

$$y_{ij} = y_{ij-1}\alpha + X_{ij}\beta + u_{ij} \tag{1}$$

where $i = 1,\ldots,N$ states are observed over $j = 1,\ldots,J$ years, y_{ij} is the *ij*th element of an $NJ \times 1$ vector of observations on the dependent variable, X_{ij} is the *ij*th row of *X* (an $NJ \times (k+1)$ matrix of observations on a set of *k* explanatory variables plus a constant), α is a scalar parameter and β a $(k+1) \times 1$ vector of parameters to be estimated, and u_{ij} is the *ij*th element of an $NJ \times 1$ vector of errors. Further, assume that u_{ij} follows a one-way error component model

$$u_{ij} = \mu_i + \varepsilon_{ij} \tag{2}$$

where $\mu_i \sim IID(0, \sigma_\mu^2)$ and $\varepsilon_{ij} \sim IID(0, \sigma_\varepsilon^2)$ are independent of each other and among themselves. The dynamic panel data model described in (1) and (2) is characterized by two sources of persistence: a) autocorrelation due to the presence of a lagged dependent variable, y_{ij-1}, among the regressors and b) the state-specific effects, μ_i, which capture heterogeneity among the states. Since y_{ij} is a function of μ_i, it follows that y_{ij-1} is also a function of μ_i. As a result, y_{ij-1} is correlated with the error term. This correlation renders ordinary least squares (OLS) and conventional simultaneous equations estimators, including two-stage least squares (2SLS), biased and inconsistent even if the ε_{ij} are not serially correlated. The fixed effects (FE) estimator uses the within transformation to eliminate the μ_i. However, the transformation still does not negate the correlation between the lagged dependent variable and error term. As discussed by Baltagi[11], $y_{ij-1} - \bar{y}_{i.-1}$, where $\bar{y}_{i.-1} = \sum_{j=2}^{J} y_{ij-1} / (J-1)$, will still be correlated with $\varepsilon_{ij} - \bar{\varepsilon}_{i.}$ even if the ε_{ij} are not serially correlated. This is because y_{ij-1} is correlated with $\bar{\varepsilon}_{i.}$ by construction. Thus, even if the ε_{ij} are not serially correlated, the FE estimator is biased and inconsistent for small *J*. If there exist strictly exogenous variables that can be used as instruments for the lagged dependent variable, then fixed-effects 2SLS (FE2SLS) will provide consistent parameter estimates[9].

Accordingly, the FE2SLS estimator has been used in many of the studies seeking to estimate the price elasticity of cigarette demand.

It is noteworthy that the FE2SLS estimator can be severely biased if the time dimension is short, which is often the case with panel data[8]. Specifically, the parameter for the lagged dependent variable will tend to be underestimated. An important consequence is that the long-run price elasticity of cigarette demand will also be underestimated since it is a function of the autoregressive parameter. Anderson and Hsiao[12], Arellano and Bond[13], Ahn and Schmidt[14], and others have developed alternative estimators that can be used to fit linear dynamic panel data models when the equations' disturbances are serially uncorrelated. The state-specific effects are eliminated through first-differencing, which makes it easier to account for the endogeneity of the lagged dependent variable. These estimators can be used when few or no regressors are strictly exogenous. Further, they do not require the specification of models for any contemporaneous endogenous regressors[15].

Baltagi and colleagues[6-8] have published a series of papers investigating the relative performance of estimators for dynamic models of cigarette demand. Their results suggest that first-difference estimators, such as those developed by Anderson and Hsiao and Arellano and Bond, should be preferred to OLS, FE, and FE2SLS. In this paper, we seek to extend the work of Baltagi and colleagues. Using more recent data, we compare the OLS and FE estimators with the Arellano-Bond first-difference estimator. However, we also consider a more efficient generalized method of moments (GMM) estimator. We relax certain of the exogeneity assumptions made by Baltagi and colleagues and, dissimilar from them, include a state-level measure of educational attainment in our models to reduce bias in the estimated income elasticity of demand. We also use a novel autoregressive model specification to account for the persistency of the series.

METHODS

Model of Aggregate Cigarette Demand

We estimate a dynamic model for cigarette demand using panel data for the 48 contiguous states and the District of Columbia over the period 1989–2001. The demand equation is specified using a modified version of the habit-persistence model specified by Hamilton[16], McGuinness and Cowling[17], Fuji[18], and Baltagi and Levin[6,7]. The estimated equation is as follows:

$$\ln C_{ij} = \beta_0 + \beta_1 \ln C_{ij-1} + \beta_2 \ln C_{ij-2} + \beta_3 \ln P_{ij} + \beta_4 \ln I_{ij} + \beta_5 \ln E_{ij} + \beta_6 \ln Pn_{ij} + \mu_i + \lambda_j + \varepsilon_{ij} \qquad (3)$$

where subscript *i* indexes state ($i = 1,\ldots,49$) and subscript *j* indexes years 1991–2001 ($j = 3,\ldots,13$). All continuous variables are in natural logarithms to allow the estimates of model parameters to be interpreted as elasticities. That is, the percentage change in the dependent variable given a 1% change in the independent variable.

C_{ij} is per capita tax-paid sales of cigarettes among members of the state resident population aged 18 years or older. To fully account for the dynamic behavior of demand, we include first- and second-order lags for cigarette sales. P_{ij} is the average retail price per pack of cigarettes in a given state. I_{ij} is per capita disposable income among members of the state resident population. E_{ij} is the percentage of the state civilian non-institutionalized population aged 25 years or older with a bachelor's degree or higher level of education. In the context of our analysis, income and education measure different things. Disposable income captures the effect of having financial resources available to purchase cigarettes. Conversely, educational attainment serves as a proxy for the health knowledge stock in a given state. The variable Pn_{ij} is the minimum price per pack of cigarettes among all neighboring states, where neighbors are defined as sharing a common border with state *i*. This variable serves as a proxy for casual "smuggling" or "bootlegging" of cigarettes across state borders. At any point in time, cigarette prices vary across states because cigarette tax rates vary. Smokers in high-price states may be tempted to purchase cigarettes from neighboring states with lower prices. Pn_{ij} represents a substitute price for the within-state price.

The disturbance term is specified as a two-way error component model, where μ_i denotes state-specific effects and λ_j denotes year-specific effects. The ε_{ij} are unobserved, random disturbances that are assumed to have zero mean. The state-specific effects can represent any state-specific characteristic. For example, the loss of tax revenues from non-Indians purchasing tax-exempt cigarettes from reservations in Montana, New Mexico, or Arizona; the loss of tax revenues due to purchases of cigarettes from tax-exempt military bases in Florida, Texas, Washington, or Georgia; the low per capita consumption of cigarettes in Utah due to the prevalence of the Mormon religion in that state; or state-specific tendencies to regulate the sale of tobacco or ban its consumption in public venues. The temporal effects (i.e., λ_j) are assumed fixed parameters to be estimated as the coefficients of time dummies for each year in the sample. These are meant to capture the effects of policy interventions, health warnings, Surgeon General's reports, and shifts in public opinion against smoking that have occurred over time.

Although Hamilton[16], Schmalensee[19], and Baltagi and Levin[6,7] considered the impact of advertising on cigarette demand, we chose not include a measure of cigarette advertising in equation (3). Cigarette advertising data are generally not available at the state level, though annual advertising expenditures for different media are available at the national level. National advertising expenditures would have been collinear with the fixed time effects and were, therefore, excluded. We investigated a state-level measure of per capita consumer magazine cigarette advertising expenditures developed for the American Stop Smoking Intervention Study (ASSIST)[20,21]; however, it was excluded from the model due to its negative correlation with cigarette sales. In recent years, cigarette manufacturers have eschewed consumer magazine promotions in favor of other media, including specialty item distribution, public entertainment, promotional allowances, point-of-sale, and retail value added. Magazines represented 20.4% of domestic cigarette advertising and promotional expenditures in 1980, 10.5% in 1989, and just 1.5% in 2001[22]. Thus, we believe that the variable served as a proxy for advertising inefficiency; it measured the effect of allocating fixed resources toward consumer magazines as opposed to more productive media. Others should think carefully before using consumer magazine advertising data for similar research purposes.

Data Sources

Monthly tax-paid cigarette sales (in thousands of packs) were obtained from the *Monthly State Cigarette Tax Reports*[23,24]. From these data, we derived calendar-year totals that were converted to per capita sales using US Census Bureau estimates of the midyear (July 1) population aged 18 or older in each state[25]. State-specific estimates for price, as of November 1 of each year, were taken from *The Tax*

Burden on Tobacco[24]. The average price of a pack of cigarettes is constructed by using weighted averages for a pack of 20 cigarettes based on the prices of single packs, cartons, and vending machine sales, where the weights are the national proportions of each type of sale. These prices include generic cigarettes and state-level sales taxes applied to cigarettes but exclude local cigarette taxes. Because prices are published as of November 1, we created a weighted average annual cigarette price measure using methods developed for project ImpacTeen[26]. State and federal excise taxes as of November 1 were subtracted from current and previous years' prices. Assuming a linear rate of change in prices, we estimated the average net-of-tax calendar-year price (approximated by price as of July 1 in a given year) as the sum of two-thirds of the price reported in November of the current year plus one-third of the price reported in November of the preceding year. We then added back the average state and federal tax for the calendar year based on the fraction of the year a given rate was in effect. Data on state-level per capita disposable income were obtained from the Bureau of Economic Analysis[27]. State education attainment levels were obtained from the US Census Bureau Current Population Reports[28]. To account for changes in relative prices over time, all economic measures were deflated by the national Consumer Price Index (1982–1984 = 100) published by the Bureau of Labor Statistics[29].

Descriptive statistics for the variables are presented in Table 1. We include T_{ij} and Tn_{ij}, which represent the total (state and federal) excise tax per pack of cigarettes and the total excise tax per pack of cigarettes in the neighboring state with the minimum cigarette price, respectively. Though not depicted in equation (3), we employ these variables in a sensitivity analysis of our results. Both measures were deflated to 1982–1984 US dollars.

Estimation Details

Our purpose here is to provide some technical nuances for the interested reader. More detailed information may be found in chapter eight of Baltagi[11], including discussions of identification, instrumentation, and moment conditions for dynamic system estimators.

Arellano and Bond[13] developed a "difference GMM" estimator that treats the model as a system of equations, one for each time period. The equations differ only in their instrument/moment condition sets. The system is transformed to first differences, and differenced endogenous variables (e.g.,

$\ln C_{ij} - \ln C_{ij-1}$) are instrumented with suitable lags of their own levels (e.g., $\ln C_{ij-2}$ or $\ln C_{ij-3}$). Strictly exogenous regressors enter the instrument set in first differences (e.g., $\ln I_{ij} - \ln I_{ij-1}$). A critical assumption of the difference GMM estimator and of related estimators[12,14,30,31] is that the disturbances in the levels equation are not autocorrelated. If this assumption is rejected, then the orthogonality conditions involving lags of the dependent variable or lags of the regressors are not valid, and the whole estimation procedure is not valid either. The assumption that the idiosyncratic error term in the levels equation is not autocorrelated has two testable implications: the disturbances in the first-differenced equation will exhibit negative first-order serial correlation and zero second- or higher-order serial correlation. Arellano and Bond[13] developed formal tests of these hypotheses.

A problem with the original Arellano-Bond estimator is that lagged levels are often poor instruments for first differences. Arellano and Bover[30] described how, if the original equations are added to the differenced system, additional moment conditions can be brought to bear to increase efficiency. In these equations, predetermined and endogenous variables in levels are instrumented with suitable lags of their own first differences. Blundell and Bond[31] articulated the necessary assumptions for this augmented estimator more precisely and tested it with Monte Carlo simulations.

In what follows, we present results using this "system GMM" estimator. The endogenous variables, C_{ij} and P_{ij}, and their lags are instrumented as described above. We also treat Pn_{ij} as endogenous under the assumption that neighbor-state cigarette prices may be influenced by the price of cigarettes in the *i*th state. We compare the system GMM results with those of the less efficient difference GMM estimator as well as with those of OLS and FE estimators treating the endogenous variables and their lags as exogenous. The sole purpose of this is to demonstrate the biases of the other estimators when applied to dynamic panel data models. The robust estimator of the covariance matrix[32,33] is calculated for the OLS and FE estimators. A finite sample correction developed by Windmeijer[34] is applied to the robust two-step covariance matrix calculated for the difference and system GMM estimators.

When using GMM estimators, the number of valid instruments increases rapidly as the number of strictly exogenous variables and the number of time-series observations increase. It is well documented that GMM estimators with too many overidentifying restrictions perform poorly in finite samples[8,35,36]. When *J* and *N* tend to infinity at the same rate, the asymptotic bias of the difference GMM estimator is $O(1/N)$ [37]. Arellano[38] shows in a later paper that the bias is $O(m/N)$ for predetermined variables and $O(mJ/N)$ for endogenous variables, where *m* is the number of instruments. The magnitude of these biases suggests that, as a rule of thumb, one should use fewer than *N* instruments, where *N* is the number of cross-sectional units (e.g., states). We accomplish this by restricting the number of lags and differences of lags used as instruments as well as by "collapsing" the instrument matrix. Specifically, we group together columns of the instrument matrix that are for the same variable and lag distance and combine them by addition in order to generate a smaller set of moment conditions. This method of reducing the bias caused by too many overidentifying restrictions was developed by David Roodman of the Center for Global Development in Washington, DC and is implemented in his *xtabond2* module for Stata[39]. Roodman[40] applies this procedure in studying the relationship between the amount of foreign aid received by a country and its economic growth.

RESULTS

Results for the cigarette demand equation (3) are presented in Table 2. Our primary results are given in the column labeled "System GMM2." Here, the only strictly exogenous variables included in the instrument matrix are income, educational attainment, and fixed effects for time. In the column labeled "System GMM3," we present the results of a sensitivity analysis in which state total tax and total tax in the neighboring state with the minimum price are also used as instruments. The former represents the more conventional/conservative approach to system GMM estimation. However, to the extent that the tax variables are valid instruments in the demand equation, one would expect efficiency gains by including them in the instrument set.

The coefficients for the first- and second-order lags of sales are positive and significant, indicating a persistence effect in the demand for cigarettes. The short-run price elasticity of demand is -0.304 ($t_{48} = 5.44, p < 0.001$). Thus, doubling cigarette prices produces a statistically significant 30.4% decrease in the immediate demand for cigarettes. The long-run price elasticity of demand is computed as follows: $\partial E(\ln C_{ij}) \big/ \partial \ln P_{ij} = \hat{\beta}_3 \big/ (1 - \hat{\beta}_1 - \hat{\beta}_2)$. We find demand to be highly price sensitive in the long run, with an estimated elasticity of -3.51 ($t_{48} = 2.36, p = 0.02$). This implies that once states have fully adjusted to a 1% increase in the price of cigarettes, demand will be reduced by 3.5%. While the coefficient for income is statistically insignificant ($t_{48} = 0.50, p = 0.62$), doubling per capita disposable income increases cigarette demand by 2.1%. The estimated elasticity for educational attainment is negative and is significant at the 10% level ($t_{48} = 1.89, p = 0.07$); thus, more educated state populations appear to purchase fewer packs of cigarettes per capita than do less educated state populations. The effect of education on smoking would be expected to be negative, while a pure income effect controlling for education could be positive[7]. The coefficient for Pn_{ij} is positive and significant at the 10% level ($t_{48} = 1.69, p = 0.09$), which implies that doubling the minimum price of cigarettes among all neighboring states increases demand within a given state by 22.7%. This is consistent with the notion that high neighbor-state prices present a disincentive to bootlegging and, therefore, increase within-state cigarette sales. Several tests provide evidence for the validity of the model specification. We perform the *J* test[41] to evaluate our overidentifying restrictions. The test fails to reject the null hypothesis that the instruments, as a group, are exogenous ($\chi^2_{28} = 28.58, p = 0.43$). Further, tests performed on the differenced residuals indicate the presence of first-order serial correlation but the absence of second-order serial correlation.

As depicted for System GMM3 in Table 2, adding moment restrictions (i.e., using state tax and neighbor-state tax as instruments) appears to improve the efficiency of the estimator when applied to the demand equation. For example, the effects of education and neighbor-state price become significant at the 5% level. However, the *C* test[42] rejects the null hypothesis that state tax and neighbor-state tax are exogenous ($\chi^2_2 = 6.96, p = 0.03$). As such, the specification is not valid.

Our results substantiate the bias that occurs when GMM estimators are used with an excessive number of moment conditions. As shown in Table 2, the demand-equation estimates for Difference GMM1 resemble those of the FE estimator, while the estimates for System GMM1 are biased toward those of OLS. Using a more reasonable number of instruments, one would expect the difference GMM estimates and system GMM estimates to be fairly close. The coefficients for the first- and second-order lags of sales for Difference GMM2 are generally similar to those for System GMM2. It is well known that the overidentification test lacks power when the instrument set is large[42,43]. Thus, the test dramatically failed to reject the null hypothesis for Difference GMM1 and System GMM1 even though the overidentifying restrictions were invalid.

DISCUSSION

Dynamic Specification

To our knowledge, ours is the first study to treat cigarette demand as a second-order autoregressive process. Previous studies[4-10,44-46] have either used the habit-persistence model, which posits that demand in the current period is a function of demand in the preceding period, or the rational-addiction model, which posits that current demand is a function of demand in the immediately preceding and succeeding periods.

According to the rational-addiction model, *individual consumers* modify their consumption patterns based on the expected level of future consumption[4,47]. It is not immediately apparent why this model should be applicable to states. The attribution of a cognitive process to states lacks face validity. Further, even if one presumes that the underlying utility maximization occurs at the individual level, the application of the model to state-level data raises concerns about aggregation bias[48]. Because of this, we elected not to use the rational-addiction model in our analyses. Instead, we presumed that current cigarette demand in a state would reflect demand in that state over time without giving consideration to the mechanism, cognitive or otherwise, producing the dynamic relationship. Our choice to model cigarette sales as a second-order autoregressive process was driven mainly by observed relationships in the data. We considered a model with an AR(1) specification. However, the AR(2) test rejected the null hypothesis of no serial correlation in the disturbances of the first-differenced equation, which argued against the validity of this specification.

Auld and Grootendorst[49] criticized the use of aggregate or cross-sectional time series data when estimating the parameters of rational-addiction models. They claimed that parameter estimates will be biased and inconsistent due to the persistency and autocorrelation present in aggregate data. They suggested that their results might not apply to studies using individual-level data, such as those conducted by Labeaga[44] and Chaloupka[45]. We believe that Auld and Grootendorst's findings are relevant only to the rational-addiction specification and, therefore, not to our work. Ferguson[50] has demonstrated both theoretically and empirically the bias of the rational-addiction model when applied to aggregate data. Conversely, he has shown that the partial adjustment (i.e., myopic-addiction, habit-persistence) model yields reasonable results in this context[51]. He concluded that one should use either a partial-adjustment model, as we did, or the more general error-correction model instead of imposing a rational-addiction specification on aggregate data[50,51].

We found that the persistence effect, measured by the sum of β_1 and β_2, was consistently overestimated to a small degree by OLS but underestimated to a large degree by FE. It is well documented that the OLS and FE estimators produce biased and inconsistent estimates when applied to dynamic panel data models and that their biases occur in opposite directions[9,13,15,30]. The bias of the OLS estimator decreases as the autoregressive parameters approach unity. However, the opposite is true of the FE estimator; its bias increases with the persistence of the series[52]. Taken together, our results suggest that cigarette sales exhibit a high degree of persistency in recent years and should be examined for unit roots. Allowing for a second-order autoregressive process with no time trend, we performed the Levin-Lin-Chu[53] panel unit root test to evaluate the series for a unit root. The null hypothesis of nonstationarity was rejected (p = 0.016). A simple cointegration test was also formulated by testing whether the sum of β_1 and β_2 as estimated by System GMM2 was different from 1.0[51,54]. The condition that $\beta_1 + \beta_2 = 1$ would be necessary for the presence of a unit root in the second-order differenced equation. With a two-tailed significance level of 0.05, the test rejected the null hypothesis of unity ($F(1, 48) = 4.64$, p = 0.036). Unit root tests for panel data are known to exhibit a limited ability to accurately reject the unit root hypothesis when applied to short, highly persistent time series. As such, the consistency of our results provides us with some degree of confidence in the stationarity of the series.

Time Period Studied

Some may be critical of our decision to limit our analyses to a recent and relatively short period of time. We desired to use calendar-year estimates of cigarette sales and prices so that these would be consistent with the intervals over which disposable income and educational attainment were measured. Combining fiscal-year and calendar-year data would have made any interpretation of parameter estimates difficult. We were unable to generate calendar-year estimates of cigarette sales for years prior to 1983 since the necessary monthly sales reports were lacking. Furthermore, our measure of educational attainment was not available prior to 1989. Also, as noted previously GMM estimators can yield substantially biased estimates when the number of instruments exceeds the number of cross-sectional units. Since the number of instruments grows with the number of time periods, the time series can become sufficiently long to render these estimators untenable. This may be the case even if one uses fewer than all available lags of the endogenous variable(s) as instruments. In light of the aforementioned concerns, we elected to restrict our focus to the period spanning 1989 to 2001.

Notwithstanding the limited availability of certain data, we feel justified in restricting our analysis to the years studied. As noted, cigarette sales exhibit a high degree of persistency. The stabilization of sales appears to be a fairly recent phenomenon, and this increased persistency calls into question whether a single model may adequately reflect the dynamic structure of sales over a longer time horizon. Nelson studied the effect of advertising bans on cigarette demand using aggregate data for 20 countries over the years 1970 to 1995. He concluded that a structural change in the demand for cigarettes occurred in 1985, with a resulting increase in the price elasticity and decrease in the income elasticity of demand following the regime shift[55]. The results of Saffer and Chaloupka[56] are also suggestive of a structural change in the demand for cigarettes in or around 1985.

With the exception of declines of approximately 6% in 1999 and 2000 following the implementation of the Master Settlement Agreement, per capita cigarette sales declined by approximately 2% each year from 1989 to 2001. This is consistent with reported declines in annual cigarette sales of 2-3% from the Economic Research Service (ERS). The ERS has attributed the slow decline in sales to diminishing opportunities for individuals to smoke in public places as well as the rising cost of cigarettes due to higher taxes[57]. It is notable that the slow decline in per capita sales is also consistent with observed trends in smoking prevalence. Estimates from the Current Population Survey indicate that 25.5% of the US population smoked in 1990 compared with 22.8% in 2001. The dynamics of adult smoking exhibit a large degree of inertia and are likely to prevail for years to come[58]. As such, we feel our model provides an accurate depiction of the recent trend in demand.

Comparison with Previous Research

Studies estimating a linear dynamic panel data model for cigarette demand[4-10,44,46] have derived long-run price elasticities ranging from -0.11 to -2.69. Akin to these studies, we found demand to be more price sensitive in the long run than the short run. However, our estimate of the long-run elasticity exceeds earlier estimates and indicates that, in the long run, demand is highly price elastic. Several factors may explain this difference.

First, the available studies differed in the measure of cigarette demand used, i.e., per capita consumption or sales. The elasticities derived from sales data are apt to be larger than those derived from consumption data due to casual smuggling. Even when a proxy for smuggling is included in the analysis, it may not fully address the problem. For instance, our bootlegging specification did not account for the fact that, in a geographically large state, cross-border shopping may have taken place in different neighboring states and not just in the minimum-price neighboring state. Further, it did not account for Internet sales or sales made on reservations. The level of cigarette consumption derived from surveys is often significantly lower than actual cigarette sales, with the degree of underreporting being positively related to the level of social disapproval of smoking. Also, consumption data tend to be less sensitive to smoking initiation than sales data. Most surveys are designed to target adults and, therefore, begin too late to detect the effect of initiation. Aggregate sales data are more sensitive to initiation to the extent that they include the cigarettes purchased by or for new initiates. Thus, over the long run, they reflect better the cumulative effect of initiation. The use of aggregate sales data to estimate demand is widespread. While individual-level data are preferable in many applications, one often trades the ability to makes claims regarding individual price sensitivity for reduced generalizability across states or time.

Second, the time series used in earlier studies were generally longer and more mature than the series used in our investigation. For example, Becker et al.[4] applied the FE2SLS estimator to state-level sales data from 1955 to 1985 and derived an average long-run price elasticity of -0.75, while Baltagi and Griffin[8] applied the difference GMM estimator to aggregate sales data from 1963 to 1992 and derived a long-run price elasticity of -2.04. Presuming the stabilization of cigarette demand is a recent phenomenon, one would expect studies using newer sales data to derive larger price elasticities than studies using older sales data. As a result, the observed differences in long-run price elasticities may be an indication that the older and newer sales data should not be pooled.

Third, the FE2SLS estimator was used in most of the existing studies. This estimator is downward biased when applied to short time series and is only consistent if the variables included in the instrument matrix are strictly exogenous. Studies may have underestimated the effect of lagged demand due to use of the FE2SLS estimator, which led to underestimation of the long-run price elasticity. It is notable that the long-run price elasticities reported by Baltagi and colleagues[6-8] when using first-difference estimators are the closest in magnitude to our own.

Fourth, the available studies differed with respect to the dynamic model specification applied, i.e., rational addiction or habit persistence. Of those studies in which a rational-addiction model was used to estimate demand, most sought to measure demand using aggregate state-level cigarette sales. As discussed previously, there is growing evidence that the rational-addiction model yields biased estimates when applied to aggregate data. This may call into question the validity of the earlier studies' findings.

Fifth, some studies[e.g., 46] have estimated the demand for cigarettes from complex survey data without accounting for the disproportionate selection probabilities of sample members. In complex surveys, participants are selected through a random process, though different participants may have different probabilities of selection. Estimates of the price elasticity of demand tend to vary with age and race/ethnicity. For instance, the demand for cigarettes among racial/ethnic minorities and lower income groups tends to be more price elastic than demand among whites and higher income groups. Consequently, the results of the earlier studies may have been biased due to failure to account for the oversampling of elderly or minority participants.

In previous research, Baltagi et al. did not adjust for the effect of education on cigarette demand. They assumed the invariance of educational levels over time and claimed to control for the effect of education through the inclusion of state-specific effects[7]. Education is positively correlated with income. Therefore, to the extent that it is also negatively correlated with cigarette demand, one would expect the income elasticity of demand to be underestimated when excluding education from the model. Our findings with respect to educational attainment suggest that Baltagi et al.'s estimates may suffer from omitted variable bias.

Baltagi et al. also used the minimum neighbor-state cigarette price as a proxy for casual smuggling. They chose to treat this variable as exogenous in their models, whereas we elected to treat it as endogenous. To us, it was more plausible to treat neighbor-state price as endogenous, and we believe that doing so was the more conservative approach. Treating the minimum neighbor-state price as exogenous, our estimates for lagged sales, price, income, and education did not change appreciably, though the last of these became significant at the 5% level. The estimated elasticity for minimum neighbor-state price was greatly reduced (0.141 vs. 0.227) and also became significant at the 5% level. The *J* test and autocorrelation tests provided no conclusive evidence against the validity of the model specification.

Our findings with respect to overparameterized GMM estimators are consistent with those of others who have studied their finite-sample biases[8,35,36]. When we restricted the number of instruments to be smaller than the number of states, the difference GMM and system GMM estimates of autocorrelation parameters were fairly similar. The former were slightly smaller than the latter, which may be attributed to the persistence of the time series. Lagged levels become weak instruments for first-differences when the series is highly persistent, and instrumental variables estimators are subject to finite-sample biases when instruments are weak[15,31]. The finite-sample bias of the first-differenced GMM estimator is downward (i.e., in the direction of the FE estimator) when it is applied to highly persistent series. By contrast, the system GMM estimator exhibits much smaller finite-sample biases and greater precision when estimating autoregressive parameters in this context[15].

CONCLUSIONS

The current study suggests that state-level cigarette demand is more price elastic than previously thought. That is, higher tobacco prices curtail consumption more than may have been suspected prior to this research. The immediate implication is that any policy efforts to reduce tobacco use will be strengthened by higher federal and state taxes on tobacco products. The current study also suggests that education is a significant deterrent to tobacco use; a more educated society is less likely to smoke. Given current positive trends in US education, this bodes well for smoking-cessation efforts.

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Table 1. Descriptive statistics for variables used in regression analyses, 1989–2001

Variable	Definition	Mean (SD)	Minimum	Maximum
C	Tax-paid cigarette sales in packs per capita state resident population aged 18 years or older.	126.54 (32.67)	49.54	243.88
P	Weighted average real price per pack of cigarettes (including generic brands and all state and federal taxes). Deflated in 1982–1984 US dollars.	1.32 (0.28)	0.90	2.37
Pn	Minimum price per pack of cigarettes among all neighboring states. Deflated in 1982–1984 US dollars.	1.20 (0.25)	0.90	2.07
I	Real disposable income per capita state resident population. Deflated in 1982–1984 US dollars.	13,121 (1,968)	9,113	21,237
E	Percentage of state civilian non-institutional population aged 25 years or older with a bachelor's degree or higher level of education.	22.75 (5.03)	11.10	42.10
T	Weighted average sum of real state and federal excise taxes per pack of cigarettes. Deflated in 1982–1984 US dollars.	0.35 (0.12)	0.14	0.82
Tn	Total excise tax per pack of cigarettes in neighboring state with minimum per-pack cigarette price. Deflated in 1982–1984 US dollars.	0.27 (0.08)	0.14	0.67

Table 2. Pooled estimation results of cigarette demand equation, 1989–2001

	OLS	FE	Difference GMM1[1,2]	Difference GMM2[1,2,5]	System GMM1[2]	System GMM2[2,4,5]	System GMM3[2,3,4,5]
Constant	-0.085	2.765	--	--	-1.38	0.397	0.094
	(0.38)	(1.82)			(1.41)	(0.84)	(0.26)
ln C_{ij-1}	0.769	0.507	0.601	0.733	0.834	0.759	0.737
	(10.70)	(6.28)	(7.00)	(12.04)	(9.32)	(12.64)	(10.59)
ln C_{ij-2}	0.204	0.183	0.180	0.141	0.211	0.154	0.182
	(2.82)	(2.90)	(2.17)	(3.75)	(2.49)	(3.63)	(3.46)
ln P_{ij}	-0.236	-0.509	-0.457	-0.241	-0.356	-0.304	-0.389
	(6.32)	(8.20)	(2.68)	(2.57)	(2.66)	(5.44)	(5.88)
ln I_{ij}	0.035	-0.137	-0.373	-0.314	0.125	0.021	0.053
	(1.49)	(0.84)	(0.88)	(2.82)	(1.42)	(0.50)	(1.55)
ln E_{ij}	-0.041	0.028	-0.027	-0.030	0.004	-0.058	-0.062
	(3.13)	(0.79)	(0.58)	(0.73)	(0.13)	(1.89)	(2.38)
ln Pn_{ij}	0.115	0.249	0.246	0.170	0.067	0.227	0.255
	(3.95)	(3.32)	(1.52)	(1.16)	(0.42)	(1.69)	(4.67)
N	49	49	49	49	49	49	49
No. Obs.	539	539	490	490	539	539	539
No. Instruments	NA	NA	207	45	251	45	47
R^2	0.973	0.948	0.924	0.947	0.969	0.972	0.972
AR(1) test	NA	NA	0.002	0.001	0.003	0.001	0.002
AR(2) test	NA	NA	0.280	0.367	0.348	0.353	0.255
Hansen *J* test	NA	NA	1.000	0.839	1.000	0.434	0.223

Notes: Numbers in parentheses are robust *t* statistics. All regressions included time dummies, which are not reported. R^2 was estimated using the square of the correlation between the fitted values and dependent variable. *p*-values are presented for the Arellano-Bond tests of AR(1) and AR(2) in first-differences as well as for the Hansen *J* test of overidentifying restrictions.

NA = not applicable

1 An additional year's worth of data was lost due to first-differencing. The constant was differenced out and was not estimated.

2 Lags of lagged endogenous variables in levels dated *j* – 1 or earlier and lags of contemporaneous endogenous variables in levels dated *j* – 2 or earlier were used as instruments for the first-difference equations. Lags of lagged endogenous variables in first-differences dated *j* or earlier and lags of contemporaneous endogenous variables in first-differences dated *j* – 1 or earlier were used as instruments for the levels equations. Income, education, and time dummies in first-differences were used as instruments for the first-difference equations and in levels as instruments for the levels equations.

3 State total tax and neighbor-state total tax in first-differences were used as instruments for the first-difference equations and in levels as instruments for the levels equations.

4 Lags of lagged endogenous variables dated earlier than *j* – 9 and lags of contemporaneous endogenous variables dated earlier than *j* – 10 were not used as instruments.

5 The instrument matrix was collapsed by adding columns for the same variable and lag distance.