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CATO
Working Paper

Do Vouchers and Tax Credits Increase Private School Regulation?

A Statistical Analysis

By Andrew J. Coulson[1]
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Abstract

School voucher and education tax credit programs have proliferated in the United States over the past two decades. Advocates have argued that they will enable families to become active consumers in a free and competitive education marketplace, but some fear that these programs may in fact bring with them a heavy regulatory burden that could stifle market forces. Until now, there has been no systematic, empirical investigation of that concern. The present paper aims to shed light on the issue by quantifying the regulations imposed on private schools both within and outside school choice programs, and then analyzing them with descriptive statistics and regression analyses. The results are tested for robustness to alternative ways of quantifying private school regulation, and to alternative regression models, and the question of causality is addressed. The study concludes that vouchers, but not tax credits, impose a substantial and statistically significant additional regulatory burden on participating private schools.

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Introduction

Over the past quarter century, scores of studies have compared the outcomes of public and private schooling. In a 2009 review of this empirical literature,[1] I found that the statistically significant results favor the private sector by a margin of eight to one. More interestingly, when the findings are winnowed down to only comparisons between minimally regulated education markets and conventional (monopolistic and heavily regulated) public school systems, the results are starker, favoring markets by a margin of 15 to one. Findings of a positive education market effect also outnumber insignificant findings by more than four to one.

One implication of this large body of empirical evidence is that, on the whole, government regulation of private schools seems, at best, to be irrelevant to their performance and quite possibly harmful to it. Indeed the superiority of minimally regulated over heavily regulated school systems extends beyond traditional educational outcomes and cost-effectiveness. When Patrick Wolf reviewed the literature comparing civic values and engagement between students in public schools and those in private (and occasionally, charter) schools, he found the same phenomenon.[2] The freely chosen, less regulated schools were clearly superior in their civic outcomes.

Whether one accepts or finds fault with this literature, whether or not one believes that some particular combination of regulations might be beneficial, it is undeniable that the overall level of regulation imposed on private schools has become a key concern among education economists, researchers, and reformers. The view that ―markets replace [ineffective] top-down accountability through regulation with [effective] bottom-up accountability to consumers”[3] is not uncommon, and it is in part because of such views that school voucher and education tax credit programs have been enacted around the United States.

But some scholars fear a Catch-22: that the very policies intended to liberalize and expand the education marketplace may ultimately lead to its regulatory suffocation. Economists Carlisle Moody and Jerry Ellig, for example, worry that ―state regulation would almost certainly follow the expenditure of state funds.... Such regulation is subject to political pressure and the public school interest groups may press the state to enforce regulations that interfere with the operation of the private school[s].”[4] When I reviewed the worldwide historical evidence on this question a decade ago, I was unable to find a single large-scale system of government funding of private elementary or secondary schooling that had escaped heavy regulation.[5]

Absent any contemporary U.S. research, however, the validity of this concern has been open to question. Some education policy experts have written that ―it's wrong to suggest [school] vouchers would open to government regulators doors not currently open to them,”[6] and that ―voucher programs do not increase the likelihood or severity of regulation of private schools.”[7]

The policy community has also failed to reach consensus on a related question: do vouchers and tax credits differ systematically in the overall regulatory burdens they impose on participating private schools? Moody and Ellig contend that education tax credits create ―less pressure for regulation,” since ―no state money is directly expended on the schools.” [Tax credit programs can either offset parents' educational costs for their own children or can offset donations to nonprofit k-12 tuition-assistance organizations serving low-income families.[8]] I have made this argument as well, also on purely theoretical grounds.[9] To date, there has been no empirical research comparing the regulatory effects of vouchers and tax credits.

Not surprisingly, the tax credits vs. vouchers question is also contentious. Disputing the view just presented, Australian economist John Humphreys wrote in 2002 that “it seems unlikely that there would be any stricter standards under a voucher system than a tax credit system, or under the current system. Indeed, in the United States, restrictions on the Milwaukee voucher programme have actually decreased and attempts to increase them again have been defeated.”[10] Though Humphreys’ latter statement was true at the time he made it, it should be noted that a set of additional regulations was imposed on the Milwaukee program in 2009.[11]

Clearly, there is a need for a systematic empirical investigation of these questions. While the amount of data available for study is regrettably small, that cannot be helped. Policy decisions are being made regarding private school choice programs every year, and many interested parties wish to know whether or not they will lead to regulatory proliferation and whether one policy would bring with it more regulation than another. Our only alternatives are to continue to fumble in the dark on these questions, or to inform ourselves as best we can with the available data. The present study attempts to do the latter.

Data and Methods

As of late September 2010, 20 voucher and tax credit programs have been enacted in 15 states and the District of Columbia to defray private school tuition for a general student population (see Table 1). These programs are the focus of the present analysis. Excluded from this study are education tax deductions and tax credits limited to non-tuition expenses. This is to avoid biasing the results in favor of conventional tax credits, since it is reasonable to expect that deductions and non-tuition credits will precipitate less regulation than full-fledged tax credits that defray tuition costs at private schools. Since there are only two such programs (both from Minnesota and both lightly regulated) they cannot be analyzed separately.[12]

Also excluded from this analysis are tax credit and voucher programs exclusively serving special needs populations, such as disabled students. Due to the highly varied circumstances of special needs children, it is effectively impossible to develop a single curriculum or testing regime that could serve them all, and hence there is substantially less political pressure to impose curriculum and testing regulations on schools serving them.

For example, Florida’s tax credit program requires participating schools to administer a norm-referenced test of their choosing, but only to their non-disabled students. Disabled students are exempt from the requirement.[13] The Florida Opportunity Scholarships voucher program required participating students to take the same test as public school students, but the public school assessment system includes alternative testing arrangements or the waiving of tests for disabled children.[14] Similarly, Florida’s McKay voucher program exclusively targeted at disabled students is exempt from any testing requirement. Clearly, serving special needs students has a distinct causal effect on regulation, independent of program type.

This puts special needs programs into a separate category from programs serving a broader student population, and at present there are too few special needs programs to allow for separate statistical analysis. Neither is it possible to combine special needs programs with the more general programs into a single analysis, for two reasons. First, doing so would require comparable data for the regulatory burdens imposed by both types of programs. But because special needs programs do not serve non-special-needs students, the level of regulation that they would impose on the education of such students is undefined. There is thus no way to directly compare the regulatory burdens imposed by these different types of programs.

Table 1. U.S. Private School Choice Programs
(Excluding those Limited to Special Needs Students)

Legislature	Type	Enacted	Ended
Arizona	scholarship donation credit (indiv.)	1997	
Arizona	scholarship donation credit (bus.)	2005	
Colorado	voucher	2003	2003
Florida	scholarship donation credit (bus.)	2001	
Florida	voucher (failing schools, statewide)	1999	2006
Georgia	scholarship donation credit	2008	
Illinois	personal credit	1999	
Indiana	scholarship donation credit	2009	
Iowa	scholarship donation credit (indiv.)	2006	
Iowa	personal credit	1987	
Louisiana	voucher (New Orleans)	2008	
Maine	voucher (small town)	1873	
National	voucher (Washington, DC)	2004	
Ohio	voucher (Cleveland)	1995	
Ohio	voucher (failing schools, statewide)	2005	
Pennsylvania	scholarship donation credit (bus.)	2001	
Rhode Island	scholarship donation credit (bus.)	2006	
Utah	voucher	2007	2007
Vermont	voucher (small town)	1869	
Wisconsin	voucher (Milwaukee)	1990	

Second, even if we could devise a single regulation metric encompassing both special needs and broader programs, there is only a single special needs tax credit program in existence; an insufficient basis for apportioning the variance in regulation among such programs between their special needs status and their program type (i.e., credit or voucher). As a result, even if it were possible to include special needs programs in a single combined analysis, doing so would impede rather than aid our effort to determine the relative regulatory impacts of generally available voucher and tax credit programs. Statistical analysis of the regulatory effects of special needs programs must therefore wait until additional data are available and a separate study can be undertaken.

To determine the regulatory impact of these programs, we begin by collecting background data on the regulations applying to all private schools in each of the states in which they operate. The chief source for these data was Christopher Hammons' very useful 2008 paper for the Foundation for Educational Choice.[15] Next we collected data on the additional regulations, if any, imposed on private schools participating in voucher or tax credit programs (by consulting the relevant enabling legislation).

To permit analysis, all of these regulations must be categorized and then quantified according to their intensity using a single set of coding rules. Those rules are laid out in Appendix A, and the corresponding data are presented in Table 2 (lower numbers and lighter colors indicate less regulation). A breakdown of the specific regulations responsible for each of these ratings, with source citations, can be found in the Excel spreadsheet file accompanying this paper.

There are, of course, many possible ways of quantifying regulatory burden. So, to ensure that the results of this study are not simply artifacts of the particular quantification we have chosen, intensive robustness testing with alternative regulation weightings is carried out in Appendix D.

Table 2. Regulation Index Values, by Program Participation and Category

State	Program	Barr. to Entry	Deliv.	Staff.	Price	Relig.	Curri.	Test.	Finan.	Admis.
Arizona	Indiv. Sch. Don.	0	2	0	0	0	0	0	0	0
Arizona	Corp. Sch. Don.	0	2	0	0	0	0	2	0	0
Florida	Corp. Sch. Don.	1	0	1	0	0	0	3	1	0
Georgia	Sch. Don.	2	2	0	0	0	2	0	0	0
Illinois	Personal Use	0	2	0	0	0	5	0	0	0
Indiana	Sch. Don.	2	3	1	0	0	3	2	0	0
Iowa	Sch. Don.	4	2	5	0	0	5	0	0	0
Iowa	Personal Use	4	2	5	0	2	5	0	5	0
Pennsylvania	Corp. Sch. Don.	3	2	0	0	0	2	0	0	0
Rhode Island	Sch. Don.	1	0	0	0	0	6	0	0	0
Ohio	Cleveland Vchr	2	2	6	3	0	2	6	0	4
Colorado	Voucher	1	4	0	0	0	2	4	1	6
National	DC Voucher	2	1	1	0	0	2	5	0	6
Florida	Voucher	2	0	1	6	3	2	4	1	6
Louisiana	Voucher	6	2	5	6	0	6	6	1	6
Maine	Voucher	2	2	6	6	6	6	4	3	0
Wisconsin	Milwaukee Vchr	4	6	1	6	3	4	4	2	6
Ohio	State Vchr	2	2	6	6	0	2	6	0	0
Utah	Voucher	0	4	1	0	0	0	3	1	0
Vermont	Voucher	3	3	4	6	6	2	4	1	0
Arizona	None	0	2	0	0	0	0	0	0	0
Colorado	None	0	2	0	0	0	2	0	0	0
Florida	None	0	0	1	0	0	0	0	0	0
Georgia	None	0	2	0	0	0	2	0	0	0
Illinois	None	0	2	0	0	0	5	0	0	0
Indiana	None	0	0	0	0	0	1	0	0	0
Iowa	None	0	2	5	0	0	3	0	0	0
Louisiana	None	0	0	4	0	0	6	0	0	0
Maine	None	0	0	0	0	0	1	0	0	0
National	None	2	1	1	0	0	2	0	0	0
Ohio	None	0	2	1	0	0	2	0	0	0
Pennsylvania	None	3	2	0	0	0	2	0	0	0
Rhode Island	None	1	0	0	0	0	6	0	0	0
Utah	None	0	0	0	0	0	0	0	0	0
Vermont	None	3	0	0	0	0	2	0	0	0
Wisconsin	None	0	2	0	0	0	2	0	0	0

A rough first estimate of the regulatory premia imposed by private school choice programs can be obtained by subtracting the default level of regulation pertaining to all private schools in a given state from the level of regulation imposed on private schools participating in a choice program. The results of that operation can be seen in Table 3.

Table 3. Regulation Premia, by Program and Category

State	Program	Barr. to Entry	Deliv.	Staff.	Price	Relig.	Curri.	Test.	Finan.	Admis.
Arizona	Indiv. Sch. Don.	0	0	0	0	0	0	0	0	0
Arizona	Corp. Sch. Don.	0	0	0	0	0	0	2	0	0
Florida	Corp. Sch. Don.	1	0	0	0	0	0	3	1	0
Georgia	Sch. Don.	2	0	0	0	0	0	0	0	0
Illinois	Personal Use	0	0	0	0	0	0	0	0	0
Indiana	Sch. Don.	2	3	1	0	0	2	2	0	0
Iowa	Sch. Don.	4	0	0	0	0	2	0	0	0
Iowa	Personal Use	4	0	0	0	2	2	0	5	0
Pennsylvania	Corp. Sch. Don.	0	0	0	0	0	0	0	0	0
Rhode Island	Sch. Don.	0	0	0	0	0	0	0	0	0
Ohio	Cleveland Vchr	2	0	5	3	0	0	6	0	4
Colorado	Voucher	1	2	0	0	0	0	4	1	6
National	DC Voucher	0	0	0	0	0	0	5	0	6
Florida	Voucher	2	0	0	6	3	2	4	1	6
Louisiana	Voucher	6	2	1	6	0	0	6	1	6
Maine	Voucher	2	2	6	6	6	5	4	3	0
Wisconsin	Milwaukee Vchr	4	4	1	6	3	2	4	2	6
Ohio	State Vchr	2	0	5	6	0	0	6	0	0
Utah	Voucher	0	4	1	0	0	0	3	1	0
Vermont	Voucher	0	3	4	6	6	0	4	1	0

At first blush, the voucher programs (bottom half of the table) seem to impose substantially more regulation than do the tax credit programs, across virtually all categories. The average regulation premium imposed by tax credits amounts to 3.8 (out of a possible 54), while vouchers impose an average regulation premium of 21.5—five times greater.

But what if programs tend to accumulate regulation over time and voucher laws are, on average, older than tax credit laws? What if legislatures that approved vouchers in the past happened to be more prone to regulation in general, merely by coincidence? There is no way to address these questions without employing multiple regression techniques.[16] To isolate school choice program effects, we must control for any other variables that might also affect private school regulation. Eight such variables were identified by the author and independent reviewers, and these are enumerated in Table 4 and explained further below.

Table 4. Control Variables for Regulation under Choice Programs

Var. Name	Description
Age	Program age in years
lnAge	Natural log of Age (to see if the effect of increasing Age diminishes over over time)
DemControl	Measure of Democratic Party control of state government in the year in which the program was passed [0-3 | +1 each for Democratic control of: house, senate, governorship]
PctPrivate	Percent of students in subject state who attend private schools
RegulationRank	*Forbes'* ranking of the subject state's regulatory climate (1 = least regulation)
TotVal	Total value of the school choice program, in dollars
AvgValPerPupil	Average per-pupil benefit value, in dollars
Enrollment	Number of students participating in the program

Given that regulations can be added to a program at any time, and not simply at the time of its initial enactment, it is reasonable to expect a positive relationship between program age and degree of regulation. Program age in years is thus a plausible control variable for the level of regulation imposed on private schools. It is also plausible, however, that once a school choice program has been in existence for a certain time, it begins to be taken for granted as is, and pressure to add further regulations may thus abate in the long term. To capture this possibility, it makes sense to include the log of the program age in years as a control.

To control for the possibility that Democrats are more or less likely than Republicans to impose regulations on private school choice programs, we include a variable for party control at the time of legislative enactment. The DemControl variable ranges from zero to three, with its value increasing by one for Democratic Party control of each of the state house, state senate, and governorship. These three measures of party control could be included separately in the model, but this would increase the likelihood of ―overfitting" (see discussion below) and, at any rate, testing revealed the three separate measures to be less significant individually than collectively (as one would expect given the need for any law to pass both legislative houses and be signed by the governor). A significant positive value would indicate that Democrats are more prone to regulating private school choice programs than are Republicans, a significant negative value would indicate the reverse, and an insignificant value would indicate no noticeable difference.

Since states that have a greater percentage of children already in private schools may be more or less prone to regulating schools participating in choice programs, we include PctPrivate among our control variables.

Another reasonable control is the general propensity for a state to enact regulations, which we measure using the *Forbes* ranking of states by regulatory climate (with 1 being the least regulated state).

On the presumption that pressure to regulate a program might be related to some measure of its magnitude, we round out our set of control variables with measures of the total amount of funding it can/does marshal (TotVal), the average size of its financial benefit per pupil (AvgValPerPupil), and the number of students it enrolls.

Table 5 shows the Regulation Index values for three groups of private schools: those in a voucher program (IsVoucher = 1), those in a tax credit program (IsCredit = 1), and those not participating in a choice program (IsVoucher = IsCredit = 0). The same table shows the values for each of the control variables listed in Table 4. Of these, only PctPrivate and RegulationRank are defined for private schools that are not participating in a choice program, because all the other controls are characteristics of choice programs.

How to analyze these data? First, we note that each of the 36 rows in Table 2 describes a particular group of private schools operating within a particular state. This hierarchical structure—groups of private schools within states—lends itself to a family of statistical analysis techniques known as multilevel modeling. In a multilevel model, we can determine how the level of regulation of private schools varies within a given state based on whether or not they participate in a school choice program, and also how the regulation of private schools in a given type of program (or outside of any program) varies between states.

This approach allows us to take maximum advantage of the fact that some states have more than one school choice program, isolating the effect of program type on regulation *within* states from confounding factors that vary *between* states. Differences in the regulation levels of the voucher and tax credit programs in Florida, for example, cannot be the result of variation in state-level causal factors, because both programs were created and operate *within* the same state. So, to the extent we have multiple program observations per state, a multilevel model removes state-level bias as a potential problem—*even bias from state-level variables that we cannot measure and do not include in the model*.

For this and other reasons,[17] the best approach to identifying the regulatory effects of vouchers and tax credits is to apply multilevel regression (see Appendix B for details) to the equation

$$\text{Regulation}_{sp} = \gamma + \alpha_0 \times \text{IsVoucher}_{sp} + \alpha_1 \times \text{IsCredit}_{sp} + \beta \times X_{sp} + \mu_s + \varepsilon_{sp}$$

where Regulation_{sp} is the Regulation Index value for private schools p in state s, γ is a constant, α_0 and α_1 are coefficients, β is an array of coefficients, X_{sp} is an array of control variables, μ_s is a state-specific error term, and ε_{sp} is an observation-specific error term. In this equation, the values of α_0 and α_1 will tell us whether and to what extent participating in voucher or tax credit programs is associated with increased private school regulation.

Table 5. Private School Regulation and Control Variables

Legislature	Is Credit	Is Voucher	Reg. Index	Age	lnAge	Dem. Cntl.	Dflt. Reg.	Pct. Private	Reg. Rank	Tot. Val.	Avg. Val. / Pupil	Enroll.
Arizona	1	0	2	13	2.64	0	2	6	38	$54,068,817	$1,909	28,324
Arizona	1	0	4	5	1.79	1	2	6	38	$7,516,746	$2,533	2,967
Arizona	0	0	2					6	38			
Colorado	0	1	18	0	0.00	0	4	8.1	22	$0	$6,261	0
Colorado	0	0	4					8.1	22			
Florida	1	0	6	9	2.30	0	1	14.7	18	$91,873,050	$3,950	23,259
Florida	0	1	25	7	2.08	0	1	14.7	18	$3,087,204	$4,206	734
Florida	0	0	1					14.7	18			
Georgia	1	0	6	2	1.10	0	4	9.5	5	$4,700,000	$4,700	1,000
Georgia	0	0	4					9.5	5			
Illinois	1	0	7	11	2.48	1	7	14.8	28	$71,014,500	$387	183,500
Illinois	0	0	7					14.8	28			
Indiana	1	0	11	1	0.69	1	1	10	15	$2,500,000	$0	0
Indiana	0	0	1					10	15			
Iowa	1	0	16	4	1.61	1.5	10	9.9	22	$7,478,872	$856	8,737
Iowa	1	0	23	23	3.18	2	10	9.9	22	$15,136,400	$79	191,600

Legislature	Is Credit	Is Voucher	Reg. Index	Age	lnAge	Dem. Cntl.	Dflt. Reg.	Pct. Private	Reg. Rank	Tot. Val.	Avg. Val. / Pupil	Enroll.
Iowa	0	0	10					9.9	22			
Louisiana	0	1	38	2	1.10	2	10	20.2	43	$4,890,912	$3,919	1,248
Louisiana	0	0	10					20.2	43			
Maine	0	1	35	137	4.93	0	1	10.8	32	$110,447,821	$8,039	13,739
Maine	0	0	1					10.8	32			
National	0	1	17	6	1.95	0	6	12	25	$11,325,600	$6,600	1,716
National	0	0	6					12	25			
Ohio	0	1	25	15	2.77	0	5	13.1	10	$17,448,704	$2,782	6,272
Ohio	0	1	24	5	1.79	0	5	13.1	10	$50,219,915	$3,959	12,685
Ohio	0	0	5					13.1	10			
Pennsylvania	1	0	7	9	2.30	0	7	18	31	$53,580,000	$1,410	38,000
Pennsylvania	0	0	7					18	31			
Rhode Island	1	0	7	4	1.61	2	7	19.1	49	$1,710,789	$5,879	291
Rhode Island	0	0	7					19.1	49			
Utah	0	1	9	0	0.00	0	0	3.6	19	$0	$2,000	0
Utah	0	0	0					3.6	19			
Vermont	0	1	29	141	4.96	0	5	13.4	33	$24,031,807	$9,773	2,459
Vermont	0	0	5					13.4	33			
Wisconsin	0	1	36	20	3.04	2	4	15.8	37	$128,268,298	$6,607	19,414
Wisconsin	0	0	4					15.8	37			

Note that, in the multilevel regression model, only the PctPrivate and RegulationRank control variables can be employed. If any of the other controls are included, the observations for private schools not participating in a choice program will automatically be dropped from the regression, because the values for those controls are only defined for school choice programs. Losing those observations eliminates the hierarchical structure of the data (because we would be left with only a single observation for most states), making multilevel analysis—and the advantages that it confers—impossible.

To determine if the multilevel model is biased by the omission of these controls, we perform a second analysis: a conventional multiple regression looking only at the choice programs themselves. While reducing the size of our dataset from 36 to 20 observations, this allows us to include all the controls listed above plus an additional one: DefaultReg, which corresponds to the Regulation Index for private schools not participating in a choice program.[18] That second equation can be expressed as

$$\text{Regulation}_i = \gamma + \alpha \times \text{IsVoucher}_i + \beta \times X_i + \varepsilon_i$$

Whereas the first model compared the level of regulation under voucher and tax credit programs to the level imposed on private schools not participating in any choice program (hence measuring the regulatory premium of each program), this second model compares the level of regulation between vouchers and tax credits (holding constant the default level of regulation on private schools). Here, the coefficient α tell us if vouchers are associated with significantly more or less regulation than tax credits.

With this new model comes a new problem: including nine control variables when we are reduced to just 20 observations (one per choice program) would guarantee ~~-o~~verfitting." Overfitting occurs when more variables are included in a model than are necessary to adequately predict the dependent variable, particularly when the ratio of the number of predictors to the number of observations is high. A model that is overfit may produce a high R-squared value on the available data set, but will usually be inferior to a more parsimonious (i.e., simpler) model in fitting new data.[19] In the context of this study, an overfit model would yield a deceptively high

R-squared while reducing our confidence that it would accurately predict the level of regulation imposed by future school choice programs.

To minimize this problem, we must identify the smallest subset of the theoretically plausible predictor variables that adequately explains the level of regulation of private school choice programs, weeding out those predictors that add little or no explanatory power to the model. The most thorough way to do this is to evaluate every possible regression equation made up of five or fewer predictors, chosen from our set of ten[20] (a process that can be easily automated using modern statistical analysis software). From the resulting 637 possible models, we identify the one that has the best combination of explanatory power and parsimony, as measured by a statistic known as Mallow's C_p.[21]

To validate the model thus identified, we also calculate two alternative measures of power and parsimony for the top three models in the Mallow's ranking just obtained. Both alternative measures, Akaike's Information Criterion (AIC) and the Bayesian Information Criterion (BIC),[22] agree with the Mallow's ranking that the optimal model is comprised of just four variables: IsVoucher, lnAge, DemControl, and RegulationRank—the other theoretically plausible control variables listed in Table 4 turn out to be poor predictors of private school regulation. Our parsimonious OLS regression equation is thus:

$$\text{Regulation}_i = \gamma + \alpha\times\text{IsVoucher}_i + \beta_0\times\text{lnAge}_i + \beta_1\times\text{DemControl}_i + \beta_2\times\text{RegulationRank}_i + \varepsilon_i$$

This model was tested to determine if it satisfies the assumptions of OLS, and the results of those tests are provided in Appendix C. The only OLS assumption that is clearly violated is the independence of the observations from one another. As already noted, several states have more than one school choice program and there may be state-specific factors affecting regulation that we have not observed and hence cannot control for with a simple OLS approach. Failing to control for the violation of this OLS assumption could lead to incorrect standard errors and confidence intervals for our explanatory variables, undermining tests of statistical significance. To deal with this issue, we produce Huber White robust standard errors, clustering our observations by state (Stata command ―cluster(StateID)").[23]

Causality

Even if the coefficients on either of our key explanatory variables (IsVoucher and IsCredit) turn out to be statistically significant, we will be left with the question: is the relationship between program type and degree of private school regulation causal? In theory, there could be an unmeasured independent variable that both causes states to prefer one type of program over the other *and* causes them to increase regulation of private schools participating in school choice programs. If so, the IsVoucher or IsCredit variables would be said to be ―endogenous," and their coefficients would no longer measure their own unique causal impact on private school regulation.

As already noted in the preceding section, our use of multi-level modeling minimizes the possibility of a confounding state-level variable that simultaneously determines both the type of program selected and the degree of regulation it imposes. That is because several states have more than one school choice program. The relationship between the level of private school regulation that these programs impose cannot be affected by variation in an unmeasured state-level variable, because the given programs are within the same state. Florida, as noted earlier, has enacted both a voucher and a tax credit program and imposed very different levels of regulation on them. That is inconsistent with the endogeneity theory, which posits that some unmeasured variable simultaneously determines both propensity to regulate choice programs *and* the type of choice program enacted.

In principle, we could also test for endogeneity by using a two-stage-least-squares model with instrumental variables, but this method is discouraged for small sample sizes such as we are faced with here.[24] Moreover, even if we had more observations, no obvious instrumental variables for program type present themselves, making this test impossible. Fortunately, there is considerable additional evidence that endogeneity is unlikely to be a problem.

First, consider that a state's general proclivity to regulate is captured by the RegulationRank variable, and this variable is controlled for in the OLS model. Furthermore, RegulationRank's correlation with IsVoucher is very small and negative (-0.07). So a state's general proclivity to regulate is associated with *neither* higher regulation of school choice programs *nor* the selection of one program type over another. It thus cannot be a source of endogeneity.

Second, consider that DefaultReg (the default level of regulation imposed on all private schools in a given state) is not a statistically significant predictor of choice program regulation (and it was dropped from our parsimonious OLS regression for that reason). Furthermore, its level of correlation with IsVoucher is also small and negative (-0.16). So even *the specific proclivity to regulate private schools* is insignificantly (and negatively) correlated with choosing vouchers over credits, and is irrelevant to how heavily regulated a choice program is. So it, too, cannot be a source of endogeneity.

Third, Illinois came very close to passing a voucher bill in May 2010 that would have imposed some extra regulations on private schools, but that state already has a tax credit program that does *not* impose any additional regulations. As with the Florida example given above, this is inconsistent with the endogeneity of IsVoucher or IsCredit.

Fourth, Arizona enacted two special needs voucher programs within a few years of enacting its tax credit programs. This suggests that the state shows no particular favoritism for one type of program over another, contrary to what we would expect under the endogeneity hypothesis.

Fifth, the DemControl variable is a significant predictor of higher program regulation, but is only very weakly (and *negatively*) correlated with IsVoucher (-0.26). This is inconsistent with the endogeneity hypothesis—even more so given the results reported in the following section.

Together, these facts and the resistance of our multilevel model to state-level omitted variable bias militate against the likelihood of endogeneity of IsVoucher and IsCredit. To the extent that IsVoucher or IsCredit is a significant predictor of private school regulatory burden, it therefore seems reasonable to conclude that the relationship is causal, though we cannot remove all uncertainty on this point.

Findings and Discussion

The detailed results of the multilevel and robust OLS regression analyses appear in appendices B and C, respectively, and are summarized in Table 5. Each row reports the coefficient value for the given variable and, in parentheses, its standard error.

Table 5. Summary Regression Results

	Multilevel (fixed effects)	OLS (robust)
IsVoucher	21.62***	18.85***
	(2.20)	(2.20)
IsCredit	3.45	.
	(2.28)	
PctPrivate	(dropped)	.
lnAge	.	3.63**
		(.93)
DemControl	.	7.99***
		(1.26)
Reg.Rank	(dropped)	-.24*
		(.086)
N	36	20
R-squared	.84	.90

* p <.05, ** p < .01, *** p < .001

In the multilevel model, the coefficient of the IsVoucher variable is highly significant ($p < .001$) and approximately equal to 22. Nineteen times out of twenty, participation in a voucher program is expected to be associated with a private school regulatory burden that is 17 to 26 points higher on the Regulation Index than the burden imposed on schools not participating in a choice program.

To put that in perspective, the average Regulation Index value for private schools not participating in a choice program is 4.6, and the standard deviation is 3.1. The IsVoucher coefficient is thus not simply highly significant, it is also large relative to the default level of regulation applying to private schools as a whole.

By contrast, the IsCredit coefficient is small and statistically insignificant at any conventional level in the multilevel regression, so we have no evidence that tax credits systematically lead to increased regulation on private schools.

When we compare the regulatory burdens associated with voucher and tax credit programs to one another, via the OLS regression, participation in a voucher program is once again found to have a large, highly statistically significant effect. This, it should be remembered, is after the consideration of a variety of theoretically plausible control variables (most of which were found to add little to the explanatory power of the model), and the inclusion of the three important controls (lnAge and DemControl and RegulationRank). The slight difference in the IsVoucher coefficient between the two models is primarily accounted for by the fact that, in the multilevel model, vouchers are being compared to private schools not participating in a choice program, and in the OLS model vouchers are compared to tax credits.

To make the meaning of IsVoucher comparable across the two models, we can subtract the (insignificant) OLS IsCredit coefficient from the IsVoucher coefficient in the multilevel model. Doing so, we are left with 18.2, which is quite close to the OLS coefficient of 18.9. This level of consistency indicates that our estimate of the regulatory impact of vouchers is ―sturdy" in the face of alternative statistical techniques and model specifications, bolstering our confidence in it.

As noted earlier, all of these results are based on the regulatory burden index developed in Appendix A. It is important to determine, therefore, whether or not they are robust to alternative measures of regulation. Do the results just reported still hold if we weight curriculum regulations up to four times less heavily than price controls? If we weight them up to four times more heavily? These questions are addressed in Appendix D, and the conclusion of that investigation is that our regression results are highly robust to wide variations in the calculation of the Regulation Index. IsVoucher remains statistically significant ($p < .01$) across every one of 2,000 different randomly weighted versions of the Regulation Index for both the OLS and multi-level models, whereas IsCredit rises to statistical significance ($p < .05$) in only 15 of those 2,000 randomized weightings—less than one percent of the cases. In other words, the results reported in this paper are almost entirely immune to wide variations in the way that regulatory burden is quantified.

Conclusion

Because of the limited number of observations available for analysis (and associated methodological considerations), we cannot be certain that the findings just described will be replicated in other states that choose to enact private school choice programs. Nevertheless, there is reason to expect our findings to generalize. Within our sample at least, the variation in regulation *within* states is much greater than the variation *between* states. Within-state factors, most notably whether or not private schools are participating in a voucher program, explain the vast majority of the variance in private school regulation in our sample. It is possible that, in states that have not yet enacted vouchers or tax credits, there could be large, new state-level effects that do not exist in our current sample and that would significantly alter the regression results. There is, however, no obvious reason to expect such effects.

In any event, to the extent that our findings can be generalized, they suggest that:

- Voucher programs are associated with large and highly statistically significant increases in the regulatory burden imposed on private schools (compared to schools not participating in choice programs). And this relationship is, more likely than not, causal.
- Tax credits do not appear to have a similar association.

These results are robust to widely differing ways of quantifying private school regulation, as demonstrated in Appendix D. Even if some kinds of regulations are viewed as much less or much more important than others, the regulatory impact of participating in a voucher program remains significant and the regulatory impact of participating in a tax credit program remains (in over 99 percent of cases) insignificant. As new programs are enacted, and existing programs are modified, these questions should of course be revisited.

In light of these findings, tax credits seem significantly less likely than vouchers to suffer the Catch-22 described in the introduction—less likely to suffocate the very markets to which they aim to expand access. But several caveats are in order. There is variation in regulatory burden within each type of program as well as between them—so it is important to evaluate programs individually.[25] And while market freedom is a very important consideration in weighing school choice policies, it is not the only consideration. Other factors, from social effects to growth rates to state constitutional hurdles, must also be considered.[26]

Appendix A. Quantifying Private School Regulation

Table A1 presents the quantification system for our Regulation Index variable. Table 2, in the body of the text, presents the Regulation Index category values for each group of private schools, by program participation and state, based on the relevant legislation.

For each of nine categories, values between 0 and 6 are assigned based on the severity/number of the corresponding regulations. Some categories, such as Price Controls, are presented as a simple list of descriptions and their scores, while others are presented as a combination of base scores and score modifiers. These two presentations are functionally equivalent, and the latter approach is used merely to save space (because, otherwise, we would have to list all the combinations of the base scores and the modifiers as separate rows in the table). A discussion of the rationale for including each regulation category in the index follows the table.

Table A1. Regulation Index Scoring, by Category

Price Controls	
No price controls	0
Partial controls	3
Fixed price, no co-pay	6
Admissions Controls	
Unfettered	0
Must use random lottery or first come first served for some grades	3
No autonomy (random lottery or FCFS admissions for all students)	6
Cannot cater to specific religious constituencies	+1
Some form of enrollment cap	+2
Curriculum Regulations (base score)	
No curriculum guidelines	0
Limited general framework	2
Extensive or detailed framework	4
Extensive and detailed framework	6
Curriculum Regulations (mod factors--min total = 0)	
Some exemptions (e.g., rules only apply > enrollment)	-1
Instruction must be in English	+1
Testing Requirements (base score)	
No testing requirement	0
Some testing required, but schools choose tests	2
Some specific tests required	4
Specific high-stakes tests required	6
Testing Requirements (mod factor--max total = 6)	
School must publish results	+1

Barriers to Entry (base score)	
No barriers to entry	0
Must be accredited/chartered by one of sev. bodies	2
Must be accredited/chartered by a single state body	4
New schools may not enter program	6
Barriers to Entry (mod factors--max total = 6)	
Simple, inexpensive government registration	+1
Moderately complex/expensive government registration	+2
Very complex/expensive government registration	+3
Local education authorities enforce rules	+1
Restrictions of Religious Freedom (base score)	
No restrictions	0
Devotional instruction cannot be required	3
School must be secular	6
Religious Restrictions (mod factors--max total = 6)	
Vouchers/credits may not defray devotional instruction costs	+2
Staffing Regulations (base score)	
No restrictions	0
College degree	1
One year of government-mandated training	3
Full teaching degree from government-accredited program	5
Staffing regulations (mod factors--max total = 6, min = 0)	
May not consider candidates' religion	+2
Mandatory collective bargaining	+3
All regulations apply only to management, not teachers	-2
Must have special skills if no college degree	+1
Maximum pupil/teacher ratio	+1
Minimum size	+1
Staffing requirements based on school size	+1
Financial Regulations (base score)	
No financial regulations	0
Subsidies discriminate against for-profit schools	2
Cannot be operated for profit	5
Financial regulations (mod factors--max total = 6)	
Surety bond or equivalent required	+1
Basic audit or equivalent required	+1
Extensive audit or equivalent required	+2
Delivery/Facilities Regulations (base score)	
No delivery/facilities regulations	0
Minimal/non-specific facilities rules	1
Modest facilities rules or some limits on virtual schools	2
Expensive/extensive facilities rules or no virtual schooling	4
Delivery/Facilities Regulations (mod factors--max total = 6)	
Loose guidelines on the number of hours per day of class time	+1
Tight rules on the number of hours per day of class time	+2

The ultimate Regulation Index value for a given group of private schools is simply the equally weighted sum of all its category scores. Any such index is necessarily arbitrary, and readers may feel that some categories should be weighted more heavily than others. To test whether or not alternative weightings affect the central results of this paper, intensive robustness testing was undertaken and its findings are reported in Appendix D. The rationales for including each category are described below.

Prices determined by supply and demand are an essential feature of market systems. They communicate to producers what consumers want and provide incentives for producers to supply the most sought-after goods and services. Without the information and incentives provided by freely determined prices, the market's operation is grossly impeded.

Specialization and the division of labor are also core features of markets. It is no coincidence that they are the first topic of extended discussion in Adam Smith's *The Wealth of Nations*. Without them, the development of specialized expertise is hobbled, and along with it efficiency and innovation. Admissions regulations that force every school to accept students on a random lottery basis when oversubscribed interfere with the ability of schools to tailor their services to particular audiences. This is, moreover, a more severe regulatory burden than that obtaining within conventional public school systems. Contrary to the common statement that "public schools accept all comers," public school systems frequently place students that they are unable to serve in private schools that specialize in educating children with their particular needs. Hundreds of thousands of students are so placed every year.[27] Even in the case of students without special needs, schools in a given district need not accept any student outside their catchment area. What the public school system guarantees is thus that every child will be served *somewhere*, not that every school will (or will be able to) serve every child.

Curriculum regulations are an obvious further imposition on specialization and the division of labor. They also undermine the power of consumer choice—if all the instructional offerings in the marketplace are homogenized, parents no longer have meaningful choice (evoking Henry Ford's: "any color you want, so long as it's black"). What is less often recognized is that state-mandated testing also exerts a homogenizing pressure on what is taught. Reporting poor results on an official test—even one that does not well reflect a school's mission—would put it at a competitive disadvantage. So an art-centric school that posts poor science scores is under pressure to increase the time and intensity of its science classes in order to avoid a black eye on official tests, which thereby takes away from its core mission. Though language learning occurs most easily in younger children, a school that opted to focus on foreign languages and history in the early grades and then turn to mathematics in the later grades would be at a grave disadvantage on official mathematics tests in the early grades, creating pressure for it to abandon its pedagogical mission.

The entry of new firms into a marketplace is a lynch-pin of innovation and productivity growth, both directly and indirectly. New firms that survive their initial start-up phase are usually better able to use the latest technology and thus to enjoy higher productivity growth than established firms. And, as Schumpeter argued[28] and subsequent research has confirmed,[29] the entry of these new firms—and even the mere threat of their entry—is enough to drive existing firms to pursue innovation and seek productivity growth internally. Those existing firms that are unable to keep up the pace of improvement are supplanted by their competitors—a process Schumpeter termed "creative destruction." Regulations inhibiting entry of new schools are thus inimical to innovation and efficiency improvements.

There is unquestionably very substantial demand for religious schooling in the United States, and religious (particularly Catholic) schools are generally found to be more efficient, have equal or better academic achievement, and have higher attainment than public schools. James Coleman[30] and later Bryk, Lee and Holland[31] ascribe some of these advantages to the ability of religious schools to create institutional cohesion and a sense of community based around their faith. Inhibiting religious freedom in education is thus not only deleterious to parental choice, it is also likely to be injurious to school effectiveness and efficiency.

A key tool that employers have for securing the faithful execution of the firm's mission is the ability to hire and retain employees who share that mission. Conversely, one of the most commonly cited reasons for the failure of pedagogical reforms to scale up within the public school sector is the inability of the reformers to ensure that principals fully understand and agree with their approach, and the inability of principals to exclusively hire and retain teachers committed to pursuing that approach.[32] Regulations that inhibit the freedom of school managers to select and retain whatever employees they deem most capable of undertaking the work at hand necessarily impede institutional success.

The profit and loss system is central to the operation of markets. The ability to distribute profits to shareholders is essential to raising investor capital for expansion, research, and development. High levels of profitability attract new entrepreneurs into a field, expanding the availability of the most valued products or services and ultimately driving down prices, raising quality or both. Forbidding profit-making schools is thus apt to inhibit the growth and dissemination of the best educational services. The imposition of other financial regulations on schools participating in choice programs—regulations to which private schools are not otherwise subject—consume resources that schools could spend in the pursuit of their mission.

Finally, regulations on the manner in which schooling is delivered—e.g., length of school days and years, permissibility of virtual schooling—circumscribe the range of offerings available to families and inhibit the development of potentially highly efficient new education delivery mechanisms.

Appendix B. Multilevel Regression Results

Multilevel generalized least squares (GLS) regressions can be performed assuming either "fixed effects" or "random effects." A fixed effects model looks only at the variation in regulation within states and ignores variation between states, while a random effects model considers variation both within and between states. The coefficients of fixed effects models are consistent but not necessarily efficient (i.e., they may have unnecessarily wide confidence intervals), while the coefficients of random effects models are efficient (narrower confidence intervals) but not necessarily consistent (i.e., possibly biased).

To minimize the risk of bias, we must use the fixed effects model unless it can be shown that the coefficients produced by the random effects model are satisfactorily similar (e.g., via a Hausman test) and also that there are indeed random effects to measure (e.g., via a Breusch and Pagan Lagrangian multiplier test). The null hypothesis of the Breusch and Pagan test is that the variance of the group error, μ_i, is equal to zero—that there are in fact no state-level ("random") effects. Tables B1 through B4 present the results of both regressions and both tests. Note that the PctPrivate and RegulationRank control variables are automatically dropped from the fixed effects regression since they do not vary within states.

Table B1. Fixed Effects Regression Results

```
xtreg regindex isvoucher iscredit pctprivate regulationrank, i(StateID) fe

Fixed-effects (within) regression               Number of obs      =        36
Group variable (i): StateID                     Number of groups   =        16

R-sq:  within  = 0.8433                         Obs per group: min =         2
       between = 0.4128                                        avg =       2.3
       overall = 0.6872                                        max =         3

                                                F(2,18)            =     48.44
corr(u_i, Xb)  = -0.0759                        Prob > F           =    0.0000

------------------------------------------------------------------------------
    regindex |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   isvoucher |   21.61746   2.203911     9.81   0.000     16.98721     26.2477
    iscredit |   3.453145   2.284079     1.51   0.148    -1.345528    8.251818
  pctprivate |  (dropped)
regulation~k |  (dropped)
       _cons |   4.674832   1.217232     3.84   0.001     2.117523    7.232142
-------------+----------------------------------------------------------------
     sigma_u |  4.9948269
     sigma_e |  4.8328722
         rho |  .51647494   (fraction of variance due to u_i)
------------------------------------------------------------------------------
F test that all u_i=0:     F(15, 18) =     1.69             Prob > F = 0.1440
```

Table B2. Random Effects Regression Results

```
xtreg regindex isvoucher iscredit pctprivate regulationrank, i(StateID) re

Random-effects GLS regression                   Number of obs      =        36
Group variable (i): StateID                     Number of groups   =        16

R-sq:  within  = 0.8424                         Obs per group: min =         2
       between = 0.6293                                        avg =       2.3
       overall = 0.7514                                        max =         3

Random effects u_i ~ Gaussian                   Wald chi2(4)       =    117.41
corr(u_i, X)       = 0 (assumed)                Prob > chi2        =    0.0000

------------------------------------------------------------------------------
    regindex |      Coef.   Std. Err.      z    P>|z|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   isvoucher |   21.25085    2.02224    10.51   0.000     17.28734    25.21437
    iscredit |    4.12928   2.051409     2.01   0.044     .1085928    8.149968
  pctprivate |   .5939145   .2820513     2.11   0.035     .0411041    1.146725
regulation~k |   .0281553   .1081711     0.26   0.795    -.1838562    .2401668
       _cons |  -3.513944   3.642051    -0.96   0.335    -10.65223    3.624344
-------------+----------------------------------------------------------------
     sigma_u |  3.0678432
     sigma_e |  4.8328722
         rho |  .28721836   (fraction of variance due to u_i)
------------------------------------------------------------------------------
```

Table B3. Hausman Test Results

```
                 ---- Coefficients ----
             |      (b)          (B)            (b-B)     sqrt(diag(V_b-V_B))
             |     fixed        random       Difference          S.E.
-------------+----------------------------------------------------------------
   isvoucher |     21.61746     21.25085        .3666048        .8762247
    iscredit |     3.453145      4.12928       -.6761351        1.004361
------------------------------------------------------------------------------
                           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(2) = (b-B)'[(V_b-V_B)^(-1)](b-B)
                          =        0.53
                Prob>chi2 =      0.7676
```

Table B4. Breusch and Pagan Test Results

```
regindex[StateID,t] = Xb + u[StateID] + e[StateID,t]

Estimated results:
                |       Var     sd = sqrt(Var)
        ---------+-----------------------------
        regindex |   116.6373       10.79988
               e |   23.35665       4.832872
               u |   9.411662       3.067843

Test:   Var(u) = 0
                            chi2(1) =     3.48
                      Prob > chi2 =     0.0623
```

Though the Hausman test would allow us to use the random effects model (because we cannot reject its null hypothesis) the Breusch and Pagan test does not allow us to reject the possibility that there are in fact no random effects to measure (because we cannot reject its null hypothesis that the variance of $\mu_i = 0$). Hence we are left with the coefficients and the confidence intervals of the fixed effects model.

Appendix C. Robust OLS Regression Results

The results for the regression with robust standard errors of the parsimonious OLS model appear in Table C1.

Table C1. Robust OLS Regression Results

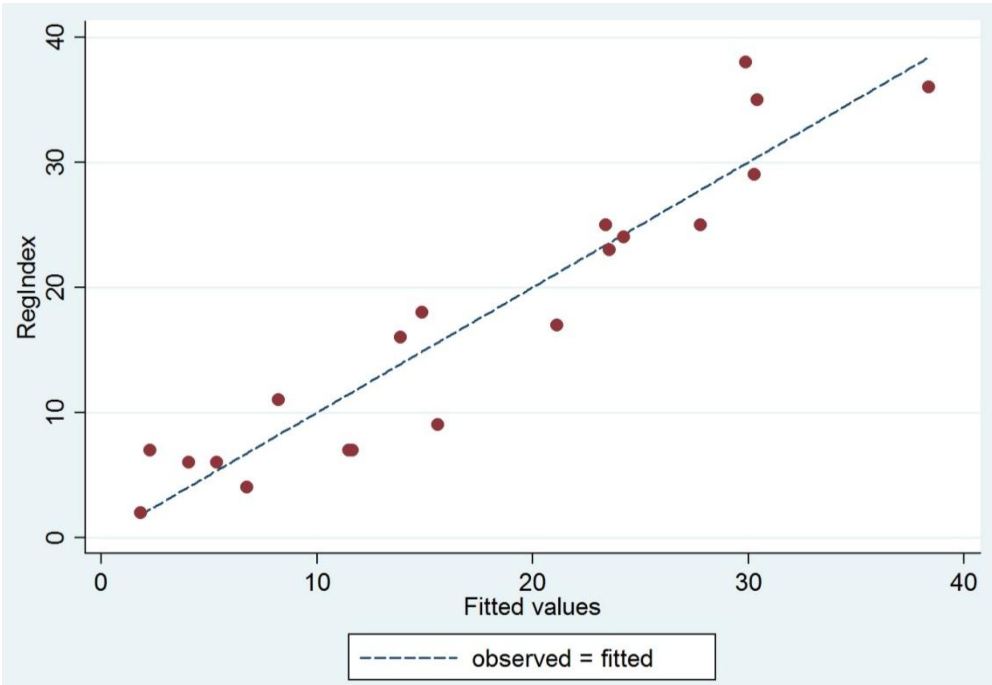
```
reg regindex isvoucher lnage demcontrol regulationrank, cluster(StateID)

Linear regression                                      Number of obs =      20
                                                       F(  4,    15) =   40.91
                                                       Prob > F      =  0.0000
                                                       R-squared     =  0.8955
Number of clusters (StateID) = 16                      Root MSE      =  4.1899
```

regindex	Coef.	Robust Std. Err.	Beta Coef.	t	P>|t|	[95% Conf.	Interval]
isvoucher	18.84905	2.207913	0.79	8.54	0.000	14.143	23.55511
lnage	3.631224	.9259558	0.44	3.92	0.001	1.657596	5.604852
demcontrol	7.994318	1.259135	0.62	6.35	0.000	5.310537	10.6781
regulation~k	-.237059	.0857815	-.26	-2.76	0.014	-.4198979	-.0542202
_cons	1.253603	2.403601	0.08	0.52	0.610	-3.869552	6.376759

The OLS regression reveals that vouchers are associated with a significantly higher level of private school regulation than are tax credit programs ($p < .001$). IsVoucher is in fact the most statistically significant variable in the model (having the highest t-score). Furthermore, as the Beta coefficients[33] make clear, IsVoucher also has the largest *magnitude* of any explanatory variable. And, despite its parsimony, this model produces a good fit, with an R-squared value of nearly 0.9 (the customary ―adjusted R-squared" figure cannot be computed when robust standard errors are used, but with only four variables in the model, R-squared itself is a reasonable measure of goodness of fit). To illustrate, a plot of the observed versus fitted regulation index values appears in Figure C1.

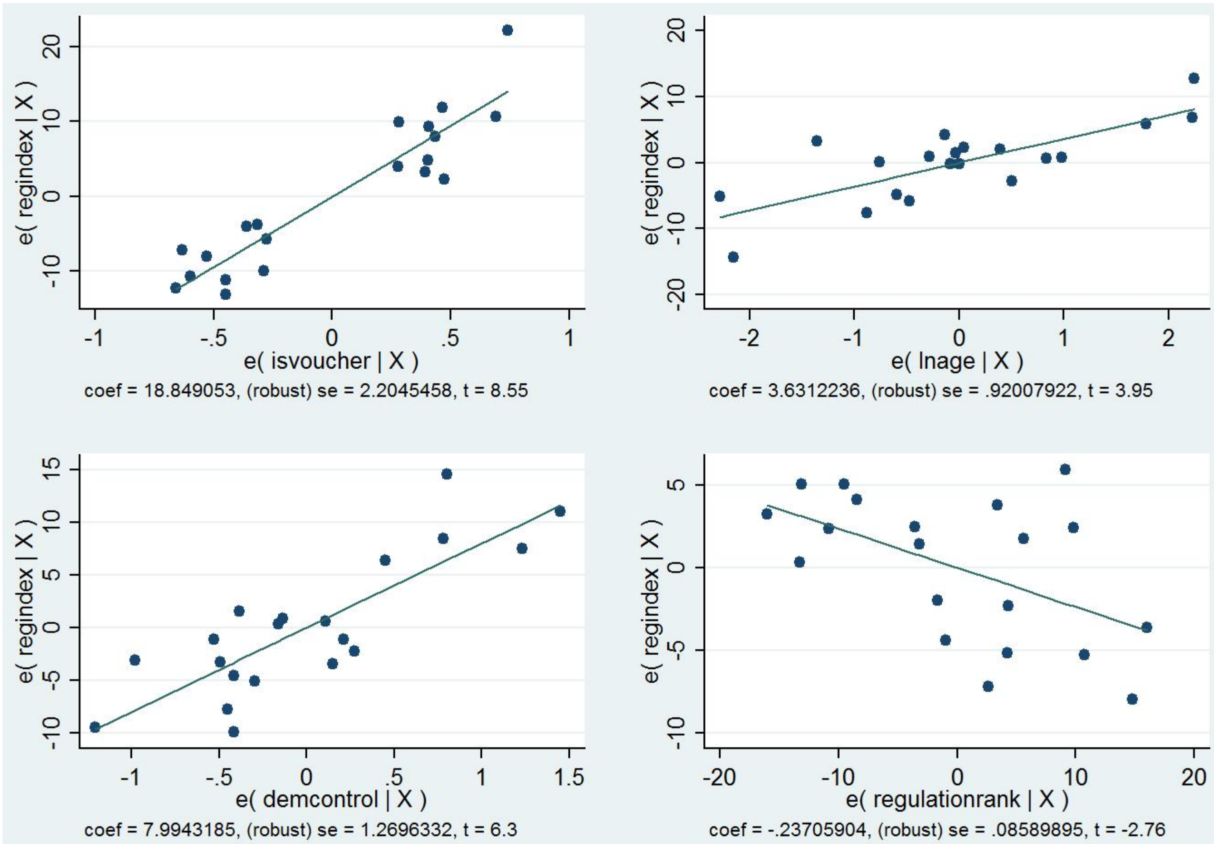
Figure C1. Observed Versus Fitted Values



The estimates in Table C1 of course depend on the validity of the assumptions of OLS regression, including: linearity of the relationships between the predictors and the dependent variable, little multicollinearity among the predictors, normality of the residuals, and lack of omitted variable bias. A scatter plot of residuals versus fitted values shows little heteroskedasticity, but we omit that plot here because the homoskedasticity assumption of OLS is relaxed when robust standard errors are used, as in the present case.

To verify the linearity of the relationships of the predictors to the dependent variable, we generate added variable plots for each of them (see Figure C2).[34] No non-linearity problems are evident.

Figure C2. Added Variable Plots



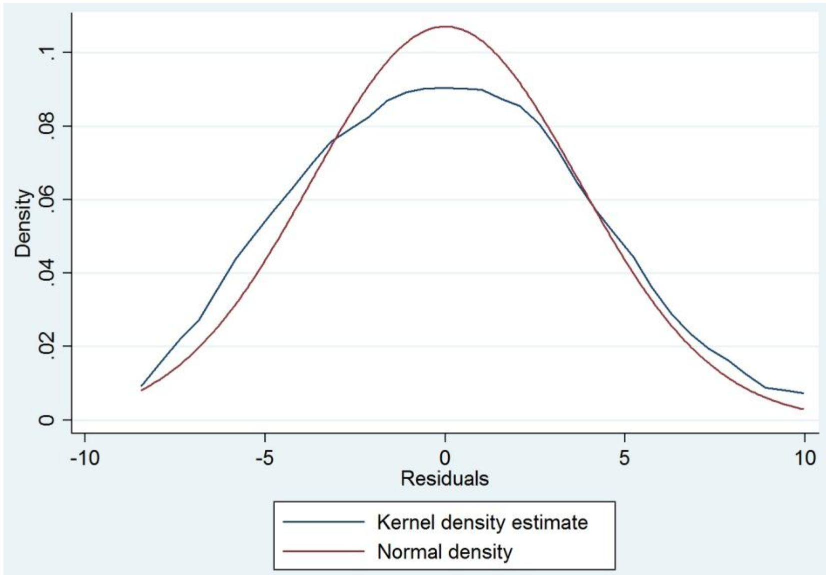
To test for multicollinearity among the predictors, we compute each of their tolerances (the proportion of their variance that is not due to other predictors, calculated as 1 minus the R-squared value resulting from the regression of the predictor of interest on the remaining predictors). These tolerances, along with their corresponding Variance Inflation Factors (equal to 1/tolerance), are presented in Table C2. Clearly, our predictors are substantially linearly independent of one another.

Table C2. Test of Multicollinearity

Variable	VIF	1/VIF
demcontrol	1.49	0.669531
regulation~k	1.49	0.671526
lnage	1.13	0.886587
isvoucher	1.10	0.912988
Mean VIF	1.30	

To test the normality of our residuals, we compute a kernel density function for them and then compare it against a normal curve (see Figure C3), finding them to conform relatively well.

Figure C3. Test for Normality of Residuals



While there is no conclusive test for the absence of omitted variable bias, two common tests are available that can detect its presence. Linktest adds the square of the predicted values as a new explanatory variable, which should not be statistically significant. If it is, we cannot reject the null hypothesis that a significant predictor has been omitted. The linktest results, given in Table C3, suggest that we can reject the null hypothesis that there are omitted variables at any conventional level of significance.

Table C3. Linktest for Omitted Variable Bias

```
      Source |       SS       df       MS              Number of obs =      20
-------------+------------------------------           F(  2,    17) =   76.95
       Model |  2269.09816     2  1134.54908           Prob > F      =  0.0000
    Residual |  250.651837    17  14.7442257           R-squared     =  0.9005
-------------+------------------------------           Adj R-squared =  0.8888
       Total |     2519.75    19  132.618421           Root MSE      =  3.8398

------------------------------------------------------------------------------
    regindex |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
        _hat |   .7197678   .3128792     2.30   0.034     .0596503    1.379885
      _hatsq |   .0076645   .0082669     0.93   0.367    -.0097771     .025106
       _cons |   1.688627   2.449259     0.69   0.500    -3.478858    6.856111
------------------------------------------------------------------------------
```

A more rigorous form of linktest is the Ramsey Reset Test, which adds several higher power terms of the predicted values as test predictors. Its results are given in Table C4.

Table C4. Ramsey Reset Test for Omitted Variable Bias

```
Ramsey RESET test using powers of the fitted values of regindex
      Ho:  model has no omitted variables
                    F(3, 12) =      2.79
                    Prob > F =      0.0862
```

Once again, we can reject the null hypothesis that there are omitted variables in our model at any standard level of statistical significance.

Appendix D. Robustness Testing

The results reported elsewhere in this paper depend on the Regulation Index described in Appendix A, which weights all nine categories of regulation equally. But some types of regulation are arguably more onerous than others, in which case varying weights should be assigned to each regulation category when the overall Index value is computed. To test whether or not this paper's findings are robust to such alternative weightings, Stata's programming language was used to create 2,000 different random weightings, compute the resulting new regulation index values for each observation, and then apply both regression models to each of the randomized runs.

These randomized runs allowed each category of regulation to receive a weight between 0.5 and 2 times the default value—so that the highest possible weight is four times the magnitude of the lowest possible weight. For example, on any given run, the weight assigned to the Barriers to Entry regulation category could be one half (or twice) the size of the weight given to the Admissions category. The results are reported in Table D1.

Table D1. Number Of Statistically Significant Findings,
by Confidence Level and Program Type
(2,000 Randomized Runs, Regulation Category Weight Range = .5 to 2)

	IsVoucher		IsCredit	
	0.01	0.05	0.01	0.05
multilevel, fixed effects	2,000	2,000	0	15
OLS, robust	2,000	2,000	N/A	N/A

The results in Table D1 demonstrate that this paper's findings are highly robust to a wide range of alternative weightings. The IsVoucher variable is highly statistically significant for all 2,000 runs at even the relatively stringent ($p < .01$) level, whereas the IsCredit variable is insignificant across 99 percent of runs at even the loose ($p < .05$) level.

[1] Andrew J. Coulson, ―Comparing Public, Private, and Market Schools," *Journal of School Choice*, vol. 3 (2009), no. 1, pp. 31-54. http://www.cato.org/pubs/articles/coulson_comparing_public_private_market_schools_jsc.pdf
[2] Patrick J. Wolf, ―Civics Exam: Schools of Choice Boost Civic Values," *Education Next*, vol. 7 (2007), no. 3 (Summer), pp. 66–72. http://educationnext.org/files/ednext_20073_66.pdf
[3] Herbert J. Walberg and Joseph Lee Bast, *Education and Capitalism: how Overcoming Our Fear of Markets and Economics Can Improve America's Schools* (Stanford, California: Hoover University Press, 2003), p. 221.
[4] Moody and Ellig, ―The Universal Tuition Tax Credit: Achieving Excellence in Education without a Tax Increase," http://www.virginiainstitute.org/tax.html

[5] Andrew J. Coulson, *Market Education: The Unknown History* (New Brunswick, NJ: Transaction Books, 1999).
[6] Bast and Walberg, p. 262.
[7] Bast and Walberg, p. 326.
[8] Examples of the former type of program can be found in Illinois and Iowa, and of the latter can be found in Florida and Pennsylvania.
[9] Coulson, *Market Education: The Unknown History*. And:
Andrew J. Coulson ―Toward Market Education: Are Vouchers or Tax Credits the Better Path?," Cato Institute Policy Analysis no. 392, February 23, 2001. http://www.cato.org/pubs/pas/pa392.pdf. And:
Andrew J. Coulson, ―Giving Credit Where It's Due Why Tax Credits Are Better Than Vouchers," *The Independent Review*, vol. 3 (2002), n.2 (fall), pp. 277-287. http://www.independent.org/pdf/tir/tir_07_2_coulson.pdf
[10] John Humphreys, ―Funding School Choice. Vouchers or Tax Credits: A Response to Buckingham," Center for Independent Studies, *Policy*, vol. 18 (2002), no. 1 (autumn). http://www.cis.org.au/Policy/aut2002/polaut02-3.pdf
[11] Lisa Kaiser, ―The School Accountability Moment. Milwaukee's choice program gets guidelines," *Express Milwaukee*, July 29, 2009. http://www.expressmilwaukee.com/article-7439-the-school-accountability-moment.html
[12] The K-12 Education Subtraction and the K-12 Education Credit. See: Minnesota Statutes, section 290.0674. http://www.taxes.state.mn.us/taxes/individ/credits_subtractions_additions/education_credits_subtractions/educ_credit_sub.shtml
[13] Florida bill SB 2126, lines 548-553, 2010.
http://myfloridahouse.gov/Sections/Documents/loaddoc.aspx?FileName=_s2126er.DOCX&DocumentType=Bill&BillNumber=2126&Session=2010
[14] See the OSP legislation: Florida Statutes 1002.38,
http://www.leg.state.fl.us/Statutes/index.cfm?App_mode=Display_Statute&Search_String=&URL=Ch1002/Sec38.HTM. Also see the public school testing regulations: Florida Statutes 1008.22,
http://www.leg.state.fl.us/Statutes/index.cfm?App_mode=Display_Statute&Search_String=&URL=Ch1008/Sec22.HTM
[15] Christopher Hammons, ―Fifty Educational Markets: a Playbook of State Laws and Regulations Governing Private Schools," The Friedman Foundation for Educational Choice, *School Choice Issues in Depth*, April 2008.
http://www.edchoice.org/downloadFile.do?id=295
[16] One anonymous reviewer commented that applying sophisticated regression models to such a small dataset is overkill, and that a few simple descriptive statistics would suffice. This is mistaken. The possibility of bias being introduced by confounding variables is independent of sample size, and so the use of multiple regression analysis is just as essential with small samples as with large ones. While the ―statistical power" of regression models is reduced with smaller data sets, this simply makes it more difficult to detect weak effects. If, using proper regression techniques, we detect significant effects with the present limited number of observations, it is clear that those effects will be substantial in magnitude.
[17] Multi-level regression also addresses a problem that we would face with Ordinary Least Squares: the lack of independence of observations from states that have more than one choice program. OLS assumes that observations are independently and identically distributed, and the independence assumption is violated when multiple observations come from the same state, potentially throwing off the standard errors for the regression coefficients. We can address this problem within OLS by using robust standard errors, but this is convincingly argued to be inferior to multi-level modeling in: Ban Chuan Cheah, ―Clustering Standard Errors or Modeling Multilevel Data?," Columbia University Department of Statistics, research note, May 2009.
http://www.stat.columbia.edu/~cook/movabletype/mlm/RevisitMoulton_2.pdf

Additionally, multi-level modeling allows us to parcel out the variance into its within-state and between-state components, and determine how substantial each is as a share of the total variance. The standard test for determining the importance of between-state variance in panel regression is Breusch and Pagan's Lagrangian multiplier test, which is used to determine whether there is sufficient between-state variance to warrant a random-effects model or, if not, that the within-state (―fixed effects") model should be employed. In the present study, we find that the within-state model is preferred by the Breusch and Pagan test, because the between-state variance is not substantial. This, in turn, bodes well for the generalizability of the findings because, at least for our observations, state-level effects matter far less than within-state variables like program type.

Finally, multi-level modeling plays a role in helping to determine causality, as discussed in the following section of the paper.

[18] The DefaultReg variable cannot be included in a regression in which the data set includes observations of private schools not participating in choice programs, because doing so would violate the recursivity rule of OLS regression—the dependent variable would be identical (and causally linked) to the DefaultReg values for all the no-choice-program observations.
[19] Douglas M. Hawkins, ―The Problem of Overfitting," *Journal of Chemical Information and Computer Sciences*, Vol. 44(2004), No. 1, pp. 1–12.
[20] The predictor of interest, IsVoucher, plus the nine control variables.
[21] For a discussion of Mallow's Cp, see: J. D. Dobson, *Applied Multivariate Data Analysis. Volume I: Regression and Experimental Design* (New York: Springer-Verlag, 1991), p. 268-69.
[22] For a brief explanation of these criteria, see: Christopher F. Baum, *An Introduction to Modern Econometrics Using Stata* (College Station, Texas: Stata Press, 2006), p. 79.
[23] For a discussion of the Stata ―cluster()" command, see: ―Stat Library: Analyzing Correlated (Clustered) Data," http://www.ats.ucla.edu/stat/stata/library/cpsu.htm.
[24] See, for instance, McFadden (1999, p. 6): ―the 2SLS estimator is also biased if we let the number of instruments grow linearly with sample size. This shows that for the IV [Instrumental Variable] asymptotic theory to be a good approximation, n must be much larger than j [where j is the number of predictors in stage 1]. One rule-of-thumb for IV is that n - j should exceed 40, and should grow linearly with n in order to have the large-sample approximations to the IV distribution work well." Daniel McFadden, *Economics 240B Course Reader*, Chapter 4, Department of Economics, University of California at Berkeley. http://elsa.berkeley.edu/~mcfadden/e240b_f01/ch4.pdf
[25] Opposed by the Obama administration and by all but a handful of Democrats in Congress, funding for this program was sunset in 2010, and the program will cease to operate when its existing students have graduated.
[26] See, for instance: Adam B. Schaeffer, ―The Public Education Tax Credit," Cato Policy Analysis no. 605, December 2007. http://www.cato.org/pub_display.php?pub_id=8812. And,
Andrew J. Coulson, ―Forging Consensus: Can the School Choice Movement Come together on an Explicit Goal and a Plan for Achieving It?" Mackinac Center for Public Policy, Research report, April 30, 2004. http://www.mackinac.org/6517
[27] Janet R. Beales and Thomas F. Bertonneau, ―Do Private Schools Serve Difficult-to-Educate Students?," Mackinac Center for Public Policy, research report, October 1, 1997. See also: ―Jay P. Greene and Greg Forster, ―Effects of Funding Incentives on Special Educaiton Enrollment," Manhattan Institute Civic Report no. 32, December 2002.
[28] Joseph Schumpeter, *Capitalism, Socialism and Democracy*, (New York: Harper and Brothers, 1950).
[29] See, for example, Eric Bartelsman, John Haltiwanger and Stefano Scarpetta, ―Microeconomic Evidence of Creative Destruction in Industrial and Developing Countries," World Bank working paper, October 2004. http://siteresources.worldbank.org/INTWDR2005/Resources/creative_destruction.pdf
[30] See, for instance: James S. Coleman, *Equality and Achievement in Education* (Boulder, Colorado: Westview Press, 1990).
[31] Anthony S. Bryk, Valerie E. Lee, and Peter B. Holland, *Catholic Schools and the Common Good* (Cambridge, Massachusetts: Harvard University Press, 1993).
[32] Andrew J. Coulson, ―With Clear Eyes, Sincere Hearts and Open Minds: A Second Look at Public Education in America," research report, Mackinac Center for Public Policy, Michigan, July 27, 2002. http://www.mackinac.org/4447
[33] In order to assess the relative magnitude of the explanatory variable coefficients, all of the variables can be standardized prior to performing the regression, so that they have a mean of 0 and a standard deviation of 1. The coefficients thus produced are called beta coefficients, and can be interpreted as follows: a one standard deviation increase in the standardized explanatory variable is associated with an increase of <beta coefficient> standard deviations in the dependent variable (which, in this case, is the level of regulation on private schools as measured by our regulatory index).
[34] A good brief explanation of added variable plots can be found in: Alan Heckert, ―Partial Regression Plot," *Data Plot Volume 1*, National Institute of Standards and Technology, April 4, 2003. http://www.itl.nist.gov/div898/software/dataplot/refman1/auxillar/partregr.htm.