The Impact of Earmarked Lottery Revenue On State Educational Expenditures

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Abstract

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, 37 other states and the District of Columbia have instituted lotteries. Gross sales of lottery tickets now exceed $37 billion a year, adding about $12 billion a year to state budgets. Many states deposit lottery profits into their general funds, but 16 states earmark lottery profits for primary and secondary education. Given the fungibility of money, economists have questioned the effectiveness of the earmarking policies. In this paper, we use a panel data set of the states with lotteries to examine the impact of earmarking lottery revenues on state educational spending. We have two primary results. First, we find that about 50 to 80 cents out of an earmarked dollar is spent on public education. These results are consistent with the large literature in public finance on the "flypaper effect." Second, states with lotteries spend a higher share of the marginal lottery dollar on education than income generated from other sources such as alcohol and cigarette taxes. For example, states spend on average about 16 percent of revenues on K-12 education, and our estimates indicate that each additional dollar raised from sin taxes increases K-12 spending by about the same fraction. In contrast, each dollar of lottery profit increases school spending by about 30 - 50 cents. 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. Our results are fairly stable across different sample periods, control groups, and different estimators.

JEL Classification codes: H52, H72

I. Introduction

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, 37 other states and the District of Columbia have instituted lotteries[1]. The gross sale of lottery tickets now exceeds $37 billion a year, adding about $12 billion a year to state budgets.

Many states deposit lottery profits back into their general funds, but most states have earmarked lottery revenues 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 16 of 21 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. Some theoretical models suggest that earmarking revenues for a particular category should increase the spending by the amount no more than project expenditures would be increased if the money were not earmarked. 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 more than an equal increase in income. However, dozens of studies have demonstrated that money "sticks where it hits." Hines and Thaler (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 in a particular category is therefore an empirical question.

Using a panel data of state expenditures on education in states with lotteries, we examine the impact of earmarking lottery for K-12 education on spending in this category. We perform three different sets of tests. First, we examine states that switched the allocation of lottery profits from the general fund into public education during the sample period. If earmarking increases spending on education, we

[1] In January 2002, South Carolina became the 38th state with a legalized state lottery.

should see an increase in K-12 spending commensurate with the amount of new monies allocated to the earmarked category once the revenues were shifted to this category. As a second test, we restrict our attention to the 9 states that have always earmarked lottery profits for K-12 education and examine whether period-to-period changes in state spending on education are correlated with changes in lottery profits over the same period. If earmarking increases spending, the coefficient on per capita lottery profits in models where per capita K-12 spending is the dependent variable should equal 1. In both of these tests, we use as comparison groups the 18 states that have always deposited their lottery profits into the general fund. 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.

The first test is subject to the criticism that the collection of states moving their allocation of lottery profits from the general fund to education is a selected sample. For example, these states may have expected greater than average growth in education spending which lead them to earmark lottery profits. As we outline below, however, our reading of the legislative history does not indicate this was the case. The second test 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. 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. Putting these two results together, we can use the shocks to lottery revenues as instruments for profits in a two-stage least square 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 revenues contributes only an additional 50 to 80 cents in school expenditures. Although some money is leaked away from the intended purpose, we do find that earmarking increases education spending by an amount larger than the amount produced by having an extra dollar in lottery profits added to the general fund. In particular, we find that a dollar increase in general revenues generated from lottery profits only lead to a 30 to 50 cent increase in school spending. 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. Our results are fairly stable across different sample periods, control groups, and different estimators.

The rest of the paper consists of eight sections. In Section II, we provide a brief history and an outline of some of the key economic issues of state lotteries. In Section III, we review the theoretical and empirical literature on earmarking. We discuss the estimation method and describe the data used in this analysis in Section IV, and in Section V, we present the basic results. In section VI, we identify events that lead to period-to-period variations in lottery profits such as the introduction of lotto games and video lotteries, and the legalization of casino-style gaming, and provide regression results employing two-stage least-square estimation. In the final section, we make some concluding remarks.

II. A Brief History of Lotteries in the U.S.[2]

Lotteries are not new to America. They were used extensively during colonial times to fund a diverse set of public projects such as roads, bridges, wharves, buildings, colleges, churches, libraries, lighthouses and the Continental Army. Most of the early lotteries in the United States were run by state and local governments, but during the 19th century, a number of private companies were hired by governments to operate and market public lotteries. After a number of celebrated cases of fraud in these

[2] The first quarter of this section draws heavily from 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)**.**

privatized lotteries, most states moved to ban lotteries. By 1894, no state permitted lotteries and 35 states had constitutional prohibitions against them.

Lotteries made their twentieth-century debut in New Hampshire in 1964. In contrast to the 19^{th} century model of privatized lotteries, the state government ran the New Hampshire program. Over the next six years, only one other state adopted a lottery, but state budget problems in the early 1970s generated a rapid coast-to-coast expansion in state-run lotteries. Table 1 lists the states that began operating state lotteries during five-year intervals over the past four decades. By 2000, lotteries were run by 37 states and the District of Columbia.

Initially, modern state lotteries were passive drawings where the winning ticket was selected from all tickets sold. These lotteries were similar in many respects to the lotteries run during colonial times. The massive growth is lotteries has however been spurred on by the introduction of active games. There are four major types of active lottery games used today: instant scratch offs, daily numbers, keno and lotto. Instant scratch-off games were introduced in the early 1970s. Tickets were sold to the public with vinyl-covered words or numbers. The covering is scratched off after purchases and the combination of words or numbers identifies if the ticket-holder wins a prize. The daily number was the first computerized game where players are allowed to choose their own three-digit or four-digit numbers, and players win if their number matches the number drawn. Lotto is similar to daily number games, but it allows the players to pick a handful of numbers from a large set, such as 6 numbers from 1 to 44 or 7 numbers from 1 to 80. If the player’s numbers match the numbers drawn, they win a jackpot that 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 drawing. Lotto jackpots can reach into the hundreds of millions of dollars. Similar to lotto, Keno also allows bettors to choose a few numbers (how many is up to the player) out of a large set. Keno drawings are held several times an hour and the game is mostly offered in lounges and bars.

Much of the year-to-year variation in state-level lottery profits is produced through the introduction of new games. Most states introduce the lottery in stages where games with smaller payoffs such as the daily numbers and/or instant scratch off tickets are offered first. For these games, revenues are enhanced by the introduction of new games. In many cases, instant scratch-off tickets featuring new themes are introduced and old ones retired every three to six months. As states gain experience with the lottery, many then introduce high-payout games such as the lotto or multi-state games. Lotto was first introduced in Massachusetts and New York in 1978. Since then, every lottery state except Maine has started its own in-state lotto game. Lotto profits are highly nonlinear in the size of the jackpot because the handle on the game grows quickly as the jackpots rise. Some smaller population states have tried to exploits the benefits of large jackpot games by forming multi-state lottos. In 1985, the first multi-state lottery, 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 since its formation. The most famous multi-state game is Lotto*America which started in 1988 and changed its name to Powerball in 1992. Powerball is now played in 23 states and Washington D.C. Another well-known multi-state game is the Big Game that started in 1996. It is played in seven non-MUSL states and Georgia. In 2002, the game was stopped and succeeded by the Mega Millions Game.

In Figure 1, we graph total lottery ticket sales and profits during fiscal years (October 1 to September 31) 1970 to 2000 in real 1996 dollars. As numbers in Figure 1 illustrate, the growth in the size of state lotteries has been staggering. In fiscal year 1970, when only New Hampshire and New York ran state lotteries, total ticket sales amounted to $201 million. By FY2000, the total lottery ticket sales reached $34.0 billion, about 0.4 percent of the GDP. In FY2000, lotteries added $10.8 billion to state treasuries.

State lotteries are one of the fastest growing segments of the legal gaming industry. In Table 2, we report net gaming revenues (revenues minus winnings) for 1982 and 2000 in constant 1996 dollars by industry sector. In 1982, lottery revenues were $3.6 billion, constituting 20.8 percent of legal gaming revenues or about half the revenues taken in by Nevada and Atlantic City casinos and a little bit shy of revenues generated from horse racing. Over the next 18 years, state lotteries have become the most profitable industry segment with 2000 net revenues just shy of $16 billion.

For every dollar sales of lottery tickets, about 50 cents is paid to players as prizes, 20 cents cover the administration costs and retailers commission, and the remaining 30 cents are returned to the state government as net proceeds. Thus the rate of return from lottery sales is about 43 percent.   Though profitable, lotteries contribute a small share of state revenues. In fiscal year 1998, lottery profits were only 1.4 percent of total state revenues.   To provide a basis of comparison, state tobacco and alcohol taxes plus liquor store profits combined generated 1.7 percent of state revenues in the same year.

Lotteries are a controversial source of public finance. It was, and is still viewed as a vice by some. To reduce opposition to lotteries, some state legislatures tried to guarantee that lottery profits would be used for a "good cause" and of the 37 states with lotteries, 21 earmark at least a portion of lottery profits for specific programs. Five states earmark lottery profits for programs such as parks and recreation, senior citizen programs and college scholarships. Currently, 16 other states earmark lottery profits for K-12 education. The other 16 states deposit lottery profits into the general fund. In Table 3, we summarize the allocation of lottery proceeds in 37 states and Washington D.C. since the inception of lottery in each state.

The state lottery program first collects earmarked lottery revenues and then profits are transferred into the appropriate accounts. States that earmark funds to K-12 education use different financial arrangements to funnel profits to schools. Some states transfer the money to particular school funds. For instance, lottery money is transferred into the Common School Fund in Illinois. Some allocate the money directly to school districts such as in California and in New York. Some spend the money on specific programs as in Georgia where lottery money has financed educational projects such as technology for educational facilities, and construction for educational facilities. In general, states spend lottery revenue on various items from teacher salaries, books and supplies, computers, to capital outlays.

It is worth noting that some of the states have changed the usage of lottery money overtime. For example, Missouri shifted lottery money from General Revenue fund to educational fund in 1992; Illinois did not earmarked lottery revenue for K-12 schools until 1985. During the sample period of 1978-98, five states have switched the allocation of lottery revenues between general revenue fund and educational fund, they are Illinois, Missouri, Montana, Oregon and Texas. Nine states have always earmarked lottery money for public education.

III. Previous Evidence on the Fungibility of Earmarked Money

Empirical approaches designed to detect the fungibility of earmarked money fall into two broad groups: one group estimates a linear system of expenditure equations derived from a specific utility function, while another class of papers estimates reduced-form models. In the first group of papers, local governments are assumed to maximize utility by meeting the objective levels of expenditure and revenue collection. Some studies on domestic fiscal response to categorical foreign aid have utilized this approach. A specific form of utility function, such as a Stone-Geary (Feyzioglu et al., 1998 and Swaroop et al., 2000) or a quadratic loss function (Franco-Rodriguez, 2000)) is assigned to the domestic government. Earmarked money enters the budget constraint and is divided into fungible and non-fungible part where the proportion that is fungible is assumed to be exogenous. The analytical solutions from the constrained maximization problem produce a system of equations where revenues and expenditures are expressed in terms of exogenous variables. The system of equations is then estimated simultaneously and the coefficient of fungibility is estimated within the system. Feyzioglu et al (1998) estimated a statistically significant negative coefficient of fungibility in a cross-country data of 14 less-developed countries while Swaroop et al. (2000) found a positive but statistically insignificant parameter using data from India. Franco-Rodriguez (2000) found a very small impact of aid on public expenditures in Costa Rica.

In reduced-form estimation models, local governments are assumed to be a collective decision-making group that represents preferences of the voters. Earmarked revenues enter the budget constraint and appear as one of the arguments in the demand function. A prototypical model is like that found in Wilde (1968) and Figure 2 summarizes this type of model.[3] Local governments maximize utility by spending revenues on one social good (S) and all other social and private expenditures (Y). In the absence of earmarked money, point A is selected as the optimal combination of S and Y. Now suppose that an amount PR is earmarked for spending on S and the budget constraint confronting the government becomes the kinked line PRT′. With this budget, the government maximizes utility at point B. Had all the earmarked funds been diverted to S, the optimal consumption bundle would have been C. Therefore, part of the earmarked money is spent on other expenditures. Notice that the optimal bundle B with earmarked funds is identical to one that would occur if an identical un-earmarked dollar amount is added to the general fund. In this case, because money is fungible, earmarking does not change expenditures.

If however the earmarked amount is greater than PE that happens to be the optimal quantity spent on S, the optimal expenditure on S will be larger if the money is earmarked. Beyond point E, for example, when PF′ is earmarked for S, the kinked budget line becomes PF′W′. The best combination available for the local government is F′, where the earmarked amount of PF′ is spent completely on S and the entire own source revenue is devoted to other expenditures. If the money is deposited into the general revenue, optimal combination F (the tangency of budget line WW′ with the indifference curve) will be chosen where less money will be spent on S comparing to point F′. However, even at point F′, the earmarked money is still fungible in the sense that if PF′ were to supplement spending on S, the total expenditure on S would be MM′ with spending on other expenditures unchanged.

[3] The graph is modified from the Figure 1 in Wilde (1968).

Pack and Pack (1990, 1993), Cashell-Cordo and Craig (1990) and Gupta (1993) have estimated the fungibility of various categories of foreign aid. Some find earmarked aid is completely fungible while others find that earmarking does alter spending by a statistically significant amount. A few authors have examined the fungibility of lottery profits for education, including Mikesell et al. (1986), Borg et al. (1990), Summers et al. (1995) and Vance et al (1999). These four papers fit the time-series data of educational expenditures for particular states with linear or quadratic trends and compare these trends before and after the introduction of lotteries with earmarked revenues. None of these papers find that earmarking has increased spending and some present suggestive evidence that earmarking has reduced state spending on education. Spindler (1995) improves on this methodology 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 for seven states[4], Spindler concludes that, on average, lottery revenue appeared fungible over the 1961-1992 period. Garrett (2001) estimates a similar model using data from Ohio but adds additional covariates to the model including such factors as net lottery revenue per capita and per capita income. Garrett finds suggestive evidence that up to half the money earmarked to education makes it there but the results are not statistically significant.

IV. A Within-Group Econometric Model

As we outline below, the data set we use for this analysis contains annual observations on state-level per capita K-12 spending and lottery profits and our empirical model is designed to exploit the structure of the data. In general, our regression model is based on the traditional analysis in Figure 2. State spending on public education is derived from the optimal behavior of the state government. It is a function of net lottery revenue and exogenous state characteristics. Lottery profits change from year to year for a variety of reasons. In some states, lotteries were introduced sometime over the panel. In these states, the obvious change in revenues is simply the introduction of the lottery. In other states, the lottery

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

was introduced prior to the first year of our panel. In these states, as well as states with newer lotteries, much of the change in lottery profits from one year to another is generated by the introduction of new games or the changing attachment 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 revenue. If earmarking changes spending patterns, we would expect education spending would grow dollar for dollar with lottery profits. We do however need to identify what states would have done with the extra money had revenues not been earmarked to education. Subsequently, we include in our model data for states with lotteries that place lottery profits directly into the general fund. Given data from various type 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 education spending and ask whether increases in lottery profits generate a differential increase in K-12 spending in earmarked versus non-earmarked states.

Consider a group of states, all with lotteries, but some that earmark lotteries (E=1) to education and some that don't (E=0). State i's lottery profits in year t are represented by L_{it} while $K12_{it}$ represents state per capita expenditures on K-12 education. The simple within-group model outlined above can be expressed by the following equation:

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

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 effects, T_t is a simple linear time trend and therefore, λ_i is a state-specific time trend. The final term is an idiosyncratic error.

In many studies, K-12 spending is denominated by the number of students, but in this case, we must use the same denominator for both the lottery profits and K-12 spending variables so 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 and α_2, which represent the additional lottery dollar devoted to K-12 education for earmarked and non-earmarked states.

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 dollar per capita on K-12 schools, while California spends about $500 per capita. Much of the between-state differences in the levels of school spending are a result of historical and political factors that cannot be fully captured by measurable covariates. As a result, we control for these persistence differences in the level of spending by including a complete set of state fixed-effects in the 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 state 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 up the coefficient on the α's. Controlling for state fixed-effects allows for the possibility that states with higher than average education spending may 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 there are also persistent differences across states in the growth rate in spending on public schools. To demonstrate this point, we ran, 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. In Figure 3, we graph the distribution of the time trends for each state. The range of the time coefficients is about $63 per capita, and the distribution of the coefficient on the time trend is skewed to the left. The time trend is negative in 20 states, and positive for 30 states. Michigan ranks the top in the annual growth of state school expenditure per capita, about $50 per capita increase per year; while North Carolina ranks the lowest with about $13 per capita decrease each year. Given this wide range in the growth of per capita K-12 spending, we also, include in our analysis state-specific time trends.

There is empirical evidence that socioeconomic and demographic characteristics of state affect state's expenditure on public education. Fernnadez et al. (2001) found that in the long-run, personal income, school enrollment, share of population aged 5-17, and share of population aged 65 and above are significant determinants in public educational expenditures. Bergstrom et al. (1982) in their estimation of demand for local public schools found evidence that black homeowners desire significantly and substantially higher education expenditures than do whites who have similar incomes and tax prices; unemployed people tended to want substantially less expenditures on education than employed. Therefore, we expect that state educational expenditures respond to variations in such variables as state personal income, public school enrollment, racial composition and age structure of state population, and education attainments of adult population. Such covariates are thus included in X_{it} in our regression equation.

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 43 states. By 1997, finance systems were declared unconstitutional in 18 states. Murray, Evans and Schwab (1998) and Murray, Evans and Schwab (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 finance of schools and the value of the tax base varies considerably across districts. A growing dissatisfaction with the property tax has lead some states to directly reform local finance of schools by de-emphasizing the property tax. For example, legislation in Michigan, Oregon and Vermont[5] has greatly reduced the local districts' dependence on the property tax and shift 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, in all models, we delete the five states that earmarked lottery revenue for purposes other than educational expenditures.[6] Second, we divide the remaining 32 states into three groups. The "K-12" group includes those nine states that have always earmarked lottery profits for K-12 education.[7] The "Changers" group includes the five states that changed the allocation of lottery profits between general revenue and education during the sample period[8]. Then, the "General Revenue" group includes the 18 lottery states that have always deposit lottery profits into general revenue fund.[9] Referring to the econometric model above, all states in the "K-12" group have E_{it}=1 for all periods, while E_{it}=0 in all years for those state in the "General Revenue" group. Finally, in the "Changers" group, E_{it} switches from 0 to 1 at some point in our panel. Sub-sample 1 includes the "Changers" and "General Revenue" groups, while the "K-12" and the "General Revenue" groups are in sub-sample 2. We use these two data sets for two reasons. First, the type of variation in L_{it} that is used to identify the model is very different in the two samples. In models with the five "Changer" states, we measure whether the unexplained within panel covariance in L_{it} and

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[5] In Oregon the passage of Measure 5 in 1990, a property tax limitation measure, required 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 form 40 percent to 60 percent.
[6] The five states are Colorado, Massachusetts, Nebraska, New Jersey, and Pennsylvania.
[7] The nine states are California, Florida, Georgia, Idaho, Michigan, New Hampshire, New Mexico, New York and Ohio.
[8] The five states are Illinois, Missouri, Montana, Oregon and Texas.

$K12_{it}$ is different before and after the money is earmarked for education. In contrast, in the "K-12" sample, 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. Second, there is sufficient concern that the collection of states that shift their allocation of lottery profits from the general fund to the earmarked categories are not a random sample that we do not want to contaminate the "K-12" group with these observations.

Within-group variation in lottery profits is used to identify its impacts on state school spending in the fixed effect estimation. Panel data estimates are sensitive to model specifications 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 effects, first differences and long-differences 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:

(2) $$\Delta_j K12_{it} = \alpha_1 * \Delta_j (L_{it} * E_{it}) + \alpha_2 * \Delta_j [L_{it} * (1-E_{it})] + \beta * \Delta_j X_{it} + v_t + \lambda_i + \varepsilon_{it} \qquad \text{for } j=1, 3, \text{ or } 5$$

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

Based on estimates of α_1 and α_2, we can test a number of hypotheses about the fungibility of lottery profits. Many of these tests, however, require the evaluation of inequality restrictions. For example, one test is simply whether the earmarked money is fungible which is simply the hypothesis that $\alpha_1 < 1$. A second test examines whether earmarking increases spending above the level that would

[9] The eighteen states are Arizona, Connecticut, Delaware, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maine, Maryland, Minnesota, Rhode Island, South Dakota, Washington, West Virginia, Wisconsin, Vermont and Virginia..

normally occur if money is not earmarked, which is captured by the hypothesis that $\alpha_1 > \alpha_2$. 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 square 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 $s^2 = (Y-X\tilde{\beta})'(Y-X\tilde{\beta})/(n-k)$) outlined above. The moments from the set of random draws that satisfied 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 (1986) shows that in this case, if there is 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 use 400,000 draws using antithetic acceleration as described in Geweke (1986).

The sample period for the analysis is FY1978-98. The revenue data is obtained from National Center for Education Statistics. The key outcome in our analysis is some measure of educational data at state level. Such data are available in terms of both expenditure and revenue. In general, state expenditure on public education includes money from not only own source but also from federal transfer, while state revenue allocated to educational spending records only own source of revenue. Since we are studying the budgetary composition within a state, the revenue data is utilized. In those states where lottery profits are designated to public education, some grant all the proceeds to elementary and secondary school systems (K-12), some divide the money between K-12 and post-secondary education. Given the fact that K-12 education is always among the beneficiaries, we construct our left hand side variable as state allocation of revenues to K-12 schools.

State demographic variables are obtained from the following sources: school enrollment from National Center for Education Statistics; state personal income from Bureau of Economic Analysis; state unemployment rate from Bureau of Labor statistics; state population, state elderly population, state white population and state adult education attainment are from Census.

The variable of interest is the net lottery revenue allocated to K-12 schools. Raw data on net lottery revenue is obtained from www.lafleurs.com, which provides lottery statistics of US and other European counties. For the states earmarked lottery money for education, some spend all the lottery revenue on K-12 schools such as New Hampshire, some divide the money between K-12 and post-secondary education. We collect detailed information on the division of lottery money between K-12 schools and post-secondary education in each state from web pages of state lotteries and construct the net lottery revenue allocated to K-12 schools.

All the money values are in terms of 1996 dollar, which is calculated using CPI-urban from Bureau of Labor Statistics. Variables are in per capita. Our sample contains 672 observations of 32 states in the United States ranging from FY1978 to FY1998. Washington D.C. is not included since there is no corresponding record of state educational revenues. Table 4 provides summary statistics of the three groups: the “Changers”, the “K-12” and the “General Revenue” states.

V. Regression Results

In Table 5, we present results for the basic within-group model using the states in the “Changers” group, plus the comparison group that contains the 18 states that have always allocated lottery profits to the general fund. In the top-half of the table we report point estimates of the impacts of lottery profits on state K-12 spending from earmarked and non-earmarked states using fixed effects, first difference, 3-year difference and 5-year difference estimator separately. In the first row of the table, we see that the coefficient on α_1 is \$0.75 to \$0.81, indicating that out of every new dollar raised in earmarked funds, about three quarters finds it way into K-12 education. In contrast, the coefficient on α_2 is uniformly smaller, indicating that an extra dollar of non-earmarked lottery profits raise K-12 spending by only 31 to 37 cents. Estimates using within-group variation produce similar results to short- and long-term differences, although the standard errors increase as the sample size shrinks in the long difference models.

In the middle of Table 5, 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 is only rejected at a p-value of 0.05 or smaller in the 5-year difference estimation. In the third panel of Table 5, we report the Bayesian inferences of two inequality constraints. These results provide more convincing evidence that earmarking increases spending but not at a dollar for dollar rate. The first constraint, $\alpha_1 < 1$, tests the hypothesis that the earmarked money is fungible. The second inequality restriction, $\alpha_1 > \alpha_2$, tests the hypothesis that contribution to public school expenditures is larger if the money is earmarked than if it is not. Though the coefficient of earmarking α_1 is not statistically different from 1, there is a 70 percent chance it is less than one. Though we cannot reject the hypothesis that the effects on school spending are the same whether the money is earmarked or not in a standard F test of linear constraints, the probability that the inequality constraint of $\alpha_1 > \alpha_2$ holds is about 90 percent and more.

We report the fixed-effect estimates of other covariates in the first column of the appendix table. 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 1.1 cents. The share of population aged 65+ has a statistically significant and negative effect on public educational expenditures as in Poterba (1997 and 1998) and Harris, Evans and Schwab (2001). A one percent increase in the elderly population reduces state budget for schools by about \$55 per capita. The share of population unemployed imposes a negative impact on education expenditures as well. A one percent increase in the population unemployed leads to about \$8.9 per capita less available for schools. The coefficient on enrollment is positive but not significant. We find significantly positive effects of both reform variables on state educational expenditures. Property tax reforms in the three states (MI, OR and VT) channel more money to public schools than court-ruled reform in other states.

In Table 6, we apply the same set of estimators and test statistics to sub-sample 2 that pool "K-12" and "General Revenue" groups together. There are more observations in the "K-12" group than in the "Changers" group, which reduces the standard errors of $\hat{\alpha}_1$. Fixed effects and 5-year difference estimates of α_1 are still statistically significant and positive and roughly the same size as the estimates obtained using the "Changers" sub-sample. First difference and 3-year difference estimates $\hat{\alpha}_1$ of are not statistically significant and smaller than the corresponding point estimates in Table 5. In the difference models the value of $\hat{\alpha}_2$ lies between \$0.19 - \$0.52.

Using fixed effects, first difference and 3-year difference estimator, the evaluated probability of the inequality constraint of $\alpha_1 > \alpha_2$ become smaller. The probability that the earmarked money is fungible ($\alpha_1 < 1$) is over 90 percent. 5-year difference estimator produces similar Bayesian test results as those using sub-sample 1.

We report fixed effects estimates for all other covariates using sub-sample 2 in the second column of the appendix table. Comparison between the estimates in the two columns of the appendix table shows that, though of similar signs and degree of significance, point estimates using two sub-samples differ in sizes from each other. Heterogeneous estimates of the effect of the same socioeconomic and demographic variables provide a post-regression support for the validity of our division of sample. However, estimates of our variable of interests are similar using the two sub-samples.

We find in our regressions that about 60-80 cents out of a dollar is spent on public education if the money is earmarked. Our findings here consistent with the large literature in public finance on the "flypaper effect." In that literature, intergovernmental grants are equivalent to increases in income so economic theory predicts these grants should not increase spending 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 $1 in income categorical spending increases by five to ten cents. In contrast, dozens of studies have demonstrated that money "sticks where it hits." Hines and Thaler (1995) review a number of empirical tests of the flypaper effect and conclude that unrestricted block grants 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 effects found in these previous studies (Gramlich, 1977).

Another interesting result is that although states spend, on average, about 17-18 percent of the total budget on public education over the sample period, the marginal non-earmarked lottery dollar generates a much larger change in spending on education. In particular, our results suggest that in the "General Revenue" group of states, each additional dollar in lottery profit generates about 30-50 cents more in education spending. The difference in the marginal and average propensity to spend on public education suggests that public school expenditure has some priority in the budgetary process when it comes to determine expenditures out of lottery revenue. 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 tobacco tax and alcoholic revenues. Alcoholic revenue is constructed from the summation of liquor store revenue and alcoholic beverage tax in each state. As we discussed above, tobacco and alcohol taxes each generate about the same amount of money to the states as lottery profits. We re-run the regressions of equations (1) and (2) adding the two variables of "sin tax" into the right hand side. Results are presented in Table 7.

The upper panel of Table 7 contains estimates using sub-sample 1, the lower panel of sub-sample 2. As we can see, adding tobacco tax and alcoholic revenue does not change the significance and point estimates of the impacts of lottery money. The effects of tobacco tax and alcoholic revenue on state school expenditures are generally not significant, and the point estimates are no larger than the average propensity to spend on schools. The only significant effect that we obtained is the 5-year difference estimate of the impact of tobacco tax, which is about 9 cent out of a dollar. As a source of revenue, net lottery profits constitute roughly the same proportion in the total state budget as tobacco tax and alcoholic revenue. However, the contribution to school expenditure is much larger from lottery revenue than it is from the other sin taxes.

It should be noted that most states experience a steady decline in the revenue from alcoholic beverage tax and liquor store revenue over the sample period. After fitting a state specific time trend, there may not be much within state variations left to identify the impact on school expenditure, which may be one of the reasons leading to the insignificant estimates of alcoholic revenue.

Some may argue that states that earmarked lottery money for education would spend more on school expenditures than those states that did not earmark even in the absence of earmarking. In other words, chances are higher for those states that have a higher propensity to spend on public education to earmark lottery revenue for education. Thus our estimates of the earmarking effects may be contaminated without controlling for the characteristics that lead to the selection. The selection bias is most relevant for those states that changed the allocation of lottery money from general revenue to school funds in our sub-sample 1. We should note that this type of selection bias would tend to generate a large coefficient for earmarked lotteries 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 coefficient may be contaminated by the selection bias, we look for the historical reasons that the five states in the sub-sample 1 switched the allocation of lottery money. As shown in Table 4, Montana changed the allocation of lottery revenue from K-12 schools to general revenue fund in 1996, while the other four state, Illinois, Missouri, Oregon and Texas, switched from general revenue to K-12 schools.

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

In Missouri, the state lottery was authorized in 1984 and although the legislation approving the lottery didn't stipulate that all of the money would be used for education, voters were led to believe it during the campaign to approve the lottery amendment.[10] Later they found out that the money was put in state general revenue and used for all state services, with elementary and secondary education getting only about 26 percent.[11] In August 1992, voters earmarked state lottery proceeds for education by passing Amendment 11. It should be pointed out that before the approval of Amendment 11, voters rejected an amendment that sought to raise property taxes for education. In the previous year, voters also rejected Proposition B to raise income tax for education. In other word, it is clear in Missouri that voters wanted lottery profits to be spent on education, it is however not clear that they wanted more money spent on education.

In the case of Texas, it is not clear either that the voters wanted to earmark funds to pay for more education. When the Texas lottery was adopted in 1992, the majority of Texans believed that the fund

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[10] 'Schools get full cut of gambling money', Fred W. Lindecke, Missouri Political Correspondent, April 26, 1993 Monday, Five Star Edition.

[11] 'Parents back tow measures amendments would aid schools with easier taxation methods', July 27, 1992, Monday, Five Star Edition

would be set aside for the Texas school system, which would have provided local property tax relief. Lottery profits were however deposited into the general funds.[12] In 1997, Governor G. W. Bush proposed to cut school property taxes while increasing state taxes. However, opposition against a tax hike was strong. Williamson R-Weatherford suggested putting $1 billion plus annual lottery proceeds into the Permanent School Fund to replace the declining oil and gas income from state land. Effective in September 1 1997, all lottery revenue was dedicated to Foundation School Fund.

The Oregon lottery was started in 1985, and originally, the proceeds were dedicated to economic development. But in 1990, Measure 5 was approved to cut local property tax. The state government was then expected to cover the budget shortfalls in schools expenditures.[13] To deal with this expected shortfall, the legislature included education as part of economic development. More than half of the lottery take was transferred to educational expenditures.[14] 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 with education and resolved the uncertainty. Measure 21 was passed by a nine-to-one margin that added financing of public education to the allowable uses of lottery proceeds. However, the case of Oregon does not pose much of a concern about selection bias for our work. First, because Oregon was one of the states with major property tax reform, we control for the shift of more spending to the state level with our property tax reform legislative dummy variables. Second, the shift to more state finance of K-12 education occurs in 1990 in Oregon, but the shift in earmarking policy is not complete until 1997, the last year in our sample.

The state that is most likely to pose a selection bias in our "Changers" sample is Illinois. In 1985, the Illinois General Assembly passed wide-ranging reforms in the state's schools. Lottery revenue together with an increase in tax on cigarettes and a tax on interstate telephone calls were collected to support the reform. If it were the case, our estimates of $\hat{\alpha}_1$ would be biased upwards. However, we re-ran

[12] 'Lottery's millions could be for schools', May 1, 1997 Thursday, ALAMO.
[13] 'Carving up the lottery', 12/11/1995, Portland Oregonian
'Hooked on Gambling', Editorial, 11/22/1996, Portland Oregonian
[14] 'Let schools win lottery', Editorial, 01/25/1995, Portland Oregonian

the regression without the state of Illinois and obtained even larger estimates for the earmarking states. As a matter of facts, excluding any one of the “Changers” states does not change the estimated coefficient significantly.

The basic results in Tables 5 and 6 are not sensitive to major alterations of the model. The time series for per capita K-12 educational revenues at the national level indicates that state revenues were flat in the early 1980s, and then increasing steadily ever since. To determine whether our results are sensitive to different sample periods, we restricted our sample period from 1978-98 to 1983–98 and re-run the regressions using the two sub-samples. As a second specification test, we also altered the composition of our control sample. In the previous two sections, we used as a control group those states with lotteries that have always earmarked revenues to the general fund. We can expand the sample to include those states without the lottery. Including these states will help establish what the underlying trend in K-12 revenues.

Results with these basic changes to the econometric model are reported in Tables 8 and 9. In these tables, we report the results for the fixed-effect, first-difference, plus 3 and 5-year lag models using alternative specifications. In Table 8, we report sensitivity tests for the “Changers” and “General Revenue” sub-sample while in Table 9, we report results for the “K-12” and “General Revenues”. In each table, we report the basic estimates of from previous tables in the first row of the table. Estimates for the shorter period are reported in the second row of results. In row 3, we expand the sample to include the non-lottery states but use the longer time period, while in row 4, we keep the larger collection of states and reduce the length of the time series.

The results in Tables 8 and 9 indicate that these alterations to our model do not change the parameter estimates much. Estimates of interest using the “Changers” and “General Revenue” sub-sample 1 are fairly stable across the alternative estimators. In Table 8, coefficient on the earmarked lottery money is in a small interval between 0.7 – 0.8. Coefficient on the lottery profits deposited into the general revenue fund falls between 03 – 0.4, except the insignificant estimates in the first difference model. In Table 10, results from estimators using the “K-12” and “General Revenue” sub-sample follow a similar pattern. Fixed effect and 5-year difference estimators always produce statistically significant coefficients on the earmarked lottery coefficient, while estimated effects are statistically insignificant using first difference and 3-year difference estimators.

The results in Tables 9 indicate that for the models that use the “K-12” sample as the treatment group, the first-difference estimates on the earmarked lottery coefficient is always different from in other models. Why is the first-difference model so different for this regression? Griliches and Hauseman (1986) note that applying the analysis of covariance approach to panel data often results “too low” and statistically insignificant coefficients. Errors of measurement in the independent variables whose relative importance gets magnified in the within dimension are usually blamed for the unsatisfactory outcome. Griliches and Hausman demonstrate that if the ‘true’ value of the independent variable is serially correlated with a declining correlogram (which is most likely the case here) and for T>2, errors of measurement will usually bias the first difference estimators downward (toward zero) by more than they will bias the within estimators. In this case, errors in measurement may be produced by the fact that our independent variable of interest may be a proxy for the variable we want to measure. For example, most states deposit lottery profits initially into a state lottery fund or a special account. Transfers from the fund or account are made annually or quarterly either to a school fund, to school districts, or to various school projects. The budgetary allocation of lottery revenue is separated from the allocation of state general revenue. Thus if the state government substitutes school money obtained from lottery revenue for other expenditure programs in its allocation of the general revenue, the allocation of general revenue should be based on the expectation of lottery revenue in the next fiscal year. It may be the case that in the first-difference model, the expected lottery revenue should be the variable of interest in the regression. If this is the case and since data on expected lottery revenues is not available, then the actual revenue ends up to be a mismeasured value of the true variable.

VI. 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 model is it leaves 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 earmarking revenues.

As we mentioned above 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 the 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 in K-12 education. Comparing the impact that the shock had on lottery profits and its eventual impact on K-12 education should provide some indication of what fraction of the marginal dollar is spent in education. For example, if the introduction of a Lotto-style game increases profits by $20 per person and K-12 profits by $15 per person, then one could conclude that 75 cents of every new dollar in lottery profit is destined for education.

There are, however a number factors that may complicate this type of analysis. First, the comparison of the "first-stage" and "reduced-forms" outlined above is straightforward when looking at one discrete even in isolation. In practice, there are many events that we can measure that should change lottery profits. Subsequently, 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 (2SLS) to tie the first-stage and reduced-forms together. In the 2SLS model, predicted lottery profit is entered into the equation of interest. Angrist, Imbens and Rubin (1996) argue that 2SLS can be thought of as a local average treatment effect that measure the impact of the covariate of interest (lottery profits) on the outcome (K-12 spending) for those observations whose behavior has been changed as a result of receiving the treatment.

In case therefore, 2SLS coefficient on lottery profits will measure the change in K-12 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. Subsequently, 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 years 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 1 in years the lottery is in operation and zero otherwise. Many states adopt lotteries in stages, first introducing instant tickets and daily numbers, then larger jackpot games such as lottos and multi-state games. The second variable we construct is another dummy variable that equals one since the first year a state runs an in-state lotto game. As Cook and Clotfelter (1989) discuss, the success of the 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 numbers of lotto players is highly nonlinear in the jackpot. Many smaller states that run lottos never see jackpots in the tens of millions of dollar range on their in-state lottos. As a result, a number of smaller states grouped together and offered a multi-state lotto games to pool population across borders and increase the likelihood of larger jackpots and hopefully, to increase profits. There are now three major multi-state lotto games: Power Ball, the Big Game and Hot Lotto. We constructed a dummy variable for years when Power Ball, the Big Game or some other multi-state lottery was in operation in a state. Some states have moved to video lottery machines in bars, restaurants and race tracks where people can play poker, blackjack, keno and bingo over the terminals and a dummy variables captures their introduction into the market. Finally, there has been a 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 a tremendous 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 allowed 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 24 states other than Nevada and New Jersey. Evans and Topoleski (2002) have constructed a master list of these casinos with their zip codes and opening dates. We used this list to identify counties with casinos and when they opened. We include as a covariate in these regressions the fraction of the people in counties with a casino.

In the bottom portion of Table 10, we report the results of the first-stage regression for states in the “K-12” and “General Revenue” samples. In the first stage regression, we control for state and year effects, state specific time trends, and all other covariates used in the previous regressions. In the table, we report the coefficients on the events that potentially should alter per capita lottery profits. We can see that the adoption of the lottery increases total profits by $23 per person and the adoption of an in-state lottery adds an addition $17 per person. In the K-12 states, the adoption of a multi-state lottery increased revenues by a statistically significant $11/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 obtained from Table 5 we constructed using the “Changers” sub-sample.

We apply the same estimation procedure to the 18 general revenue states and add video lottery game (VLG) as another instrument for the general revenue group. There are four states in this group that introduced video lottery games in the early 90s (Delaware, South Dakota, Rhode Island and West Virginia) and the introduction of VLTs has a large, positive and statistically significant impact on lottery profits. We again obtain statistically significantly and positive impact of in-state lotto on profits, but in this sample, we find little evidence that the multi-state lotto increased profits. 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 of the general revenue deposits is 0.534, which is close to our previous estimates using sub-sample 1. In both the “K-12” and “General Revenue” sub-samples, the p-values on the test of over-identifying restrictions are rather large we cannot reject the null hypothesis that the models are correctly specified.

It is worth noting that although Powerball[15] and the Big Game[16] are the well-known lotto games among states, introducing these games did not guarantee a boost in lottery profits as we see in the general revenue group. Just examining the time series of lottery profits in a few states, there was a noticeable drop in lottery profits when 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 games like Powerball and Big Game cannibalizes other lottery games.

VII. Conclusion

Although state lotteries have been a financial success, many voters and elected officials were 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 37 states with lotteries, 21 states currently earmark monies with the vast 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 by the history of the Montana lottery. When Montana began their lottery in 1987, lottery profits were earmarked for public education. In 1995, the state legislature decoupled lottery revenue from school financing. As the President of the Montana Education Association noted, it wan an “illusion” that lottery were a big help to public schools. Although lottery transferred almost $42 million directly into school fund accounts between 1987 and 1994, the amount is less than 1 percent of state school budgets during the period.[17]

[15] Four states in the K-12 group joined the Powerball, they are Georgia, Idaho, New Hampshire, New Mexico, and Ohio. Fourteen states in the general revenue groups joined the Powerball game, they are: Arizona, Connecticut, Delaware, Indiana, Iowa, Kansas, Kentucky, Louisiana, Minnesota, Rhode Island, South Dakota, Wisconsin, West Virginia.

[16] Georgia and Michigan in the K-12 group, Maryland and Virginia in the general revenue group joined the Big Game.

Whether earmarking increases spending or not is in the end an empirical issue, and in this paper, we use the experiences of lottery states over the past 20 years to examine this issue. Given previous evidence on the "Flypaper Effects", it is not surprising that we find about half to three quarters of an earmarked lottery profits find their way to public schools. We have shown that 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 higher share of the marginal lottery dollar on education than income generated from other sources such as alcohol and cigarette taxes. Though our findings suggest that earmarking lottery revenue to K-12 education increases 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. It should be pointed out that lottery revenues are a small fraction of state spending on education and total state revenues. Even though earmarking makes a large proportion of lottery profits available to public schools, lottery profits can never become the major source of finance for public education.

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Table 1
Expansion of Lotteries in the U.S.,
FY1960 – FY2000

	States initiating lotteries within the interval
FY1960-65	NH
FY1965-70	NY
FY1970-75	NJ, CT, PA, MA, MI, MD, ME, IL, RI, OH
FY1975-80	DE, VT
FY1980-85	AZ, DC, WA, CO, OR
FY1985-90	IA, CA, WV, MO, MT, SD, KS, FL, WI, VA, KY, ID
FY1990-95	IN, MN, LA, TX, GA, NE
FY1995-00	NM

Source: State Lottery Websites.

Table 2
Gambling Revenue by Industry, 1982 and 2000

Industry	Revenues in Billions of constant 1996 dollars (Industry market share) 1982	2000
Nevada/Atlantic City Casinos	$6.9	$12.5
	(40.3%)	(22.2%)
Lotteries	$3.6	$15.8
	(20.8%)	(28.1%)
Horse racing	$3.7	$3.0
	(21.6%)	(5.4%)
Native American gambling	-----	$9.5
		(17.0%)
River boat casinos	-----	$8.2
		(14.6%)
Other gambling	$2.9	$7.1
	(17.2%)	(12.7%)
Total	$17.1	$56.2

Source: Christiansen Capital Advisors

Table 3
State Allocation of Lottery Proceeds

State	Year lottery began	Initial allocation	Year Allocation Switched	Current Allocation
AZ	1981	General Revenue[1]	-	
CA	1985	K-12 Education	-	
CO	1983	State Capital Construction Fund	1992	Parks and recreation
CT	1972	General Revenue	-	
DC	1982	General Revenue	-	
DE	1975	General Revenue		
FL	1988	Education	-	
GA	1993	Education		
ID	1989	Education		
IL	1974	General Revenue	1985	K-12 Education
IN	1989	General Revenue[1]	-	
IA	1985	General Revenue	-	
KS	1987	General Revenue[1]	-	
KY	1989	General Revenue[1]	-	
LA	1991	General Revenue	-	
ME	1974	General Revenue	-	
MD	1973	General Revenue	-	
MA	1972	Cities and Towns	-	
MI	1972	K-12 Education	-	
MN	1990	General Revenue[1]	-	
MO	1986	General Revenue	1992	Education
MT	1987	K-12 Education	1995	General Revenue
NE	1993	Environment, Education, Compulsive Gamblers	-	
NH	1964	K-12 Education	-	
NJ	1970	Education and Institution	-	
NM	1996	Education	-	
NY	1967	K-12 Education	-	
OH	1974	K-12 Education	-	
OR	1985	General Revenue	1997	K-12 Education (15%)
PA	1972	Senior Citizens Program	-	
RI	1974	General Revenue[1]	-	
SD	1987	General Revenue[1]	-	
TX	1992	General Revenue	1997	K-12 Education
VT	1978	General Revenue	1998	K-12 Education
VA	1988	General Revenue	1999	K-12 Education
WA	1982	General Revenue	2001	K-12 Education
WV	1986	General Revenue	-	
WI	1988	General Revenue[1]	-	

[1] Lottery revenues 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 pages

Table 4
Summary Statistics, Selected Lottery States, FY1978-FY1998

Means and (Standard Deviations)

Variable	Changers States	K-12 States	General Revenue
State educational revenue per capita	385	453	462
	(106)	(188)	(146)
State personal income per capita	20,806	21,503	21,110
	(2,376)	(3,322)	(3,572)
Lottery profit per capita	22	28	26
	(27)	(27)	(30)
Share of population currently enrolled in school	0.179	0.177	0.174
	(0.016)	(0.022)	(0.016)
Share of population aged 65+	0.121	0.119	0.124
	(0.015)	(0.024)	(0.015)
Share of population unemployed	0.034	0.033	0.030
	(0.008)	(0.009)	(0.009)
Share of white population	0.891	0.865	0.902
	(0.050)	(0.084)	(0.088)
Share of population with some high school	0.110	0.123	0.115
	(0.021)	(0.024)	(0.031)
Share of population with high school diploma	0.371	0.367	0.386
	(0.038)	(0.045)	(0.048)
Share of population with some college	0.200	0.194	0.176
	(0.050)	(0.050)	(0.054)
Share of population with college diploma	0.201	0.199	0.197
	(0.028)	(0.038)	(0.051)
Number of observations	105	189	378

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

Table 5
State Educational Revenues Per Capita Equations:
Changers and General Revenue Groups

Parameter Estimates and (Standard Errors)

Covariates	Fixed Effects	First Differences	3-years Differences	5-years Differences
α_1 (Lottery profits in earmarked states)	0.786	0.801	0.814	0.749
	(0.340)	(0.383)	(0.405)	(0.380)
α_2 (Lottery Profits in non-earmarked states)	0.372	0.172	0.359	0.308
	(0.129)	(0.189)	(0.154)	(0.147)
F-test (p-value) on Hypothesis H_0: $\alpha_1 = \alpha_2$	1.603	3.581	1.661	5.451
	(0.206)	(0.059)	(0.198)	(0.020)
Bayesian Inferences:				
Prob ($\alpha_1 < 1$)	0.737	0.699	0.676	0.744
Prob ($\alpha_1 > \alpha_2$)	0.898	0.970	0.892	0.894
N	483	460	414	368
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 include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 6
State Educational Revenue Per Capita Equations:
K-12 and General Revenue Groups

Parameter Estimates and (Standard Errors)

	Fixed Effects	First Differences	3-years Differences	5-years Differences
α_1 (Lottery profits in earmarked states)	0.615	0.015	0.494	0.779
	(0.284)	(0.379)	(0.318)	(0.281)
α_2 (Lottery Profits in non-earmarked states)	0.524	0.194	0.440	0.506
	(0.143)	(0.210)	(0.169)	(0.160)
F-test (p-value) on Hypothesis H_0: $\alpha_1 = \alpha_2$	1.519	0.225	0.447	14.156
	(0.218)	(0.635)	(0.504)	(<0.001)
Bayesian Inferences				
Prob ($\alpha_1 < 1$)	0.913	0.995	0.945	0.783
Prob ($\alpha_1 > \alpha_2$)	0.612	0.340	0.560	0.805
N	567	540	486	432
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 include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65^+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 7
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
"Changers" and General Revenue Lottery States				
Alcoholic Revenue	0.093	0.183	0.141	0.054
	(0.146)	(0.190)	(0.165)	(0.163)
Tobacco Tax	0.073	0.046	0.043	0.052
	(0.074)	(0.076)	(0.083)	(0.079)
Lottery Profits – earmarked states	0.819	0.812	0.817	0.751
	(0.342)	(0.384)	(0.406)	(0.380)
Lottery Profits – non-earmarked states	0.412	0.188	0.387	0.335
	(0.134)	(0.190)	(0.156)	(0.152)
K-12 and General Revenue Lottery States				
Alcoholic Revenue	-0.035	0.117	0.054	-0.094
	(0.070)	(0.092)	(0.163)	(0.076)
Tobacco Tax	0.023	0.017	0.052	0.088
	(0.040)	(0.048)	(0.079)	(0.040)
Lottery Profits – earmarked states	0.563	0.083	0.751	0.639
	(0.300)	(0.384)	(0.380)	(0.296)
Lottery Profits – non-earmarked states	0.522	0.203	0.335	0.508
	(0.144)	(0.210)	(0.152)	(0.161)

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 8
Alternative Estimates for Changers and General Revenue Groups

Parameter Estimates and (Standard Errors)

Covariates	Fixed Effects	First Differences	3-years Differences	5-years Differences
Basic Results in Table 6				
α_1 (Lottery profits in earmarked states)	0.786	0.801	0.814	0.749
	(0.340)	(0.383)	(0.405)	(0.380)
α_2 (Lottery Profits in non-earmarked states)	0.372	0.172	0.359	0.308
	(0.129)	(0.189)	(0.154)	(0.147)
Reduce years to FY1983-98				
α_1 (Lottery profits in earmarked states)	0.793	0.772	0.881	0.749
	(0.322)	(0.386)	(0.409)	(0.380)
α_2 (Lottery Profits in non-earmarked states)	0.405	0.191	0.410	0.308
	(0.141)	(0.194)	(0.158)	(0.147)
Add non-lottery states, FY1978-98				
α_1 (Lottery profits in earmarked states)	0.814	0.840	0.698	0.755
	(0.428)	(0.453)	(0.505)	(0.508)
α_2 (Lottery Profits in non-earmarked states)	0.321	0.137	0.284	0.197
	(0.162)	(0.224)	(0.192)	(0.195)
Add non-lottery states, reduce years to FY1983-98				
α_1 (Lottery profits in earmarked states)	0.652	0.776	0.675	0.755
	(0.398)	(0.453)	(0.500)	(0.508)
α_2 (Lottery Profits in non-earmarked states)	0.300	0.133	0.288	0.197
	(0.174)	(0.228)	(0.192)	(0.195)

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65^+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 9
Alternative Estimates for K-12 and General Revenue Groups

Parameter Estimates and (Standard Errors)

Covariates	Fixed Effects	First Differences	3-years Differences	5-years Differences
		Basic Results in Table 8		
α_1 (Lottery profits in earmarked states)	0.615	0.015	0.494	0.779
	(0.284)	(0.379)	(0.318)	(0.281)
α_2 (Lottery Profits in non-earmarked states)	0.524	0.194	0.440	0.506
	(0.143)	(0.210)	(0.169)	(0.160)
		Reduce years to FY1983-98		
α_1 (Lottery profits in earmarked states)	0.590	0.071	0.558	0.779
	(0.285)	(0.361)	(0.319)	(0.281)
α_2 (Lottery Profits in non-earmarked states)	0.438	0.231	0.495	0.506
	(0.147)	(0.201)	(0.169)	(0.160)
		Add non-lottery states, FY1978-98		
α_1 (Lottery profits in earmarked states)	0.661	-0.023	0.479	0.738
	(0.350)	(0.430)	(0.387)	(0.369)
α_2 (Lottery Profits in non-earmarked states)	0.466	0.199	0.395	0.407
	(0.174)	(0.239)	(0.206)	(0.208)
		Add non-lottery states, reduce year to FY1983-98		
α_1 (Lottery profits in earmarked states)	0.523	-0.001	0.459	0.738
	(0.345)	(0.423)	(0.385)	(0.369)
α_2 (Lottery Profits in non-earmarked states)	0.400	0.229	0.420	0.407
	(0.179)	(0.235)	(0.204)	(0.208)

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65^+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Table 10
2SLS Estimates using lottery, Lotto Games (and Video Game)
Population of counties with Casinos as instruments

Parameter Estimates and (Standard Errors)

Covariate	K-12 Group	General Revenue Group
	2SLS Estimates	
L_{it}	0.785	0.534
	(0.441)	(0.248)
	First Stage Estimates	
Lottery	22.677	15.889
	(4.638)	(2.933)
In-state Lotto	17.045	9.289
	(3.698)	(2.542)
Multi-state Lotto	11.037	-0.506
	(5.346)	(2.403)
Video Lottery Game	-	45.549
	-	(4.427)
Population of counties with Casinos	-40.948	-22.327
	(19.910)	(8.591)
Partial F	33.93	36.35
($F_{.001}$)	(4.91)	(4.22)
Over-identification Test	1.62	0.66
(P value)	(0.188)	(0.620)
N	189	378

In all models, we include state and year effects and in the fixed-effects model, we include state-specific time trends. Other covariates include per capita income, share of population currently enrolled in K-12 schools, share of population aged 65^+, share of population unemployed, share of white 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, a set of dummy variables for state educational finance reform, and a set of dummy variables for property tax reform.

Figure 1

Gross Revenue and Net profit from State Lottery

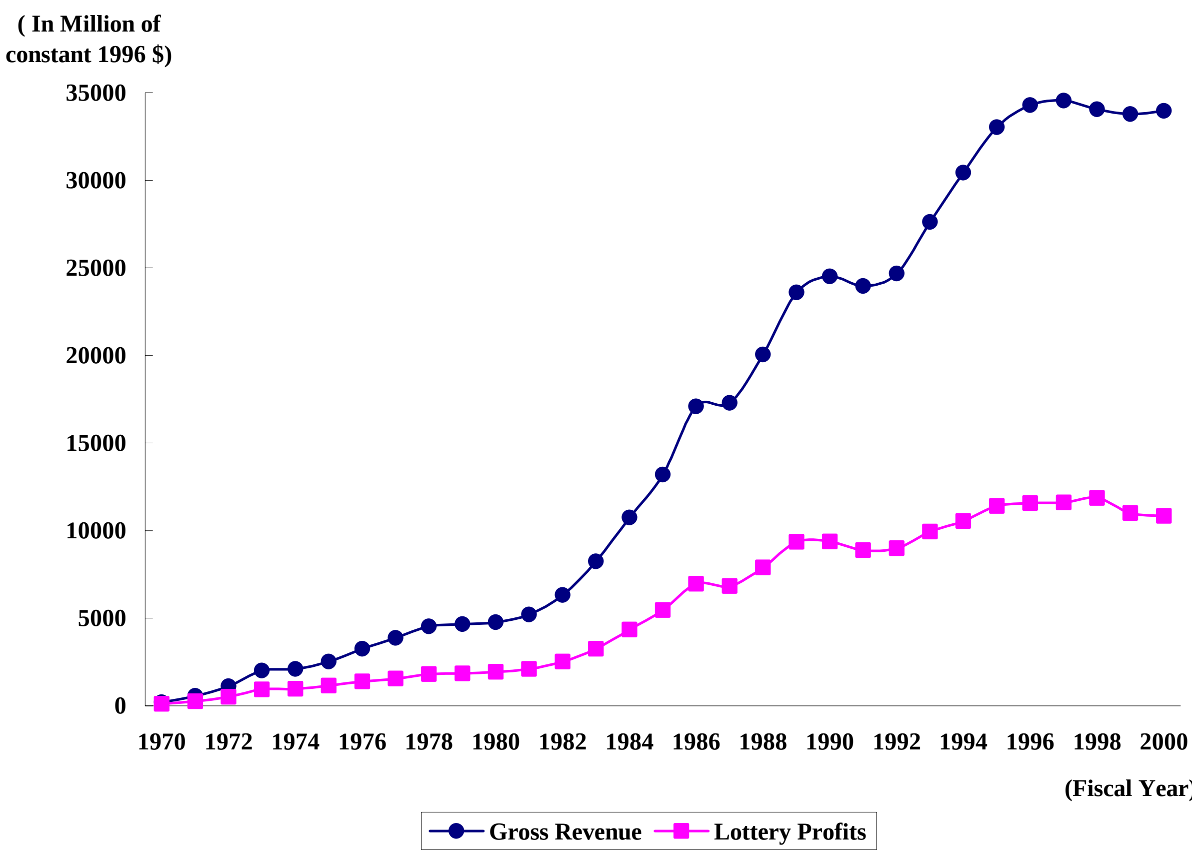


Figure 2

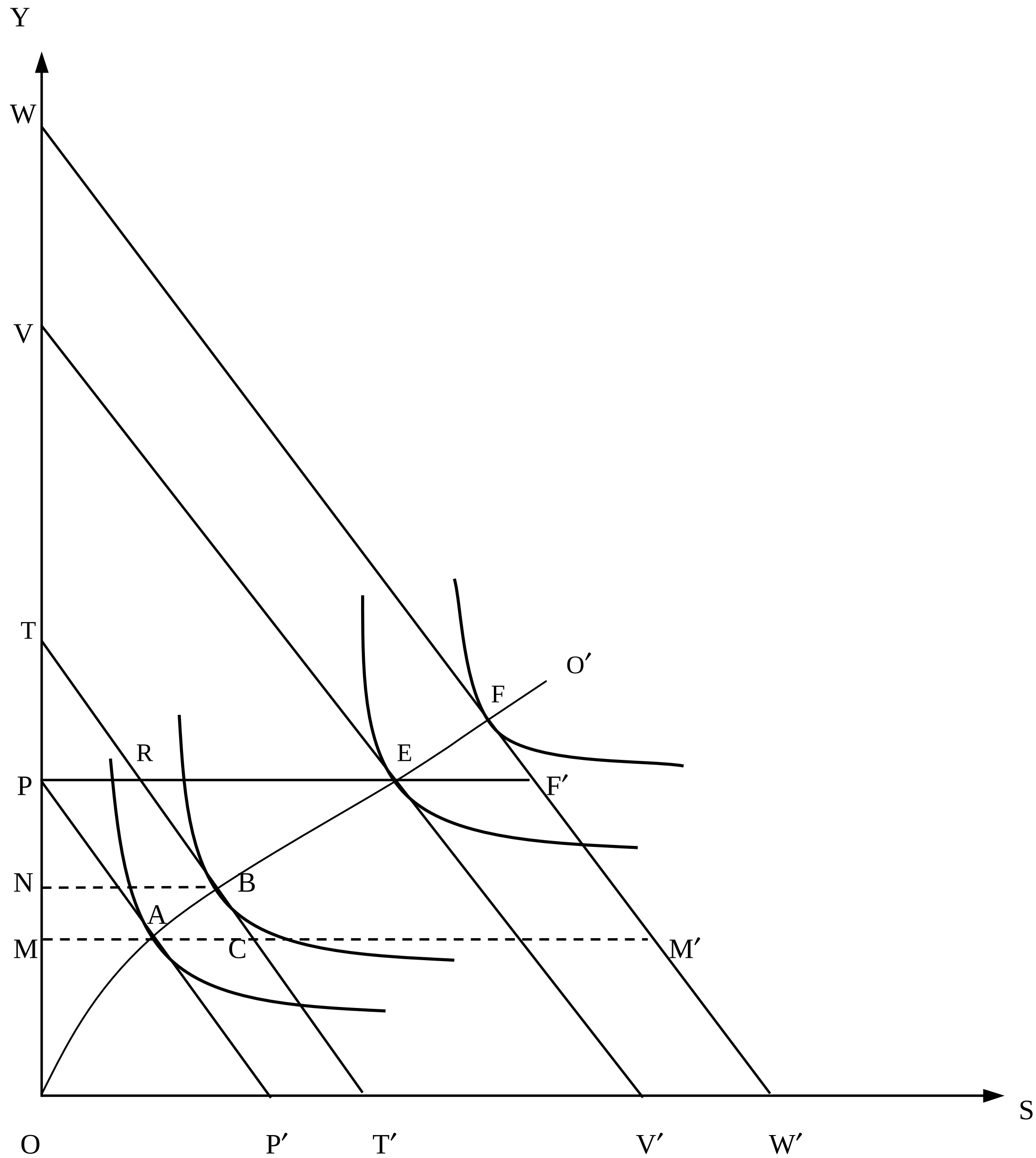
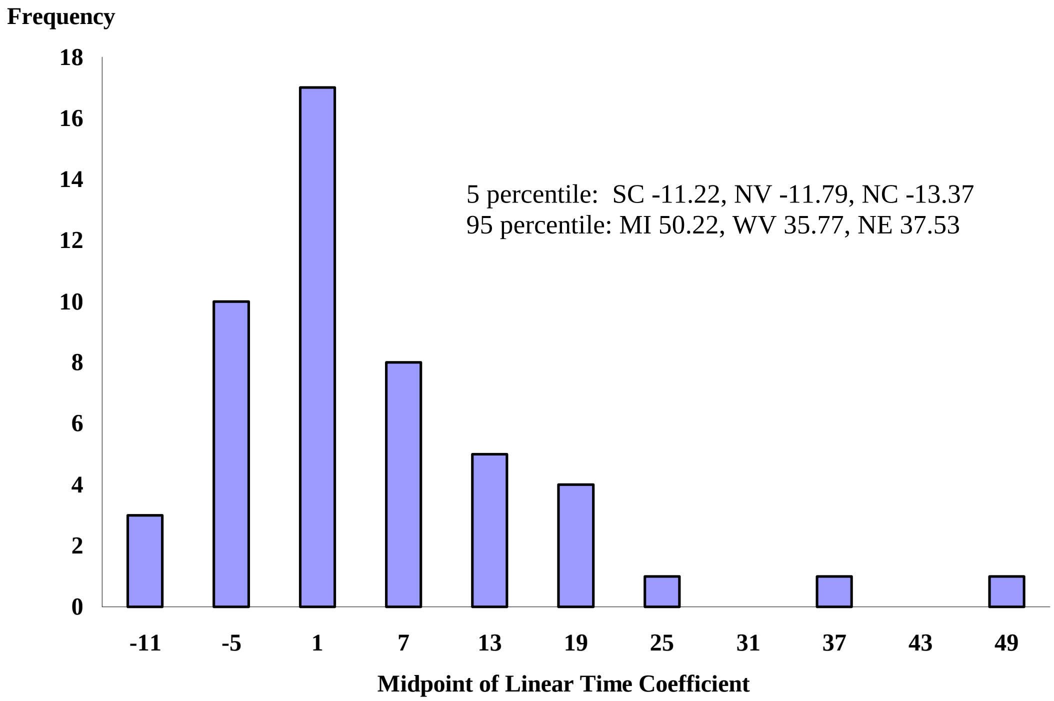


Figure 3

Histogram of State Specific Time Trend



Appendix Table 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	-3533.27
	(394.71)	(1168.70)
Share of population ages 65+	-5479.22	-865.80
	(1209.77)	(356.63)
Share of white population	18.40	4.42
	(95.70)	(84.92)
Share of population with some high school	89.30	204.09
	(188.54)	(183.62)
Share of population with high school degree	133.01	240.85
	(142.63)	(133.59)
Share of population with some college	309.02	398.36
	(159.87)	(151.13)
Share of population with college degree	93.50	227.91
	(145.65)	(139.73)
$Lottery_{earmarked}$ (K-12 earmarking)	0.79	0.61
	(0.34)	(0.28)
$Lottery_{g\text{-}fund}$ (General revenue)	0.37	0.52
	(0.13)	(0.14)

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

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