Choosing to Save

The Fiscal Impact of Education Tax Credits on the State of Nevada

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1. Executive Summary

This study analyzes the net fiscal impact that a large-scale education tax credit program would have on the state of Nevada. It does so by estimating the number of students who would migrate from public to independent schools as a result of lower effective tuition costs, and then calculating the impact that such migration would have on both districts and the state treasury. The impact of tax credits claimed on behalf of students already enrolled in private schools is also taken into account.

The particular education tax credit program evaluated here combines credits for donations to non-profit scholarship organizations serving low-income families with credits for parents who directly assume the cost of their own children's education (both of which already exist, separately, in several states). In other words, the program will cut taxes on any individual or business that pays for the independent schooling of an eligible child. This program, developed by the Washington, D.C.-based Cato Institute, is known as the Public Education Tax Credit Program.

In order to calculate the district-level impact of the program, it is necessary to know the marginal cost of a public school student. The marginal cost is the additional spending required to serve one additional student, and also the savings from having to serve one fewer student. Nevada's marginal cost of public schooling was not previously available in the literature and so it has been calculated for the present study by analyzing public school data for the nine most recent years available.

Total per-pupil spending in Nevada public schools is roughly $10,019, and the marginal cost is estimated here at $8,576. This figure, roughly 85 percent of average spending per pupil, is comparable to the 80 percent figure arrived at in a 2006 analysis of South Carolina.

Assuming that students already enrolled in private schools become eligible gradually over the first seven years of the program, the Public Education Tax Credit is estimated to save nearly $1 billion in its first 10 years. Because the marginal cost on which that savings calculation is based is itself only an estimate, it is preferable to consider the total program savings as a spectrum rather than a point. Taking the lower and upper bounds of the 95 percent confidence interval around the estimated marginal cost, total tax credit program savings are expected to be between $590 million and $1.3 billion over the first 10 years. This study's conclusion that the PETC would generate substantial savings is shown to be robust to a variety of alternative assumptions in the model.

While the PETC program is confidently estimated to yield substantial long-term savings, its impact during the first few years after implementation is more sensitive to the particular parameters of the legislation and to the assumptions made in the model. To ensure savings during these first few years, eligibility for students already enrolled in private schools can be phased in gradually over time, and the rate of that phase-in monitored and adjusted by the legislature, if necessary, based on real-world conditions.

2. The "Public Education Tax Credit" Program

Though the terms "Public Education" and "Public Schooling" have come to be treated as synonymous, it makes sense to draw a distinction between them. Public schooling refers to a particular institution: a district-based system of government-owned, tax-funded schools. That institution exists for a purpose: to serve our individual educational needs and to advance our shared educational goals. Americans want their schools to prepare children not just for success in private life but also for participation in public life. We want all children to have access to a good education, regardless of family income. And we expect schools to help foster harmonious social relations among the many different groups that constitute our pluralistic nation — or at a minimum to not cause tensions between them. It's useful to have a name for that set of goals and ideals, and it seems reasonable to call them the aspirations of public education.

Seen this way, "public schooling" is a means and "public education" is an end. The first is nothing more nor less than a tool for trying to fulfill the second. The Public Education Tax Credit program analyzed in this paper is simply an alternative tool for advancing the ideals of public education. It does this by offering tax credits that provide all families with an easy choice of district or independent schools, religious or secular schools, traditional or progressive schools.[1] Public Education Tax Credits reduce the state and local taxes owed by anyone who pays for the private schooling of an eligible child. Parents can claim credits for their own children's educational costs, and other taxpayers (including businesses) can claim credits when they pay for the education of someone else's child, either directly or by donating to a nonprofit, scholarship-granting organization.

Both kinds of credits already exist, separately, in various states. Arizona, Florida, Georgia, Iowa, Pennsylvania and Rhode Island all provide tax credits for donations to non profit, scholarship-granting organizations (SGOs). Individuals or businesses donate money to the SGOs, and receive tax credits (essentially a tax cut) covering most or all of that amount. The SGOs, in turn, distribute the money as independent school tuition assistance to families who need it. Illinois and Iowa have "personal use" tax credits, which allow parents to receive a credit when they directly assume the cost of their own children's education. The Public Education Tax Credit act combines both scholarship donation and personal use credits, to ensure that all families have access to real educational choice. The total benefit that any child may enjoy from the program (any personal use credits claimed by their parents plus any scholarship funds received from an SGO) is capped based on family income. Children from the lowest-income families are eligible for the largest benefit, and those of progressively wealthier parents are eligible for a progressively smaller total benefit. This scaling of the benefit is both to ensure fiscal soundness and because the goal of the program is to ensure universal access to the educational marketplace, not to create universal dependence on a state program.

The PETC has been presented in detail by the Cato Institute's Adam Schaeffer, and readers are encouraged to refer to his treatment for the specifics of the legislation and the rationale behind it.[2] In the case of Nevada, it is assumed here that the PETC would apply to state sales taxes and local property taxes already committed to education.

3. Methodology

[Author's Note: This section was adapted from Andrew J. Coulson and Anca M. Cotet, "The Fiscal Impact of a Large-Scale Education Tax Credit Program," Cato Institute Policy Analysis no. 618, July 1, 2008.]

Outline of the Model

The fiscal impact of the Public Education Tax Credit act will depend on the extent to which credits lead to migration of public school students to independent schools, the extent to which existing private school families claim credits, and the effect that each of these has on tax revenues and public school spending.

When a student uses a credit (or a credit-funded scholarship) to leave the public sector for an independent school, the state can retain the funds that it would otherwise have allocated to that student's public school district. So raw state savings are equal to the average state-level per-pupil allocation times the number of migrating students. Conversely, every time a credit is claimed against state taxes — either for a migrating student or a student who had already been enrolled in private school — state tax revenues are reduced. The net *state-level* fiscal impact is thus the sum of savings from migration and revenue reductions due to credit claims against state taxes.

In order to calculate the *total* fiscal impact of education tax credits, however, it is necessary to look beyond the state level. If we are to compare savings and losses under the program with the figures that would exist without the program, we must also consider the impact on school districts. Districts will see their state funding reduced in direct proportion to the number of migrating students. This reduction will be exactly equal to the state-level savings from migration, because every dollar saved by the state is a dollar no longer received by the district. District revenues will be further reduced by credits claimed against local property taxes that would otherwise have gone to fund public school operations.[3]

Note that when computing these district revenue reductions it is important to separate out federal funding. That is because the Elementary and Secondary Education Act allocates funding based on a district's school-age population and poverty ratios rather than its public school enrollment. Hence, federal funding will not decline in proportion to migration of students to the private sector.

On the other side of the ledger, school districts will have to serve fewer students, and will thus see their costs reduced. To estimate the amount that public school districts will save, we must multiply their marginal cost by the number of students who migrate to the private sector. A district's marginal cost is the extra amount it must spend to serve one additional student, and, consequently, also the savings that it enjoys from serving one fewer student. The marginal cost of public schooling can be estimated from available data, and this is done for the state of Nevada in Appendix A.

This study assumes that marginal cost will remain constant as district enrollment falls. In other words, the savings from serving one fewer student in a district enrolling 30,000 children will be the same as those from serving one fewer student in a district enrolling 15,000 children. That is not a problematic assumption so long as migration happens gradually over time, because, in the long run, costs that we currently think of as "fixed" (i.e., independent of enrollment) become "variable" (i.e., tied to enrollment). For example, when enough children have migrated that entire school facilities can be closed and sold off, the formerly "fixed" costs of heating, cooling and maintaining those schools go away and in fact new revenue is generated by the sale.

Importantly, the fiscal impact calculation presented in this paper does *not* take into account such long-term savings from the recoupment of formerly "fixed" costs, so its savings estimates are to be considered conservative lower bounds when looking at the medium to long term. There would be plenty of time to recoup such costs, though, since experience with other school choice programs both in the United States and abroad indicates that migration to the private sector is indeed a gradual process. Families have to become aware of the program, existing private schools need time to expand their facilities, and educational entrepreneurs need time to create new schools.

Estimating Total Migration Using Elasticity of Demand

To this point we have discussed how to figure savings and revenue reductions due to the PETC program based on migration from public to private schools. But we have yet to discuss how to estimate the total amount of migration likely to occur. This study determines the propensity to migrate based on the increased attractiveness of enrolling in an independent school as a result of the PETC program reducing the out-of-pocket cost of private schooling. The lower the net private school tuition, the higher the expected migration will be. According to the Public Education Tax Credit act, the extent to which tuition is reduced by the credits depends on family income. Evaluating families' response to lower tuition rates is equivalent to measuring consumers' response to a change in the price of any other good or service and is generally measured by the price elasticity of demand. Elasticity tells us how much demand is likely to rise as price falls, and how much demand will ebb as price rises.

For example, an elasticity of demand for private education of -1 indicates that for a one percent decrease in the net tuition in private schools, there is a 1 percent increase in enrollment in those schools. There are numerous published estimates of the elasticity of demand for private education in the United States, and this paper uses the average of those figures, which is -1.1.[4] Note that the use of a substantially smaller elasticity figure, such as Chiswick and Koutroumanes' -0.48, does not alter the central conclusions of this study, as is explained in the Results and Discussion section.

This analysis predicts the total expected migration under the PETC based on the relationship between tuition after credits are taken into account and migration, given the legislation's provisions regarding the availability of tax credits and their impact on net tuition.

Calculating migration on the basis of the estimated elasticity of demand assumes that the demand function has the same elasticity at every price (Chiswick and Koutroumanes, for instance, use this assumption to estimate the elasticity of demand[5]). Quantity demanded, q, is expressed as a function of price, P (tuition in this case):

$$q = \alpha P^{\varepsilon} ,$$

where ε is the estimated elasticity of demand, ε = -1.1, and α is a scaling variable that depends on demographic characteristics.[6] If the tuition changes from P_0 to P_1, the resulting change in quantity demanded Δq may be expressed as

$$\Delta q = \alpha P_1^{\varepsilon} - \alpha P_0^{\varepsilon}$$

where P_0 is the existing tuition in private schools and P_1 is the net-of-tax-credit tuition. The scaling variable α is calculated for each income category of students (see the preceding endnote), but the elasticity ε is assumed to be the same for all families. To predict the total amount of migration, a forecast of the impact of the education tax credits on net tuition is made for each income class (this is done in row 126 of the accompanying Fiscal Impact Calculator spreadsheet file).

It is conventionally assumed that any good with a constant, negative price elasticity will be consumed by everyone if its price reaches zero. In plain English this means that consumers will consume any good about which they are price-sensitive if it becomes free — hence the ubiquitous use of "free" in advertising slogans. So, if the effective price of private schooling is reduced to zero, and it has a negative, constant price elasticity of demand, then private schools will enroll all students when their price reaches zero. But is the elasticity of demand for private schooling constant with respect to price? Chiswick and Koutroumanes, as noted above, assume that it is in their elasticity computation,[7] but from an abundance of caution the present study relaxes that assumption somewhat in estimating savings from the PETC. Specifically, the present analysis does not assume that, when effective tuition falls to zero, private sector market share will rise to 100 percent.

The question then becomes: by how much should this value be lowered to inject a healthy skepticism into the model. What should we assume private market share to rise to when effective tuition falls to zero, if not 100 percent? Some guidance is afforded by the Dutch experience, in which even heavily regulated "free" private schools secure a roughly 75 percent market share despite the presence of equally "free" public schools, nearly a century after that nation's school choice program came into existence. Since it seems likely that private schools less stifled by rules and regulations could more easily differentiate themselves to appeal to varied audiences, and thus provide a greater incentive to migrate to the private sector, the corresponding number for the PETC program should be somewhere between 75 percent and 100 percent. The present study assumes a 90 percent private market share when effective tuition reaches zero, but readers should note that no value in this range will reverse the general conclusion of this study, as will become evident from the robustness calculations presented in the Results and Discussion section. Readers wishing to explore the impact of alternative assumptions may change this 90 percent value in the "Constants" section of the Fiscal Impact Calculator Excel spreadsheet.

Estimating the Rate of Migration

While elasticity of demand allows us to estimate the *ultimate* amount of migration to independent schools, it does not tell us the duration of the migration process. Determining the rate at which the predicted migration takes place is admittedly somewhat speculative, but there is both relevant evidence and theory to guide us. Both the Netherlands and Chile have large-scale school choice programs that substantially reduce the perceived cost of private schooling. A key difference between those programs and the PETC is that the foreign programs impose considerable restrictions on participating private schools. In the Netherlands, for instance, teacher salaries, teacher training, curriculum, testing, and even school mission are constrained by the state. Participating schools must also be organized as non profits. These sorts of restrictions serve to reduce the degree of specialization and differentiation possible under the Dutch and Chilean programs, and thus the incentive families have to opt for private schooling. Because the design of the PETC makes schools and scholarship organizations directly accountable to parents and taxpayers, there is no need for intrusive regulation of the content and management of schools and thus greater opportunity for differentiation. This, in turn, will increase the perceived value-added of opting for an independent school, due to the greater likelihood of finding something other than what is already offered in the government schools. With greater justification for migration, the rate of migration would likely be faster.

In the Netherlands, private sector share had reached 70 percent by 1982 (and had been close to that level for some time), 65 years after enactment of its national school choice program.[8] Today, more than a quarter century later, private sector enrollment stands at close to 75 percent. In Chile, majority private enrollment was reached in roughly 20 years, though the pace of that growth has slowed somewhat in recent years. If these experiences are any guide, private sector enrollment growth seems to continue over the long term when the financial advantage enjoyed by "free" public schools is reduced or eliminated, but it does so at a slowing rate after the first few decades.

This is consistent with the business literature on the rate of diffusion of new options in the marketplace, which tends to follow an s-shaped (or "sigmoid") curve. Adoption is initially somewhat slow, as information about the product or service has not yet been widely disseminated. Adoption then accelerates as people who are interested come to know about it, and finally, adoption slows as most of those who are interested have already adopted it.[9] The Fiscal Impact Calculator spreadsheet accompanying this report includes a graph and set of constants depicting the particular migration curve used here. Its constants were chosen so that 90 percent of total predicted migration would be reached by year 20, while beginning with a modest level of migration in year one (less than 2 percent of public school enrollment). The precise slope of this curve is determined by a set of constants that are presented and explained in the spreadsheet, and readers wishing to explore alternative assumptions may modify those constants accordingly. (Note that we are dealing here only with the *rate* at which predicted migration will take place, not the total amount of migration, which was the subject of the preceding section.)

Now that we have both an estimate of the total ultimate migration and a curve describing the rate at which that migration is expected to occur, we can compute an estimate for the number of students who will have migrated by the n^{th} year after passage of the PETC. This allows the fiscal impact of the program to be estimated year by year, and that is how the results are presented later in this report and in the accompanying spreadsheet file.

Phase-In

To ensure a conservative savings estimate, it is best to assume that every family with children already enrolled in private school will claim a credit as soon as it is eligible to do so. This assumption, coupled with the gradual migration from public to private schools predicted by the model, could result in net losses during the first few years of operation if nothing is done to avoid that eventuality.

It is possible to eliminate this potential problem by phasing in eligibility of students already enrolled in private schools over several years. In the simplest example, it might be decided that, in year one of the program, existing private school students are eligible to participate only if they are in elementary school, while all existing private school students would become eligible in year two of the program. This concept can be generalized so that existing private school students gradually become eligible to benefit from the tax credits over any number of years — based on their age or grade. In this way, the fiscal impact of their credit claims can be spread out over a sufficient number of years to be more than offset by savings from students migrating from public to private school.

To account for this policy option, the Fiscal Impact Calculator spreadsheet allows the input of a number from 1 to 10, indicating how many years over which eligibility of existing private school students will be phased in. A value of 1 implies that all existing private school students are immediately eligible from the first year the program is enacted, while a value of 10 implies a 10-year phase-in. The "Savings(Loss) Calculations" section of the spreadsheet indicates, for each year of operation, the grade range of existing private school students who are eligible to participate (see also Table 1 in the Results and Discussion section of this paper).

The inclusion of this phase-in parameter is not meant to prejudge legislators' decisions. Some legislatures, under certain economic conditions, might decide that it is proper to allow all children to participate in the program immediately, even if this leads to net losses in the first three or four years. Alternately, lawmakers may decide that any negative fiscal impact is unacceptable, and hence opt for a phase-in sufficiently long to ensure savings from the first year of the program.

General Notes on the Spreadsheet File

The PETC Calculator spreadsheet accompanying this report uses a basic feature of Excel known as "cell names." A cell name is a user-defined term that refers to a particular cell in the spreadsheet.

Names are used in Excel formulas in place of normal cell references (such as “B2”), so that the purpose of the formulas can more easily be understood.

You can see whether or not a given cell has a name by selecting that cell and then looking at the “name box” just to the left of the Excel formula bar. This is where the cell’s name will appear, if it has one. If it is not named, a normal cell reference will be displayed.

To make matters easier, the PETC Calculator spreadsheet lists the cell name for each entry field in parentheses and italic font. For example, the very first entry field in the spreadsheet, cell E14, is named *Private_Phase_In*.

For further guidance on how to easily trace what is going on in Excel formulas, please search Excel’s built-in help facility for the terms “trace dependents,” “trace precedents” and “evaluate formula.”

4. Results and Discussion

The most recent audited figures available for spending and enrollment in Nevada public schools are for the 2006-07 school year. Summing total district spending across all 17 districts ($4,025,166,998), dividing it by enrollment (425,872), and adjusting the quotient for inflation to 2008 dollars, yields a total per-pupil value of $10,019.[10] The marginal cost of a Nevada public school student is estimated in Appendix A as $8,576, with a 95 percent confidence interval of $7,996 to $9,147. Private school enrollment for the state, extrapolating from historical data, is estimated to have been 30,745 students in 2008. Median Nevada private school tuition in that year is estimated at $5,965.

Plugging these values into the Fiscal Impact Calculator spreadsheet file, along with family income distribution and state tax data, allows us to estimate the impact of the PETC program, were it to be implemented today. That impact, over the first 25 years of the program, is presented in Figure 1 and Table 1. Note that "year 0" in Table 1 refers to present conditions, prior to the enactment of the PETC, and that a seven-year phase-in of existing private school students is assumed.

Figure 1.
Net Fiscal Impact of PETCs on Nevada, by Year

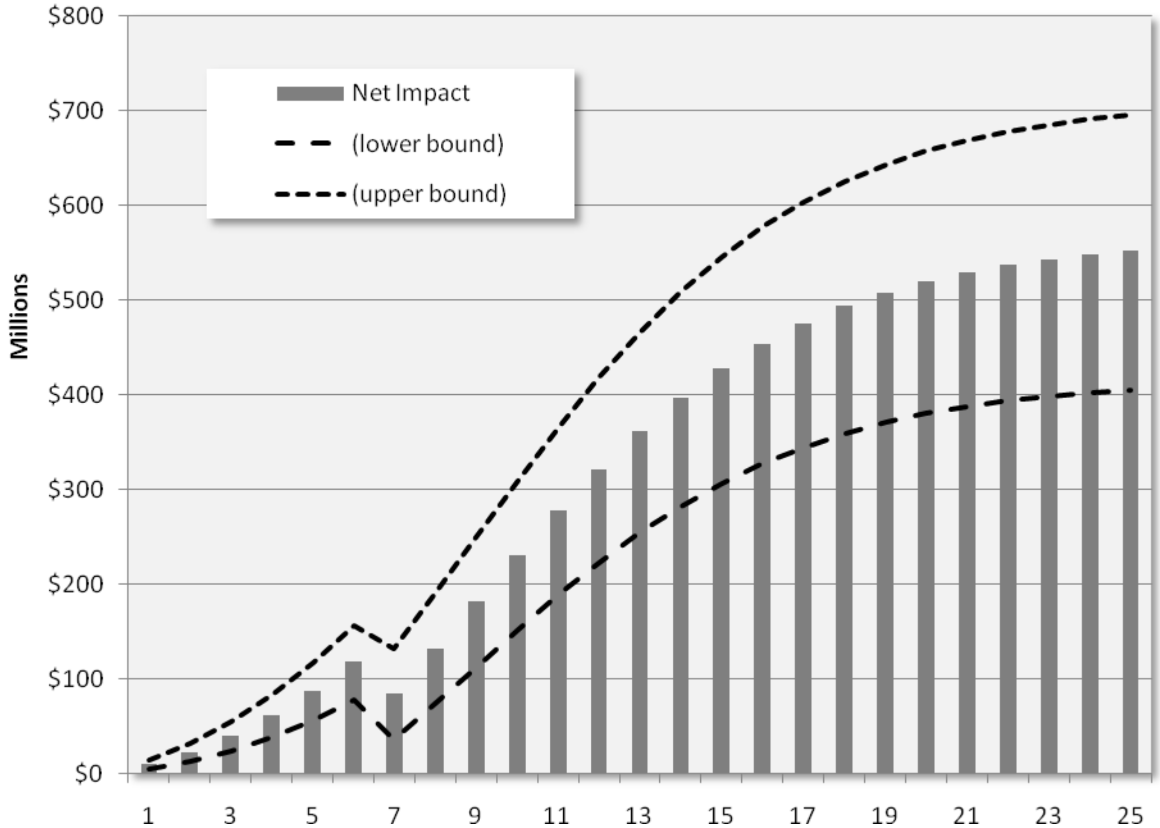


Table 1.

Net Fiscal Impact of PETCs on Nevada, by Year

(Positive impact numbers equal savings)

Year	Net Fiscal Impact	Total Public School Spending per Pupil	Private School Enrollment	Public School Enrollment	Existing Priv. Students Eligible if prev. year grade <=
0	0	$10,019	30,745	425,872	N/A
1	$9,398,565	$10,105	38,217	418,400	Kindergarten
2	$21,839,268	$10,208	46,788	409,829	1
3	$38,725,997	$10,336	56,966	399,651	2
4	$60,300,016	$10,493	68,838	387,779	3
5	$86,552,205	$10,686	82,402	374,215	4
6	$117,142,801	$10,917	97,534	359,083	5
7	$83,723,768	$11,192	113,974	342,643	12
8	$131,741,506	$11,513	131,331	325,286	12
9	$180,922,914	$11,879	149,108	307,509	12
10	$229,757,936	$12,286	166,760	289,857	12
11	$276,778,346	$12,729	183,756	272,861	12
12	$320,728,025	$13,195	199,642	256,975	12
13	$360,682,852	$13,672	214,085	242,532	12
14	$396,099,637	$14,145	226,886	229,731	12
15	$426,797,480	$14,599	237,983	218,634	12
16	$452,892,091	$15,024	247,415	209,202	12
17	$474,709,114	$15,409	255,301	201,316	12
18	$492,698,393	$15,750	261,803	194,814	12
19	$507,362,544	$16,046	267,104	189,513	12
20	$519,204,957	$16,297	271,385	185,232	12
21	$528,696,613	$16,506	274,815	181,802	12
22	$536,258,177	$16,679	277,549	179,068	12
23	$542,253,093	$16,820	279,716	176,901	12
24	$546,987,769	$16,933	281,427	175,190	12
25	$550,715,826	$17,024	282,775	173,842	12

These numbers are of course only estimates, depending on, among other things, the marginal cost calculation in Appendix A. To make that caveat more concrete, Figure 1 displays the expected program savings under both the lower and the upper bounds of the 95 percent confidence interval around the estimated marginal cost. The conclusion that Public Education Tax Credits would save a substantial amount of taxpayers' money is robust to any marginal cost figure within that range.

A further test of the robustness of these findings is to drop the magnitude of the elasticity figure used in the calculations from the average of published estimates (-1.1) to the modest Chiswick and Koutroumanes estimate (-.48). To make the scenario still more conservative, we can further lower the estimate of ultimate private school enrollment at zero tuition from 90 percent (already below the default 100 percent assumption) down to 75 percent (a level already achieved in the Netherlands, even with very heavily regulated private schools). If we couple these two assumptions with the use of our lower bound for the marginal cost of public schooling, and plug all three into the Fiscal Impact Calculator spreadsheet, we can compute a very conservative lower bound estimate on the program's 10 year savings. That value is roughly $225 million. By year 18 of the program, under all of these hostile assumptions, the *annual* savings would reach $229 million. The ability of Public Education Tax Credits to generate substantial savings thus appears highly robust to alternative assumptions.

Note that the one-time decline in expected savings shown in Table 1 from year six to year seven is due to the jump in eligibility of existing private school students. As shown in the right-most column of the table, eligibility of existing private school students is phased in gradually based on their grade in the previous year. In year six, only private school students who were enrolled in grades k-5 in the preceding year would be eligible for credits. In year seven, all private school students would become eligible, and would be expected to have credits claimed on their behalf in that year, hence the hiccup in the otherwise monotonically increasing savings chart.

By year 25 after program passage, the model estimates that 62 percent of all students would be enrolled in independent schools. This seems reasonable given the international experiences discussed earlier for programs that offer less scope for schools to differentiate themselves and cater to different audiences (and hence less incentive for families to choose independent schools).

Another important result is that per-pupil spending in public schools is estimated to rise steadily along with migration from public to independent schools. That is the natural result of using marginal cost as the basis for calculating savings. For each student who migrates, districts are expected to reduce spending by the marginal cost of the student, which is only 85 percent of the average total per-pupil cost. As more students migrate, and as that migration overwhelms revenue reductions due to credits for pre-existing private school students, the public school system enjoys a higher and higher level of per-pupil funding.

Legislators should be aware that the savings from Public Education Tax Credits will not automatically be evenly allocated between the district and the state level. As can be seen in the accompanying spreadsheet (rows 86 and 87), the state would, by default, enjoy considerable savings in each year of the program while the districts would suffer smaller losses. In order for districts to be "held harmless" by the PETC program, it would be necessary for the legislature to transfer to them a portion of its savings equal to the districts' losses. The net savings reported in the Table and Figure above are the savings left over *after the state has transferred the funds to hold districts harmless*. In the end, the districts would be left with rising per-pupil funding, and the state would be left with the *net* savings depicted in Table 1.

Summing the predicted savings over the first 10 years of the program yields an estimate of $960 million, with lower and upper bounds (based on the 95 percent confidence interval of the marginal cost) of $590 million and $1.3 billion, respectively. Readers should recall, of course, that in the long term most currently "fixed" costs will become "variable," and hence the total long-term savings from the program will likely be considerably larger than those estimated here using a marginal cost that is constant over time and does not capture these ultimately variable costs.

Because of the above observation and the demonstrable robustness of the long term savings to alternative assumptions, the chief concern for legislators should thus be the program's first few years of implementation. If savings are sought right from the first year of the program, it will be necessary to phase-in eligibility of families whose children are already enrolled in private schools over several years, as was assumed in this analysis.

Indeed legislators may wish to take advantage of the flexibility conferred by a gradual phase-in of existing private school students to respond to the actual impact of the program as it progresses. Consider two scenarios. First, based on the model and assumptions presented here, a seven year phase-in of eligibility for existing private school students would ensure savings in every year of the program. If, in practice, the early savings turn out to be greater than those predicted by this study, legislators could shorten the phase-in period so that all students would become eligible more quickly than initially planned. On the other hand, if actual savings are lower than those predicted here, the planned phase-in process could be slowed to ensure continued savings until migration numbers rise to predicted levels and continued savings are assured. The gradualness and flexibility of PETC phase-in thus provides a budgetary safety net.

5. Conclusions

It is apparent from the results of this study that a Public Education Tax Credit program would generate very substantial savings for Nevada, and that these savings are robust to a variety of alternative assumptions. The expected savings are centered on roughly one billion dollars over the program's first 10 years, and the annual savings figure is expected to rise substantially over time, reaching $229 million in year 10, $426 million in year 15, and $520 million in year 20.

At a time when most states, including Nevada, face significant challenges in order to balance their budgets, the Public Education Tax Credit act presents an attractive alternative. It would not only save taxpayers a considerable sum of money but would actually *expand* the range of educational options available to Nevada families.

Appendix A: Marginal Cost of a Nevada Public School Student

Full and detailed explanations regarding the method of estimating the marginal cost of education in public schools are already available.[11] The starting point for the approach taken here is the work of Lindsay, 2005, and that of Grecu and Lindsay, 2006.[12] Lindsay originally estimated the marginal cost of public schooling in South Carolina by pooling three years worth of school-level data in an Ordinary Least Squares regression. He regressed school-level spending against school enrollment, enrollment squared, and enrollment cubed, with period effect dummy variables for the first and second years as well as controls for cost factors such as the grade range of the school and the average teacher salary. The marginal cost was thus obtained from the coefficients of the enrollment terms.

One concern with Lindsay's original model is that teacher salary might well be endogenous — which is to say that if teacher salaries and school spending are linked, it may be as a result of intrinsically higher spending districts choosing to spend more on salaries and not simply that a more expensive local teaching workforce exogenously drives up the cost of public schooling. In other words, the causality explaining the relationship may go both ways, which violates an assumption of OLS regression. Such endogeneity can be dealt with using Two Stage Least Squares (2SLS) regression in which an "instrument" (i.e., proxy variable) is developed that is exogenous but also highly correlated with the endogenous independent variable.

Because of the endogeneity issue apparent in the South Carolina data, Grecu and Lindsay (2006) took a 2SLS approach to deal with teacher cost. In the Nevada data, this step is unnecessary since there is no statistically significant correlation between salaries and spending, once other variables are taken into account (as the reader will see below). Not only is there no problematic two-way causal relationship, there is no significant relationship at all, and hence no need for instrumentation of the salary variable.

The other notable difference between the original Lindsay study and the Grecu and Lindsay follow-up is that the latter analysis adds a control variable based on student test scores. This variable is included for consistency with the economic cost function literature, in which it is assumed that quality and cost are related, and so, to isolate the true relationship between cost and other variables, it is necessary to control quality.

There is, regrettably, no entirely suitable quality measure available for the nine-year span of Nevada data used in the present study. The potentially suitable test score data, Nevada's High School Proficiency Exam and its criterion-referenced tests in the earlier grades, are either not available for the entire span of years or were altered at one or more points during that span in ways that do not guarantee year-to-year comparability.[13] Is this a problem? It appears not. As Grecu and Lindsay found, their educational quality control variable had "no significant effect on marginal cost."[14] This is consistent with the literature.[15] There is no consistent statistically significant correlation between per-pupil spending in public schools and student achievement after controls for student and family characteristics. Furthermore, reviewing Grecu and Lindsay's results, we find that their marginal cost estimates are insignificantly *higher* when their quality control variable is included. So if the absence of such a control biases the present Nevada marginal cost estimate at all, it likely makes it lower and hence more conservative. Some may find the insignificance of the quality control term surprising, but in the context of a public school cost function it makes good theoretical sense.[16]

An additional concern expressed by Lindsay (2005), but not addressed in his subsequent work with Grecu, is that the use of school-level data makes it impossible to capture enrollment-related changes in spending that occur at the district level. To remedy that shortcoming, the present study of Nevada uses the school district as the subject of analysis. The use of district-level data in Nevada introduces its own problem, however: the limited number of observations in any given year. There are only 17 public school districts in the state, one for each county. Therefore, to obtain a sufficient number of observations to achieve meaningful regression results and precise estimates, this study analyzes nine years of data (from 1998-99 through 2006-07, inclusive).

The dependent variable used here is total district expenditures, adjusted for inflation to constant 2008 dollars. The key explanatory variable is district enrollment, and controls (also adjusted for inflation, where applicable) are included for a variety of factors that might affect spending independently of enrollment: average teachers' salaries, median house asking price from the 2000 Census (a measure of local willingness/ability to spend money on education), the percentage of students with special needs (who often require additional spending), the percentage of students in "gifted" programs (who may also engender higher costs), the percentage of Hispanic students (as a proxy for any added costs due to second-language instruction), and a set of dummy variables indicating the year in which the observation was made (to control for period effects — policy and other changes that may have induced spending changes across districts in a particular year).

What regression technique should be applied to these data? Lindsay applied OLS, while Grecu and Lindsay applied 2SLS to deal with the endogeneity issues in their model (which are not present here). Neither approach, however, takes advantage of the additional information contained in our deeper longitudinal data set: we not only have information on how separate districts differ from one another, we have information on how individual districts differ over time. To make use of that additional information, it is customary to apply "panel" or "time-series" regression.

Consider the OLS regression:

$$Y_{it} = \beta X_{it} + \varepsilon_{it}\,,$$

Where Y_{it} is total spending in district i at time t, X_{it} is an array of independent variables for district i at time t, β is an array of coefficients of those variables, and ε is the error term. If we assume that ε has an unobserved district-specific component that is constant over time (μ_i), and a unique district-year component (e_{it}), the above equation can be rewritten as:

$$Y_{it} = \beta X_{it} + \mu_i + e_{it}\,,$$

The OLS estimate of β will be biased if μ_i is correlated with any of our independent variables. To control for that possibility, we can add dummy variables to X for the first n-1 districts (where n is the number of districts). The resulting equation is called a "fixed-effects" panel model, because the μ_i error terms are fixed with respect to time. It is also known as a "within-effects" model because it examines only those changes that occur within districts, over time — ignoring information about variation between districts.

One shortcoming of the fixed-effects model is that it cannot explore the significance of explanatory variables that do not change over time. Variations in urbanicity from one district to the next, for example, might have some effect on spending and would not vary substantially within districts over a nine-year period. Similarly, our median house asking price control variable, coming as it does from the 2000 census, is invariant over time and is automatically dropped from a fixed effects model. Variations of these kind are called "between" variations, since they are constant over time, but vary between districts.

The "random-effects" panel regression model is designed to deal with situations in which there are both "within" and "between" effects. There are, furthermore, tests to determine the preferability of one model over the other. These tests, conducted in Appendix B, indicate that the random-effects model is preferred. After applying a log-log transformation[17] to the data to normalize the residuals (a requirement of linear regression), the regression results are as shown in Table A1.

Table A1.

Random Effects Panel Regression

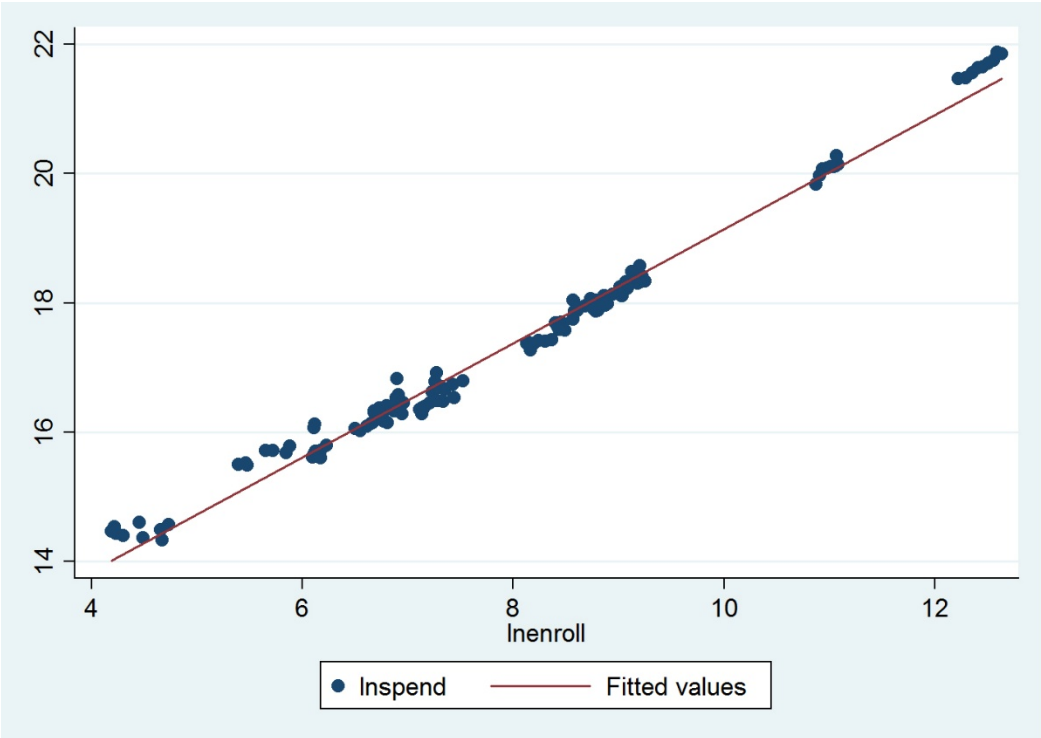
Dependent Variable: Log Total District Spending, Constant 2008 Dollars

lnenroll	0.856	Log of district enrollment
	(29.19)**	
lnsal	0.174	Log of average teacher salary
	(0.65)	
lnhouse	0.118	Log of median house asking price
	(0.66)	
lnpctdis	0.554	Log of (pct disabled/special needs students +1)
	(1.96)*	
lnpctgift	-0.926	Log of (pct "gifted" students + 1)
	(1.24)	
lnpcth	0.145	Log of (pct Hispanic students + 1)
	(0.38)	
Y0	-0.161	Year 1998-99
	(4.34)**	
Y1	-0.163	Year 1999-00
	(4.63)**	
Y2	-0.126	Year 2000-01
	(3.62)**	
Y3	-0.093	Year 2001-02
	(2.81)**	
Y4	-0.090	Year 2002-03
	(2.82)**	
Y5	-0.091	Year 2003-04
	(2.96)**	
Y6	-0.056	Year 2004-05
	(1.82)	
Y7	-0.024	Year 2005-06
	(0.73)	
Constant	7.260	
	(2.13)*	

Observations: 151
Number of DistrictID: 17
Absolute value of z statistics in parentheses
* significant at 5%; ** significant at 1%

Note that the enrollment squared and enrollment cubed terms used in Grecu and Lindsay are dropped automatically from the model after the log-log transformation due to collinearity, since log(x^b) is equal to b*log(x), a linear multiple of log(x). This does not appear to be problematic given the quite linear relationship between the log of district spending and the log of district enrollment (see Figure A1).

Figure A1.
Scatter of Log Spending on Log Enrollment



To test for multicollinearity among the independent variables we can compute each of their tolerances (i.e., the proportion of the variance for the variable in question that is not due to other independent variables, which is 1 minus the R-squared value resulting from the regression of the independent variable of interest on the remaining independent variables). These tolerances, along with their corresponding Variance Inflation Factors (equal to 1/Tolerance), are presented in Table A2.

Table A2.
Test for Multi-collinearity

Variable	Tolerance	VIF
Lnhouse	0.362	2.76
Lnenroll	0.380	2.63
y0	0.541	1.85
y1	0.550	1.82
y2	0.553	1.81
y4	0.554	1.81
lnpctgift	0.555	1.80
y7	0.556	1.80
y3	0.558	1.79
y5	0.569	1.76
y6	0.569	1.76
Lnpcth	0.666	1.50
Lnpctdis	0.731	1.37
Lnsal	0.798	1.25

VIF values above 10 are generally considered problematic, and all VIFs in the model are well below that level.

In addition to normalizing the residuals, the log-log transformation allows for the straightforward interpretation of coefficients. If β_1 is the coefficient of the *lnenroll* term, then a 1 percent increase in enrollment produces a β_1 percent increase in spending.[18] Based on the regression results in Table A1, a 1 percent increase in enrollment leads to a .856 percent increase in spending, so the marginal cost of public schooling in Nevada would be roughly 85 percent of the average per-pupil cost — with a 95 percent confidence interval of between 80 and 91 percent of average cost. This is consistent with Grecu and Lindsay, who estimate marginal cost in South Carolina at roughly 80 percent of average cost.[19]

To obtain the Nevada marginal cost estimate in dollars, we multiply the coefficient of *lnenroll* by the average total per-pupil spending at the district level ($10,019, in 2008 dollars, for the 2006-07 school year — the most recent available).[20] So the marginal cost would be roughly $8,576, with a 95 percent confidence interval of $7,996 to $9,147.

These estimates, as well as those of Grecu and Lindsay and others, suggest that public schools do amend their spending levels in response to even relatively small changes in enrollment. As a result, we can be confident that they will be able to do the same with larger shifts in enrollment such as those expected under an education tax credit program, which give greater scope for realizing savings. These findings contradict the belief, posited by some,[21] that public school costs are largely fixed and hence not conducive to savings from falling enrollment.

Appendix B:
Fixed vs. Random Effects Panel Models

The Hausman test is the principal means of determining the preferability of fixed or random effects models. Fixed effects models produce consistent estimates, while random effects models produce efficient estimates. So, if the coefficients for the two models are sufficiently similar overall, it is preferable to use the random effects model, which offers more precise estimates. A Hausman test can be run in Stata by first running the two models and then the "Hausman" command. Tables B1 and B2 present the regression results and Figure B1 presents the Hausman test results.

Table B1. Fixed Effects Panel Regression

```
Fixed-effects (within) regression               Number of obs      =       151
Group variable (i): districtid                  Number of groups   =        17

R-sq:  within  = 0.5261                         Obs per group: min =         7
       between = 0.9897                                        avg =       8.9
       overall = 0.9877                                        max =         9

                                                F(13,121)          =     10.33
corr(u_i, Xb)  = 0.9438                         Prob > F           =    0.0000

------------------------------------------------------------------------------
     lnspend |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
    lnenroll |   .6291436   .0756869     8.31   0.000     .4793015    .7789858
       lnsal |   .1697405   .2980799     0.57   0.570    -.4203872    .7598682
     lnhouse |  (dropped)
    lnpctdis |   .6034166   .2744698     2.20   0.030     .0600312    1.146802
   lnpctgift |  -1.104291   .7354481    -1.50   0.136    -2.560304    .3517227
      lnpcth |   .2301655   .4320077     0.53   0.595    -.6251076    1.085439
          y0 |  -.1368292   .0383985    -3.56   0.001    -.2128492   -.0608093
          y1 |   -.149089   .0357812    -4.17   0.000    -.2199273   -.0782508
          y2 |   -.115564   .0349184    -3.31   0.001    -.1846942   -.0464338
          y3 |   -.088761   .0328014    -2.71   0.008    -.1536999    -.023822
          y4 |   -.094163   .0307611    -3.06   0.003    -.1550628   -.0332633
          y5 |  -.0958973   .0294896    -3.25   0.001    -.1542799   -.0375148
          y6 |  -.0579346   .0294228    -1.97   0.051    -.1161847    .0003156
          y7 |  -.0200911   .0324456    -0.62   0.537    -.0843258    .0441435
       _cons |   10.48348   3.362436     3.12   0.002     3.826646     17.1403
-------------+----------------------------------------------------------------
     sigma_u |  .52985031
     sigma_e |  .08103527
         rho |  .97714399   (fraction of variance due to u_i)
------------------------------------------------------------------------------
F test that all u_i=0:     F(16, 121) =    39.41             Prob > F = 0.0000
```

Table B2. Random Effects Panel Regression

```
Random-effects GLS regression                   Number of obs      =       151
Group variable (i): districtid                  Number of groups   =        17

R-sq:  within  = 0.5147                         Obs per group: min =         7
       between = 0.9894                                        avg =       8.9
       overall = 0.9876                                        max =         9

Random effects u_i ~ Gaussian                   Wald chi2(14)      =   1779.88
corr(u_i, X)       = 0 (assumed)                Prob > chi2        =    0.0000

------------------------------------------------------------------------------
     lnspend |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
    lnenroll |   .8555619   .0293137    29.19   0.000      .798108    .9130158
       lnsal |   .1736086   .2672788     0.65   0.516    -.3502482    .6974654
     lnhouse |   .1175186   .1782384     0.66   0.510    -.2318222    .4668594
    lnpctdis |   .5541809   .2825033     1.96   0.050     .0004847    1.107877
   lnpctgift |  -.9256805   .7468685    -1.24   0.215    -2.389516     .538155
      lnpcth |   .1451643   .3859962     0.38   0.707    -.6113745     .901703
          y0 |   -.160724   .0370325    -4.34   0.000    -.2333065   -.0881416
          y1 |  -.1634268   .0352738    -4.63   0.000    -.2325622   -.0942915
          y2 |  -.1256321   .0347452    -3.62   0.000    -.1937315   -.0575327
          y3 |  -.0933045    .033236    -2.81   0.005    -.1584459   -.0281631
          y4 |  -.0898823   .0318193    -2.82   0.005    -.1522471   -.0275176
          y5 |  -.0909924   .0307651    -2.96   0.003     -.151291   -.0306938
          y6 |  -.0557744   .0306684    -1.82   0.069    -.1158833    .0043346
          y7 |  -.0242939   .0331597    -0.73   0.464    -.0892858     .040698
       _cons |   7.260422   3.403355     2.13   0.033     .5899691    13.93087
-------------+----------------------------------------------------------------
     sigma_u |  .15691115
     sigma_e |  .08103527
         rho |  .78944619   (fraction of variance due to u_i)
------------------------------------------------------------------------------
```

Figure B1. Hausman Test of Fixed Versus Random Effects

```
                 ---- Coefficients ----
             |      (b)          (B)            (b-B)     sqrt(diag(V_b-V_B))
             |       fe           re         Difference          S.E.
-------------+----------------------------------------------------------------
    lnenroll |    .6291436     .8555619       -.2264182        .0697797
       lnsal |    .1697405     .1736086       -.0038681        .1319608
    lnpctdis |    .6034166     .5541809        .0492356               .
   lnpctgift |   -1.104291    -.9256805       -.1786102               .
      lnpcth |    .2301655     .1451643        .0850013        .1940039
          y0 |   -.1368292     -.160724        .0238948        .0101506
          y1 |    -.149089    -.1634268        .0143378        .0060042
          y2 |    -.115564    -.1256321        .0100681        .0034735
          y3 |    -.088761    -.0933045        .0045435               .
          y4 |    -.094163    -.0898823       -.0042807               .
          y5 |   -.0958973    -.0909924       -.0049049               .
          y6 |   -.0579346    -.0557744       -.0021602               .
          y7 |   -.0200911    -.0242939        .0042028               .
------------------------------------------------------------------------------
                           b = consistent under Ho and Ha; obtained from xtreg
            B = inconsistent under Ha, efficient under Ho; obtained from xtreg

    Test:  Ho:  difference in coefficients not systematic

                 chi2(13) = (b-B)'[(V_b-V_B)^(-1)](b-B)
                          =       11.87
                Prob>chi2 =      0.5381
                (V_b-V_B is not positive definite)
```

These results indicate that we should prefer the more efficient random effects model because we cannot reject H_o of the Hausman test.

A further test of the appropriateness of the random effects model is to test its null hypothesis that the variance in u_i across districts (σ_u^2) is equal to zero. This can be done with the Breusch and Pagan Lagrangian multiplier test presented in figure B2 (the "xtest0" command in Stata).

Figure B2. Breusch Pagan Test for Random Effects

```
lnspend[districtid,t] = Xb + u[districtid] + e[districtid,t]

Estimated results:
                 |       Var     sd = sqrt(Var)
        ---------+-----------------------------
         lnspend |   2.823349       1.680282
               e |   .0065667       .0810353
               u |   .0246211       .1569112

Test:   Var(u) = 0
                             chi2(1) =   338.06
                         Prob > chi2 =   0.0000
```

Since we can reject the null hypothesis that the variance of u_i is equal to zero, it is clear that there are random effects present in the data and we must therefore use the random effects panel model rather than a fixed effects or simple OLS model.

Endnotes

[1] For an international and historical discussion of the relative merits of market versus monopoly school systems in satisfying the goals of public education, please see: Andrew J. Coulson, *Market Education: The Unknown History* (New Brunswick, NY, Transaction Books: 1999). A recent discussion of the relative social effects of alternative school systems in the United States is offered in Neal McCluskey, "Why We Fight: How Public Schools Cause Social Conflict," Cato Institute Policy Analysis no. 587, January 23, 2007. http://www.cato.org/pub_display.php?pub_id=7040. For a recent worldwide survey of the scientific evidence comparing academic achievement, efficiency, parental satisfaction and other outcomes between market and monopoly school systems, see: Andrew J. Coulson, "Markets vs. Monopolies in Education: A Global Review of the Evidence," Cato Institute Policy Analysis no. 620, September 10, 2008. http://www.cato.org/pub_display.php?pub_id=9634.

[2] Adam B. Schaeffer, "The Public Education Tax Credit," Cato Institute Policy Analysis no. 605, December 5, 2007. http://www.cato.org/pub_display.php?pub_id=8812

[3] Since PETCs can be applied to both state and local taxes, and since we cannot know in advance exactly what share of total credits will be applied to taxes at each level of government, we must come up with a working assumption about this distribution. In this analysis we assume that the proportion of all credits that would be claimed against state (as opposed to local) taxes will equal the ratio of revenues generated by the state-level taxes to which PETCs can be applied to the total revenue generated by all taxes to which PETCs can be applied. This approximation works very well if there is no difference in the cost (to taxpayers) of claiming a credit at the state versus the local level. In other words, it works well if it is just as easy to claim a credit against state taxes as it is to claim a credit against local taxes. The validity of that assumption depends on how the state chooses to implement the legislation.

[4] Erekson, Homer O. 1982. "Equity Targets in School Finance, Tuition Tax Credits, and the Public-Private Choice." Journal of Educational Finance 7:436-449; West, Edwin G. and Halldor Palsson. 1988. "Parental Choice of Schooling Characteristics: Estimation Using State-Wide Data." Economic Inquiry 26:725-740; Megna, Richard and Tong Hun Lee. 1990. "Estimation of the Demand for Local Public Education under a Kinked Budget Constraint." The Review of Economics and Statistics 72(4):596-602; Lankford, Hamilton and James Wychoff. 1992. "Primary and Secondary School Choice among Public and Religious Alternatives." Economics of Education Review 11:317-337; Keeler, Andrew and Warren Kriesel. 1994. "School Choice in Rural Georgia: An Empirical Analysis". Journal of Agriculture and Applied Economics. 26. 526-534; Chiswick, B.R. and S. Koutroumanes. 1996. "An Econometric Estimate of the

Demand for Private Schooling." Research in Labor Economics 15:209-237; Keeler, Andrew, Warren Kriesel, and Marc White. 1996. "School Choice Policy- Estimates of Supply and Demand Response in Private Education". Southern Rural Development Center. April; Gwartney, James D. and Richard L. Stroup. 1997. Economics: Private and Public Choice (8th edition). Dryden, New York.

[5] Chiswick and Koutroumanes.

[6] Alpha can be obtained algebraically by solving the migration equation, $\Delta q = \alpha P_1^{\varepsilon} - \alpha P_0^{\varepsilon}$, for alpha, where delta q is the change in quantity demanded (i.e., the change in private school enrollment), and then plugging in prior data for private school tuition and enrollment. Hence, $\alpha = \Delta q / (P_1^{\varepsilon} - P_0^{\varepsilon})$, and, assuming that enrollment went from zero at some initial time to its present value, we can say that initial price was infinity (i.e., that there was no price high enough at that time to allow consumption of private schooling). Since infinity to a negative power (the value of Elasticity is negative) is zero, the P_0 term drops out of the equation and we are left with

α = current enrollment / (current price)$^{\varepsilon}$

Current enrollment for each income bracket is then calculated as total private school enrollment times the share of families in the given income bracket. Note that if the delta q calculation shown above generates a number larger than current public school enrollment in the given bracket, all public students in that bracket are expected to migrate to the private sector.

[7] Barry Chiswick and Stella Koutroumanes, "An Econometric Analysis of the Demand for Private Schooling," *Research in Labor Economics* 15 (1996): 209–37.

[8] Alex Heard, "Vouchers in Holland Have Led to Control of Private Schools," *Education Week*, April 28, 1982. http://www.edweek.org/ew/articles/1982/04/28/02230049.h01.html

[9] See, for instance: Vijay Mahajan, Etan Muller, and Frank M. Bass, "Diffusion of New Products: Empirical Generalizations and Managerial Uses," *Marketing Science*, vol. 14 (1995), no. 3, part 2 of 2.

[10] See note 19 for sources.

[11] Alex Grecu and Cotton M. Lindsay, "Cost Savings from Pupil Migration to Private Schools," Clemson University (2006), http://ssrn.com/abstract=1027763; and Cotton M. Lindsay, "The Marginal Cost of a Student: A further Analysis," working paper, Clemson University, 2005, "http://www.scpolicycouncil.com/publications/61.pdf."

[12] Cotton M. Lindsay, "The Marginal Cost of a Student: A Further Analysis," research report, The South Carolina Policy Council Education Foundation, 2005. http://www.scpolicycouncil.com/publications/61.pdf. And,

Alex Grecu and Cotton M. Lindsay, "Cost Savings from Pupil Migration to Private Schools," Social Science Research Network, SSRN abstract no. 1027763. http://ssrn.com/abstract=1027763

[13] "In the 1999 Session, the Legislature... required that the HSPE be revised to measure the performance of students on the academic standards starting with the class graduating in 2003." Carol M. Stonefield, ed., "2007 Nevada Education Data Book," Research Division, Legislative Counsel Bureau of Nevada & Fiscal Analysis Division, Legislative Counsel Bureau of Nevada, February 2007, section VIII, p. 137.
http://www.leg.state.nv.us/lcb/research/EducationDataBook/2007EDB.cfm

[14] Grecu and Lindsay, p. 5.

[15] See, for instance: Eric A. Hanushek, Steven G. Rivkin and Lori L. Taylor, "Aggregation and the Estimated Effects of School Resources," The Review of Economics and Statistics, v. 78, no. 4 (November 1996), p. 626. And,

Stephen Childs and Charol Shakeshaft, "A Meta-Analysis of Research on the Relationship Between Educational Expenditures and Student Achievement," Journal of Education Finance, vol. 12, no. 3 (1986): 260. And,

Eric A. Hanushek, "The Economics of Schooling: Production and Efficiency in Public Schools," Journal of Economic Literature, v. 24 (September 1986), pp. 1141-1177. And,

Richard Vedder, Joshua Hall, and Michael Melander, "Determinants of Ohio Student Performance," working paper (Athens, Ohio: Department of Economics, Ohio University, January 15, 1998).

[16] The economic cost function literature developed to study situations in which cost and quality *should* be related. In competitive markets, producers can generally sell higher quality goods at a premium. Because higher quality thus means higher revenue, it makes sense, up to a point, to spend more on the production of higher quality goods. Based on the market's incentive structure, there *should* be a relationship between cost of production and

product quality. But is the same true in public schooling? To answer in the affirmative it would be necessary to show that public school revenues are related to school quality, controlling for other factors.

But the revenue generation mechanisms and funding formulae of public school systems have no consistent relationship with anyone's perception of quality. While districts with higher test scores are sought-after by homebuyers, driving up housing prices and thus local property tax receipts, it is also the case that low quality is often (and successfully) used as an argument to justify higher spending by public school officials. Furthermore, state school funding formulae are usually designed to compensate districts with relatively low local property tax revenue. Finally, even in very high-spending and high-scoring suburban districts it is not clear that high scores are caused by the higher spending, or are simply the result of the most affluent and well-educated parents moving to these areas and their children being relatively advantaged in their educational performance due to their home environment. It is therefore not at all clear that there *should* be relationship between quality and cost in public schooling. The empirically demonstrated absence of such a relationship is thus eminently reasonable.

[17] This implies taking the log (natural, in this case) of the terms on both sides of the equation.

[18] William H. Crown, *Statistical Models for the Social and Behavioral Sciences* (Westport, Connecticut: Greenwood Publishing Group, 1998), p. 61.

[19] Alex Grecu and Cotton M. Lindsay, “Cost Savings from Pupil Migration to Private Schools,” Clemson University (2006), http://ssrn.com/abstract=1027763.

[20] Summing up total district spending for the 2006-07 school year yields $4,025,166,998, which, divided by that year's total enrollment of 425,872, yields a per-pupil expenditure of $9,452. Adjusted for inflation into 2008 dollars using the average of the BLS inflation adjustments for 2006 and 2007 yields $10,019. Source for the expenditure data: Nevada Department of Education fiscal accountability director, Bill Arensdorf, via Geoffrey Lawrence of the Nevada Policy Research Institute. Source for enrollment data: "Nevada Department of Education Research Bulletin: Student Enrollment and Licensed Personnel Information," 2007 edition, Nevada Department of Education. http://nde.doe.nv.gov/Resources/Bulletin-FY2007.pdf

[21] For instance, Harry Miley, “Marginal Cost and the Fiscal Impact of a Proposed Tuition Tax Credit in South Carolina,” working paper prepared for The South Carolina School Boards Association and The South Carolina Association of School Administrators, Miley & Associates, 2005, http://www.scsba.org/acrobat/050207_mileystudy/050207_mileystudy_complete.pdf