ABSTRACT

Title of Dissertation / Thesis: **ECONOMIC ANALYSIS OF STATE LOTTERIES IN THE UNITED STATES**

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Modern lotteries in the United States are run exclusively by state governments. In many cases, states establish separate lottery agencies to administer and promote the games. One statutory duty of many lottery agencies is to maximize the net revenue of the games, hence, all agencies engage in advertising. There is however constant pressure from state legislatures to reduce advertising budgets because of the concerns about the efficacy of advertising in increasing sales, as well as the distaste for the state government's promotion of lottery. Existing literature suggests that the marginal effectiveness of advertising decreases as the quantity of advertising increases. To provide empirical evidence on whether an additional advertising dollar increases lottery sales, we examine quasi-experiments in three states (Illinois, Washington, and Massachusetts) where advertising budgets of state lotteries were exogenously curtailed by the state legislature. We find that the elasticity of advertising is 0.07-0.16, suggesting that a one-dollar decrease in advertising spending could cost the state government $9-10 of the net revenue at the margin. Contrary to the belief of some legislatures that state lotteries spend

too much on advertising, our results suggest that they may advertise too little in terms of maximizing the profit.

Out of the thirty-eight states with lotteries, sixteen earmark lottery profits for primary and secondary education. Given the fungibility of money, economists have questioned the effectiveness of the earmarking policies. Using a panel data set of states with lotteries, we find that 60-80 cents out of an earmarked dollar is spent on public education. In contrast, each dollar of lottery profit increases school spending by about 50 cents in states that deposit profits into the general fund, and by only 30 cents in states that earmark profits for areas other than education. Using a Bayesian estimation procedure for inequality restrictions in the normal linear least squares model, we find there is a high likelihood that a dollar of earmarked lottery profits generates less than a dollar of spending on K-12 education, but more than the spending generated from a dollar of lottery profits put into the general fund.

ECONOMIC ANALYSIS OF STATE LOTTERIES IN THE UNITED STATES

By

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Chapter 1: Overview

Since 1964 when New Hampshire first started the state-wide sweepstakes, thirty-seven other states plus Washington D.C. have legalized lotteries, with more states poised to follow.[1] The resurgence of state lotteries is spurred by the increasing demand for more or improved public services and strong opposition to tax hikes. The legalization of lotteries tends to ripple within regions of the country. As one state starts a lottery, its bordering neighbors tend to follow suit in order to reduce the potential loss of tax revenues to their neighbor. In the thirty-eight states where lottery games are currently offered, lottery sales amount to about $38 billion a year. They contribute about $12 billion to state coffers annually, accounting for 1.4 percent of the total state revenue, more than the tax revenue collected from sales of tobacco and alcoholic beverage together.

The controversial nature of lottery profits has aroused public concerns on almost every policy issue related to it: whether a state should legalize the lottery, what programs lottery profits should benefit, whether lotteries prey on economically disadvantaged population, and how much state governments should allow lotteries to spend on advertisement. Some issues are fairly common in the public finance realm such as the effectiveness of earmarked revenues in increasing spending on designated programs; others are unique in the field of public finance, such as the determination of the proper scale of lottery advertisement by state government.

[1] North Dakota and Tennessee approved lotteries in the November, 2002 election.

Although state lotteries have been in business for forty years, research on some issues related to state lottery policies is not ample. A comprehensive evaluation of the impact of state lotteries on citizens and governments is beyond the scope of this dissertation and requires academic investments from a variety of different fields in the social sciences. In this dissertation, I focus on two more limited but very important aspects of lottery operations. In the first half of the dissertation, I study the efficacy of advertising in increasing lottery sales, while in the second half, I estimate the impact of earmarked lottery profits on state educational expenditures.

1.1 Is too much money spent on lottery advertising?

Most lotteries are operated by a specific state government agency. One statutory duty of these agencies is to maximize the net revenue generated from lottery sales.[2] To bolster profits, each state lottery agency engages in advertising. Advertising budgets are financed through lottery ticket sales but the budget must be approved annually by the legislature. Legislatures in some states are concerned that lottery advertising budgets are too high. Proponents of this view typically make two arguments in support of their case. First, some argue that lottery advertising has saturated the market and it no longer

[2] Here are some examples of texts taken out of state lottery laws specifying such an objective. In Arizona Revised Statutes Title 5 Chapter 5 Article 1 5-504B, it is stated that "the commission shall oversee a state lottery to produce the maximum amount of net revenue consonant with the dignity of the state." In Article 3 Sections 8880.25 of California State Lottery Act of 1984 (California Government Code Title 2 Division 1 Chapter 12.5), it is stipulated that "the Lottery shall be initiated and operated so as to produce the maximum amount of net revenues to supplement the total amount of money allocated for public education in California." We also find the following text in Georgia Code Title 50 50-27-2 (3) that "lottery games shall be operated and managed in a manner which provides continuing entertainment to the public, maximizes revenues, and ensures that the lottery is operated with integrity and dignity and free of political influence." In New Jersey State Lottery Law (N.J.S.A 5:9-7) it is also states that "…lottery shall produce the maximum amount of net revenues for State institutions and State aid for education consonant with the dignity of the State and the general welfare of the people."

expands ticket sales. Second, others find it distasteful that states encourage participation in gambling.

While this dissertation will have little to say about the second point, the first concern is obviously an empirical question. The existing literature on the impact of advertising on sales suggests that the marginal effectiveness of advertising decreases as the quantity of advertising increases, and that “saturation stages” are different for different products. To determine whether an additional advertising dollar increases lottery sales, we examine quasi-experiments in three states (Illinois, Washington, and Massachusetts) where advertising budgets of state lotteries were exogenously curtailed by the state. The basic estimation method employed is a difference-in-difference model. We measure changes in advertising spending and sales before and after the legislative decision to cut the budget, and determine whether both advertising spending and sales are lower during the treatment period. To obtain direct measurements of advertising effects such as elasticity of sales with respect to advertising, we also treat legislative decisions as instruments for advertising spending and apply the two-stage least squares method to combine the results above.

We find that the reduction in advertising spending in these three states reduced lottery ticket sales. Our calculated elasticity of sales with respect to advertising ranges between 0.07-0.16. Though of different products, our estimated elasticity is in line with previous studies. For example, Seldon et al (1989) find that the elasticity of cigarette sales with respect to advertising is between 0.1-0.25, and Valdés (1993) reports an estimated elasticity of 0.08-0.09 in the cigarettes industry in Spain.

Contrary to the belief of some that too much is spent on lottery advertising, our results suggest that if the goal of a lottery agency is to maximize profits, most states are spending too little on advertising. We calculate the gross rate of return to advertisement and find that a one-cent decrease in monthly advertising spending per capita results in a decrease in gross sales of 31-37 cents. Assuming that thirty percent of sales are retained in state coffers as profit, a one-dollar decrease in advertising spending could cost state government $9-11 of net revenue at the margin.

The results are not surprising since advertising expenditure accounts for only 1.17 percent of total sales, an advertising/sales ratio that is much lower than other industries. For example, casinos on Las Vegas Boulevard spend 2.4 to 4 percent of the total hotel-casinos' revenues on advertising (Zheng 1999), restaurant owners spend 3.2 percent of their total revenues on advertising, beverage manufacturers 7.5 percent, cosmetics companies 8.8 percent, and candy makers 12.7 percent.[3] State lotteries are constrained in means that could be used to maximize profits. In addition to the annual legislative review of advertising budgets, some states explicitly cap the advertising budget at a small percentage of gross sales in the legislation;[4] some states stipulate that certain fraction of total sales should be returned to the public in the form of prizes.[5] It is therefore not unreasonable that we find that state lotteries fall far down the advertising response curve away from the saturation stage.

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[3] The statistics is quote from the online information provided by the North American Association of State and Provincial Lotteries available from the World Wide Web: http://www.naspl.org/faq.html.

[4] Lottery advertising budget is capped at 4 percent of gross sales in Arizona, 3.5 percent of projected sales in California, 3.05 percent of lottery sales in Idaho, 1 percent of lottery sales in Iowa, 2.75 percent of gross sales in Minnesota, and 3.5 percent of projected sales in Oregon.

[5] For example, Michigan Lottery Act specifies that "not less than 45% of the total annual revenue accruing from the sale of lottery tickets or shares shall be apportioned for payment of prizes to the holders of winning tickets or shares." The Act also stipulates that the lottery commissioner appointed by the governor shall promulgate rules regarding "(a) The type of lottery to be conducted. (b) The price of tickets or shares in the lottery. (c) The number or size of the prizes on winning tickets or shares."

1.2 Is earmarked lottery profit fungible?

Out of the thirty-eight states with lotteries, sixteen earmark lottery profits for primary and secondary education. Given the fungibility of money, many economists and political observers have questioned the effectiveness of earmarking policies. The fungibility of money is, however, rejected in a variety of empirical applications in public finance, most notably, the large literature on the “flypaper effect.” Whether earmarking increases spending is, in the end, an empirical question. In the second half of the dissertation, we address this issue by examining the experiences of lottery states in the past twenty years.

Using a panel data set of states with lotteries, we perform three different sets of tests. First, we restrict our attention to the nine states that have always earmarked lottery profits for K-12 education, and examine whether year-to-year changes in state spending on education are correlated with changes in lottery profits over the same period. If earmarking increases spending dollar for dollar, the coefficient on per capita lottery profits in models where per capita K-12 spending is the dependent variable should equal 1. Second, we examine states that switched the allocation of lottery profits between the general fund and public education during the sample period. If earmarking increases spending on education dollar for dollar, we should see an increase in K-12 spending commensurate with the amount of new monies allocated to the earmarked category once the profits were shifted to this category. In both of these tests, we use as a comparison group the fifteen states that have always deposited their lottery profits into the general fund.

In the third empirical model, we first model discrete events that should alter the lottery profits generated by states in “first-stage” regressions, then examine how K-12 spending is correlated with these events in “reduced-form” models. Drawing on these two results, we can use these shocks as instruments for lottery profits in a two-stage least squares model and estimate what fraction of the marginal dollar produced in these events is spent on education.

Given previous evidence on the “flypaper effect,” it is not surprising that we find up to three quarters of an earmarked lottery dollar finds its way to public schools. In contrast, each dollar of lottery profit increases school spending by about 50 cents in states that deposit profits into the general fund, and by only 30 cents in states that earmark profits for areas other than education. There is a high probability that a dollar of earmarked lottery profits generates more spending on K-12 schools than the spending generated from a dollar of lottery profits put into the general fund.

Chapter 2: Lotteries in America

In this chapter, we briefly review the history of lotteries in the America since the colonial time,[6] and discuss the regulatory structure of modern state lotteries. We also sketch the evolvement of product lines of modern lotteries, and weigh the contribution of state lotteries to the general revenue budget of state governments.

2.1 Brief History of Lotteries in America

Lotteries are not new to America. They were used extensively during colonial times to fund a diverse set of public services and infrastructures such as roads, bridges, wharves, buildings, colleges, churches, libraries, lighthouses, and the Colonial army. For instance, the Virginia Company of London, the financier of Jamestown in Virginia, was permitted by the Crown to hold lotteries to raise money for the company's colonial ventures.

Lotteries were most active during the period following the adoption of the Constitution and prior to the establishment of effective means of local taxation in the 1830s. From 1790 to the Civil War, fifty colleges, three hundred schools, and two hundred churches were erected with lottery proceeds. Among them were some of the nation's earliest and most prestigious universities – Harvard, Yale, Columbia, Dartmouth, Princeton, and William and Mary. Between 1790 and 1860, twenty-four out of thirty-

[6] Detailed history on lotteries in the United States can be found in Clotfelter and Cook's excellent book, *Selling Hope: State Lotteries in America* (1989). For a shorter discussion of the history and economic issues associated with state lotteries, see Clotfelter and Cook (1999).

three states financed civic improvements such as courthouses, jails, hospitals, orphanages, and libraries through lotteries.

During the first quarter of nineteenth century, the federal government begun to have access to revenue from tariffs, excise taxes on alcohol, and proceeds from sales of land. State and local governments could tax general property. Subsequently, the lottery gradually faded as a revenue source and taxation took its place in public finance.

State and local governments ran most of the early lotteries in the United States, but during the nineteenth century, private companies were hired by governments to operate and market public lotteries. However, these privately run lotteries had some celebrated cases of fraud that eventually caused their downfall. For example, in 1823, Congress passed a private lottery for the beautification of Washington D.C. In the end, the organizers absconded with the proceeds and the winner was never paid. The most notorious case occurred in 1868 where Louisiana, like other Southern states, turned to lotteries to generate revenue to rebuild the war-ravaged region. The Louisiana Lottery Company was authorized by the state and granted a 25-year charter. A carpetbagger criminal syndicate from New York bribed the legislature into passing the lottery law and establishing the syndicate as the sole lottery provider. In 1895 when the lottery was disbanded, it was discovered that promoters had made huge ill-gotten gains.[7] Concern about the corruption present in many games led states to prohibit state lotteries. By the end of the nineteenth century, thirty-five states had constitutional prohibitions against lotteries and no state permitted the operation of lotteries.

In 1964, lotteries made their twentieth-century debut in New Hampshire. To avoid federal anti-lottery statutes, the lottery was modeled after the Irish sweepstakes and tied

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[7] From "Gambling in California", California Research Bureau Report CRB-97-003, by Roger Dunstan.

to horse races. Winners are the lucky ones who purchased one of the few tickets tied to the horse who would win the race. Over the next six years, only one state, New York, adopted a lottery. In 1970, New Jersey started a "state" lottery. In contrast to the nineteenth-century model of privatized lotteries, the New Jersey Lottery was administered by a commission appointed by the Government, a model that most state lotteries follow today. Academic researchers usually recognize the New Jersey Lottery as the first modern state lottery.

In the early 1970s, state budgetary problems coupled with growing opposition to tax hikes generated a rapid, coast-to-coast expansion in state-run lotteries. By the end of the 1970s, most northeastern states had established lotteries; states on the West Coast followed suit in the 1980s; southern states jumped on the bandwagon in the early 1990s. Today, lotteries are operated in thirty-eight states and the District of Columbia. In Table 2-1, we list the states with lotteries and the years when lotteries began operation.

2.2 State Monopoly of Lotteries

2.2.1 Federal Regulation

Since the notorious scandal of the Louisiana lottery, the federal government outlawed the use of the U.S. mail for lotteries in 1890 and in 1895, invoked the Commerce Clause to forbid the shipment of lottery tickets across state lines. As a result, Title 18 of the United States Code § 1301 - 1304 banned transporting or mailing of lottery tickets or related matter, together with advertisement of or information concerning any forms of gambling, including lottery.

In 1975, exceptions were provided to state lotteries. Section § 1307 was added to Title 18 Chapter 61, allowing lotteries and its advertising to be conducted within a state that legalized lottery. However, cross-border sales of lottery tickets are still outlawed.

2.2.2 State Laws Regulating the Operation of Lotteries

Modern lotteries have been the unique province of state governments. State lottery agencies are in charge of the daily operation. These agencies differ in their administrative nature. Out of the thirty-eight state lotteries, eleven are placed within an existing state department, usually in the state's revenue department. The remaining twenty-three are established as separate organizations. We summarize in Table 2-2 the administrative nature of state lottery agencies and state codes regulating operations of lotteries as of 2003. The regulatory structures are remarkably similar across states. Despite minor variation in their administrative form, all state lotteries share a common subordination to the elected state legislature, with the responsibility for the form, goals, and operations of lotteries firmly in the hands of the latter.

Each state grants its lottery agency a monopoly; no competitors are allowed within the state. Part of the impetus behind the exclusivity is to ensure that the state can capture monopoly profits. It is the statutory duty of the lottery agency to maximize net revenue. To fulfill this obligation, most of the lottery agencies are organized in an entrepreneurial and business-like manner.

2.3 Lottery Product Mix

The product lines of state lotteries have been evolving continuously over the past forty years. In December 1970, New Jersey Lottery offered the first 50-cent weekly draw lottery ticket of modern state lotteries. Each ticket was preprinted with a potential winning number combination that was determined at weekly drawings. The game was a passive drawing game since it did not require any effort of players except purchasing the ticket. It did not differ much from the games offered during the colonial time. To attract more purchases, new games have been invented that involve active play by players. Now, three major groups of games are offered: instant scratch games, online games, and video lottery games.

In this section, we provide a general description of these games and track their evolution over the past forty years. Detailed descriptions on specific games can be found on web sites of state lotteries.

2.3.1 Instant Scratch Games

Massachusetts introduced the first instant scratch-off ticket in 1974. Tickets were sold to the public with vinyl-covered words or numbers. The covering can be easily scratched off with a coin, and the revealed words or numbers identify if the ticket holder wins a prize. Compared to the passive drawing games, the instant game offers players immediate gratification and immediate cash payoff from the authorized sales representative. To sustain players' interest, different game themes and prize structures are periodically introduced.

For years, most instant scratch off games had ticket costs of between fifty cents and one dollar. Beginning in the 1990s, states have instituted instant games with higher ticket costs, such as tickets of $2, $3, $5, $7 and $10. For example, Oregon offered its first $2 game, “Double Header,” in September 1990; Massachusetts introduced its first $5 ticket, “Holiday Bonus,” in November 1992; Kentucky Lottery introduced “Surprise Package,” its first $3 instant ticket, in September 1996; and in November 1997 Michigan Lottery’s first $10 instant ticket, “$1,000,000 Gift,” went on sale with a top prize of $1,000,000. Payout ratios of the new multi-dollar instant games are usually higher than the original $1 game.

2.3.2 Online Games

An online game is a game where tickets are purchased through a network of computer terminals located at retail outlets. The terminals are linked to a central computer that records the wagers. Examples of on-line games include numbers games, lotto and keno.

A. Daily Numbers Games

Lottery players wanted games in which they could actively participate by choosing their own numbers. In response, the New Jersey Lottery introduced the nation’s first online daily numbers game, “Pick-it,” on May 22, 1975. The daily numbers games allow players to choose their own three- or four-digit numbers, and to make various types of bets with different probabilities and payouts, through computer terminals. Today, all states offer a daily 3- and/or 4-digit number game. In some states such as Delaware and

Maryland, daily numbers are drawn twice a day at noon and in the evening. In other states such as Massachusetts and Minnesota, the numbers games are drawn once a day, seven days a week.

B. Lotto Games

Lotto was first introduced in Massachusetts and New York in 1978. The lotto game offered in Massachusetts was a pick of six numbers out of 49. If the player's numbers match the numbers drawn, he or she wins a jackpot which is a share of the dollars played for that drawing. If there is no winner on a particular drawing, the jackpot is rolled over until the next period. In recent years as lotto games have grown in size, rollover jackpots have reached hundreds of millions of dollars. By now, every lottery state except Maine has its own in-state lotto game.

Lotto profits are highly nonlinear in the size of the jackpot because the ticket sales grow quickly as jackpots rise. States with smaller populations have tried to exploit the benefits of large jackpot games by joining multi-state lotto games. In 1985, the first multi-state lotto game, Tri-State Lotto, was introduced in Maine, New Hampshire and Vermont. Later, in 1988, five states (Oregon, Iowa, Kansas, Rhode Island and West Virginia) plus the District of Columbia formed the Multi-State Lottery Association (MUSL), which has offered a series of multi-state lotto games. The most famous multi-state lotto game is Powerball. The game was started in 1988 as Lotto*America and was changed to Powerball in 1992. Powerball is now played in twenty-six states, Washington D.C and US Virgin Islands. Another well-known multi-state lotto game is the Big Game that started in 1996. It is played in eleven non-MUSL states. In May 2002, the Big Game was replaced with Mega Millions.

C. Keno Games

Keno was first introduced by the New York Lottery in 1988, and is now played in nine other states. Similar to a lotto game, keno allows bettors to choose a few numbers (how many is up to the player) out of a large set. Keno drawings are held more frequently, usually several times an hour. The game is mostly offered in lounges and bars. For example, the keno game offered in Maryland now allows players to select up to ten numbers from 1 through 80. The Lottery's computer randomly chooses twenty winning numbers between 1 and 80 for each game of keno and displays those winning numbers on the Keno Information Monitor. Players win based upon how many winning numbers they match. Drawings take place every four minutes.

2.3.3 Video Lottery Games

Video lottery games are close relative of slot machines. Like arcade video machines, video lottery terminals (VLTs) are usually programmed to carry a wider variety of games, such as poker, blackjack, and bingo, and offer players the chance to play a game and win immediate payouts for winning bets. South Dakota is the first state to license and regulate video lottery games. Currently, VLTs are permitted in seven states: Delaware, Louisiana, Montana, Oregon, Rhode Island, South Dakota, and West Virginia.

Most states introduce the lottery in stages where games with smaller payoffs such as the daily numbers and/or instant scratch-off games are offered first. Then, after a time, they introduce the larger-payoff games such as lotto or even a multi-state lotto. Instant games, daily numbers games, and lotto games are offered in all state lotteries, while keno and VLTs are only permitted in selective states due to concerns that such high-frequency games may entice some to play more than they can afford.

2.4 Who Plays the Lottery?

Much of the public concern about lotteries comes from the belief that playing lottery places a burden on economically disadvantaged population. Using the survey of Gambling Impact and Behavior Study 1997-1999 conducted by the National Opinion Research Corporation, Clotfelter et al (1999) estimate the demographic and socioeconomic patterns of lottery players, and conclude that lottery expenditures represent a much larger burden on household budget for low-income families than for those with high incomes. They find that although lottery participation rates increase with respect to household income, players with income less than $50,000 spend a larger fraction of their income on the lottery than players from other income categories, the lowest income category (households with income less than $10,000) has the highest per capita spending of $597. They also find that the degree of involvement is highly concentrated in a small fraction of players. The top 5 percent of players account for 54 percent of the total sales, and the top 20 percent account for 82 percent of the total sales.

Lottery play also differs systematically among demographic groups. They find that men are a bit more likely to play, and play somewhat more on the average, than women. Singles spend less on lottery plays than married or divorced people. Spending per player increases with age; per capita spending is the highest for the middle-age category of 45-64. Though participation rates are almost identical across ethnic groups, average spending by blacks who play is much larger than for other groups. Participation rates do not differ much by education, but spending by players drops as the player's educational attainment goes up. In summary, they find that males, blacks, high-school dropouts, and people in the lowest-come category are heavily over-represented among the top 20 percent of lottery players.

This is clearly a regressive mechanism to raise taxes. However, the regressivity may be restrained somewhat once we add the expenditure side of lottery finance into consideration. Since all lottery profits are collected into state coffers and are expected to be spent on public services, it is possible that those heavy players also benefit more from the existence of lotteries. This is more likely to be true when lottery profits are earmarked for specific purposes and where the earmarking policy has a discernible impact on state spending pattern.

2.5 Lottery Profits: a Special Revenue Source for State Government

The expansion of lotteries across states and the innovation of new games produced a rapid growth in lottery sales nationwide. In Figure 2-1, we graph total lottery ticket sales and profits. The growth in the size of state lotteries has been staggering. In FY1970, when only New Hampshire and New York ran lotteries, total ticket sales amounted to $201 million. In FY2000, the total sales reached $34.0 billion, about 0.4 percent of the Gross Domestic Products, adding $10.8 billion to state budgets.

State lotteries are one of the fastest growing segments of the legal gaming industry. As we can see in Table 2-3, lottery net revenues (gross revenue minus payout) were $3.6 billion (in terms of 1996 dollars) in 1982, constituting 20.8 percent of legal gaming net revenues. The net lottery revenues were about half the net revenues taken in by Nevada and Atlantic City casinos and a little bit shy of net revenues generated from horse racing. In 2000, lottery net revenues were slightly below $16 billion, accounting for 28.1 percent of industry net revenues. Net revenues from lotteries are greater than any other segment of the industry, such as Nevada and Atlantic City casinos, horse racing, Native American gambling and riverboat casinos.

For every dollar of lottery ticket sales, about 50 cents are paid back to players as prizes, 20 cents are spent on administration costs and retailers' commission, and the remaining 30 cents are returned to the state government as net proceeds. Though highly profitable, lottery profits only account for a small share of total state revenues. In FY1998, lottery profits constitute only 1.4 percent of total state revenues in states with lotteries, which are of the same scale as the state property tax; or the sum of state tobacco and alcohol taxes and liquor store profits. By contrast, state general sales tax and income tax each accounts for about one fifth of total state general revenue.

2.6 Lottery Profits Increase Spending on Public Goods

As a source of state revenue, the lottery is often compared to other tax instruments in term of economic efficiency. Some scholars question whether it is an entirely fair comparison. Clotfelter et al (1987) suggest that we compare the legalization and provision of lottery products to the situation where lotteries are prohibited. Because the legalization of lotteries enlarges the consumption bundle set of individual consumers and creates consumer surplus, lotteries are welfare-enhancing.[8]

Because the purchase of lottery tickets is voluntary, no deadweight loss is created when the state government is allowed to have access to lottery profits. Pigou's conjecture suggests that a move to less distortionary taxation would lead to greater expenditure on public goods. Hamilton (1986) proves in his model that grants from higher-level governments will result in increases in local spending above those which increased consumer incomes would produce if the federal taxes create less distortion than local taxes. Following the same logic, we would expect that the less distortionary lottery profits generate an increase in spending on public goods above the level of public spending without lotteries.

Morgan (2000) also predicts higher spending on public goods out of lottery profits, while he compares lottery profits to voluntary contribution schemes. State governments wishing to increase revenues face implicit or explicit limitations on the taxation schemes available to them, such as Proposition 13 in California and the Headlee Amendment in Michigan, two major initiatives that greatly reduced the dependence on property tax. The resurgence of state lotteries is a response to public resistance to any sort of tax increases. Under such situation, Morgan (2000) argues that lottery profits may not be a substitute for confiscatory tax instruments when these are politically feasible; rather, lotteries are often used in lieu of other voluntary contribution by private charities and governments when taxes are not feasible. Comparing lottery with other voluntary schemes, Morgan shows in his model that lottery is a practical way of overcoming the

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[8] The conclusion is based on assumptions that individuals have full information on lottery products and there is no externality associated with individual play of lotteries.

free-rider problem in the decentralized allocation of public goods. His findings suggest that relative to voluntary contributions, lotteries increase the provision of the public goods, improve welfare, and provide levels of the public good close to first-best as the lottery prize increases.

We should note that lottery profits account for less than 1.5 per cent of total state revenues. Even if the theoretical prediction of increased spending on public goods holds in realities, the level of spending increase due to influx of lottery profits will be small.

Chapter 3: Over- or Under-Advertising by State Lotteries

3.1 Overview

As was noted in Chapter 2, state lotteries were commonplace during the 1800s. But unlike current lotteries, private firms typically ran these lotteries. A series of scandals forced these lotteries to shut down and there were no state lotteries for about eighty years. The modern lottery was reintroduced in New Hampshire 1964, and since then, thirty-seven other states plus Washington D.C. have started state lottery games. Learning from the lessons from the notoriously privately-run lotteries in the last century, state governments formed special government agencies to be responsible for the operation of lotteries. In many states, the statutes establishing the lottery specify that the duty of the lottery agency is to maximize net revenue. To fulfill its obligation, all state lottery agencies engage in advertising. Advertising budgets are financed through lottery ticket sales, but the advertising budget must be approved annually by the legislature.

State legislatures often find themselves conflicted about the appropriate level of the advertising budget. Most state lottery charters stipulate that lottery agencies should maximize the net revenues to the state so it is no surprise that lotteries advertise. Some legislatures claim that advertising is the driving force in the growth of lottery sales, and suggest that advertising levels are too low given new competition from other forms of gambling such as riverboat and Native American casinos. They claim that any decision to cut advertising budgets is equivalent to killing the goose that lays golden eggs. In some states, however, legislatures are concerned that states are advertising too much. The arguments for this camp tend to take two different forms. Some states wonder whether lottery advertising has saturated the market and no longer expands ticket sales. Others worry about the moral implications of advertising by the state lottery – should states be encouraging their residents to gamble?

Determining the budget for lottery advertising is an important issue because it could impact the millions of dollars of revenues the state government takes in annually from lottery sales. In the thirty-eight states where lottery games are offered, lottery sales amount to about $38 billion a year. They contribute about $12 billion to state coffers annually, accounting for 1.4 percent of the total state revenue, more than the tax revenue collected from sales of tobacco and alcoholic beverage together.

There is no clear-cut evidence that state legislatures can rely on in determining the proper size of the lottery's advertising budget. Existing literature assumes that the marginal effectiveness of advertising decreases as the quantity of advertising increases. The advertising response curve is assumed to be either S-shape or concave (Little 1979, Rao et al 1975, and Vakratsas et al. 2004). Sales are said to enter into the saturation stage when the marginal effect of advertising becomes asymptotically zero. If the advertising expenditure of the state lottery lands on the rising segment of the advertising response curve, an extra dollar on advertisement could bring in more sale. However, if it enters into the saturation stage where the response curve is flat, further spending on advertising will not increase sales significantly. Existing literature suggests that saturation stages are different for different products (Rao et al 1975). It is therefore an empirical question to determine whether an additional advertising dollar will increase lottery sales.

There have been quite a few academic and industry studies examining the impact of advertising on sales. Most studies examine a firm's advertising spending in a competitive or oligopoly market, where the advertising expenditures of other firms could affect sales of the firm directly or indirectly through strategic interactions. Because federal regulation prohibits sales of lottery tickets across state borders, each state lottery agency is a monopoly within its territory. There is little competition among state lotteries for each other's market, and therefore advertising campaigns are not employed as a non-price competition strategy. Consequently, we can safely assume away any interaction among state lotteries that is of first order significance in the determination of advertising expenditures, and treat each state lottery as an independent observation.

Furthermore, since sales of lottery tickets across state borders are outlawed, the increase in sales generated from more advertising will come mainly through market expansion and not re-allocation of market shares. This is in contrast to much of the existing literature on advertising and market shares that has a difficult time determining whether the positive covariance between advertising and market share is attributable to the expansion of market or re-arrangement of existing customers.

The unique market structure of the lottery industry mentioned above greatly simplifies our econometric model. However, there are other features of state lotteries that complicate the analysis. In many states, advertising budgets for lotteries are a specified fraction of the previous year's sales. Therefore, a simple regression of lottery sales on advertising would be subject to an omitted variable bias. This potential endogenous relation between sales and advertising was first pointed out in Schmalensee (1972). To identify the impact of advertising on ticket sales, we utilize discontinuities in advertising expenses generated by state regulations as an exogenous source of variation in advertising. In particular, we use temporary or permanent legislated reductions in advertising in three states in just this manner. As we outline in detail below, the legislative decisions to reduce advertising were not related to any projected change in lottery sales. Therefore, we can treat the events as quasi-experiments and estimate their impact on lottery sales.

For example, the Washington State Lottery eliminated TV commercials during the summers of 1993 and 1994 in order to test the effectiveness of advertising on sales. In Massachusetts, State Democratic Senator Thomas F. Birmingham waged a fierce public battle with Republican Treasurer Joseph D. Malone over lottery advertising since early 1993. After a three year crusade, the lottery's advertising budget was cut from $11.6 million in FY1993 to $5.6 million in FY1994, to $2.8 million in FY1995 and had been capped at $400,000 a year since FY1996. Although Democrats claimed the cut "positive not personal," Republicans deemed it "a slap at Joe Malone." In January 1999, George Ryan was sworn in as the Governor of the state of Illinois. To finance his $12 billion "Illinois First" public works projects, TV advertising campaigns of the State Lottery were reduced in FY2000 and FY2001. Some press hinted that Lori Montana, the Lottery Director appointed by the former governor, was being dealt a bad hand.

We examine the three quasi-experiments separately in the paper. The basic estimation method employed is a difference-in-difference model. We first measure changes in advertising spending and sales before and after the legislative decision to cut the budget, and determine whether both advertising spending and sales are significantly lower during the treatment period. Comparing the impact that the negative shock has on advertising and sales could produce some indication of the change in sales due to a marginal dollar reduction in advertising spending as a result of the treatment. We can also treat the legislative decision as an instrument for advertising spending, and apply the two-stage least squares method to obtain direct measurements of advertising effects on sales.

The key in the difference-in-difference analysis is identifying what would have happened to advertising and sales absent the intervention. These values are estimated by including data on states with no such change in advertising policy as control states. The selection of control states is critical in the evaluation of the quasi-experiments. Given that we are studying cases where the advertising spending pattern was interrupted exogenously, we first use as controls those states that trend similarly as the treatment state both in sales and advertising before the interruption. Then, we exclude states that share borders with the treatment state to rule out possible neighborhood effects between the state and its neighbors.

We also select control states according to their game structures. For example, Mega Millions (the Big Game), a multi-state lotto game, is offered in the three treatment states. Illinois and Massachusetts joined this multi-state lotto game when it was started in September 1996. Washington joined later in September 2002. Seven other states also started offering the Mega Millions game during the sample period from 1992 to 2002. We thus include in the control group those states that offer the Mega Millions game before or at the same time as the treatment state. The state lottery of Washington and Massachusetts also offer keno games with ten other states. Similarly, we use as controls those states that offer keno games before or at the same time as the two treatment states. For Illinois, state lotteries without keno games are included as controls. For the purpose of completeness, we also use all lottery states except treatment states as controls.

We find significant reductions in both advertising expenditures and sales in the three quasi-experiments. In Illinois, monthly advertising spending per capita is reduced by 2.5 cents during the TV commercial restriction period, a reduction of 40 percent from the pre-restriction mean; and monthly sales per capita fall by 9 percent. The estimated semi-elasticity of sales with respect to advertising is 4.05. The calculated elasticity of sales with respect to advertising is 0.16, which suggests that a one percent decrease in monthly advertising spending results in 0.16 percent decrease in sales per capita. In the state of Washington, we find that monthly per capita spending on advertisement falls by 2.5 cents during the summer TV hiatus, a reduction of 70 percent from the non-treatment mean, and lottery sales drop about 16 percent. The estimated semi-elasticity with respect to sales is 6.41; the calculated elasticity of sales is 0.08. In the case of Massachusetts, we find that monthly advertising spending per capita falls by 6.8 cents after 1994, almost a complete elimination of advertising. Monthly sales of traditional online games fall by 22.8 percent after 1994. The calculated elasticity is 0.01, which implies that a one percent decrease in advertising spending results in 0.01 percent decrease in sales of traditional online games.

However, the estimates may not be accurate if we do not consider the persistent effect of advertising. Most of the literature recognizes that advertising expenditures may not have their full impact on sales in the period when they occur. The depreciation rate of advertising effects varies across products. After surveying more than seventy published results, Clark (1976) finds that the effect of advertising on sales of mature, frequently purchased, low-priced products has a high depreciation rate: 90 percent of the cumulative effect of advertising depreciates within three to nine months. The effect of lottery advertisement could be short-lived if the advertising is game-specific and informative, such as providing information on the current size of the jackpot or the prize structure of a new scratch game. Advertisement of this type becomes obsolete quickly when the jackpot rolls over or the scratch game phases out. On the other hand, the effect of advertisement could be long-lived if advertising changes the preferences of a random citizen and turns him or her into a more frequent player. Although some state laws suggest that the advertisement of lotteries should focus on provision of game information, we do not have enough data to separate the impact of informative advertisement from persuasive one. Thus, our estimated depreciation rate is the combined effect of both types of advertisement. Following the estimation method employed in Berndt et al (1995), we find evidence that 90 percent of the effect of lottery advertising depreciates after six months.

The dependent variable in our analysis is a panel of monthly lottery sales at the state level. A simple Durbin-Watson test suggests the existence of serial correlation within each state. Although the existence of serial correlation does not affect the consistency of our estimates, the standard errors calculated using ordinary least squares are not robust. To correct the serial correlation in the panel analysis, we follow the estimation method provided in Anderson & Hsiao (1982) and report the corresponding robust standard errors in regression results.

This chapter proceeds as follows. We first review some previous research studying the impact of advertising on sales, and then describe the dataset used in the analysis. In the fourth section, we present background information from local press and/or state legislative publications on the three quasi-experiments to support our claim that the advertising budgets were exogenously altered. In the fifth section, we describe the econometric models and explain in detail how we select control groups for each treatment state. In this section, we also describe how we estimate the depreciation rate of advertising and how we correct for serial correlation of monthly sales within each state. Estimated results are presented separately for each treatment state in the sixth section. We draw conclusions in the final section.

3.2 Literature Review

Literature studying the relation between advertising and sales is abundant. However, there is no published study examining the impact of lottery advertising on ticket sales. A comprehensive review of the whole advertising literature is beyond the purpose of this paper. Instead, given the quasi-experiments we examine in this paper, we focus on studies that examine the impact of government regulation of advertising on sales.

A fair amount of literature examines whether advertising restrictions for tobacco and alcohol have reduced aggregate consumption of these products. A number of researchers pooled cross-national time series data from the OECD countries over the time period of 1960 to 1990 to estimate the impact of various advertising bans and restrictions. The results from these analyses are mixed. Some (Laugesen et al 1991, Saffer 1991, Saffer et al 2000, 2002) find advertising bans reduce cigarette or alcohol consumption by a statistically significant amount, while Stewart (1993) and Nelson (2003) find that advertising bans have no effect on cigarette consumption.

In early studies, most authors treated the enactment of advertising bans as an exogenous shock. However, Young (1993) suggests the possibility that the enactment of an advertising ban could be endogenous. Likewise, Saffer et al (2002) finds evidence that alcohol consumption has a positive effect on total advertising bans, while Nelson (2003) finds that a decline in smoking prevalence among males leads to stronger restrictions on cigarette advertising. Taking into consideration the endogenous enactment of bans, Saffer et al (2002) still finds a statistically significant negative effect of advertising bans on alcohol consumption. Nelson (2003) finds that advertising bans have no effect on cigarette consumption.

In Washington State Lottery Report 95-6, the Legislative Budget Committee of Washington State conducted a study of the Washington State Lottery and found that the TV advertising hiatus during the summer of 1993-94 resulted in a potential loss in sales of $4.5 million. The report also found that the presence of TV advertising positively influences sales.

In this paper, our identification comes from discontinuities in advertising expenditures due to government regulations. But such regulations are not as prevalent across states as the cigarette and alcohol advertising bans in OECD countries that are discussed above. We therefore study the quasi-experiments on a case-by-case basis. We claim that the discontinuities can be treated as exogenous variation and provide detailed information in section 3.4 to support the claim. In the literature on advertising bans, the strictness of bans is measured by the number of media banning advertising. However, it is questionable whether bans on the same media are comparable across different countries. The literature gave little attention to whether bans lower advertising spending. If the authors could obtain data on advertising spending of sample countries and provide some direct evidence on the comparability of bans across different countries and on the efficacy of bans in reducing advertising expenditures, the estimated results would be more convincing. In this paper, we provide statistical evidence and regression results on changes of advertising spending due to government regulations in the treatment states.

We are able to estimate the sales elasticity with respect to advertising using two-stage least squares (2SLS). However, the estimates will not be accurate without considering the persistent effect of advertising. Most of the literature recognizes that advertising expenditures may not have their full impact on sales in the period when they are incurred. Nerlove and Arrow (1962) assume that advertising builds a stock of firm-specific goodwill that affects both current and future consumption. To estimate the persistence of advertising impacts, assumptions about the depreciation process are required. The typical assumption is that a fixed fraction of the advertising effects in this period is retained in the next period.

One commonly applied method to estimate depreciation rates is to replace the cumulative effects of all the previous advertising with the lagged dependent variable. The coefficient on the lagged dependent variable is the estimate of the persistent effect of previous advertising. This method is called the Koyck transformation. Detailed descriptions of Koyck model and the derivation of estimable equations from Nerlove-Arrow model can be found in Clark (1976), Bass et al (1983), Blattberg et al (1981), and Rao (1986). Although the Koyck model is widely applied in the estimation of advertising depreciation, some critics argue that the Koyck model can also represent habit-persistent behavior not necessary related to advertising (Clark 1976).

To pin down the depreciation of advertising effects, Berndt et al (1995) estimate the depreciation rate using the non-linear two-stage least squares (NL-2SLS) method, where they include lagged advertising expenditures in the regression and restrict the coefficients on the lagged advertising variables so that effects of lagged advertising depreciate exponentially over time. In this paper, we follow the method used in Berndt et al (1995) to estimate the depreciation of the effects of lottery advertising.

3.3 Data

The data set used in this paper is compiled from different sources. Monthly lottery sales data are aggregated from weekly sales data obtained from some state lottery agencies[9] and La Fleur's, a private company that provides lottery data for the United States and a number of European countries. The data contain monthly sales of the thirty-seven state lotteries from 1992 to 2002. For most of the states, we have a full panel of observations over the sample period.[10]

The advertising data were more difficult to obtain. For a number of states, we requested advertising budget records from state lotteries directly. Though some agencies keep records of annual advertising budget, records of the monthly spending on advertising are unavailable. Therefore, we obtained the monthly advertising spending data of state lotteries from Competitive Media Reporting (CMR). CMR is a private company that monitors advertisements that appear in fifteen different media nationwide and provides occurrence and expenditure data on advertisement of certain brands or products. The lottery advertising expenditure data provided by CMR cover media such as

[9] We want to express our special thanks to Lisa McDonald, from the Media Relations of Massachusetts State Lottery, for her kind help to provide us the lottery sales data of Massachusetts.

[10] For states such as Georgia, Nebraska, New Mexico, and Texas where lottery did not start until after 1992, the panel is considered complete in the sense that monthly sales are reported after the lottery initiated. Sales data are missing for Colorado and Indiana in 1992, Minnesota in 1993, Louisiana in 1994 and Rhode Island from 1992 to 1994.

TV, radio, magazine, newspaper and outdoor service. On average, spot TV advertising accounts for about two thirds of the total lottery advertising expenditure; outdoor service, spot radio and local newspaper consume the rest. Some of the state lotteries also run commercials in national newspapers and nationally syndicated television shows, but these are infrequent outlets for state lottery advertising dollars.

Using the CMR data, we are able to construct a panel of monthly advertising spending by state lotteries and break down the spending by various media. The calculation of the CMR advertising expenditure data is based on the occurrence of advertisement in CMR-tracked media and on the advertising rate per unit estimated by CMR. Thus the data do not add up to the state advertising budget figure. Comparing the annual advertising budget obtained from some of the state lotteries, we find that the aggregate expenditure figure calculated using monthly data from CMR is generally smaller. The result is not surprising considering that some of the state budget will be spent on consultants, running the state advertising office, etc. However, we are reluctant to gauge the extent of underestimation using data from CMR because the annual advertising budget records from state lottery agencies are not comparable across states.[11]

The introduction of new games is an important factor that affects lottery sales. During the 1980s, all state lotteries offered at some point three types of games: instant game, daily numbers and lotto games. The 1990s saw a blossoming of new games. For example, instant tickets are offered in face value of \$2, \$3, \$5, \$7 and \$10 in addition to the traditional \$1 ticket. Some states adopted keno. Almost all state lotteries joined at

[11] Some state lotteries record the allocated budget ex ante, some record the advertising expense ex post. It is also not clear from the data source whether promotion expenditure is included in the advertising budget.

least one of the multi-state lotto games to take advantage of the larger jackpot sizes.[12] To account for the different game structures of state lotteries, we construct a set of dummy variables representing the introduction of each new game during the sample period. The variables include three dummy variables that equal 1 for the first, second, and third month separately after the introduction of the game, and a fourth dummy variable that equals 1 for the rest of months after the introduction of the game. The step structure of the dummy variable is designed to capture some of the non-linear effects on sales of new games. Information on game structures are obtained from various publications and web sites of state lotteries.

Another factor that also affects ticket sales is the jackpot size in games such as lotto. Cook et al. (1993) argues that lotto sales increase with the scales of operations presumably because that sales are sensitive to the size of the jackpot and that larger jackpots come with a larger population base. The authors estimated the elasticity of lotto sales in Massachusetts with respect to the jackpot to be 0.35. Using lotto sales data from New York State, DeBoer (1991) finds evidence that the relation is not linear: lotto sales accelerate as jackpots grow. To account for such a non-linear relation, we collect data on jackpot sizes of the two largest multi-state lotto games: Powerball and Mega Millions (the Big Game),[13] and include polynomial terms of the jackpot size in regression models.

The sample contains 4682 monthly observations of thirty-seven lottery states from 1992-2002. Variables in dollars are deflated using CPI-Urban index from the Bureau of

[12] The only two states not offering multi-state lotto games are California and Florida.

[13] Special thanks to Dr. Melissa Kearney from the Department of Economics of Wellesley College, who kindly shared her data on jackpot sizes of lotto game. Special thanks to Ms. Dot Colvin from Maryland State Lottery, who provided data on recent jackpot sizes of Mega Millions from 2000 to 2002. Special thanks to Mr. William Sawchuck, from DC Lottery, who provided data on recent jackpot sizes of Powerball from 2000 to 2002.

Labor Statistics. The base month is January 1996. All variables are in per capita term. In Table 3-1, we summarize the sample characteristics. Monthly sales per capita average $10.45, while advertising spending per capita is about 4 cents per month. Large standard deviations of monthly sales and advertising spending suggest heterogeneous performance of lottery agencies across states.

State unemployment rates in the sample panel are monthly observations collected from the Bureau of Labor Statistics. State personal income per capita is the monthly average calculated using quarterly state personal income (SPI) from the Bureau of Economic Analysis (BEA). The rest of the state demographic features included in the sample panel are annual observations of state population, state population by age, gender, race, Hispanic origin, education level, marital status, and SMSA status. All demographic data are collected from the Census Bureau.

3.4 Background Information on the Three Quasi-experiments

Because lotteries are operated by state governments, their advertising practices are not regulated by the Federal Trade Commission. However, all the lottery states adopt formal laws or establish standards or guidelines as to what is appropriate for lottery advertising. Some states even explicitly cap the advertising budget at a certain percentage of gross sales in the legislation. The average advertising/sales ratio of lotteries across states is kept at a fairly low level compared to other industries. According to National American Association of State and Provincial Lotteries (NASPL), "in 1996 North American lotteries spent $400 million on advertising and received $34 billion in sales. Advertising expenditures accounted for 1.17 percent of total sales. By contrast, restaurant owners spent 3.2 percent of their sales on advertising, beverage manufacturers 7.5 percent, cosmetics companies 8.8 percent, and candy makers 12.7 percent. Advertising accounts for less of the cost of a lottery ticket than virtually any other consumer product."[14] Casinos on Las Vegas Boulevard spend 2.4 to 4 percent of the total hotel-casinos' sales on advertising (Zheng 1999).

Although state lotteries are self-funded and self-governing, their advertising budgets must be approved by state legislatures. In many states, there is constant pressure to suppress the lottery advertising. Some legislatures find it distasteful that public funds are used to promote gambling while others question the efficacy of advertising in increasing sales. In some states, the lottery advertising budget has been explicitly reduced by the state legislature.

During the 1994 legislative session, the legislature of Washington State directed the Legislative Budget Committee to conduct a study of the Washington State Lottery. To determine the effectiveness of lottery advertising, the State Lottery of Washington eliminated TV advertising for three to four months during the summer of 1993 and 1994. In 1993 the hiatus was 13 weeks and in 1994 the hiatus was 17 weeks.[15] Although we label the data from the three states we examine as quasi-experiments, the events in Washington were in fact an experiment!

In the state of Massachusetts, the debate over the size of the lottery advertising budget became more confrontational than in any other state. Beginning in 1993, State Senator Thomas F. Birmingham (D-Chelsea), Ways and Means chairman, waged a fierce

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[14] From http://www.naspl.org/faq.html#lotadver.
[15] From *Washington State Lottery Report 95-6*, The State of Washington Legislative Budget Committee, 02/15/1995.

public battle with Republican Treasurer Joseph D. Malone over lottery advertising.[16] Senator Birmingham successfully fulfilled a three-year crusade and persuaded the legislature to cut the Lottery's advertising budget from $11.6 million in FY1993 to $5.6 million in FY1994, to $2.8 million in FY 1995 and cap the lottery advertising at $400,000 a year since FY1996 and restrict the use of the money to in-store displays only.[17] Birmingham argued that he was motivated only by a conviction that the lottery was overspending on "tawdry enticement" to poor, gullible people.[9] Although the Senate President William Bulger said the cuts were "positive not personal,"[18] Senate Republican leader Brian O. Lees of East Longmeadow dismissed these arguments noting that the democratic senate approved lottery advertising budgets of up to $14 million annually when Democrat Robert Q. Crane was the treasurer in the 1980s. Lees said, "This is a slap at Joe Malone, pure and simple." Birmingham acknowledged Malone "is not the most popular person among legislators," but said that should not protect him from legitimate budget cutbacks.[19] As a result, the advertising budget of the Massachusetts State Lottery had been capped at $400,000 since FY1996, was down to a little more than $100,000 a year in FY2003.[20] The advertising budget is back to $5 million after Senator Birmingham's departure at the end of 2002.

The third legislated reduction in advertising that we consider happened in the State of Illinois. In January 1999, George Ryan was sworn in as the Governor of the state of Illinois. To finance his $12 billion "Illinois First" public works projects, the new

[16] "Senate proposal guts lottery ad budget," by Peter J. Howe, *the Boston Globe*, 06/08/1994.
[17] "Lottery ad budget hits a new low: Funding cutback drops Mass. to lowest in the US," by Peter J. Howe, *the Boston Globe*, 06/14/1995.
[18] "Malone blasts plans to curtail Lottery ads," by Peter J. Howe, *the Boston Globe*, 06/22/1993.
[19] "Lottery cut survives Senate budget debate, Day 1," by Peter J. Howe, *the Boston Globe*, 06/14/1994.
[20] "As Lottery marks holiday with ads, some critics object," by Raphael Lewis, *the Boston Globe*, 12/17/2003.

governor proposed tax hikes on liquor and vehicle fee increases, as well as an expansion of the state's ability to borrow money.[21] Amid the increases in fees and "sin tax" hike, the advertising budget of the State Lottery also felt the pressure to become leaner. On May 13, 1999, Dave Urbanek, the gubernatorial spokesman, said that although there was disagreement on whether all TV advertising would be cut, officials have decided that the current level of advertising (referring to television) is too much. On the same day, lottery spokesman Mike Lang disputed claims that all TV advertising would be curtailed after the new budget goes into effect on July. But he admitted that lottery is cutting about $5.5 million from its overall $23 million marketing budget. At the suggestion of gubernatorial officials, the State Lottery of Illinois did a major overhaul of the marketing scheme.[22] Some press hinted that Lori Montana, the Lottery Director appointed by the former governor, was being dealt a bad hand since the legislature cut the Lottery's advertising budget and decreased their appropriations.[23]

The most recent example of legislative cut of lottery advertising is found in Missouri[24] where the advertising budget has been cut consecutively for two years since 2003. The Missouri case is left out here because our sample only covers the period from 1992 to 2002.

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[21] "Governor signs tax increases, bonding authority for Illinois FIRST," *AM Cycle*, the Associated Press State & Local Wire, 06/15/1999.

[22] "Lottery ads will be pared," by Kurt Erickson, *the Pantagraph*, 05/13/1999.

[23] *Chicago Sun-Times*, 08/03/1999.

[24] "In the FY2002, the lottery received $8.25 million for advertising. This year that was cut to $6.25 million. Lawmakers, noting that sales have remained fairly steady, decided that the lottery could make do with even less advertising money for next year. The proposed budget for fiscal 2004 would reduce lottery advertising to $3.05 million. House members initially had sought an even larger cut but were persuaded by Senate negotiators to accept the roughly 50 percent reduction." "Luck runs out on lottery ads," by David A. Lieb, *BC Cycle*, the Associated Press State & Local Wire, 05/06, 2003.

3.5 Econometric Models

3.5.1 Difference-in-Difference Estimation

To measure the impact of advertising reduction on lottery sales, we would like to compare sales of the treatment state lottery in the period when the advertising budget was reduced to sales that the state lottery would have produced during that period had the budget not been curtailed. A simple comparison of sales before and after the treatment could produce a biased estimate of the treatment effect if we are unable to control for factors that occur at the same time as the treatment. To estimate the counterfactual growth of sales that would have occurred in the treatment state in the absence of the interventions, we include other non-treatment lottery states in the comparison group. We hope that by carefully selecting comparable control states, we are able to project the counterfactual change in sales of the treatment state using the estimated change in sales of the control states before and after the treatment. This is a standard difference-in-difference model that compares the change in sales of the treatment state before and after the treatment to the estimated change in sales of control states. To determine the impact of the advertising restriction on sales, we estimate the following equation:

$$S_{iym} = \theta * D_{iym} + \beta_1 X_{iym} + \beta_2 Z_{iy} + u_i + v_y + w_m + \varepsilon_{iym} \qquad (3\text{-}1)$$

The dependent variable S_{iym} is the natural log of monthly sales per capita of state i, in year y and month m. D_{iym} is the treatment dummy variable of interest, which equals 1 for the treatment state during the treatment period, and 0 otherwise. θ is the treatment effect, which measures percentage change in sales of the treatment state during the treatment period. u_i, v_y, and w_m represent state, year and month fixed effects, ε_{iym} is the error term.

There is substantial variation in monthly sales per capita both across and within state lotteries. The average monthly sales of all state lotteries are \$10 per capita. While the State Lottery of Massachusetts ranks at the top with monthly sales of \$41, the State Lottery of Montana ranks in the bottom with monthly sales of \$3. To account for the unobserved time-invariant heterogeneity in lottery sales across states, we include state fixed effects in the regression. Monthly fixed effects are included to filter out the seasonality of sales, and year fixed effects to control for shocks to sales that are common to all state lotteries but vary across years, such as national recessions.

X_{iym} is a vector of covariates capturing state economic features and lottery characteristics that could affect sales. The state economic characteristics included in X_{iym} are the natural log of monthly state personal income per capita[25] and the monthly state unemployment rate. To capture the impact of introduction of new games on sales, we include in X_{iym} a set of dummy variables that represents whether a state operates a game in a particular month. Some of the new games introduced during the sample period are keno,[26] Tri-state Win Cash,[27] the Big Game,[28] Powerball,[29] Daily Million,[30] Cash 4

[25] Monthly state personal income per capita is calculated from quarterly SPI from BEA.

[26] California, Colorado, Georgia, Kansas, Massachusetts, Maryland, New York, Washington, West Virginia offered keno games at different point of time in the sample period, Michigan, Oregon, and Rhode Island offered keno before 1992.

[27] Tri-State Win Cash started in Maine, New Hampshire, and Vermont in April 1992.

[28] Georgia, Illinois, Massachusetts, Maryland, Michigan, New Jersey, New York, Ohio, Virginia, and Washington joined the Big Game/Mega Millions consortium during the sample period at different point of time.

Life,[31] Rolldown,[32] Hot Lotto,[33] Tri-West Lotto,[34] Lotto South,[35] 2By2,[36] and higher-stakes instant games with ticket costs of $2, $3, $5, $7 and $10.[37] We also include in X_{iym} the jackpot size of the two most popular multi-state lotto games – Powerball and Mega Millions (the former Big Game). To capture the nonlinear relation between sales and the size of jackpot, we include polynomial terms of the jackpot size of order three.

We include in Z_{iy} state demographic variables observed annually such as age, gender, and racial composition of the state population, share of state population of Hispanic origins, education attainment of the state population, share of state population married, and share of state population lived in the SMSA areas.

It is important to demonstrate the effectiveness of legislative decisions in reducing lottery's spending on advertising. We thus apply the same difference-in-difference estimation to monthly advertising expenditures. The estimated equation is the following:

$$A_{iym} = \phi^* D_{iym} + \alpha_1 X_{iym} + \alpha_2 Z_{iy} + u_i + v_y + w_m + \mu_{iym} \qquad (3\text{-}2)$$

A_{iym} represents the per capita monthly spending on advertising of state i, in year y and month m. The variable is not in log terms because of zero advertising for some state

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[29] Arizona, Colorado, Connecticut, Georgia, Louisiana, Nebraska, New Hampshire, New Mexico, Pennsylvania, and Wisconsin joined the Powerball consortium during the sample period at different point of time.

[30] The game was offered in Indiana, Iowa, Louisiana, Minnesota, Missouri, Montana, Nebraska, New Hampshire, New Mexico, Rhode Island, South Dakota, West Virginia, and Wisconsin in 1996-98.

[31] The game was offered in Deleware, Indiana, Iowa, Minnesota, Montana, Nebraska, New Hampshire, Rhode Island, South Dakota, West Virginia, and Wisconsin in 1998-2000.

[32] Rolldown was offered in Deleware, Idaho, Iowa, Louisiana, Minnesota, Montana, Nebraska, New Hampshire, South Dakota, and West Virginia in 2000-02.

[33] Hot Lotto was offered in Iowa, Minnesota, Montana, New Hampshire, South Dakota, and West Virginia in 2002.

[34] Tri-West Lotto started in Idaho, Montana, and South Dakota in February 1994.

[35] Lotto South started in Georgia, Kentucky, and Virginia in September 2001.

[36] 2by2 started in Kansas and Nebraska in June 2002.

[37] A timetable of state's adoption of higher-stakes instant games is available at the web site of La Fleur's.

lotteries in some months. All the covariates in equation (3-1) are included in the regression of advertising spending. μ_{iym} is the error term. The coefficient ϕ estimates the reduction in monthly advertising spending of the treatment state during the treatment period.

3.5.2 Two-Stage Least Squares Estimation

What we are interested in measuring is the effect of advertising spending on sales. Using the panel data, we could estimate equation (3-3) directly.

$$S_{iym} = \lambda * A_{iym} + \gamma_1 X_{iym} + \gamma_2 Z_{iy} + u_i + v_y + w_m + \omega_{iym} \qquad (3\text{-}3)$$

However, the estimated coefficient λ would overstate the effect of advertising since advertising spending is a function of sales. The consistent estimate of λ can be obtained using the two-stage least squares method where we employ the quasi-experiments as exogenous shocks to advertising spending. In the 2SLS estimation where A_{iym} is instrumented with D_{iym}, predicted advertising spending enters into the estimation of equation (3-3).

The coefficient λ estimated using 2SLS can be thought of as a local average treatment effect that measures the change of sales with respect to advertising for the treatment state during the treatment period. Another way to think about equation (3-2) is that the estimated effect ϕ is the "first-stage" relation between the negative shock of the legislative decision and advertising spending. If advertising has any impact on sales, such a negative shock to lottery advertising should eventually lead to changes in lottery sales. The estimated effect θ in equation (3-1) can be thought as the "reduced-form" relation between the negative shock and lottery sales. Comparing the impact that the negative shock has on advertising and sales should produce some indication of the percentage change in sales due to a marginal dollar reduction in advertising spending as a result of the treatment.

3.5.3 Estimation of Advertising Depreciation

Although we have identified three quasi-experiments that can be used to identify an equation such as (3-3), the estimated semi-elasticity in equation (3-3) may not be accurate without considering the persistent effect of advertising. Most of the literature recognizes that advertising expenditures may not have their full impact on sales in the period when they are incurred. We should include previous advertising spending in the regression in order to estimate the persistent effect. There is no rule of thumb to determine how many periods of lagged advertising spending should be included. Some studies assume the number is infinite and apply a Koyck transformation to substitute the effects of all lagged advertising with the lagged dependent variable. The coefficient on the lagged dependent variable is thus an estimate of the carry-over effect of advertising. Although the Koyck transformation simplifies the estimation, the coefficient on the lagged dependent variable can also be an estimate of habit persistence not related to advertising at all.

Other studies assume that carry-over effects of advertising are statistically detectible within a certain period and include in the regression all the lagged advertisings during that period. Clark (1976) finds that 90 percent of the cumulative effect of advertising on sales of mature, frequently purchased, low-priced products depreciates within three to nine months. We assume that the cumulative effect of lottery advertising depreciates completely within a year and include the monthly advertising spending of the previous year in the regression. Following the estimation method employed in Berndt et al (1995), we assume that a fixed fraction of the effect of current advertising retains in the next period, and estimate the retention rate using non-linear two-stage least squares method. The estimated equation is the following:

$$S_{iym} = \lambda_0 * \sum_{j=0}^{11} [\delta^j * Lag_j (A_{iym})] + \gamma_1 X_{iym} + \gamma_2 Z_{iy} + u_i + v_y + w_m + \varepsilon_{iym} \qquad (3\text{-}4)$$

$Lag_j (A_{iym})$ is the j-month lag of current advertising spending per capita, the maximum number of lagged advertising included in the regression is 11. The coefficient δ measures the retention rate of the advertising effect. The coefficient λ_0 is the estimated short-term semi-elasticity of sales with respect to advertising.

Instruments for $Lag_j (A_{iym})$ are the treatment dummy variable D_{iym}, and the interaction of the treatment dummy variable D_{iym} with $Lag_{(j+12)} (A_{iym})$, the twelve-month lag of each of $Lag_j (A_{iym})$ in the regression. Given the conventional wisdom that advertising effects depreciate completely within a year, the twelve-month lag of current advertising spending does not have any direct effect on current sales, which qualifies it as a potential instrument for the current advertising spending in the regression equation (3-4). Similarly, the twelve-month lag of $Lag_j (A_{iym})$ where j =1, … , 11 can also be used as instrument for $Lag_j (A_{iym})$ in the regression. Because we are interested in measuring the retention rate of advertising (δ) of the treatment state as a result of the treatment, we use the interaction of the treatment dummy variable with the twelve-month lags as instruments.

3.5.4 Correction for Serial Correlation in the Error Terms

Analyzing a panel of high-frequency data such as the monthly lottery sales at state level, we are concerned with possible serial correlations of residuals within state. Although the existence of serial correlation does not affect the consistency of our estimates, the standard errors calculated using the ordinary least squares method are not robust. To obtain robust estimates, we follow the econometric method suggested by Anderson & Hsiao (1982) in the estimation of models with serial correlation and report robust standard errors in the corresponding estimated results.

The method is a multi-step procedure.

- Step 1, we estimate equation (3-3) assuming the error terms are i.i.d., and obtain estimates of residuals.
- Step 2, we estimate the serial correlation coefficients by regression current residuals on its lagged terms.
- Step 3, we apply the estimated serial correlation coefficients to the dependent variable and all the covariates, and obtain a panel of "serial-correlation-corrected" variables.
- Step 4, using the "serial-correlation-corrected" panel data, we re-estimate equation (3-3) and report estimates of both coefficients and robust standard errors.

The estimates are asymptotically efficient when the number of time series observations tends to infinity. Proofs on properties of the estimates are available in Anderson & Hsiao (1982). Although we include state fixed effects in the regression to represent time-invariant difference in per capita lottery sales, we are concerned about the heteroscedasticity across states, and allow the serial correlation coefficients to vary across states.[38]

3.5.5 Selection of Control States

The comparison states for each of the three treatment states are selected separately according to various standards. Because the advertising spending pattern of the treatment state was interrupted exogenously during the treatment period, we first select as controls those states that trend similarly as the treatment state in sales and advertising before the interruption. Specifically, we pair the treatment state with any other state with lotteries except Massachusetts, Illinois and Washington, and keep the time series observations before the legislated reduction in advertising. Then, we estimate the following equations:

$$S_{iym} = \rho^*T + \tau^* (O_{iym}^*T) + \beta_1 X_{iym} + \beta_2 Z_{iy} + u_i + v_y + w_m + \varepsilon_{iym} \qquad (3\text{-}5)$$

$$A_{iym} = \sigma^*T + \upsilon^* (O_{iym}^*T) + \alpha_1 X_{iym} + \alpha_2 Z_{iy} + u_i + v_y + w_m + \mu_{iym} \qquad (3\text{-}6)$$

[38] We also estimate standard errors using cluster method. Saffer et al (2000) and Saffer et al (2002) compute standard errors with Huber or White estimator using country as the cluster variable to correct the serial correlation. However, the estimated standard error is asymptotically efficient only when N (the cross sectional units) tends to infinity. Angrist & Lavy (2002) and Wooldridge (2003) provide evidence that this procedure does not work well when panels numbers in the 40s or 50s. Given that our data contain limited number of states, such assumption is not necessary satisfied. We thus do not report the corresponding estimates.

where T is a monthly trend variable, O_{iym} is a dummy variable that equals 0 for the treatment state and 1 for the other lottery state. Covariates in X_{iym} and Z_{iy} are the same as in equation (3-1) and (3-2). The estimated coefficient ρ measures the common time coefficient of sales in the two states, while the estimated coefficient τ detects whether sales of the other state trend differently. Similarly, υ tells us whether advertising spending trends differently in the two states. If both τ and υ are not statistically different from zero, we include the candidate state in the control group.

Although each state lottery is a monopoly within its territory, some may be concerned that the state lottery may respond to the advertisement of its neighboring state lottery in order to attract players across the border and/or keep players within the border. The volume of cross-border sales could be large for areas such as DC Metro area since Maryland, D.C., and Virginia all run different lotteries and have a different collection of games. However, we do not believe that the volume of cross-border purchases is large enough in our three treatment states (Illinois, Massachusetts, and Washington) that it could trigger advertising competition among neighboring lotteries. To be on the safe side, however, we exclude from the control group those states that share borders with the treatment state.

As a second method to select control states, we pick as comparisons those states that have the same mix of games. All state lotteries offer three types of games: instant scratch game, daily numbers, and lotto. The first major difference across states is which multi-state lotto consortium it belongs to. Among a dozen of the consortia, Powerball and the Mega Millions game (the former Big Game) enroll the largest number of member states and offer huge jackpots usually not matched by than any other consortium in

America. Our three treatment states belong to the Mega Millions consortium. The state lotteries of Illinois and Massachusetts joined the Big Game in September 1996. The Washington State Lottery joined later in September 2002. Control states are therefore selected among the members of Mega Millions consortium: four states that joined the consortium in September 1996 are included as controls for Illinois and Massachusetts,[39] three states that joined the consortium later around 2002 are included as controls for Washington.[40]

The second major difference in game mix across states is whether the state lottery offers keno games. Massachusetts and Washington are among the twelve states that offer keno games, while Illinois belongs to the rest of state lotteries without keno games. The State Lottery of Washington began to offer keno on November 1992. Massachusetts started at the beginning of 1994. States that offered keno games before or at the same time as either of them are included in the corresponding control group.[41] State lotteries without keno games are included as controls for Illinois State Lottery. For the purpose of completeness, we also include in the control group all the other state lotteries except Illinois, Washington, and Massachusetts.

3.6 Estimated Results

We first discuss regression results from the two quasi-experiments in Illinois and Washington. The case of Massachusetts is analyzed later since it is more complicated.

[39] The other four Lotteries are Georgia, Maryland, Michigan, and Virginia.
[40] The three states are New Jersey, New York, and Ohio.
[41] The controls for Massachusetts are California, Kansas, Maryland, Michigan, Oregon, and West Virginia. The controls for Washington are California, Kansas, Michigan, and Oregon.

3.6.1 Estimated Impacts of TV Commercials Restriction in Illinois

In Figure 3-1, we plot the monthly advertising expenditure per capita for the Illinois State Lottery. As we can see, per capita spending on TV commercials as well as the total per capita expenditure on advertisement were greatly reduced during the period of July 1999 to June 2002. Monthly spending on TV advertising shrank 78 percent (from 5 cents per capita to 1 cent per capita). Although the State Lottery compensated for the reduction in TV advertising by more than doubling expenditures on other media, total monthly advertising spending dropped 32 percent (from 6 cents per capita to 4 cents). Monthly sales per capita are plotted in Figure 3-2. During the period of less TV advertisement, monthly sales decreased by 20 percent (from $11 per capita to $9).

We should note that total monthly spending on advertising did rise sharply in May 2000 and May 2002. The first hike was the result of the largest lottery jackpot ever offered in North American history ─ $363 million from the “Big Game”. The second occurred when the “Big Game” was renamed “Mega Millions” in May 2002. In the regression models, we control for game structures of different lotteries as well as jackpot sizes of two major multi-state lotto games ─ Powerball and Mega Millions.

In Table 3-2, we compare average monthly sales and advertising spending before and after the TV commercial restriction. We find that average monthly spending on TV advertising and total advertising are significantly lower after the restriction. Average monthly sales also fall short compared to the average before the restriction. All the statistics are significant with 95 percent confidence.

We notice that in Figure 3-2, sales seem to trend downward during the whole sample period. We thus apply the interrupted time series analysis to the time series data of Illinois State Lottery to determine if sales and advertising expenditure trend further downward after the TV commercial restriction. The regression equations are as follows:

$$S_{ym} = c + \varphi * T + \pi * (D_{ym} * T) + \varepsilon_{ym} \qquad (3\text{-}7)$$

$$A_{ym} = c + \eta * T + \iota * (D_{ym} * T) + \mu_{ym} \qquad (3\text{-}8)$$

S_{ym} is per capita sales of Illinois State Lottery in natural log in year y and month m. A_{ym} is per capita advertising spending of Illinois State Lottery in year y and month m. T is the monthly time trend. D_{ym} is the dummy variable that equals 1 during the TV commercial restriction period and 0 otherwise.

The estimated time coefficients are reported in the upper panel of Table 3-3. Though advertising spending trends significantly downward during the TV commercial restriction period, the negative trend in sales during the treatment is not statistically significant. In the lower panel of the table, we report the Durbin-Watson (D-W) test statistics on serial correlation among residual terms. The statistics suggest the existence of serial correlation in the error terms. Though we cannot determine the order of serial correlation solely based on the D-W statistics, it seems that monthly advertising spending follows an AR (1) process, while monthly sales per capita follows an autocorrelation of higher order such as AR (3).

We apply the D-W tests to the time series data of sales of other state lotteries allowing the maximum order of autocorrelation to be 13. In some states, the first significant D-W test statistics occurs at order 1, while in most states, the first significant D-W test statistics occurs at order 2 or 3. It is counter-intuitive that we encounter the irregular autocorrelation pattern of monthly sales. If we assume the purchase of frequent players follows an AR (1) process, there should exist some mechanism that alters the buying pattern. Such mechanism could come from promotion tactics employed by state lotteries. To promote sales, some lotteries offer purchase of advanced plays. For example, Arizona state lottery allows players to purchase tickets for online games such as "Fantasy 5" in advance for twelve consecutive drawings (which last for more than a month). Illinois state lottery allows its players to subscribe to online lotto games. The subscription offers players to pick their numbers for 26/52/104 consecutive drawings (which lasts for a quarter/half a year/a year). Massachusetts state lottery offers season tickets at a discounted price such as $25 for 26 consecutive drawings, $50 for 52 drawings, and $100 for $104 drawings. Such promotion techniques reduce the transaction costs for frequent players, which could alter their buying pattern correspondingly. We therefore observe the abnormal pattern of autocorrelation in lottery sales in those states. The pattern of serial correlation across states lines up fairly well with the advance purchase scheme offered by state lotteries. Later in the estimation of robust standard errors, we assume the residuals follow AR (3) to accommodate for all possible serial correlation patterns.

Using as controls those states with similar pre-treatment trends in advertising spending and sales as the Illinois State Lottery, we apply the difference-in-difference method to estimate the treatment effects of TV commercial restriction. Estimates for this model are reported in the upper panel of Table 3-4. The left column records estimates using OLS method. The “first-stage” estimate of the treatment effect is obtained from the regression of equation (3-2), the “reduce-form” estimate is obtained from the regression of equation (3-1), and the 2SLS estimate from equation (3-3).

Our results suggest that monthly advertising spending per capita are reduced by 2.3 cents during the restriction period, a reduction of 40 percent from the pre-restriction mean, and monthly sales per capita fall by 10 percent. The estimated semi-elasticity of sales with respect to advertising is 4.24, which suggests that a one-cent decrease in monthly advertising spending per capita results in 4.24 percent decrease in sales per capita. All the estimated effects are statistically significant at a p-value of 0.05.

In order to compare our estimated results with results from other literature, we calculate in Table 3-5 the elasticity of sales with respect to advertising by multiplying the estimated semi-elasticity with the average monthly advertising expenditure per capita during the treatment period. The elasticity calculated is 0.16, which suggests that a ten percent decrease in advertising spending results in 1.6 percent decrease in sales. We also calculate the gross rate of return to advertising in the last raw of Table 3-5. A one-cent decrease in monthly advertisement spending per capita decreases sales by 38 cents.

To correct the serial correlation and heteroscedasticity in error terms, we apply the estimation method suggested in Anderson & Hsiao (1982) and report the robust OLS estimates in the right column of Table 3-4. The estimated coefficients are essentially the same as they are in the left column. The robust standard errors are larger than their counterparts in the left column as we have expected. The “reduced-form” and the 2SLS estimates now have p-values of 0.05 and 0.07 respectively.

In the lower panel of Table 3-4, we exclude the neighboring states of Illinois from the control group and apply the same estimation procedures to the smaller dataset. The "first-stage" estimates are almost identical to the results in the upper panel. The "reduced-form" estimate of the effect on sales and the 2SLS estimate of the semi-elasticity are essentially the same as their counterparts in the upper panel.

In Table 3-6, we estimate equations (3-1) - (3-3) using as controls observations from states that offer a similar mix of major games. In the upper panel, four states (Georgia, Maryland, Michigan, and Virginia) that joined the Big Game Consortium at the same time as Illinois are in the control group. In the lower panel, twenty-two states that do not offer keno games are in the control group. Although the estimated impacts of TV commercial restriction in the "first-stage" and "reduced-form" regressions are different from the estimates in Table 3-4, the 2SLS estimates of semi-elasticity are similar.

For the purpose of completeness, we report in the upper panel of Table 3-7 estimates using all lottery states except Massachusetts and Washington as controls, and in the lower panel estimates using all non-neighboring states except Massachusetts and Washington as controls. All the estimates are statistically significant at a p-value of 0.05. The point estimates of the semi-elasticity are almost twice the size of their counterparts in Table 3-4. The estimated standard errors also double the size of their counterpart in Table 3-4.

As we have discussed in the previous section, our estimate of the semi-elasticity of sales with respect to advertising is not precise if the effects of advertising last more than one month. To measure the persistent effects of lottery advertising, we estimate the model outlined in equation (3-4). We start by including only current advertising and one-month lag of advertising among the covariates, and gradually increase the number of lagged advertising included to eleven. The convergence criterion of the iteration process is set at 1E-7. We report the estimated retention rate (δ) in Table 3-8. Including advertising expenditures of the past twelve months in the regression, our estimate of the retention rate of advertising is 0.627 with a p-value of 0.06. The estimates are fairly stable if we include advertising spending of the past eight, nine, ten, and eleven months into the regression.

The estimate suggests that 62.7 percent of the effect of current advertising is carried over to the next month. After two months, 39.3 percent of the effect is retained.[42] After six months, only 10 percent of the effect is left.[43] In other words, 90 percent of the cumulative effect of advertising depreciates within six months. Our estimated results here line up fairly well with previous studies surveyed in Clark (1976), which suggest that 90 percent of the cumulative effect of advertising on sales of mature, frequently purchased, low-priced products depreciates within three to nine months.

The effects of lottery advertisement could be short-lived if advertising is game-specific and informative, such as providing information on the current size of the jackpot or information on the prize structure of a new scratch game. Advertisements of this type become obsolete quickly when the jackpot rolls over or the scratch game phases out. On the other hand, the effect of advertisement could be long-lived if the advertisement changes the preferences of a random player and turns him or her into a frequent player. Although some state laws suggest state lotteries should only advertise game information, we do not have enough data to separate the impact of informative advertisement versus

[42] The cumulative effect of current advertising after two months is calculated as $0.627^{2} = 0.393$.
[43] The cumulative effect of current advertising after six months is calculated as $0.627^{5} = 0.097$.

persuasive one. Thus, our estimated retention rate is the combined effect of both types of advertising.

The estimated short-term semi-elasticity is 1.405 at a p-value of 0.22, which suggests that a one-cent decrease in advertising spending leads to 1.4 percent decrease in current sales. If we sum up the cumulative effects of advertising over six months, a one-cent decrease in current advertising spending could cause sales fall by 3.54 percent in the long term.[44]

3.6.2 Estimated Impacts of Summer TV Hiatus in Washington

The summer TV hiatus of lottery advertisement occurred both in 1993 and 1994 in the State of Washington. In Figure 3-3, we plot monthly per capita spending on advertising for the Washington State Lottery from 1992-1994. In this graph, the solid horizontal lines are average total monthly advertising spending per capita before and during the summer hiatus separately. Before the summer TV hiatus, total monthly advertising expenditure averaged 4.2 cents per capita. During the hiatus, per capita spending dropped to 1.2 cents, a 71 percent reduction. TV advertisement fell by about 95 percent from 3.8 cents per capita to about 0.2 cents per capita. It should be noted that expenditures on non-television advertising (such as radio, billboard, and print) increased during the same period from 0.4 cents per capita to 0.9 cents per capita.

In Figure 3-4, we plot monthly sales per capita of the State Lottery of Washington from 1992 to 1994. The solid horizontal lines represent average monthly sales per capita

[44] The long-term semi-elasticity is calculated as $1.405*(1+ 0.627 + 0.627^2 + 0.627^3 + 0.627^4 + 0.627^5) = 3.537$.

before and during the summer hiatus separately. The monthly average sales per capita fell from $6 before the hiatus to $5.37 during the hiatus, a reduction of about 11 percent.

In Table 3-9, we compare the average monthly sales and advertising spending per capita of the Washington State Lottery in and out of the summer hiatus. Total spending on advertising during the summer hiatus is significantly lower than in the rest of the sample period, presumably because of a sharp reduction in TV commercials. Compared to the rest of the sample period, advertising spending on other media goes up during the hiatus summers, but the increase is not statistically significant at the 95 percent level. Lottery sales fall flat during the hiatus, but the difference is not statistically significant.

We then apply the interrupted time series analysis to the time series data of advertising expenditures and sales of Washington State Lottery, and report the estimated coefficients on the time trend variables in the upper panel of Table 3-10. The estimated equations are (3-7) and (3-8). We are able to detect a significantly negative trend of advertising spending during the period of TV hiatus, and a negative but statistically insignificant trend of sales in the treatment period. The D-W test statistics in the lower panel suggest that monthly advertising expenditures are AR (1). No statistically significant auto-correlation is detected in residuals of sales series. We should point out that in this sample data, there are few states that exhibit no serial correlation.

In Table 3-11, we report the difference-in-difference estimates of the effects of TV hiatus on advertising spending and sales together with the 2SLS estimates of the semi-elasticity of sales with respect to advertising. The first month of the summer TV hiatus occurred in July 1993. We are not able to select control states according to the pre-treatment trend criterion because only sixteen observations are available for each state lottery before the treatment. Thus, we use all state lotteries as controls in regressions in the upper panel, and all non-neighboring states as controls in regressions in the lower panel. The estimated results are similar in the two panels. Using non-neighboring states as controls, we find that monthly per capita spending on advertisement fall by 2.4 cents in Washington during the summer TV hiatus; lottery sales drop by about 1.4 percent. The estimated semi-elasticity is 5.9, which implies that a one-cent reduction in monthly advertising spending per capita results in 5.9 percent decrease in sales. Given that the treatment only lasts for six months, the estimated semi-elasticity is statistically significant with 90 percent confidence; though estimated effects in the "first-stage" and "reduced-form" regressions are significant with 95 percent confidence.

Based on estimates from the lower panel of Table 3-11, we calculate the elasticity of sales with respect to advertising and the gross rate of return to advertising in Table 3-12. The elasticity calculated is 0.07, which suggests that a ten percent decrease in advertising expenditure leads to 0.7 percent decrease in sales. The gross rate of return is 31.49, which suggests that a one-cent decrease in monthly advertising expenditure per capita produces about 31 cents decrease in monthly lottery sales per capita.

In Table 3-13, we report the set of estimates from equation (3-1) – (3-3) using as controls observation from states with a similar game mix as Washington. In the upper panel, three states (New Jersey, New York, and Ohio) that joined the Big Game Consortium around the same time as Washington are in the control group. In the lower panel, four states (California, Kansas, Michigan, and Oregon) that offered keno games before or during the same year as Washington are in the control group. In the lower panel, we can identify significantly negative "first-stage" impacts on monthly advertising expenditures, and negative but statistically insignificant impacts of summer hiatus on sales. The point estimates of the semi-elasticity are not very different from what we obtain in Table 3-11, but only statistically significant at a p-value of 0.20. In the upper panel, none of the “first-stage” or “reduced-form” estimates is significant at a p-value of 0.10, and the 2SLS estimate of semi-elasticity is only significant at a p-value of 0.30.

To estimate the retention rate of advertising in equation (3-4), we need twelve-month lags of advertising. However, the first summer TV hiatus occurred in July 1993 and our sample starts from January 1992. We therefore do not have enough data points to estimate the retention rate of advertising in this experiment.

3.6.3 Estimated Impacts of Advertising Caps in Massachusetts

The advertising budget of the Massachusetts State Lottery was cut drastically in 1994 and has been kept at a low level ever since. In Figure 3-5, we plot the monthly advertising expenditure per capita by the Massachusetts State Lottery. The solid horizontal lines represent average monthly advertising spending before and after the budget cuts separately. Before the budget cut in 1994, the state lottery spent 6 cents per capita per month on advertising. The figure dropped by 95 percent to 0.3 cents per month after the cut; spending on both TV and non-TV media were slashed as a result of the advertising restrictions.

Anecdotal evidence suggests that the Massachusetts State Lottery allocated the reduced budget unevenly over different games. Eric Fehrnstrom, the State Treasurer’s spokesman, said that instant games received the bulk of the lottery’s advertising effort, while Megabucks and MassCash games (online lotto games) received the least advertising efforts.[45] We plot the monthly sales per capita of instant and online games in Figure 3-6. Monthly sales of online games decreased by 31 percent from $12 to $8 after the budget cutbacks, while sales of instant games increased by about 50 percent from $19 to $28. In Figure 3-7, we plot total monthly sales per capita over the sample period. Rather than a drop in sales, total monthly sales per capita increased from $31 to $43 after 1994.

We compare the monthly averages of advertising spending (total and by media), total sales, sales of traditional online games, and sales of instant games before and after the advertising caps in Table 3-14. Although monthly averages of total and media-specific advertising expenditures are significantly reduced after the treatment, we can only detect a significant fall in the average sales of traditional online games. Monthly average of total sales and sales of instant games are significantly higher after 1994 than before.

When we apply the interrupted time series analysis to the sales and advertising series of Massachusetts Lottery in Table 3-15, we find a statistically significant negative trend in advertising spending after 1994, a negative and statistically insignificant trend in sales of traditional online game after 1994, and positive and statistically insignificant trends on both total sales and sales of instant games after 1994.

A couple of reasons led to the rising trend in total sales and sales of instant games. During the 1990s, state lotteries began offering instant game tickets in face value of multiple dollars (for example, $2, $3, $5, $7 and $10). The new multi-dollar instant games offer higher payout ratios than the original $1 ticket. Among all state lotteries, Massachusetts lottery's instant games offer the highest prize payout in the country,

[45] "Lottery strikes its rich with big revenue," by Jack Meyers, *the Boston Herald*, 067/14/1994.

ranging from 69 to 80 percent. For instance, the ten-dollar instant game "Spectacular" in Massachusetts offers an 80 percent return to players. Innovation of such high payout games bolstered sales of instant games, which could overshadow the negative impact of reduced advertisement. Simple mean comparison of sales before and after the treatment and trend comparison using interrupted time series analysis cannot isolate the treatment effect we are interested in. Later in this section, we include a set of game dummy variables in regression models to separate the effects of game innovations on sales from the treatment effect.

Another reason behind the rising sales is the introduction of keno games. As shown in the lower part of Figure 3-7, keno started in Massachusetts in 1994. The concurrence of the keno introduction and advertising caps contaminates the evaluation of this quasi-experiment. Unlike the Illinois and Washington experiments, the Massachusetts case has an additional "treatment" – the introduction of keno – that potentially contaminates the evaluation of the experiment. To separate the impacts of the two shocks, we include in the regression a set of dummy variables representing the introduction of keno games. Luckily, we have eleven other state lotteries that also offer keno games. Among those, eight introduced keno at some point during the sample period.[46] Assuming the effects of keno games on lottery sales are similar across these states, we can pick up the common impact of keno games on sales through the set of keno dummy variables, and capture the impact of advertising caps on sales by the treatment dummy variable.

[46] The eight states are California, Colorado, Georgia, Kansas, Maryland, New York, Washington, and West Virginia.

To understand the impact of keno on total sales and sales of other games, we run a separate regression for each state that started offering keno games during the sample period. The dependent variables are the natural logs of monthly total sales per capita, monthly sales of traditional online games per capita, and monthly sales of instant games per capita. Covariates on the right hand side are the state personal income, state unemployment rate and the dummy variable that equals one in the months when the keno game was introduced. We report the coefficients on the dummy variable in Table 3-16. In models where the outcome is total sales, we find a statistically significant and positive coefficient on the keno dummy variable in five of seven cases. The coefficient on the keno dummy is statistically significant and positive in three of seven cases when the dependent variable is sales of instant sales, and statistically significant and positive in two of seven cases when the dependent variable is sales of traditional online games. Pooling the seven states together, we apply the fixed effects estimation and find that the introduction of keno has positive impacts on sales of the three different game combinations. The estimated positive impacts are statistically significant on total sales and instant sales. We hope that including the set of keno dummy variables can help us single out the treatment effect.

Using as controls states with similar pre-treatment trends in advertising spending and sales,[47] we estimate equations (3-1) – (3-3) with the log of monthly sales of traditional online games as the dependent variable and report the OLS and robust estimates of key coefficients in the upper panel of Table 3-17. We find that monthly advertising spending per capita fall by 6.6 cents after 1994; monthly sales of traditional

[47] We apply the selection criterion to the four dependent variables: monthly advertising spending per capita, monthly total sales per capita, monthly instant sales per capita, and monthly sales of traditional online games per capita. States that meet the criterion in all four regressions are selected into the control group.

online games fall by 28.3 percent after 1994. The estimated semi-elasticity is 4.275. All estimates are statistically significant with 95 percent confidence.

We calculate the elasticity of sales with respect to advertising and gross rate of return to advertising in Table 3-18. The calculated elasticity is 0.011, which implies that a ten percent decrease in advertising spending results in 0.11 percent decrease in sales of traditional online games. The gross rate of return is 35.92, which implies that a one-cent decrease in advertising spending results in 35.92 cents decrease in sales of traditional online games. We should be careful in the interpretation of the estimated semi-elasticity in Table 3-17. It is not comparable to the ones obtained from the quasi-experiments in Illinois and Washington in Table 3-4 and 3-11. Because we do not have information on how much budget is allocated to advertisement for traditional online games, our "first-stage" estimate measures the average reduction in total advertising spending after 1994. Given the press information on the uneven distribution of the budget over different games, our "first-stage" estimates could under- or over- estimate the reduction of advertising on traditional online games

In the lower panel of Table 3-17, we use non-neighboring states with similar pre-treatment trends in sales and advertising expenditure as the Massachusetts Lottery as control states. The point estimates are essentially the same as their counterparts in the upper panel, and all are statistically significant with 95 percent confidence. In Table 3-20, we report the set of estimates from equation (3-1) – (3-3) using as controls observation from states with similar game mix as Massachusetts. In the upper panel, four states that joined the Big Game Consortium at the same time as Massachusetts are in the control group. The estimated impacts of the "first-stage" and "reduced-form" regressions are negative and statistically significant at a p-value of 0.05. The 2SLS estimate of semi-elasticity is also significant with 95 percent confidence, although the size of the estimate and the standard error are larger than the estimates in Table 3-17. In the lower panel, seven states that offered keno games before or during the same year as Massachusetts are in the control group. We are able to identify significantly negative “first-stage” impacts on monthly advertising expenditures, and negative but statistically insignificant impacts on sales. The estimated semi-elasticity is not statistically significant.

In Table 3-21, we report estimates using all lottery states except Illinois and Washington as controls in the upper panel, and estimates using all non-neighboring states except Illinois and Washington as controls in the lower panel. All the estimates are statistically significant at a p-value of 0.05. The point estimates of the semi-elasticity are close to the estimates in Table 3-17.

To determine the persistent effect of advertisement on sales, we estimate a series of equation (3-4) allowing different numbers of past advertising expenditures entering the regression. The estimated retention rates (δ) are reported in Table 3-22. In the upper panel, we include advertising expenditures of the past nine months or above in the regression. The estimated retention rate bounces between -1.832 and 1.460; the estimated short-term advertising effects are not statistically different from zero with large p-values. We then estimate equation (3-4) including advertising expenditures of the past five, six, seven and eight months in the regression. The corresponding estimates are recorded in the lower panel of Table 3-21. The estimated retention rates in the lower panel are fairly stable in the range of 0.626-0.777, with p-values of 0.024 – 0.131.

When we include advertising spending of the past six months in the regression of equation (3-4), the estimated rate of retention is 0.679 at a p-value of 0.024. The result suggests that 67.9 percent of the effect of current advertising is carried over into the next month. After six months, only 10 percent of the effect is left. In other words, 90 percent of the advertising effect depreciates after six months. The estimated short-term semi-elasticity is 3.108 at a p-value of 0.076, which suggests that a one-cent decrease in advertising spending leads to 3.1 percent decrease in current sales. If we sum up the cumulative effects of advertising over six months, a one-cent decrease in current advertising spending could cause sales fall by 8.73 percent in the long term.[48]

We also apply the whole set of estimation procedures to sales of instant games and total sales. With sales of instant games as the dependent variable, we are able to identify negative impacts on sales in the “reduced-form” regressions, but the estimates are not statistically significant in most of the regressions. Subsequently, the estimates of the semi-elasticity are not statistically significant either. With total sales as the independent variables, the estimated impacts on sales of advertising caps are positive and statistically insignificant. To limit the length of the paper, we do not report these estimates.

3.7 Conclusion

Using a difference-in-difference model, we find that reductions in advertising spending in the three quasi-experimental states reduce lottery sales. After correcting for

[48] The long-term semi-elasticity is calculated as $3.108*(1+ 0.679 + 0.679^{2} + 0.679^{3} + 0.679^{4} +0.679^{5}) =8.733$.

serial correlation in residual terms within states, we calculate the elasticity of sales with respect to advertising in the range of 0.07 to 0.16. Though of different products, our estimated elasticity is in line with previous studies: Seldon et al (1989) find that the elasticity of cigarette sales with respect to advertising ranges between 0.1-0.25; and Valdés (1993) reports an estimated elasticity of 0.08-0.09 in the cigarettes industry in Spain.

Although most state lotteries have been on the market for more than thirty years, our estimated results show that advertisement can still affect sales significantly. Contrary to the belief of some state legislatures that state lotteries spend too much on advertising, our results suggest that they maybe advertising too little in terms of maximizing net revenue. We find that a one-cent decrease in monthly advertising spending per capita results in a decrease in gross sales of 31-38 cents. Assuming that thirty percent of sales are retained in state coffers as profit, a one dollar decrease in advertising spending could cost state government $9-10 of net revenue at the margin.

The results are not surprising if we recall that advertising expenditure accounts for only 1.17 percent of total sales, while casinos on Las Vegas Boulevard spend 2.4 to 4 percent of the total hotel-casinos' sales on advertising, restaurant owners spent 3.2 percent of their sales on advertising, beverage manufacturers 7.5 percent, cosmetics companies 8.8 percent, and candy makers 12.7 percent. Advertising of state lotteries is well below what other industries are spending. State lotteries are limited in their ability to maximize profits due to state regulations such specification of payout ratios in the state codes, explicitly caps on the advertising budget at no more than a small percentage of gross sales, and the annual legislative review of advertising budgets. It is therefore not unreasonable that we find that state lotteries fall far down the advertising response curve away from the saturation stage.

It is also interesting that we find lottery advertisement is short-lived in our three quasi-experiments. Most of the effects of advertising depreciate within half a year. Our explanation is that informative advertisement providing game-specific information tends to be short-lived compared to persuasive advertisement designed to cultivate players' preferences, and that informative advertisement has a better chance to survive budget cuts than persuasive ones. Therefore, our estimated retention rate of lottery advertisement during the period of budgetary stringency could reflect the dominating effect of informative advertisement. However, we do not have enough data to support the argument. Obtaining micro-level data to estimate the differential effects of informative and persuasive advertisement is on the future research agenda.

Chapter 4: The Impact of Earmarked Lottery Profits on State Educational Expenditures

4.1 Overview

Over the past four decades there has been a rapid growth in both the number and size of state lotteries in the United States. In 1964, New Hampshire became the first state since the late 1800s to run a lottery. Since then, thirty-seven other states and the District of Columbia have instituted lotteries. The gross sale of lottery tickets now exceeds $37 billion per year, adding about $12 billion per year to state budgets.

Some states deposit lottery profits back into their general funds, but most states have earmarked lottery profits to fund particular projects such as parks and recreation, environmental improvements and programs for senior citizens. The most popular destination for earmarked lottery funds is, however, public schools with eighteen of twenty-five states that earmark funds using profits for K-12 education. Given the fungibility of money, many economists and political observers have questioned the effectiveness of earmarking policies. The fungibility of money is, however, rejected in a variety of empirical applications in public finance, most notably, the large literature on the “flypaper effect.” Intergovernmental grants are equivalent to increases in income, so economic theory predicts these grants should not increase spending by an amount more than an equal increase in income. However, dozens of studies have demonstrated that money “sticks where it hits.” Hines et al. (1995) review a number of empirical tests of the flypaper effect and conclude that, while local spending increases by about five to ten cents for every dollar increase in income, unrestricted block grants increase spending dollar for dollar. Whether earmarking can increase spending on a particular category is therefore an empirical question.

State lotteries provide an excellent opportunity to examine the impact of earmarking. First, there are variations in earmarking policies both across states and within states over time that can be exploited in an econometric model. Among the thirty-seven state lotteries in our sample period of 1978-1998, nine states have always earmarked lottery profits for public education; five have changed the allocation between the general fund and educational earmarking; and eight states have earmarked lottery profits for programs other than education. The rest of the fifteen states have always deposited lottery profits into the state general fund. These fifteen states can be used as a comparison group in a difference-in-difference type estimator. Second, lottery profits are driven up or down by the introduction of new games and/or the legalization of other forms of gambling within the state. Variation in lottery profits within states can be employed to identify their impact on public educational spending. Third, lottery profits are a small portion of state budgets, thus providing a "fair" test of the fungibility of money.

Using a panel of state expenditures on public education in states with lotteries, we examine the impact of earmarking lottery profits for K-12 education on spending in this category. We perform three different sets of tests. First, we restrict our attention to the nine states that have always earmarked lottery profits for K-12 education, and examine whether year-to-year changes in state spending on education are correlated with changes in lottery profits over the same period. If earmarking increases spending dollar for dollar, the coefficient on per capita lottery profits in models where per capita K-12 spending is the dependent variable should equal 1. Second, we examine states that switched the allocation of lottery profits between the general fund and public education during the sample period. If earmarking increases spending on education dollar for dollar, we should see an increase in K-12 spending commensurate with the amount of new monies allocated to the earmarked category once the profits were shifted to this category. In both of these tests, we use as a comparison group the fifteen states that have always deposited their lottery profits into the general fund. We also examine the changes in K-12 spending in states that have always earmarked lottery profits for non-educational programs. To control for heterogeneity across states and over time in per capita state spending on education, we include state and year effects, plus state-specific time trends in our statistical model.

The first test, using earmarking states, is limited because the model is identified by unexplained period-to-period changes in lottery profits. We simply examine whether K-12 spending increased when lottery profits rose. The second test, using states switching between earmarking and non-earmarking, is subject to the criticism that the group of states moving their allocation of lottery profits from the general fund to educational fund is a selected sample. For example, these states may have expected greater than average growth in educational spending, leading them to earmark lottery profits. As we outline below, however, our reading of the legislative history does not indicate this was the case. To deal directly with these limitations, we offer a third empirical model where we examine particular events that should alter the lottery profits generated by states. For example, in “first-stage” regressions, we show that the introduction of lotto games and video lotteries generally increases per capita lottery profits while the legalization of casino-style gaming in a state reduces lottery profits. In reduced-form models, we can also examine how K-12 spending is correlated with these events. Drawing on these two results, we can use the shocks to lottery sales as instruments for profits in a two-stage least squares model.

In all three empirical tests, we find very similar results. First, we find that earmarked money is partially fungible. A dollar increase in the earmarked profits contributes an additional 60 to 80 cents to school expenditures. Although some money is leaked away from the intended purpose, we do find that earmarking increases educational spending by an amount larger than the value produced by having an extra dollar in lottery profits added to the general fund. In particular, each dollar of lottery profits increases school spending by about 40 to 50 cents in states that deposit profits into the general fund and by only 30 cents in states that earmark profits for areas other than education. Using a Bayesian estimation procedure for inequality restrictions in the normal linear least squares model, we find there is a high likelihood that a dollar of earmarked lottery profits generates less than a dollar of spending on K-12 education, but more than the spending generated from a dollar of lottery profits allocated to the general fund. Our results are fairly stable across different sample periods, control groups, and different estimators. For example, the results are very similar whether we use a fixed-effects specification or "long-differences" that exploit 5-year lags.

4.2 Earmarking of Lottery Profits

Lottery profits are a controversial source of state revenue. In order to reduce opposition to the legalization of lotteries, state legislatures tried to guarantee that lottery profits would be used for a "good cause". Of the thirty-seven states with lotteries in the year 1998, twenty-two states earmark at least a portion of lottery profits for specific programs: eight of them earmark lottery profits for programs such as parks and recreation, senior citizens, property tax relief, and college scholarships; fourteen earmark lottery profits for K-12 education. The remaining fifteen states deposit lottery profits into the general revenue fund. In Table 2-1, we summarize the allocation of lottery proceeds in thirty-seven states and Washington D.C. since the inception of the lottery in each state.

In some cases, it is not clear into which category each state falls. Although some states earmark funds for particular purposes, the legislation does not specify what fraction should be devoted to particular categories. In these instances, we define the usage of lottery profits as general revenue. For example, Arizona earmarks lottery profits for five projects and the general fund, Indiana earmarks lottery funds for five projects, Kentucky earmarks lottery money for a scholarship program and the general fund, Rhode Island earmarks for distressed city and towns plus the general fund, South Dakota earmarks for two other projects and the general fund, and West Virginia has earmarked for three different projects since 1989. None of these states indicates what fraction of funds should be devoted to each category. Therefore we classify these states as placing lottery profits into the general revenue fund.

It is worth noting that some of the states have altered the use of lottery money over time. For example, Missouri shifted lottery profits from its general revenue fund to an educational fund in 1992, and Illinois did not earmark lottery profits for K-12 schools until 1985. During the sample period of 1978 to 1998, five states have switched the allocation of lottery profits between the general revenue and educational funds. Nine states have always earmarked lottery money for public education.

4.3 Previous Evidence on the Fungibility of Earmarked Money

Some theoretical models suggest that earmarking revenues for a particular category should not increase the targeted spending by an amount more than it would be increased if the money were not earmarked. A prototypical model can be found in Wilde (1968). However, empirical evidence on the fungibility of earmarked money is not consistent with the theoretical predictions. Hines et al. (1995) survey a number of empirical papers on intergovernmental grants and conclude that, while local spending increases by about five to ten cents for every dollar increase in income, unrestricted block grants increase spending dollar for dollar. Because money "sticks where it hits," this phenomenon has been called the flypaper effect. Researchers studying domestic fiscal response to categorical foreign aid find mixed empirical results. Some find that the earmarked aid increases total public expenditures (Pack et al. 1990, Cashell-Cordo et al. 1990 and Feyzioglu et al. 1998), some find that aid is highly fungible (Franco-Rodriguez 2000), while others find foreign aid has no significant impact on domestic revenues or expenditures (Swaroop et al. 2000 and Gupta 1993).

A few authors have examined the fungibility of lottery profits earmarked for education in the United States. Studies by Mikesell et al. (1986), Borg et al. (1990), Summers et al. (1995) and Vance et al (1999) examine educational expenditures in states that earmark lottery profits for public education. They fit time-series data with linear or quadratic trends and compare these trends before and after the introduction of lotteries.

None of them finds positive effects on school spending. Some findings suggest that earmarking reduces state spending on schools. Spindler (1995) improves the identification strategy by modeling state educational spending as an AR (1) process and allowing state spending to shift according to a step function after lotteries are introduced. Using data from seven states, Spindler concludes that lottery profits on average are fungible over the period of 1961-1992.[49] Garrett (2001) estimates a similar model using data from Ohio State. He adds additional covariates into the regression model, such as lottery profits per capita and per capita income. Garrett finds suggestive evidence that up to half of the money earmarked for education finds it way to school spending, but the results are not statistically significant.

Instead of focusing on one state at a time, we examine a panel data set of all states with lotteries. Within state variation in lottery profits is used to identify its impact on public educational expenditures. To determine the effectiveness of the earmarking policy, we compare the impact of lottery profits on school spending in earmarking and non-earmarking states. Our work is most similar to that of Novarro (2002), who independently estimates models similar to those in this paper. Novarro finds that a dollar of lottery profit earmarked for education increases current educational spending by roughly 36 cents more than a non-earmarking dollar and 60 cents more than a dollar earmarked for non-educational expenditures. The dependent variable in Novarro's work is state-level per student current expenditures, which includes spending on instruction, operation and maintenance, and other day-to-day costs. Current expenditures are "spent" by local school districts and monies for operation are from local, state and federal

[49] The seven states are New York, New Hampshire, Ohio, Michigan, California, Montana, and Florida.

sources. Her test, therefore, is a joint test that lottery profits and grants to local districts are fungible.

4.4 A Within-Group Econometric Model

4.4.1 Fixed Effects Estimation

In general, our regression model is based on traditional analysis as in Wilde (1968). State spending on public education is a function of net lottery revenue and exogenous state characteristics, which can be derived from the utility maximization of the state government. The data set we use for this analysis contains annual observations at state-level per capita K-12 spending and lottery profits. Lottery profits vary from year to year for a variety of reasons. In states where lotteries were introduced at some time during the panel, the obvious cause of the change in profits is simply the introduction of the lottery. In all lottery states, profits change from one year to another through the introduction of new games or the changing affinity the state's population may have for gambling. Thus, the impact we are identifying is the marginal propensity of the state government to spend on K-12 schools out of an extra dollar available from lottery profits. If earmarking changes spending patterns, we would expect educational spending to grow dollar for dollar with lottery profits.

The counterfactual we need to identify is what states would have done with the extra money had lottery profits not been earmarked for education. In order to do so, we include in all models data from states that place lottery profits directly into the general revenue fund. As a separate test, we also include in some models data from states that earmark lottery profits for expenditures on government services other than K-12 schools. For this final group, if earmarked money sticks where it hits, we would expect little change in K-12 spending. Given that we have data from various types of states (those that earmark and those that do not), the most natural model to estimate is a within-group estimator where we hold constant permanent differences in state educational spending and test whether increases in lottery profits generate a differential increase in K-12 spending in earmarking versus non-earmarking states.

Consider a pool of states, all with lotteries. Some earmark lotteries for education ($E_{it}=1$), some earmark lotteries for other expenditures ($O_{it}=1$), and the rest deposit lottery profits into the general fund ($G_{it}=1$). Let L_{it} represent state lottery profits per capita in state i year t, while $K12_{it}$ represent state per capita expenditures on K-12 education in state i year t. The simple within-group model outlined above can be expressed by the following equation:

$$K12_{it} = \alpha_1 * L_{it} * E_{it} + \alpha_2 * L_{it} * G_{it} + \alpha_3 * L_{it} * O_{it} + \beta * X_{it} + u_i + v_t + \lambda_i T_t + \varepsilon_{it} \quad (4\text{-}1)$$

where X_{it} is a vector of socioeconomic, demographic and legal characteristics that describe the state's willingness and ability to devote resources to K-12 education, u_i and v_t are state and year fixed effects. T_t is a simple linear time trend, and therefore λ_i is a state-specific time trend coefficient. The final term is an idiosyncratic error.

In many studies where school spending is the outcome of interest, K-12 spending is denominated by the number of students. However, in this case, we must use the same denominator for both the lottery profits and K-12 spending variables, so that the two key variables in our analysis are per capita K-12 spending and per capita lottery profits. The key parameters of interest are α_1, α_2, and α_3, which represent the additional lottery dollar spent on K-12 education in states that earmark lottery profits for K-12 schools, place profits in the general fund, and earmark profits for other government services, respectively.

The state fixed-effects are required for a number of reasons. First, state per capita educational expenditures vary considerably across states. New Hampshire spends on average less than a hundred dollars per capita on K-12 schools, while California spends about $500 per capita. Many of the between-state differences in levels of school spending are results of historical and political factors that cannot be fully captured by measurable covariates. Therefore, we control for these permanent differences by including a complete set of state fixed-effects in the regression model. Second, one might be concerned that the level of K-12 spending and lottery profits might be correlated. For example, suppose that larger, more urban states have higher than average spending on education. Suppose also that these states can more effectively exploit their size by offering a larger selection of lottery games and, hence, generate more lottery profits. In a simple cross-section of data, this type of correlation would bias upwards the coefficient on the α's. Controlling for state fixed-effects allows for the possibility that states with higher than average educational spending have a different level of lottery profits. The year effects control for shocks to spending that are common to states but vary across years, such as national recessions.

There are not only permanent differences across states in the level of K-12 spending, but also persistent differences across states in the growth rate of spending on public schools. To demonstrate this point, we run, for each state, a simple time-series regression where the dependent variable is state per capita K-12 spending and the three covariates are the fraction of the state population that is enrolled in public schools, per capita income, and a simple time trend. For these regressions, we used data from 1978 through 1998. The time trend is negative in 20 states, and positive for 30 states. The range of the time coefficients is about $63 per capita. Michigan ranks at the top in terms of the annual growth of state school expenditure per capita, about $50 per capita growth annually; North Carolina ranks the lowest with about $13 per capita decrease each year. Given the wide range of growth of per capita K-12 spending, we also include in our analysis state-specific time trends. These time trends could also help control for a possible non-random selection of states into the earmarked category. One might be concerned that states with expected above average growth in educational spending would be more likely to earmark profits for education. By including the state-specific time trends, we control to some degree for this possible problem.

We include a variety of socioeconomic characteristics that have been found to be correlated with state spending on education in previous studies, such as state personal income, the share of the population unemployed, the share of the population enrolled in public schools, the racial composition and age structure of the state population, and the education attainments of the adult population. We also include in X_{it} two sets of variables that measure legislative and legal reforms that may have altered state support for K-12 education. Starting in the 1970s, the constitutionality of education finance formulas was challenged in forty-three states. By 1997, education finance systems were declared unconstitutional in eighteen states. Murray et al. (1998, 2000) demonstrate that one of the impacts of these decisions was to increase state support for public education. In this case, we capture the impact of finance reform by including a set of dummy variables that equal 1 in the first, second, third, fourth, fifth and sixth or more full year after the court decision. Most of the court cases were initiated because of large disparities across districts in per pupil spending. Much of these disparities are produced because local school districts rely heavily on the property tax for local financing of schools and the value of this tax base varies considerably across districts. A growing dissatisfaction with the property tax has led some states to directly reform local finance of schools by de-emphasizing the property tax. For example, legislation in Michigan, Oregon, and Vermont[50] has greatly reduced local districts' dependence on the property tax and shifted much of the school finance to the state level. In these states, we also construct a series of dummy variables that parallel in structure the values for court-mandated finance.

In order to compare the impact of earmarked lottery profits with the impact of money from general revenue fund, we group the panel of lottery states into two sub-samples. First, thirty-seven states with lotteries are divided into four groups. The "K-12" group includes the nine states that have always earmarked lottery profits for K-12 education.[51] The "Changers" group includes the five states that changed the allocation of lottery profits between the general revenue and educational funds during the sample period.[52] The "Earmark Non-Education" group includes the eight states that earmark

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[50] In Oregon the passage of Measure 5 in 1990, a property tax limitation measure, requires the state legislature to offset lost property tax revenue with money from state general fund. In Michigan, Proposal A in 1994 has reduce total school property taxes by about 33 percent and increased the state share of total revenue for K-12 education. In Vermont, Act 60 in 1997 set a statewide property tax rate of $1.10 for all towns and the share of state block grants has increased from 40 percent to 60 percent.

[51] The nine states are California, Florida, Georgia, Idaho, Michigan, New Hampshire, New Mexico, New York and Ohio.

[52] The five states are Illinois, Missouri, Montana, Oregon and Texas.

lottery profits for government services other than education.[53] The "General Revenue" group includes the fifteen lottery states that have always deposited lottery profits into the general revenue fund.[54] Referring to the econometric model above, if state i belongs to the "K-12" group in year t, then E_{it} equals 1, O_{it} and G_{it} equal 0. If state i belongs to the "Earmark Non-Education" group in year t, then O_{it} equals 1, E_{it} and G_{it} equal 0. If state i belongs to the "General Revenue" group in year t, then G_{it} equals 1, E_{it} and O_{it} equal 0. If state i is in the "Changers" group, then E_{it} equals 1 when the state earmarks lottery profits for education, and equals 0 when the state deposits lottery profits into the general fund; G_{it} equals 1 minus E_{it}; and O_{it} equals 0. The four groups are then combined into two sub-samples: sub-sample 1 includes the "K-12," the "Earmark Non-Education," and the "General Revenue" groups; sub-sample 2 includes the "Changers" and the "General Revenue" groups.

We examine the two data sets separately for two reasons. First, the type of variation in lottery profits L_{it} that is used to identify the model is very different in the two sub-samples. In models using sub-sample 1, we are measuring whether unexplained movements in L_{it} are more correlated with $K12_{it}$ in states that earmark profits for education versus states that do not. In contrast, in the "Changers" sub-sample, we measure whether the unexplained within panel covariance in L_{it} and $K12_{it}$ is different before and after the money is earmarked for education. Second, there is sufficient concern that the group of states that shift their allocation of lottery profits from the

[53] The eight states are Colorado, Kansas, Massachusetts, Minnesota, Nebraska, New Jersey, Pennsylvania and Wisconsin.

[54] The fifteen states are Arizona, Connecticut, Delaware, Indiana, Iowa, Kentucky, Louisiana, Maine, Maryland, Rhode Island, South Dakota, Vermont, Virginia, Washington and West Virginia.

general fund to the earmarked category is not a random sample. We do not want to potentially contaminate the “K-12” group with these observations.

4.4.2 Robust Estimation of Standard Error in Fixed Effects Model

Bertrand et al (2002) points out that in a fixed-effects model such as equation (4-1), standard errors estimated using Ordinary Least Squares (OLS) method can be biased because of possible serial correlation in error terms. If the error term follows an AR (1) process with positive serial correlation, the estimated standard error using OLS method will be biased downwards. Consequently, the under-estimated standard error could contribute to a higher false negative t-test statistics.

To obtain robust estimates of the standard error, we follow the same procedure employed in Chapter 3, the econometric method suggested by Anderson & Hsiao (1982) to correct for autocorrelation in error terms. Given that the dependent variable is annual school spending per capita, we assume the error term follows an AR (1) process. Computationally, we first estimate the first-order autocorrelation coefficient of the residuals from the estimation of equation (4-1) by running a regression of the residual on its lags. We allow each state to have its own autocorrelation parameter. Then we use these estimated coefficients to obtain robust estimates of standard errors as we have done in the previous chapter.

4.4.3 First-, 3-year, and 5-year Differences Estimations

Within-group variation in lottery profits is used to identify the impact of lottery profits on state school spending in the fixed effect estimation. Panel data estimates are sensitive to model specification if the regression is subject to contamination from measurement error, omitted time-varying characteristics, or omitted lagged effects. To deal with these issues, McKinnish (2000) suggests obtaining estimates using fixed effect, first difference, and long difference models. If the regression is correctly specified, the models should produce similar coefficient estimates. Therefore we also estimate first-difference, 3-year difference, and 5-year difference models of the form:

$$\Delta_j K12_{it} = \alpha_1 * \Delta_j (L_{it} * E_{it}) + \alpha_2 * \Delta_j (L_{it} * G_{it}) + \alpha_3 * \Delta_j (L_{it} * O_{it}) + \beta * \Delta_j X_{it} + v_t + \lambda_i + \mu_{it} \quad (4\text{-}2)$$

where j=1, 3, or 5. v_t absorbs variation in common shocks to all states in a given year, and λ_i captures the state specific time trend. State fixed effects are canceled out in the difference calculation.

4.4.4 Bayesian Inference of Hypothesis Tests

Based on estimates of α_1, α_2, and α_3, we can test a number of hypotheses about the fungibility of lottery profits. Specifically, we can test whether earmarked money increases spending dollar for dollar, the hypothesis is $\alpha_1 = 1$. We can also test whether earmarking has no impact on school spending by examining the null hypothesis $\alpha_1 = \alpha_2 = \alpha_3$. In addition, we can examine alternative hypotheses that involve more detailed tests of the parameters. For example, the probability that earmarking does not increase spending dollar for dollar is simply the probability that $\alpha_1 < 1$. Likewise, if earmarking has some impact on spending, we would expect a dollar increase in lottery profits to raise spending most in a “K-12” state and least in an “Earmark Non-Education” state, which suggests that $\alpha_1 > \alpha_2 > \alpha_3$.

Usually, t and F statistics are calculated to test equality constraints in linear regression models. However, such tests do not extend readily to inequality constrained linear models in a classical statistical framework. Geweke (1986) demonstrates that tests of inequality restrictions in the normal linear least squares model are easily incorporated into a Bayesian framework. Consider a simple linear regression model for n observations and k exogenous variables of the form $Y = X\beta + \varepsilon$, where ε is assumed to be normally distributed with a mean 0 and a variance/covariance matrix $\sigma^2 I_n$. Geweke assumes a diffuse prior on σ, and a simple indicator function $q(\beta)$ as a prior on β that equals 1 when the inequality restriction is binding and zero otherwise. Because the prior on β is improper, there is no closed-form representation for the posterior. However, numeric estimates of the moments of the posterior can easily be obtained via Monte Carlo integration. Specifically, after integrating out σ, Geweke demonstrates that the posterior distribution for β is proportional to the product of a multivariate t-distribution and the indicator function $q(\beta)$. Random draws of the parameter vector β are then made to a multivariate t distribution with parameters $\tilde{\beta} = (X'X)^{-1}(X'Y)$ and variance $s^2 = (Y-X\tilde{\beta})'(Y-X\tilde{\beta})/(n-k)$. The moments from the set of random draws that satisfy the inequality restrictions are the exact moments of the constrained estimator. The fraction of draws that satisfy the inequality restriction is an estimate of the probability that the inequality restriction is true. The number of random draws determines the numerical accuracy of a simulated moment. Geweke shows that in this case, if there are m random draws, the numeric standard error of posterior moment is $1/m^{1/2}$ times the standard deviation of the simulated moment. So with only 10,000 draws, the measurement error generated by simulating the mean of the distribution is only 1 percent of the value of the standard deviation of all random draws. In our work, we make 400,000 draws using antithetic acceleration as described in Geweke's paper.

4.4.5 Data

Data on state expenditures on K-12 education in this analysis are obtained from the National Center for Education Statistics (NCES). The key outcome in our analysis should be some measure of the allocation of funds to education at the state level. Such data are available in terms of both expenditures and revenues. In general, state expenditures on public education include not only revenues of own source but also from federal transfer and/or grants and local sources of school spending. In contrast, state revenue data record only own source revenues allocated to educational spending. Since we are interested in the fungibility of lottery profits at the state level, the revenue data are used. In those states where lottery profits are allocated to public education, some grant all the proceeds to elementary and secondary school systems (K-12), and others divide the money between K-12 and post-secondary education. Since K-12 education is always among the beneficiaries, state revenues allocated to K-12 schools are examined as the dependent variable.

The variable of interest in the regression is the net lottery revenue allocated to K-12 schools. Raw data on net lottery revenue were obtained from La Fleur's,[55] a private company that provides lottery statistics. Of the states earmarking lottery money for education, some spend all the lottery profits on K-12 schools, such as New Hampshire

[55] The web address of the company is http://www.lafleurs.com/default.htm.

and Michigan, while states such as Idaho divide the money between K-12 and post-secondary education. We collect detailed information on the division of lottery profits between K-12 schools and post-secondary education in each state and construct the independent variable E_{it} as the net lottery revenue allocated to K-12 schools.

State demographic variables are obtained from the following sources: school enrollment from the NCES; state personal income from the Bureau of Economic Analysis; state unemployment rates from the Bureau of Labor Statistics (BLS); state population, age/race composition, and state adult educational attainments from the United States Bureau of Census.

All the money values are real 1996 dollars, calculated using the CPI-urban index from the BLS. All the variables are in per capita terms. The sample contains 756 observations of 36 states in the United States over the period of 1978 to 1998.[56] Table 4-1 provides summary statistics of the four state groups: the "K-12," the "General Revenue," the "Earmark Non-Education," and the "Changers" groups.

4.5 Regression Results

4.5.1 Estimates and Test Statistics

In Table 4-2, we report estimates of equations (4-1) and (4-2) using sub-sample 1 where the "K-12," the "General Revenue," and the "Earmark Non-Education" groups are pooled together. In the top panel of the table, we report point estimates of the impacts of lottery profits on state K-12 spending for each group using fixed effect, first difference,

[56] Washington D.C. is not included in the sample.

3-year difference, and 5-year difference estimator separately. In the bottom panel of the table, we report statistical tests associated with the parameters α_1, α_2, and α_3. For brevity, we report the coefficients on the other covariates in X_{it} in the appendices.[57]

In the fixed effect model, an additional dollar of lottery profits increases spending by 60 cents in "K-12" states, an amount 10 cents larger than the increase in spending in general revenue states and 30 cents larger than the increase in states that earmark lottery profits for non-educational purposes. Given the large standard errors on α_3, we cannot reject the null hypothesis that these three coefficients are the same. The probability that earmarking increases spending at less than a dollar for dollar rate is 0.92, the probability that a higher amount per dollar of lottery profits is spent on schools in "K-12" states than in "General Revenue" states is 0.62, the probability that more is spent on schools in "General Revenue" states than in "Earmark Non-Education" states is 0.74. However, the probability of $\alpha_1 > \alpha_2 > \alpha_3$ is only 0.5 because of the large standard errors on α_3.

The within-group estimator produces similar results to short-term and long-term difference estimators, although standard errors increase as the sample size shrinks in the long difference models. Fixed effect and 5-year difference estimates on α_1 and α_2 are statistically significant and positive and of the same scale. First difference and 3-year

[57] Our estimated coefficients are similar to results obtained in other research. A one-dollar increase in state personal income per capita increases K-12 school spending by about 1.4 cents. The share of the population aged 65+ has a statistically significant and negative effect on public educational expenditures as in Poterba (1997 and 1998) and Harris et al. (2001). A one percent increase in the elderly population reduces state budgets for schools by about $28 per capita. The share of the population unemployed imposes a negative impact on education expenditures as well. A one percent increase in the population unemployed leads to about $11.1 per capita less available for schools. The coefficient on enrollment is positive and significant. A one percent increase in enrollment increases school spending by about $9. We find significantly positive effects of both reform variables on state educational expenditures. Property tax reforms in the three states (Michigan, Oregon and Vermont) channel more money to public schools than court-ruled reform in other states.

difference estimates of α_1 and α_2 are smaller and not statistically significant.[58] The coefficient estimate of α_3 is about 0.30, though not statistically significant in any model. We find it likely that earmarking increases spending at less than a dollar for dollar rate, which suggests that a fraction of the earmarked dollar may leak out and be spent on other purposes. However, large standard errors prevent us from measuring the effect precisely. Using first difference and 3-year difference estimator, the evaluated probability of $\alpha_1 > \alpha_2$ becomes smaller and the probability that the earmarked money is fungible ($\alpha_1 < 1$) is about 90 percent. However, the 5-year difference estimator produces a higher probability of $\alpha_1 > \alpha_2$ and a lower probability of $\alpha_1 < 1$, compared to the fixed effect estimator.

In Table 4-3, we present estimated coefficients of equation (4-1) and (4-2) using sub-sample 2 of the "Changers" and the "General Revenue" groups. We find the coefficient on α_1 ranging between 0.72 and 0.78. Out of every extra dollar of earmarked lottery profits, about three quarters find its way into K-12 education. In contrast, the coefficient on α_2 is uniformly smaller, indicating that an extra dollar out of non-earmarked lottery profits raises K-12 spending by only 37 cents. In this sub-sample, results are more similar across the four specifications — the within-group estimator produces similar results to the short- and long-term difference estimators.

In the middle of Table 4-3, we report the F-test on the hypothesis that impacts on educational expenditures are the same whether the money is earmarked or not. Although the point estimate of α_1 is almost twice the scale of α_2, the hypothesis cannot be rejected

[58]Although our regression results suggest that earmarking funnels a larger fraction of a lottery dollar into education, we can reject the hypothesis that K-12 spending increases dollar for dollar with earmarked profits. In other words, substitution of earmarked money for other purposes exists to a certain extent. It is reasonable to assume such adjustments in state budgets take place over a longer period. Thus we decide to focus on estimates obtained using long-term difference.

with a p-value of 0.05 because of the large standard error on the coefficient α_1. The large standard error is driven by the fact that we have only nine states in the treatment group. The Bayesian inference probabilities provide more convincing evidence that the probability of $\alpha_1 > \alpha_2$ is about 87 percent. There is a 70 percent chance that α_1 is less than one, which suggests that earmarking increases spending, though not at a dollar for dollar rate.

Overall, we find in our regressions that about 60-80 cents out of every dollar is spent on public education if the money is earmarked for K-12 schools. Our findings are consistent with the large literature in public finance on the “flypaper effect,” as well as the results in Novarro (2002). According to economic theory, intergovernmental grants are equivalent to increases in income and these grants should not increase spending by more than an equal increase in income. In a regression with some per capita spending category as the outcome variable, the coefficient on per capita income is usually somewhere in the range of 0.05 to 0.10 — indicating that for every dollar in income, categorical spending increases by five to ten cents. However, dozens of studies in the “flypaper effect” literature have demonstrated that money “sticks where it hits.” Hines et al. (1995) review a number of empirical tests of the flypaper effect and conclude that unrestricted block grants do increase spending dollar for dollar. Though in a different context, the stickiness of the money found in our regression falls into the range of the flypaper effect found in the previous studies (Gramlich 1977).

Another result we find interesting is that states on average spend about 17 to 18 percent of their total budget on public education, but the marginal non-earmarked lottery dollar generates greater spending on public education. In particular, our results suggest that in the states of the "General Revenue" group, each additional dollar from lottery profits generates about 40 to 50 cents in educational spending. The difference between the marginal and average propensities to spend on public education suggests that public school spending has some priority in the budgetary process when it comes to determining expenditures out of lottery profits. To provide further evidence on this distinction, we compare the impacts on school expenditures of lottery money with the impacts of other "sin taxes," such as the tobacco tax and alcohol revenues. Alcohol revenues are constructed from the summation of liquor store profits and revenues from alcoholic beverage taxes in each state. As indicated above, tobacco and alcohol revenues together generate about the same amount of money for states as lottery profits. We re-run the regressions of equations (4-1) and (4-2) adding the variable of "sin taxes." Results are presented in Table 4-4.

The upper panel of Table 4-4 contains estimates using sub-sample 1, while the lower panel contains estimates using sub-sample 2. As we can see, adding sin taxes into the regression does not change the significance and scale of the point estimates of α_1, α_2, and α_3. The effects of sin taxes on state school expenditures are generally not significant, and the point estimates are on average not larger than the average propensity to spend on schools. As a source of revenue, lottery profits constitute about the same proportion of the total state budget as tobacco tax and alcohol revenue. However, the contribution to school expenditure is larger from lottery profits than from the other sin taxes.

It should be noted that most states experienced a steady decline in alcohol revenues during the sample period, which in many cases was offset by rising cigarette taxes. After fitting a state specific time trend, there may not be much within state variation left to identify its impact on school expenditure.

As we discussed in the previous section, standard errors estimated in the conventional fixed effects model could be biased due to serial correlation in error terms. To obtain robust estimates of the standard error, we apply the econometric method suggested by Anderson & Hsiao (1982) to correct for serial correlation in error terms, and report the robust estimates in the right column of Table 4-5. Corresponding estimates from conventional fixed effects model are recorded in the left column. As expected, conventional fixed-effects estimates tends to understate the standard error. Although estimates obtained from robust estimation are less significant than their counterparts from conventional fixed effects model, the scale of point estimates of α_1 is still larger than that of the point estimate of α_2.

In the upper panel we report estimates obtained using sub-sample 1. The robust estimate of α_1 is of the same scale as the fixed effects estimates, statistically significant with a p-value of 0.031. The robust estimate of α_2 is smaller than its counterpart in the left column, and the robust estimate of α_3 is larger than the corresponding fixed effects estimates. Both the robust estimates of α_2 and α_3 are not statistically significant at 95 percent confidence. In the lower panel, we report robust estimates using sub-sample 2. The robust estimates of α_1 and α_2 are smaller than the conventional fixed effects estimates, with a p-value of 0.083 and 0.389 separately.

4.5.2 Anecdotal Evidence on Possible Selection Bias

Some may argue that states earmarking lottery profits for education would spend more overall on schools than states not earmarking, even in the absence of earmarking. In other words, those states that have a higher propensity to spend on public education are more likely to earmark lottery profits for education anyway. If this were the case, our estimates of the earmarking effects would be contaminated without controlling for factors that lead to the selection. The selection bias is most relevant in states that changed the allocation of lottery profits from general revenue to school funds in sub-sample 2. We should note that this type of selection bias would tend to generate a large coefficient for earmarked lottery dollars over non-earmarked dollars, but it would not explain why we obtain a coefficient on earmarked dollars that is less than 1.

To get an idea of the extent to which our estimated coefficients may be contaminated, we look for historical reasons that the five states in sub-sample 2 may have altered the allocation of lottery profits. As shown in Table 2-1, Montana changed the allocation of net lottery revenue from K-12 schools to the general revenue fund in 1996. The other four states, Illinois, Missouri, Oregon and Texas, switched from general revenue to K-12 school funds.

As outlined in the conclusion, Montana moved from earmarking to non-earmarking because some educators believed that earmarking was generating the illusion that the state was spending more on K-12 schools than it actually was.

In Missouri, when the state lottery was authorized in 1984, the legislation did not stipulate that all lottery profits would be used for education. However, voters were led to believe so during the campaign for the initiation of lottery.[59] Later, voters found out that the money was put into state general revenues and was used for all state services, with elementary and secondary education getting only about 26 percent.[60] In August 1992, Amendment 11 was passed, earmarking state lottery proceeds for public education. It should be pointed out that prior to the approval of Amendment 11, voters rejected an amendment seeking to raise property taxes for education. In the previous year, voters also rejected Proposition B to raise income tax for education. In other words, it is clear that in Missouri, voters wanted lottery profits spent on education; it is, however, not clear that they wanted more money overall spent on education.

When the Texas lottery was adopted in 1992, the majority of Texans believed that the fund would be set aside for the Texas school system. Lottery profits were, however, deposited into the general fund.[61] In 1997, Governor G. W. Bush proposed to cut school property taxes and at the same time increase the state tax. However, opposition to a tax hike was so strong that State Representative Williamson suggested putting $1 billion plus annual lottery proceeds into the Permanent School Fund.[62] Effective September 1, 1997, all lottery profits was allocated to the Foundation School Fund.

In Oregon, lottery proceeds had been allocated to economic development since 1985. However, in 1990, Measure 5 was approved to cut local property tax. The state government was then expected to cover budget shortfalls in school expenditures.[63] To deal with this anticipated shortfall, the legislature included educational expenditures as

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[59] "Schools get full cut of gambling money," 04/26/1993, Five Star Edition.
[60] "Parents back tow measures amendments would aid schools with easier taxation methods," 07/27/1992, Five Star Edition.
[61] "Lottery's millions could be for schools," 05/01/1997, ALAMO.
[62] "Lottery earnings proposed for school fund," 03/06/1997, Austin American-Statesman.
[63] "Carving up the lottery," 12/11/1995, Portland Oregonian.
"Hooked on Gambling," 11/22/1996, Portland Oregonian.

part of economic development programs. More than half of the lottery profits were spent on education.[64] Lawmakers worried about legal challenges based on the expansive definition of economic development, so they proposed a constitutional amendment (Measure 21) that linked the lottery to education and resolved the uncertainty. Measure 21 was passed and the financing of public education was added to the allowable uses of lottery proceeds. The case of Oregon does not pose much of a concern about selection bias in our work. First, because Oregon is one of the states with major property tax reform, we control for the shift of more spending at the state level with our property tax reform legislative dummy variables. Second, the shift to more state support for K-12 education occurs in 1990 in Oregon, but the earmarking policy did not start until 1997, during the last fiscal year of our sample.

The state that is most likely to cause selection bias in our "Changers" sample is Illinois. In 1985, the Illinois General Assembly passed wide-ranging reforms in state schools. Lottery profits, together with an increase in the tax on cigarettes and a tax on interstate telephone calls, were collected to support the reform. If Illinois posed selection bias, then our estimates of α_1 would be biased upwards. However, we re-run the regression without the state of Illinois and obtain even larger estimates for the earmarking states.

The basic results in Tables 4-2 and 4-3 are not sensitive to major alterations of the model. If we aggregate state revenues allocated to K-12 education over all states, we can see that total state revenues per capita allocated to K-12 education are flat in the early 1980s, and then increase steadily thereafter. To determine whether our results are

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[64] "Let schools win lottery," 01/25/1995, Portland Oregonian.

sensitive to different sample periods, we restrict our sample period 1983–1998, and re-run the regressions using the two sub-samples. We find no significant change in our estimates. As a second specification test, we alter the composition of our control sample. In the previous regressions, we use the "General Revenue" group as a control group. To help establish the underlying trend in K-12 revenues, we expand the sample to include those states without the lottery and find no significant change in the estimates of the impacts of lottery profits.

4.6 An Alternative Model: Two-Stage Least Squares

The models outlined in the previous section are intuitively appealing because they measure within state correlation in lottery profits and K-12 spending where the unexplained period-to-period movements in lottery profits identify the models. The shortcoming of the models is that, they leave unexplained exactly why there are changes in lottery profits. In this section, we model more closely the period-to-period changes in lottery profits and use this information to examine the impact of earmarked profits.

As mentioned in section II, there are several major within-state developments over time that can quickly change per capita lottery profits, including the introduction of a state lottery, the introduction of new games, and the legalization of other gambling within a state. These discrete events should lead to a "first-stage" relationship between the events and lottery profits. The positive or negative shock to lottery profits should eventually lead to a change in expenditures on K-12 education. Comparing the impact that the shock has on lottery profits and its eventual impact on K-12 education should provide some indication of what fraction of the marginal dollar is spent on education. For example, if the introduction of a Lotto-style game increases profits by $20 per person and K-12 spending by $15 per person, then one could conclude that 75 cents of every new dollar in lottery profits is destined for education.

There are, however, several factors that may complicate this type of analysis. First, the comparison of the "first-stage" and "reduced-forms" outlined above is straightforward only when looking at one discrete event in isolation. In practice, there are many events that we can measure that should change lottery profits. Consequently, we need an alternative model that will allow us to aggregate results over many events. In this instance, we can think of the discrete events as instruments for lottery profits in an educational expenditure equation and use two-stage least squares to tie the first-stage and reduced-form together. In the 2SLS model, predicted lottery profit is entered into the equation of interest. Angrist et al. (1996) argue that 2SLS can be thought of as a local average treatment effect that measures the impact of the covariate of interest (lottery profits) on the outcome (K-12 spending) for those observations whose behavior has changed as a result of receiving the treatment. In this case, the 2SLS coefficient on lottery profits will measure the change in K-12 spending when lottery profits change by a dollar as a result of the discrete events. A second complicating factor is that we have two types of states (those that earmark profits and those that do not) and, given the results in the previous section, we suspect that the first-stage and reduced-form relationships might vary across these groups. Consequently, we will estimate models for these two groups separately.

We have identified a number of events that should alter per capita lottery profits in an immediate and measurable manner. Given the time range in our sample, some states have lotteries throughout the entire period whereas some states have only introduced a lottery recently. The first variable we construct is a simple indicator that equals one in years the lottery is in operation and zero otherwise. Many states adopt lotteries in stages, first introducing instant games and daily numbers, then larger jackpot games, such as lotto and multi-state games. The second variable we construct is another dummy variable that equals one beginning in the first year a state runs an in-state lotto game. As Cook and Clotfelter (1989) discuss, the success of lotto games is predicated on having large jackpots. When the jackpot is not claimed in a particular drawing, the prize rolls over to the next drawing. The number of lotto players is highly nonlinear in the size of the jackpot. Many smaller states that run lotto games never see jackpots in the tens-of-millions-of-dollar-range on their in-state lotto games. As a result, a number of smaller states joined together in order to offer multi-state lotto games, pooling population across borders and increasing the likelihood of larger jackpots in an attempt to increase profits. There are now three major multi-state lotto games: Power Ball, the Big Game and Hot Lotto. We construct a dummy variable that equals one when a multi-state lotto game is in operation in a state. Some states have installed video lottery terminals (VLTs) in bars, restaurants and racetracks where people can play poker, blackjack, keno and bingo over the terminals. We include a dummy variable that equals one beginning at the time of the installation of VLTs in a state. Finally, there has been tremendous growth in casino style gaming in this country since 1978. Prior to that time, casinos were only legal in Nevada. In 1978, the casinos in Atlantic City opened for the first time. Since then, there has been an explosion in the number of states that allow casino gaming. There are two major types of gaming operations outside of Nevada and Atlantic City. First, passage of the Indian

Gaming and Regulatory Act in 1978 allows tribes in some states to run casinos. There are more than 220 Las Vegas-style casinos on Indian reservations. Second, some states have also allowed gaming on riverboats and racetracks. There are now casinos operations in twenty-four states other than Nevada and New Jersey. Evans et al. (2002) have constructed a master list of these casinos with their zip odes and opening dates. We use this list to identify counties with casinos and the date at which they opened. We include as a covariate in these regressions the fraction of people living in counties with a casino.

In the bottom portion of Table 4-6, we report the results of the first-stage regression for states in the “K-12” and “General Revenue” groups separately. As in the previous regressions, state and year fixed effects, state specific time trends, and all other covariates used are included in the regression. In the table, we report the coefficients on the events that potentially could alter per capita lottery profits. In the left column of K-12 states, we can see that the adoption of the lottery increases total profits by $23 per person, the adoption of an in-state lotto game adds an additional $17 per person, and the adoption of a multi-state lotto game increases net revenues by a statistically significant $11 per person. The p-value for the F-test on the significance of the instruments in the first-stage regression is less than 0.001. Using 2SLS, our estimated earmarking effect is 0.785, close to the within-group estimates reported in Table 4-4.

We apply the same estimation procedure to the fifteen “General Revenue” states and add VLTs as another instrument. There are four states in this group that introduced video lottery games in the early 90s: Delaware, South Dakota, Rhode Island and West Virginia. The introduction of VLTs increases lottery profits by $47 per person. We find a significantly positive impact of in-state lotto games on profits, but insignificant impacts of multi-state lotto games. The p-value for the F-test on the significance of the instruments in the first-stage regression is less than 0.001. The coefficient on lottery profits that are deposited into the general fund is 0.585, which is close to our previous estimates from Table 4-2. In the 2SLS estimations using the "K-12" and "General Revenue" groups separately, the p-values on the test of over-identifying restrictions are rather large and we cannot reject the null hypothesis that the models are correctly specified.

It is worth noting that although Powerball[65] and the Big Game[66] are the well-known lotto games among states, introducing these games do not guarantee a boost in lottery profits. Just examining the time series of lottery profits in a few states, we find noticeable drops in lottery profits after Maryland and Virginia adopted the Big Game and a sharp decline in profits in Idaho after it adopted Lotto America, the precursor to Powerball. It may be the case that multi-state lotto games like Powerball and the Big Game cannibalize other lottery games.

4.7 Conclusion

Although state lotteries have been a financial success, some voters and elected officials are hesitant to allow states into the gambling business. A popular device to encourage support for lotteries has been to earmark profits for particular program. Of the thirty-eight states with lotteries, twenty-five states currently earmark monies with the vast

[65] In the "K-12" group, four states have joined the Powerball, including Georgia, Idaho, New Hampshire, and New Mexico. In the "General Revenue" group, ten states have joined the Powerball game, including Arizona, Connecticut, Delaware, Indiana, Iowa, Kentucky, Louisiana, Rhode Island, South Dakota, and West Virginia.
[66] Georgia and Michigan in the "K-12" group, and Maryland and Virginia in the "General Revenue" group have joined the Big Game.

majority of these states using profits for K-12 education. The effectiveness of earmarking has, however, been questioned by many. The concerns about earmarking are illustrated nicely in the history of the Montana lottery. When Montana began the lottery in 1987, lottery profits were earmarked for public education. In 1995, the state legislature decoupled lottery profits from school financing. As the President of the Montana Education Association noted, it was an "illusion" that lottery profits were a big help to public schools. Although the lottery transferred almost $42 million directly into school funding between 1987 and 1994, the amount was less than 1 percent of the total state school budget during the same period.[67]

Whether earmarking increases spending is, in the end, an empirical question. Our paper addresses this issue by examining the experiences of lottery states over the past twenty years. Given previous evidence on the "flypaper effect," it is not surprising that we find up to three quarters of an earmarked lottery dollar finds its way to public schools. There is a high probability that a dollar of earmarked lottery profits generates more spending on K-12 schools than the spending generated from a dollar of lottery profits put into the general fund. It is also interesting to note that, states with lotteries spend a larger share of the marginal lottery dollar on education than income generated from other sources, such as alcohol and cigarette taxes.[68] Though our findings suggest that earmarking lottery profits for K-12 education increases school spending, a handsome fraction of earmarked money is fungible. There is a high likelihood that a dollar of earmarked lottery profits generates less than a dollar of spending on K-12 education.

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[67] "Chancey revenues: A review of gambling in Montana," 01/01/1995, Montana Business Quarterly.

[68] It should be pointed out that lottery profits are a small fraction of total spending on public education. Even though earmarking makes a large proportion of lottery profits available for public schools, lottery profits can never be the major revenue source financing public schools.

Chapter 5: Concluding Remarks

Lotteries are not only a popular source of state revenue, but also are products marketed and sold to the general public directly by the state government. Although it is the statutory duty of a lottery agency to maximize net revenues, the lottery agency's ability to maximize profits is limited due to state regulations such as specifications of lottery payout ratios, caps on lottery advertising budgets, and the annual review of advertising budgets. Contrary to the public concern that states over-spend on lottery advertising, our results suggest that if the goal is to maximize profits, most states spend too little on lottery advertising. We should note that our conclusions are drawn on positive analysis, the social optimal scale of lottery advertising might be smaller than the scale that maximizes profits if the distributional impact of lottery provision is taken into consideration.

Another interesting result of this dissertation is that we find evidence that earmarking alters a state's spending pattern. When a state decides to earmark lottery profits for public education, it is usually stipulated in state codes that lottery profits should supplement not substitute current spending on public schools. But it is often doubted that earmarked money is fungible. Analyzing the experiences of state lotteries in the past twenty years, we find that earmarking is a binding constraint on state budgetary decisions, and that earmarked lottery profits increase state spending on targeted educational programs.

Table 2-1 State Allocation of Lottery Profits as of 2001

State	Lottery Begun	Initial Allocation	Allocation Switched	Current Allocation
Arizona	1981	General Revenue[1]	-	
California	1985	K-12 Education	-	
Colorado	1983	State Capital Construction Fund	1992	Parks and recreation
Connecticut	1972	General Revenue	-	
Washington DC	1982	General Revenue	-	
Delaware	1975	General Revenue		
Florida	1988	Education	-	
Georgia	1993	Education	-	
Idaho	1989	Education (K-12 50%)	-	
Illinois	1974	General Revenue	1985	K-12 Education
Indiana	1989	General Revenue[1]	-	
Iowa	1985	General Revenue	-	
Kansas	1987	Economic Development (85%) Prison (15%)	-	
Kentucky	1989	General Revenue[1]	-	
Louisiana	1991	General Revenue	-	
Maine	1974	General Revenue	-	
Maryland	1973	General Revenue	-	
Massachusetts	1972	Cities and Towns	-	
Michigan	1972	K-12 Education	-	
Minnesota	1990	General Revenue (60%) Environment (40%)	-	

Table 2-1 State Allocation of Lottery Profits as of 2001 (Continued)

State	Lottery Begun	Initial Allocation	Allocation Switched	Current Allocation
Missouri	1986	General Revenue	1992	Education
Montana	1987	K-12 Education	1995	General Revenue
Nebraska	1993	Environment, Education (49.5%), Compulsive Gamblers	-	
New Hampshire	1964	K-12 Education	-	
New Jersey	1970	Education and Institution	-	
New Mexico	1996	Education	-	
New York	1967	K-12 Education	-	
Ohio	1974	K-12 Education	-	
Oregon	1985	General Revenue	1997	K-12 Education (15%)
Pennsylvania	1972	Senior Citizens Program	-	
Rhode Island	1974	General Revenue[1]	-	
South Carolina	2002	Education	-	
South Dakota	1987	General Revenue[1]	-	
Texas	1992	General Revenue	1997	K-12 Education
Vermont	1978	General Revenue	1998	K-12 Education
Virginia	1988	General Revenue	1999	K-12 Education
Washington	1982	General Revenue	2001	K-12 Education

Source: State Lottery webpage, State Lottery Publications, and State Lottery Laws.

[1] Lottery profits are treated as general revenue if the money is allocated to general revenue fund and other expenditure funds without a specific share arrangement. Source: State lottery web sites

Table 2-2 State Law on Lottery

State	State Codes	Administrative Nature
Arizona	Title 5 Chap. 5	Independent agency
California	Title 2 Div. 1 Chap.12.5	Independent agency
Colorado	24-35-205(7) C.R.S.	Within Dept. of Revenue
Connecticut	Chap. 229a	Within Dept. of Revenue Services
Delaware	Chap. 348, Vol. 59.	Within Dept. of Finance
Florida	Title IV Chap. 25	Independent agency
Georgia	50-27	Independent agency
Idaho	Title 67	Independent agency
Illinois	20 ILCS 1605	Independent agency
Indiana	IC4-30-1 Chap. 1	Independent agency
Iowa	Chap. 99E	Within Dept. of Revenue & Finance
Kansas	K.S.A. 74-8701 -- 8732	Independent agency
Kentucky	154A.	Independent agency
Louisiana	RS 47: §9001	Independent agency
Maine	Title 8 Chap.14A	Within Dept. of Administrative and Financial Services
Maryland	Art. 88D §§ 7, 8	Independent agency
Massachusetts		Within Dept. of State Treasurer
Michigan	Lottery Act 432	Within the Department of Treasury
Minnesota	349A	Independent agency
Missouri	313	Within Dept. of Revenue
Montana	23-7	Independent agency

Table 2-2 State Law on Lottery (Continued)

State	State Codes	Administrative Nature
Nebraska	REG 408	Within Dept. of Revenue
New Hampshire	XXIV 284: 21	Independent agency
New Jersey	5:09	Within the Dept. of the Treasury
New Mexico	6-24	Independent agency
New York	Tax Law 1909Chap. 60 34 s1600-20	Within the Dept. of Taxation and Finance
Ohio	37-70	Independent agency
Oregon	ORS461.2	Independent agency
Pennsylvania	PA Code V	Independent agency
Rhode Island	42-61	Independent agency
South Carolina	59-150	Independent agency
South Dakota	42-7A	Independent agency
Texas	Code, Chap. 466.	Independent agency
Vermont	31-14	Independent agency
Virginia	58.1-402.2E.	Independent agency
Washington	RCW 67.70	Independent agency
West Virginia	29-22	Independent agency
Wisconsin	Chap. 565	Within Dept. of Revenue

Source: State Codes/Laws/Statutes.

Table 2-3 Net Gambling Revenues by Industry, 1982 and 2000

Industry	Net Revenues in Billions of constant 1995-6 dollars (Industry market share) 1982	2000
Nevada/Atlantic City Casinos	$6.9 (40.3%)	$12.5 (22.2%)
Lotteries	$3.6 (20.8%)	$15.8 (28.1%)
Horse racing	$3.7 (21.6%)	$3.0 (5.4%)
Native American gambling	-----	$9.5 (17.0%)
River boat casinos	-----	$8.2 (14.6%)
Other gambling	$2.9 (17.2%)	$7.1 (12.7%)
Total	$17.1	$56.2

Source: Christiansen Capital Advisors

Table 3-1 Sample Characteristics

Variable	Mean (Standard Deviation)
Total monthly ticket sales per capita	10.452 (7.084)
Total monthly advertising per capita	0.039 (0.031)
Monthly state personal income (average)	8,235.09 (1,241.90)
Monthly state unemployment rate	5.024 (1.503)
Share of state population aged 18-21	0.075 (0.011)
Share of state population aged 22-29	0.148 (0.020)
Share of state population aged 30-44	0.327 (0.025)
Share of state population aged 45-65	0.285 (0.024)
Share of state white population	0.883 (0.083)
Share of state Black population	0.085 (0.081)
Share of state Hispanic population	0.061 (0.079)
Share of state population with some high-school education	0.104 (0.021)
Share of state population of high-school graduates	0.344 (0.042)
Share of state population with some college education	0.265 (0.037)
Share of state population of college graduates	0.224 (0.043)
Share of population living in the central city and balance of MSA area	0.697 (0.220)
Share of state male population	0.481 (0.012)
Share of state married population	0.593 (0.030)
Number of states	37
Number of observations	4682

Sources: La Fleur's
Competitive Media Reporting Company
Bureau of Economic Analysis
U.S. Census Bureau
Bureau of Labor Statistics

Table 3-2 Comparison of Average Monthly Sales and Advertising Spending of Illinois State Lottery Before and After the TV Commercial Restriction

Mean and (Standard Deviation)				
($ per capita)	Before Treatment	After Treatment	Change in Percentage	T Value (H_0: Mean $_{after}$ = Mean $_{before}$)
Total Ads	0.057 (0.018)	0.039 (0.024)	-31.6%	-4.14
TV Ads	0.049 (0.018)	0.011 (0.011)	-77.6%	-14.83
Ads on other media	0.009 (0.004)	0.028 (0.020)	211.1%	5.78
Total Sales	11.269 (1.806)	8.967 (1.389)	-20.4%	-6.87
N	90	36	---	---

Table 3-3 Interrupted Time Series Analysis of Sales and Advertising Spending of Illinois State Lottery Before and After the TV Commercial Restriction

Parameter Estimates and (Standard Errors)		
	Ads Spending Per Capita	Log (Sales Per Capita)
Common Time Coefficient	0.00012	-0.0027
	(0.00008)	(0.0006)
Treatment Time Coefficient	-0.00023	-0.0005
	(0.00006)	(0.0004)
Durbin-Watson Test Statistics (P-value for testing positive autocorrelation)		
of order 1	1.100	2.159
	(<.0001)	(0.763)
of order 2	1.822	2.166
	(0.138)	(0.802)
of order 3	1.910	1.435
	(0.310)	(0.001)
Mean of the Dependent Variable	0.052	2.345
Number of Observations	126	126
R^2	0.151	0.412

The covariates included in the regression model are monthly time trend and the interaction term of advertising reduction dummy variable with the monthly time trend. Estimated parameters of monthly time trend and D-W test statistics do not change significantly by adding monthly fixed effects.

Table 3-4 2SLS Estimates of the Effect of Advertising on Sales Using TV Commercial Restriction in Illinois as Instruments (States with Similar Pre-treatment Trends as Controls)

Parameter Estimates and (Standard Errors)

Variable	Coefficient	
	OLS	Robust OLS [a]
Control by states with similar pre-treatment trend in log(per capita sales) and per capita Ads spending: AZ CA CT FL ID IN IA KY LA ME MN MT NE NH NJ NM OR.		
First-Stage Estimate		
Treatment Dummy	-0.023 (0.004)	-0.022 (0.006)
Reduced-Form Estimate		
Treatment Dummy	-0.099 (0.038)	-0.091 (0.046)
2SLS Estimate		
Per Capita Advertising Expenditure	4.242 (1.722)	4.117 (2.239)
R^2	0.888	0.915
Number of Observations	1974	1889
Control by states with similar pre-treatment trend in log(per capita sales) and per capita Ads spending excluding neighbor states: AZ CA CT FL ID LA ME MN MT NE NH NJ NM OR.		
First-Stage Estimate		
Treatment Dummy	-0.023 (0.004)	-0.022 (0.006)
Reduced-Form Estimates		
Treatment Dummy	-0.093 (0.040)	-0.088 (0.048)
2SLS Estimate		
Per Capita Advertising Expenditure	4.114 (1.881)	4.051 (2.389)
R^2	0.894	0.990
Number of Observations	1608	1532

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-5 Calculated Sales Elasticity and Gross Rate of Return to Advertising __ Illinois Case

Parameter Estimates and (Standard Errors)	OLS Estimates	Robust OLS Estimates
Semi-elasticity Estimated in the Upper Panel of Table 3-4	4.242 (1.722)	4.117 (2.239)
Average Ads Spending and Sales in the Treatment Period		
Average Ads Spending per capita in the Treatment Period	0.039 (0.024)	0.039 (0.024)
Average Sales per capita in the Treatment Period	8.967 (1.389)	8.967 (1.389)
Calculated Elasticity and Gross Rate of Return to Advertising		
Elasticity of Sales to Advertising	0.165	0.161
Gross Rate of Return to Advertising	38.04	36.92

Table 3-6 2SLS Estimates of the Effect of Advertising on Sales Using TV Commercial Restriction in Illinois as Instruments (States with Similar Game Mix as Controls)

Parameter Estimates and (Standard Errors)		
Variable	Coefficient	
	OLS	Robust OLS [a]
Control by states with Big Game: GA MD MI VA as controls.		
First-Stage Estimate		
Treatment Dummy	-0.033 (0.008)	-0.034 (0009)
Reduced-Form Estimates		
Treatment Dummy	-0.113 (0.045)	-0.116 (0.040)
2SLS Estimate		
Per Capita Advertising Expenditure	3.461 (1.569)	3.374 (1.350)
R^2	0.776	0.757
Number of Observations	610	592
Control by states without Keno Game: AZ CT DE FL ID IN IA KY LA ME MN MO MT NE NH NJ NM OH PA TX VA WI.		
First-Stage Estimate		
Treatment Dummy	-0.014 (0.004)	-0.017 (0.006)
Reduced-Form Estimates		
Treatment Dummy	-0.069 (0.038)	-0.089 (0.042)
2SLS Estimate		
Per Capita Advertising Expenditure	4.854 (2.899)	5.198 (2.907)
R^2	0.864	0.925
Number of Observations	2521	2421

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-7 2SLS Estimates of the Effect of Advertising on Sales Using TV Commercial Restriction in Illinois as Instruments

Parameter Estimates and (Standard Errors)		
Variable	Coefficient	
	OLS	Robust OLS [a]
Control by all lottery states except MA and WA.		
First-Stage Estimate		
Treatment Dummy	-0.014 (0.005)	0.016 (0.005)
Reduced-Form Estimates		
Treatment Dummy	-0.136 (0.036)	-0.132 (0.039)
2SLS Estimate		
Per Capita Advertising Expenditure	9.804 (3.943)	8.127 (3.478)
R^2	0.767	0.876
Number of Observations	3713	3580
Control by non-neighboring states: all states except IA IN KY MO WI MA and WA.		
First-Stage Estimate		
Treatment Dummy	-0.013 (0.005)	-0.017 (0.006)
Reduced-Form Estimates		
Treatment Dummy	-0.114 (0.037)	-0.110 (0.043)
2SLS Estimate		
Per Capita Advertising Expenditure	8.531 (3.922)	6.488 (3.258)
R^2	0.799	0.881
Number of Observations	3095	2977

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-8 Estimates of the Retention Rate of Advertising __ Illinois Case

Parameter Estimates and (Standard Errors)

	Estimated Coefficient
Including Ads Spending of the Past 12 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.405 (1.146)
Retention Rate (δ)	0.627 (0.334)
Including Ads Spending of the Past 11 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.420 (1.136)
Retention Rate (δ)	0.626 (0.329)
Including Ads Spending of the Past 10 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.256(1.065)
Retention Rate (δ)	0.664 (0.323)
Including Ads Spending of the Past 9 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.650 (1.330)
Retention Rate (δ)	0.554 (0.372)
Including Ads Spending of the Past 8 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.564 (1.272)
Retention Rate (δ)	0.579 (0.364)

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

Table 3-9 Comparison of Average Monthly Sales and Advertising Spending of Washington State Lottery in and out of the Summer TV Hiatus

Mean and (Standard Deviation)

($ per capita)	Out of Treatment	In Treatment	Change in Percentage	T Value (H_0: Mean $_{in}$ = Mean $_{out}$)
Total Ads	0.037 (0.019)	0.012 (0.006)	-67.6%	-8.55
TV Ads	0.032 (0.018)	0.002 (0.006)	-93.8%	-10.51
Ads on other media	0.005 (0.005)	0.009 (0.006)	80%	1.81
Total Sales	5.927 (1.298)	5.369 (1.295)	-9.4%	-1.03
N	126	6	---	---

Table 3-10 Interrupted Time Series Analysis of Sales and Advertising Spending of Washington State Lottery in and out of Summer TV Hiatus

Parameter Estimates and (Standard Errors)

		Ad. Spending Per Capita	Log (Sales Per Capita)
Common Time Coefficient		-0.0001	-0.0004
		(0.00004)	(0.00054)
Treatment Time Coefficient		-0.001	-0.0036
		(0.0003)	(0.003)
Durbin-Watson Test Statistics (P-value for testing positive autocorrelation)			
	of order 1	1.102	2.073
		(<.0001)	(0.598)
	of order 2	1.576	1.939
		(0.006)	(0.337)
	of order 3	1.805	2.020
		(0.137)	(0.558)
Mean of the Dependent Variable		0.036	1.755
Number of Observations		132	132
R^2		0.145	0.011

The covariates included in the regression model are monthly time trend and the interaction term of advertising reduction dummy variable with the monthly time trend. Estimated parameters of monthly time trend and D-W test statistics do not change significantly by adding monthly fixed effects.

Table 3-11 2SLS Estimates of the Effect of Advertising on Sales Using TV Hiatus in Washington as Instruments

Parameter Estimates and (Standard Errors)

Variable	Coefficient	
	OLS	Robust OLS [a]
Control by all lottery states except IL and MA.		
First-Stage Estimate		
Treatment Dummy	-0.024 (0.009)	-0.023 (0.009)
Reduced-Form Estimates		
Treatment Dummy	-0.160 (0.073)	-0.145 (0.063)
2SLS Estimate		
Per Capita Advertising Expenditure	6.632 (3.695)	6.162 (3.390)
R^2	0.833	0.909
Number of Observations	3906	3772
Control by non-neighboring states: all lottery states except ID, OR, MA and IL.		
First-Stage Estimate		
Treatment Dummy	-0.025 (0.009)	-0.024 (0.009)
Reduced-Form Estimates		
Treatment Dummy	-0.160 (0.074)	-0.139 (0.063)
2SLS Estimate		
Per Capita Advertising Expenditure	6.406 (3.571)	5.865 (3.325)
R^2	0.837	0.917
Number of Observations	3660	3535

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-12 Calculated Sales Elasticity and Gross Rate of Return to Advertising __ Washington Case

Parameter Estimates and (Standard Errors)		
	OLS	Robust OLS
Semi-elasticity Estimated in the Lower Panel of Table 3-10	6.406 (3.571)	5.865 (3.325)
Average Ads Spending and Sales in the Treatment Period		
Average Ads Spending per capita in the Treatment Period	0.012 (0.006)	0.012 (0.006)
Average Sales per capita in the Treatment Period	5.369 (1.295)	5.369 (1.295)
Calculated Elasticity and Gross Rate of Return to Advertising		
Elasticity of Sales to Advertising	0.077	0.070
Gross Rate of Return to Advertising	34.394	31.489

Table 3-13 2SLS Estimates of the Effect of Advertising on Sales Using TV Hiatus in Washington as Instruments (States with Similar Game Mix as Controls)

Parameter Estimates and (Standard Errors)

Variable	Coefficient	
	OLS	Robust OLS [a]
Control by states with Big Game: NJ NY OH as controls.		
First-Stage Estimate		
Treatment Dummy	-0.020 (0.013)	-0.019 (0.013)
Reduced-Form Estimates		
Treatment Dummy	-0.091 (0.071)	-0.090 (0.069)
2SLS Estimate		
Per Capita Advertising Expenditure	4.584 (4.408)	4.813 (4.707)
R^2	0.873	0.934
Number of Observations	528	516
Control by state Keno Game before 1993: CA KS MI OR.		
First-Stage Estimate		
Treatment Dummy	-0.026 (0.008)	-0.027 (0.008)
Reduced-Form Estimates		
Treatment Dummy	-0.111 (0.077)	-0.100 (0.075)
2SLS Estimate		
Per Capita Advertising Expenditure	4.205 (3.063)	3.771 (2.940)
R^2	0.815	0.677
Number of Observations	660	645

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-14 Comparison of Average Monthly Sales and Advertising Spending of Massachusetts State Lottery Before and After Advertising Caps

Mean and (Standard Deviation)

($ per capita)	Before treatment	After Treatment	Change in Percentage	T Value (H_0: Mean $_{before}$ = Mean $_{after}$)
Total Ads	0.062 (0.039)	0.0032 (0.0042)	-94.8%	-7.41
TV Ads	0.049 (0.034)	0.0025 (0.0027)	-94.9%	-6.80
Ads on other media	0.013 (0.010)	0.0017 (0.0036)	-86.9%	-5.25
Total Sales	31.318 (5.812)	42.573 (6.216)	37.4%	8.11
Sales of Traditional Online Games	12.292 (2.195)	8.321 (2.475)	-31.0%	-7.27
Sales of Instant Games	19.025 (2.571)	28.270 (4.904)	49.4%	8.45
N	24	107	---	---

Table 3-15 Interrupted Time Series Analysis of Sales and Advertising Spending of Massachusetts State Lottery before and after Advertising Caps

Parameter Estimates and (Standard Errors)

	Ad. Spending	Log (Total Sales)	Log (Traditional Online Game)	Log (Instant Games)
Common Time Coefficient	0.001 (0.0004)	-0.001 (0.003)	0.0002 (0.005)	0.002 (0.003)
Treatment Time Coefficient	-0.002 (0.0004)	0.004 (0.003)	-0.005 (0.005)	0.002 (0.003)
Durbin-Watson Test Statistics (P-value for testing positive autocorrelation)				
of order 1	0.934 (<.0001)	2.006 (0.447)	2.838 (1.000)	1.519 (0.002)
of order 2	1.209 (<.0001)	1.837 (0.157)	2.201 (0.861)	1.556 (0.004)
of order 3	1.275 (<.0001)	1.330 (<.0001)	1.193 (<.0001)	1.248 (<.0001)
Mean of the Dep. Variable	0.014	3.684	2.175	3.253
Number of Observations	131	131	131	131
R^2	0.476	0.443	0.621	0.498

The covariates included in the regression model are monthly time trend and the interaction term of advertising reduction dummy variable with the monthly time trend. Estimated parameters of monthly time trend and D-W test statistics do not change significantly by adding monthly fixed effects.

Table 3-16 Estimates of Impacts of Keno Introduction on Sales of Different Games

Parameter Estimates and (Standard Errors)

Keno States	Log(Total Sales)	Log (Instant Games)	Log (Traditional Online Game)
		State-specific Regression [a]	
California	0.233 (0.074)	0.301 (0.066)	-0.105 (0.111)
Colorado	0.066 (0.065)	0.068 (0.054)	0.086 (0.120)
Georgia	0.055 (0.055)	0.069 (0.083)	-0.019 (0.099)
Kansas	0.623 (0.090)	0.391 (0.096)	0.344 (0.154)
Maryland	0.149 (0.057)	0.093 (0.080)	-0.122 (0.083)
New York	0.309 (0.053)	0.564 (0.122)	0.044 (0.053)
West Virginia	0.336 (0.091)	0.002 (0.075)	0.259 (0.143)
		Pool of Keno States [b]	
All Keno states	0.119 (0.022)	0.142 (0.031)	0.007 (0.037)

a. We include state personal income per capita, state unemployment rate and a dummy variable of keno introduction in the state-specific regression.

b. We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

Table 3-17 2SLS Estimates of the Effect of Advertising on Sales of Traditional Online Games Using Advertising Caps in Massachusetts as Instruments (States with Similar Pre-treatment Trend as Controls)

Parameter Estimates and (Standard Errors)

Variable	Coefficient OLS	Coefficient Robust OLS [a]
Control by states with similar pre-treatment trend in log(per capita sales) and per capita Ads spending: all lottery states except AZ FL GA ID IL LA NY OR TX VA WA.		
First-Stage Estimate		
Treatment Dummy	-0.075 (0.006)	-0.066 (0.007)
Reduced-Form Estimates		
Treatment Dummy	-0.316 (0.063)	-0.283 (0.066)
2SLS Estimate		
Per Capita Advertising Expenditure	4.208 (0.889)	4.275 (1.055)
R^2	0.907	0.926
Number of Observations	2766	2668
Control by states with similar pre-treatment trend in log(per capita sales) and per capita Ads spending excluding neighbor states: all lottery states except AZ FL GA ID IL LA OR TX VA WA NY CT RI NH VT		
First-Stage Estimate		
Treatment Dummy	-0.076 (0.006)	-0.068 (0.007)
Reduced-Form Estimates		
Treatment Dummy	-0.271 (0.064)	-0.228 (0.064)
2SLS Estimate		
Per Capita Advertising Expenditure	3.592 (0.884)	3.357 (0.964)
R^2	0.915	0.926
Number of Observations	2454	2364

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-18 Calculated Sales Elasticity and Gross Rate of Return to Advertising __ Massachusetts Case

Parameter Estimates and (Standard Errors)		
	OLS	Robust OLS [a]
Semi-elasticity Estimated in the Upper Panel of Table 3-10	4.208 (0.889)	4.275 (1.055)
Average Ads Spending and Sales in the Treatment Period		
Average Ads Spending per capita in the Treatment Period	0.0025 (0.0027)	0.0025 (0.0027)
Average Sales of Traditional Online Games per capita in the Treatment Period	8.403 (1.933)	8.403 (1.933)
Calculated Elasticity and Gross Rate of Return to Advertising		
Elasticity of Sales to Advertising	0.011	0.011
Gross Rate of Return to Advertising	35.36	35.92

Table 3-19 2SLS Estimates of the Effect of Advertising on Sales of Traditional Online Games Using Advertising Caps in Massachusetts as Instruments (States with Similar Game Mix as Controls)

Parameter Estimates and (Standard Errors)

Variable	Coefficient OLS	Coefficient Robust OLS [a]
Control by states with Big Game before 1996: GA MD MI VA as controls.		
First-Stage Estimate		
Treatment Dummy	-0.045 (0.012)	-0.046 (0.012)
Reduced-Form Estimates		
Treatment Dummy	-0.316 (0.099)	-0.254 (0.086)
2SLS Estimate		
Per Capita Advertising Expenditure	6.911 (2.700)	5.597 (2.237)
R^2	0.511	0.559
Number of Observations	638	617
Control by states with Keno Games before 1994: CA KS MD MI OR RI WV.		
First-Stage Estimate		
Treatment Dummy	-0.077 (0.009)	-0.070 (0.011)
Reduced-Form Estimates		
Treatment Dummy	-0.108 (0.114)	-0.029 (0.102)
2SLS Estimate		
Per Capita Advertising Expenditure	1.140 (1.479)	0.413 (1.460)
R^2	0.876	0.874
Number of Observations	1017	987

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-20 2SLS Estimates of the Effect of Advertising on Sales of Traditional Online Games Using Advertising Caps in Massachusetts as Instruments

Parameter Estimates and (Standard Errors)

Variable	Coefficient	
	OLS	Robust OLS [a]
Control by all lottery states except IL and WA.		
First-Stage Estimate		
Treatment Dummy	-0.081 (0.006)	-0.072 (0.007)
Reduced-Form Estimates		
Treatment Dummy	-0.325 (0.060)	-0.236 (0.061)
2SLS Estimate		
Per Capita Advertising Expenditure	3.985 (0.773)	3.280 (0.875)
R^2	0.898	0.907
Number of Observations	3892	3755
Control by non-neighboring states: all lottery states except IL WA NY CT RI NH and VT.		
First-Stage Estimate		
Treatment Dummy	-0.068 (0.006)	-0.058 (0.007)
Reduced-Form Estimates		
Treatment Dummy	-0.309 (0.063)	-0.204 (0.065)
2SLS Estimate		
Per Capita Advertising Expenditure	4.509 (0.964)	3.526 (1.154)
R^2	0.896	0.901
Number of Observations	3448	3323

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

a. Robust OLS regression corrects for serial correlations in residual terms for each state separately.

Table 3-21 Estimates of the Retention Rate of Advertising __ Massachusetts Case

Parameter Estimates and (Standard Errors)

	Estimated Coefficient
Including Ads Spending of the Past 12 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	0.321 (1.148)
Retention Rate (δ)	1.126 (0.552)
Including Ads Spending of the Past 11 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	1.570 (2.074)
Retention Rate (δ)	0.849 (0.309)
Including Ads Spending of the Past 10 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	0.09 (0.412)
Retention Rate (δ)	1.460 (0.911)
Including Ads Spending of the Past 9 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	0.104 (0.500)
Retention Rate (δ)	-1.832 (1.083)
Including Ads Spending of the Past 8 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	2.529 (1.823)
Retention Rate (δ)	0.744 (0.266)
Including Ads Spending of the Past 7 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	2.550 (1.665)
Retention Rate (δ)	0.777 (0.249)
Including Ads Spending of the Past 6 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	3.108 (1.751)
Retention Rate (δ)	0.679 (0.301)
Including Ads Spending of the Past 5 Months in the Regression of Equation (3-4)	
Short-term Advertising Effect (λ_0)	3.364 (1.949)
Retention Rate (δ)	0.626 (0.414)

We include year and month effects in the model. Other covariates include state personal income per capita, state unemployment rate, share of population aged 18-21, share of population aged 22-29, share of population aged 30-44, share of population aged 45-65, share of white population, share of Black population, share of Hispanic population, share of population living in the central city or balance of the MSA area, share of population married, share of male population, share of population with some high school, share of population with high school diploma, share of population with some college, share of population with college diploma, linear, quadratic and cubic term of jackpot size of the Big Game and Powerball, and a set of dummy variables of lottery game structures.

Table 4-1 Summary Statistics of Selected Lottery States

Means and (Standard Deviations)

Variable	K-12 States	General Revenue	Earmark Non-Educ.	Changers States
State educational revenue per capita	453	456	434	385
	(188)	(148)	(122)	(106)
State personal income per capita	21,503	21,013	22,798	20,806
	(3,322)	(3,808)	(3,156)	(2,376)
Lottery profit per capita	27.74	29.52	30.93	22.11
	(27.10)	(31.51)	(33.31)	(27.03)
Share of the population currently enrolled in school	0.177	0.174	0.167	0.179
	(0.022)	(0.017)	(0.016)	(0.016)
Share of the population aged 65+	0.119	0.123	0.128	0.121
	(0.024)	(0.016)	(0.016)	(0.015)
Share of the population unemployed	0.033	0.031	0.028	0.034
	(0.009)	(0.009)	(0.008)	(0.008)
Share of white population	0.865	0.895	0.923	0.890
	(0.084)	(0.094)	(0.041)	(0.050)
Share of the population with some high school	0.123	0.120	0.099	0.110
	(0.024)	(0.030)	(0.026)	(0.021)
Share of the population with high school diploma	0.367	0.380	0.399	0.370
	(0.045)	(0.048)	(0.045)	(0.038)
Share of the population with some college	0.194	0.174	0.181	0.200
	(0.050)	(0.055)	(0.052)	(0.050)
Share of the population with college diploma	0.199	0.195	0.217	0.201
	(0.038)	(0.053)	(0.051)	(0.028)
Number of observations	189	315	168	105

Sources: National Center fro Education Statistics
Bureau of Economic Analysis
U.S. Census Bureau
Bureau of Labor Statistics

Table 4-2 State Educational Revenue Per Capita Equations: K-12, Earmark Non-Education and General Revenue Groups

Parameter Estimates and (Standard Errors)	Fixed Effects	First Differences	3-years Differences	5-years Differences
α_1 (Lottery profits in K-12 states)	0.603 (0.286)	0.012 (0.380)	0.453 (0.326)	0.779 (0.288)
α_2 (Lottery Profits in General Revenue states)	0.504 (0.142)	0.196 (0.209)	0.417 (0.171)	0.516 (0.164)
α_3 (Lottery profits in Earmark Non-Education states)	0.302 (0.280)	0.380 (0.440)	0.560 (0.332)	0.230 (0.292)
F-test (p-value) on Hypothesis H_0: $\alpha_1 = \alpha_2 = \alpha_3$	0.256 (0.774)	0.161 (0.851)	0.078 (0.925)	0.709 (0.493)
Bayesian Inferences				
Prob ($\alpha_1 < 1$)	0.917	0.995	0.953	0.779
Prob ($\alpha_1 > \alpha_2$)	0.622	0.335	0.539	0.789
Prob ($\alpha_2 > \alpha_3$)	0.741	0.353	0.350	0.807
Prob ($\alpha_3 > 0$)	0.860	0.805	0.953	0.784
Prob ($\alpha_1 > \alpha_2 > \alpha_3$)	0.465	0.118	0.191	0.636
Prob ($1 > \alpha_1 > \alpha_2 > \alpha_3$)	0.391	0.116	0.165	0.441
Prob ($1 > \alpha_1 > \alpha_2 > \alpha_3 > 0$)	0.309	0.036	0.136	0.322
N	672	640	576	512
Fiscal Years	1978-98	1979-98	1981-98	1983-98

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates included in the regression are state personal income, the share of the population enrolled in K-12 schools, the share of the population aged 65^+, the share of the population unemployed, the share of white population, the share of the population with some high school, the share of the population with high school diploma, the share of the population with some college, the share of the population with college diploma, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 4-3 State Educational Revenue Per Capita Equations: Changers and General Revenue Groups

Parameter Estimates and (Standard Errors)

	Fixed Effects	First Differences	3-years Differences	5-years Differences
α_1 (Lottery profits in K-12 states)	0.723 (0.313)	0.770 (0.374)	0.783 (0.376)	0.734 (0.356)
α_2 (Lottery Profits in General Revenue states)	0.378 (0.122)	0.161 (0.188)	0.370 (0.146)	0.366 (0.141)
F-test (p-value) on Hypothesis H_0: $\alpha_1 = \alpha_2$	0.819 (0.366)	3.421 (0.065)	1.727 (0.190)	1.243 (0.266)
Bayesian Inferences:				
Prob ($\alpha_1 < 1$)	0.811	0.732	0.718	0.772
Prob ($\alpha_1 > \alpha_2$)	0.877	0.969	0.886	0.869
Prob ($1 > \alpha_1 > \alpha_2$)	0.690	0.700	0.605	0.640
N	441	420	378	336
Fiscal Years	1978-98	1979-98	1981-98	1983-98

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates included in the regression are state personal income, the share of the population enrolled in K-12 schools, the share of the population aged 65+, the share of the population unemployed, the share of white population, the share of the population with some high school, the share of the population with high school diploma, the share of the population with some college, the share of the population with college diploma, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 4-4 Comparison of Impacts on State Educational Revenue of Alternative Sin Taxes

Parameter Estimates and (Standard Errors)				
	Fixed Effects	First Differences	3-years Differences	5-years Differences
K-12, Earmarking Non-Education, and General Revenue Groups				
Sin Taxes	0.135	0.301	0.328	0.204
	(0.197)	(0.251)	(0.230)	(0.228)
α_1 (Lottery profits in K-12 states)	0.5990	0.006	0.436	0.774
	(0.286)	(0.380)	(0.325)	(0.288)
α_2 (Lottery Profits in General Revenue states)	0.510	0.200	0.430	0.527
	(0.143)	(0.209)	(0.171)	(0.164)
α_3 (Lottery profits in Earmark Non-Education states)	0.314	0.380	0.569	0.241
	(0.281)	(0.440)	(0.332)	(0.292)
Changers and General Revenue Groups				
Sin Taxes	0.226	0.434	0.194	-0.030
	(0.256)	(0.307)	(0.289)	(0.284)
α_1 (Lottery profits in K-12 states)	0.723	0.778	0.786	0.735
	(0.313)	(0.374)	(0.377)	(0.356)
α_2 (Lottery Profits in General Revenue states)	0.381	0.167	0.375	0.365
	(0.122)	(0.188)	(0.146)	(0.142)

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates included in the regression are state personal income, the share of the population enrolled in K-12 schools, the share of the population aged 65^+, the share of the population unemployed, the share of white population, the share of the population with some high school, the share of the population with high school diploma, the share of the population with some college, the share of the population with college diploma, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 4-5 Estimated Effects after Correction of Serial Correlation in Residuals

Parameter Estimates and (Standard Errors)

	Fixed Effects	Robust OLS
K-12, Earmark Non-Education and General Revenue Groups		
α_1	0.603	0.553
(Lottery profits in K-12 states)	(0.286)	(0.256)
α_2	0.504	0.281
(Lottery Profits in General Revenue states)	(0.142)	(0.162)
α_3	0.302	0.515
(Lottery profits in Earmark Non-Education states)	(0.280)	(0.294)
Changers and General Revenue Groups		
α_1	0.723	0.481
(Lottery profits in K-12 states)	(0.313)	(0.277)
α_2	0.378	0.118
(Lottery Profits in General Revenue states)	(0.122)	(0.136)

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates included in the regression are state personal income, the share of the population enrolled in K-12 schools, the share of the population aged 65^+, the share of the population unemployed, the share of white population, the share of the population with some high school, the share of the population with high school diploma, the share of the population with some college, the share of the population with college diploma, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 4-6 2SLS Estimates Using Lottery, Lotto Games (and Video Game), and Population of Counties with Casinos as Instruments

Parameter Estimates and (Standard Errors)		
	K-12 Group	General Revenue Group
	2SLS Estimates	
L_{it}	0.785	0.585
	(0.441)	(0.231)
	First-Stage Estimates	
Introduction of Lottery	22.677	14.415
	(4.461)	(3.265)
In-state Lotto	17.045	11.095
	(3.498)	(3.020)
Multi-state Lotto	11.037	-1.093
	(5.346)	(2.632)
Video Lottery Games	-	47.021
	-	(4.652)
Population of counties with Casinos	-40.950	-16.778
	(19.910)	(9.353)
Partial F	33.93	32.21
($F_{.001}$)	(4.24)	(4.92)
Over-identification Test	1.62	0.11
(P value)	(0.188)	(0.979)
N	189	336

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates included in the regression are state personal income, the share of the population enrolled in K-12 schools, the share of the population aged 65^+, the share of the population unemployed, the share of white population, the share of the population with some high school, the share of the population with high school diploma, the share of the population with some college, the share of the population with college diploma, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Figure 2-1 Annual Sales and Profits of State Lotteries in the United States

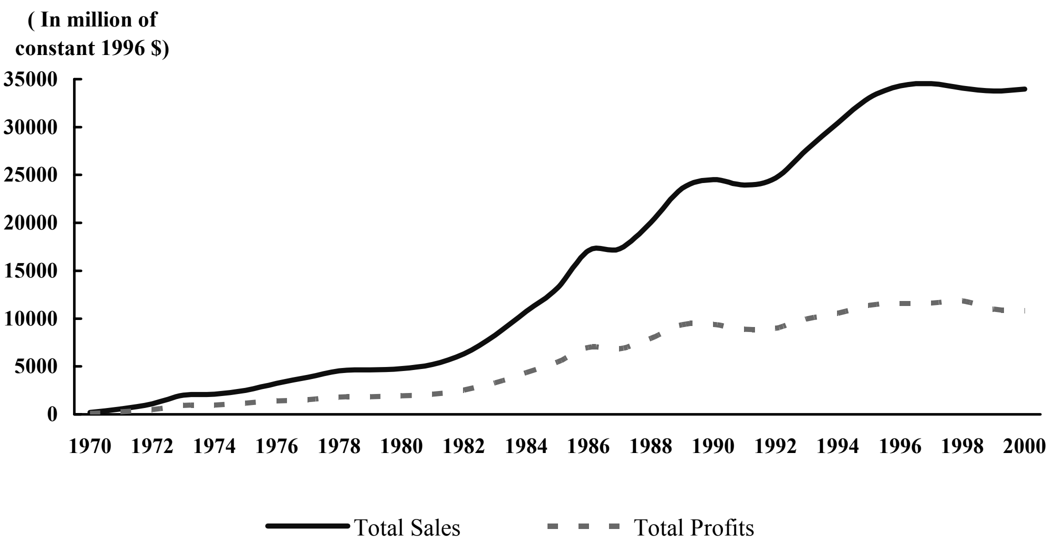


Figure 3-1 Monthly Advertising Spending Per Capita in Illinois

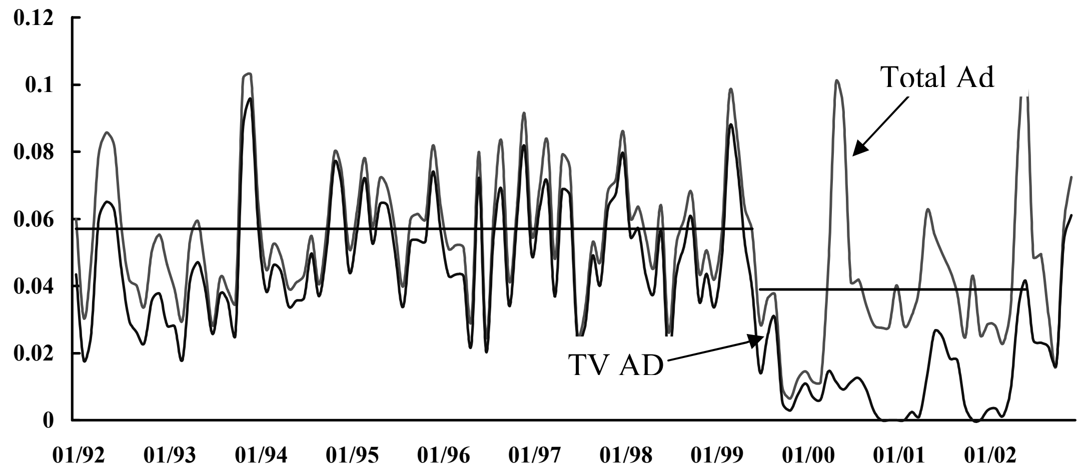


Figure 3-2 Monthly Total Sales Per Capita in Illinois

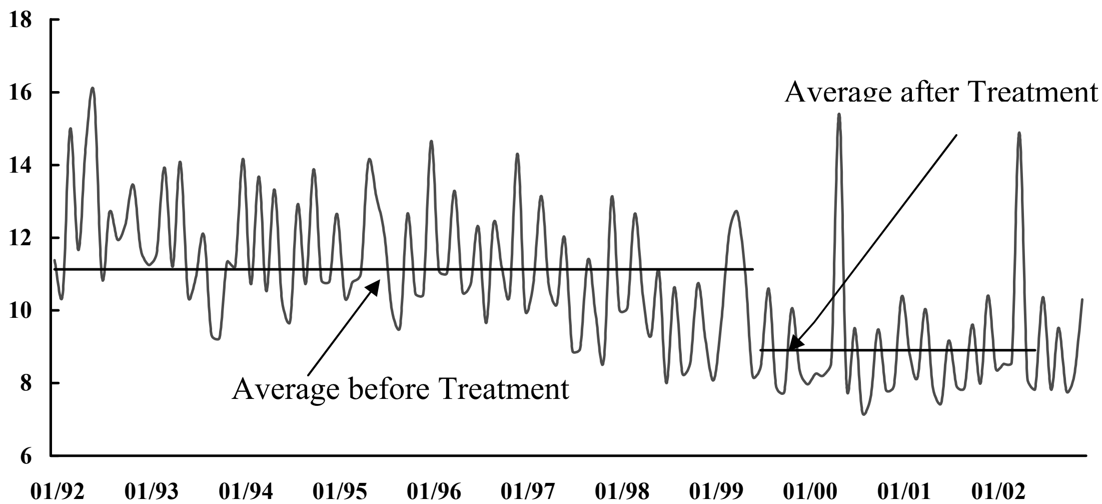


Figure 3-3 Monthly Advertising Spending Per Capita in Washington

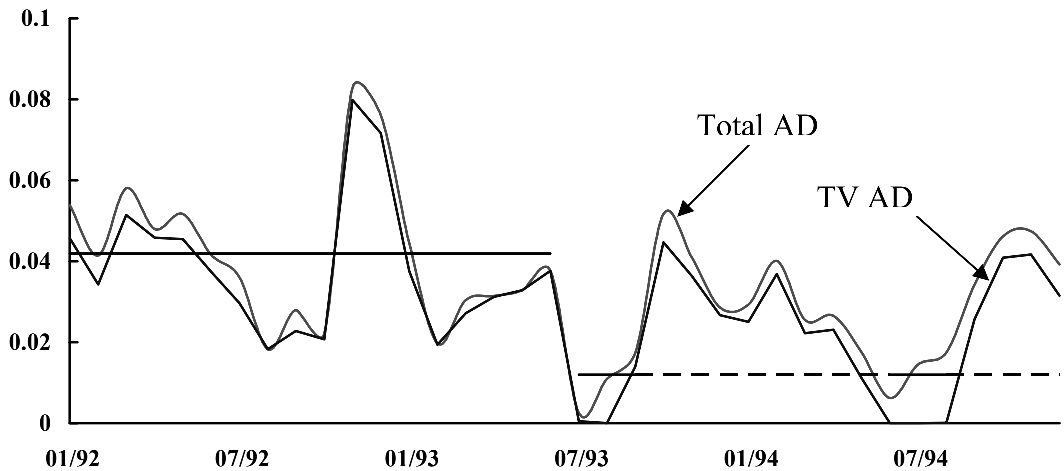


Figure 3-4 Monthly Total Sales Per Capita in Washington

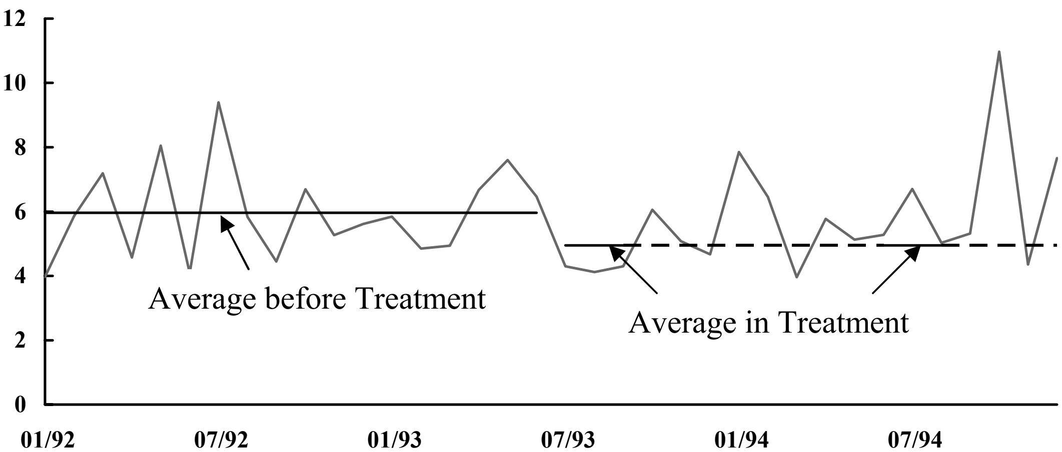


Figure 3-5 Monthly Advertising Spending Per Capita in Massachusetts

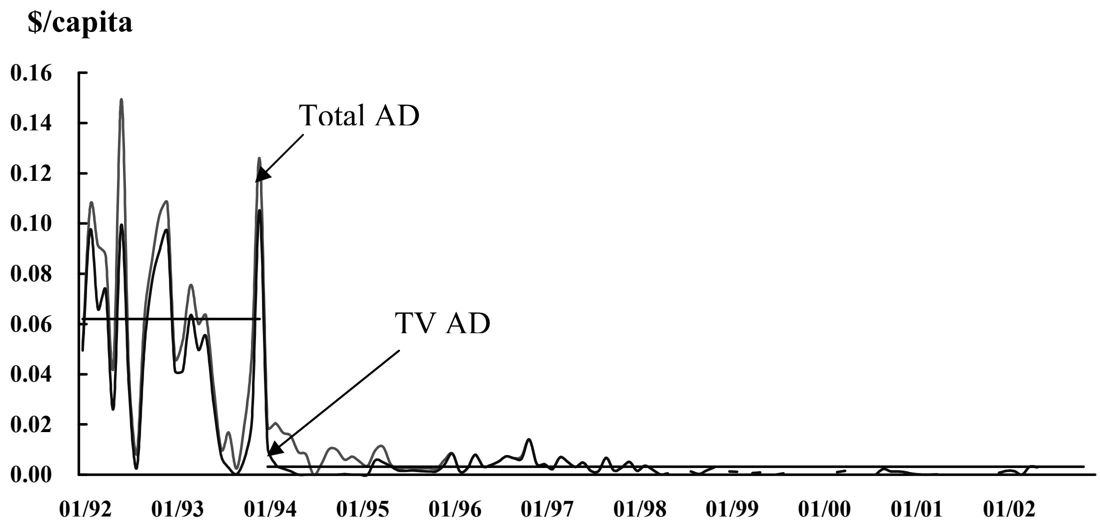


Figure 3-6 Monthly Sales Per Capita by Games in Massachusetts

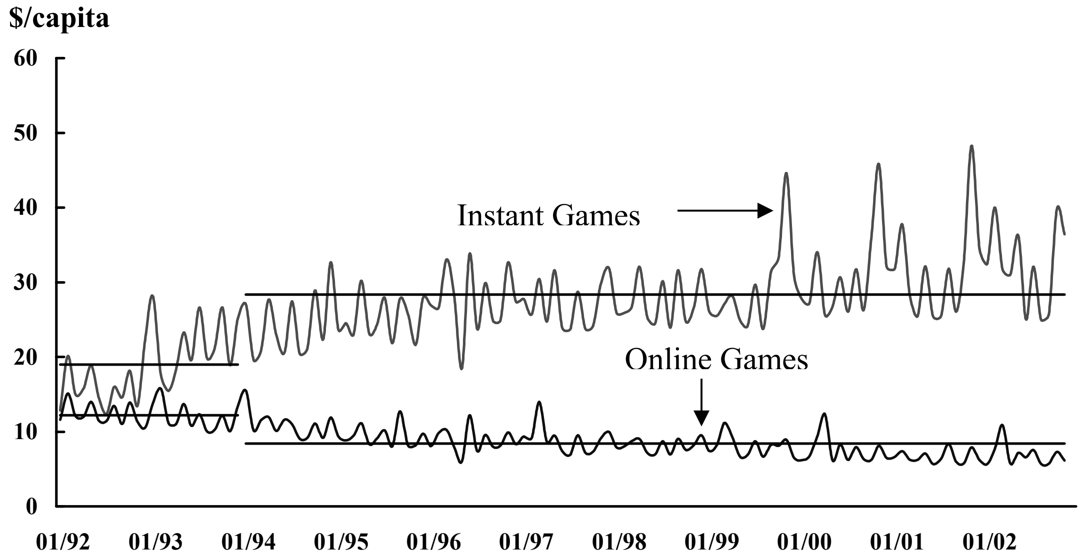
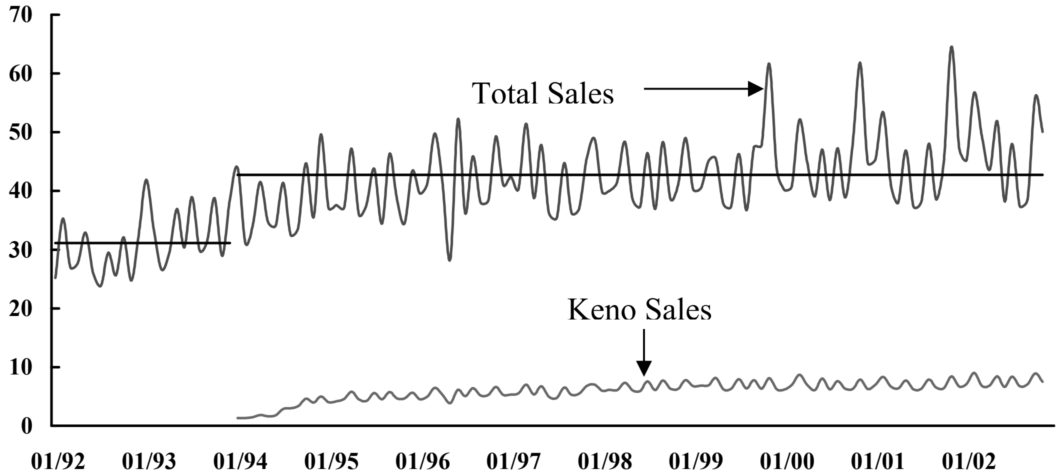


Figure 3-7 Monthly Total Sales Per Capita in Massachusetts



Appendices

Table A-1 Fixed Effects Estimates for K-12 Group and Changers Group Pooled With General Revenue Group

Parameter Estimates and (Standard Errors)

	Sub-sample 1 Changers & General Revenue Groups	Sub-sample 2 K-12 & General Revenue Groups
Share of population enrolled	546.63	933.05
	(550.04)	(502.01)
Per capita income	0.0106	0.0148
	(0.0045)	(0.0043)
Share of population unemployed	-891.06 (394.71)	-3533.27 (1168.70)
Share of population ages 65+	-5479.22 (1209.77)	-865.80 (356.63)
Share of white population	18.40	4.42
	(95.70)	(84.92)
Share of population with some high school	89.30 (188.54)	204.09 (183.62)
Share of population with high school degree	133.01 (142.63)	240.85 (133.59)
Share of population with some college	309.02 (159.87)	398.36 (151.13)
Share of population with college degree	93.50 (145.65)	227.91 (139.73)
$Lottery_{earmarked}$ (K-12 earmarking)	0.79 (0.34)	0.61 (0.28)
$Lottery_{g\text{-}fund}$ (General revenue)	0.37 (0.13)	0.52 (0.14)

Table A-1 Fixed Effects Estimates for K-12 Group and Changers Group Pooled With General Revenue Group (continued)

Parameter Estimates and (Standard Errors)

	Sub-sample 1 Changer & General Revenue Groups	Sub-sample 2 K-12 & General Revenue Groups
1 year after education finance reform	20.26 (12.99)	32.34 (14.80)
2 year after education finance reform	36.08 (14.07)	44.06 (15.03)
3 year after education finance reform	34.89 (14.42)	35.51 (15.31)
4 year after education finance reform	36.94 (15.59)	45.40 (16.69)
5 year after education finance reform	34.46 (15.75)	45.21 (17.96)
6 year after education finance reform	33.34 (14.02)	40.33 (15.98)
1 year after property tax reform for education finance	52.98 (37.82)	35.53 (37.21)
2 year after property tax reform for education finance	114.11 (40.12)	530.18 (38.21)
3 year after property tax reform for education finance	107.16 (40.91)	549.08 (39.37)
4 year after property tax reform for education finance	180.38 (42.43)	547.73 (40.25)
5 year after property tax reform for education finance	244.01 (43.96)	571.93 (42.93)
6 year after property tax reform for education finance	257.65 (41.18)	-----
R^2	0.96	0.97
N	483	567
States	23	27

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