# Where Does School-Choice Funding Go? How Large-Scale Choice Programs Affect Private-School Revenue, Enrollment, and Prices

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**Abstract**

Using a new dataset constructed from nonprofit tax-returns, this paper explores how several recent large-scale choice programs in the US affected the fiscal health of private schools and the accessibility of a private education. We find that school choice programs created a large transfer of public funding to private schools, suggesting that every dollar of funding raised revenue by a dollar or more. Turning to the incidence of school choice and the impact of choice on enrollment, we find that the effects of choice depended crucially on the type of program introduced, with programs based on corporate tax credits creating relatively large enrollment gains and small price increases. We compare results for religious and secular schools, calculate elasticities of demand and supply for private schools, and discuss welfare effects. Back-of-the-envelope calculations suggest that the deadweight loss created by school choice programs is reasonably small: about 5 cents for every dollar of funding.

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# Introduction

The United States is currently undergoing a revolution in the promotion of school choice, with dozens of states considering or recently enacting large-scale laws that use public resources to promote attendance at private elementary and secondary schools. Currently, over a million U.S. families participate in such programs and recent legislation in several states will push this number higher in coming years (Friedman Foundation, 2013).

But the proliferation of these programs across the country should not be taken as evidence that observers now agree on how school choice works. Several elements of school choice remain highly controversial. In particular, arguments on school choice often focus on the *financial* implications of these policies. On the one hand, school choice programs are sometimes depicted as expanding choice for disadvantaged families (Governor Mitch Daniels, 2011; Governor Jan Brewer, 2011). Alternately, choice programs could simply transfer income to wealthy families whose children would attend private school in any event (Reese, 2009; People for the American Way, 2003). Families are not the only potential beneficiaries of school choice; some observers emphasize that choice represents a new and unprecedented source of financial support for schools themselves (Crothers, 2014). The validity of these descriptions depends upon how choice impacts the cost and prevalence of private education, and whether choice funding ultimately goes to families or schools.

Prior research offers little evidence regarding the effect of school choice on these outcomes. Of course, there is a large literature exploring the impact of school choice programs, and in particular voucher programs, on economically important activities; Figlio (2009), Hoxby (2003), and the Center on Education Policy (2011) provide useful background on research on school choice. But most of this prior work considers outcomes such as student achievement, Tiebout sorting, and public-school performance. Using sophisticated theoretical models, some studies predict how vouchers might impact private school enrollment or tuition (Epple and Romano, 1998; Ferreyra, 2007; and Nechyba, 2000) but the empirical validity of these studies is largely unproven. Evidence for how school choice affects private school finances, or evidence on the incidence of school choice generally, is difficult to find. The goal of this paper is contribute such evidence.

A key challenge in doing so is finding data on private-school finances. For this, we turn to a largely-unnoticed source of data: nonprofit tax returns. Using a dataset of IRS Form 990 returns made available by the National Center for Charitable Statistics, we construct a panel dataset with detailed financial information for thousands of private schools each year. Our sample covers about 20% of all private school enrollment in the country. While some important groups of private schools, such as Catholic schools, are not included in the sample, we provide several pieces of evidence which give us optimism that estimates from this sample could be informative for all private schools.

We first investigate whether school revenue increases when a policy to subsidize private schooling is implemented. We focus on a type of policy that has been relatively unstudied in economic research—tax-credits for private schooling. These laws provide credits to either individuals for private-school expenditures, or credits to individuals or corporations for donations to scholarship-tuition organizations that award scholarships to private-school students. These laws are essential for understanding school choice given their extraordinary (and ongoing) proliferation, and their large size suggests that they could have important effects on school enrollment and finances even at the state level, unlike the smaller-scale voucher programs that have been the focus of past work. Our data cover the years 1991 to 2009; there are several states that enacted large-scale programs to subsidize private-school-tuition that we use in our baseline estimates. We discuss these programs and their enactment in more detail below.

We find robust evidence that school choice policies raise revenue for the private schools in our sample and that the magnitude of this effect is large; even conservative extrapolations to all private schools indicate that a dollar of choice funding raises private school revenue by a dollar or more. However, we find that the impact of school choice depends crucially on the type of program enacted. The corporate tax credits we study either mandate that corporations cannot designate scholarship recipients or restrict the type of student that can qualify (e.g., making scholarships means-tested) or both. In contrast, individual tax credits often explicitly or in practice allow families to receive subsidies for expenditures on their own children and are not means tested. We consequently find several differences between individual-tax-credits and other programs.

We find that private schools raise their tuition in response to individual credits but not for other programs. We also find evidence of private school enrollment growth following the establishment of a corporate credit program, but not for individual credits. Thus both programs raise school revenue, but in very different ways. For individual credits we cannot reject that the incidence of choice falls entirely on families while for other choice laws we cannot reject that it falls entirely on schools. These results are consistent with several depictions of private education, as we discuss below.

About half of our sample consists of religious schools; we compare the effects of programs on religious and private schools. Our estimates suggest that school choice leads to higher revenue for both types of school, although there is some evidence that tuition increases in response to individual credits are greater for religious schools. We also extend our sample to consider several large voucher laws enacted at the end of the time period we study. These estimates again show that corporate credit programs have relatively large effects on enrollment and small effects on the cost of private school. Our estimates for the most recent voucher programs are imprecise, although this may reflect the fact that these programs were enacted at the very end of our sample and were relatively small during the period we study.

Finally, we construct simple estimates of the price elasticity of demand for private schooling and of the deadweight loss created for each dollar of public revenue spent on school choice. Our results suggest a price elasticity of -0.52; this is slightly larger in absolute value than the baseline estimate in Dynarski, Gruber, and Li (2009), although one could argue that the population they study would be less price sensitive than the population affected by choice. Our simple estimate of deadweight loss is five cents per dollar spent; we know of no comparable estimate for this number.

The rest of this paper is organized as follows. The next section describes the school choice laws used here. Section 3 discusses the data, section 4 the specifications, section 5 the results, and section 6 concludes.

## 2. School Choice Laws

The main estimates in this paper focus on what we refer to as "school choice laws." By this, we mean laws that use government funds to subsidize private school tuition. We focus on several large statewide laws that affected reasonably large groups of students between 1991 and 2009. Along with one large voucher program—the McKay Voucher program in Florida—the laws used in the main estimates of this paper are primarily tax-credit laws. With the exception of Chan (2006), who looks at the effect of a tax-credit law in Ontario on public school performance, we know of no economic research on these laws.[1] The discussion here draws on discussions in Welner (2008), the Friedman Foundation (2013), Schaeffer (2009), and a report compiled by the Research Department of the Minnesota House of Representatives (2011).

While technically distinct from traditional school vouchers, tuition tax credits serve the same purpose and work in a very similar way. A typical voucher program consists of a government agency distributing money, which has been collected through taxation, to the families of students on the condition that the money will be spent on private schooling. In a typical tax credit program, the government reduces the tax liability, often dollar for dollar up to some limit, of a taxpayer that has provided funding to support students' private schooling. In both cases, states dedicate public resources to private school attendance for select students. The similarities between vouchers and tuition tax credits have led some to refer to these tax credits as "neovouchers" (Welner, 2008), or sometimes simply as "vouchers" (e.g., Caputo, 2013).

Tax credit laws for private school tuition fall into two areas: credits for individuals and credits for corporations. Corporate tax credit programs provide an income-tax credit for corporate donations to a scholarship-tuition-organization, or STO, which then uses the donated funds to award scholarships to children attending private schools.[2] There are three states which enacted corporate tax credits of this kind during

[1] Some state-government reports consider the financial effect of these laws on *public* school funding. That is not the focus of this paper, although it is certainly an important topic. Studies considering issues related to public finance and public education include Murray, Evans, and Schwab (1998) and Fischel (2006).

[2] In recent years the tax base for some corporate credits has been expanded; for example in 2009 (beyond the period of study here) Arizona and Florida both expanded corporate tax credit programs to the insurance premiums tax.

the period of study here: Arizona, Florida, and Pennsylvania. These programs use credits, not deductions, so that the financial incentive created by the program can be quite large even if the state income tax rate is small. For example, in Florida during the time of this study the corporate income tax rate was 5.5%, but Florida's tax credit program awards a credit dollar-for-dollar with respect to donations to STOs. A program in Florida that used a tax *deduction* for corporate donations would thus lower the opportunity cost of a dollar given to STOs down from one dollar to 95 cents, but the tax *credit* program lowers the cost of this donation from one dollar to zero.[3]

Individual income tax credit programs may either award a credit for donations made to STOs, or for a household's own spending on private school tuition and other private school expenses. There are three states with programs of this kind during the period of our study: Arizona, Iowa, and Illinois. Even though many state's individual-income tax rates are relatively low (compared to federal rates), these programs create large financial subsidies since they use credits rather than deductions. But individual credits are different from corporate credits in that they (a) allow individuals to claim credits for private school tuition for their own children and (b) are not means tested, unlike most corporate programs.

Table 1 provides information on the laws used in this study. There are seven laws that are the main focus here: an individual and corporate credit in Arizona, a voucher program and corporate credit in Florida, individual credits in Illinois and Pennsylvania, and a corporate credit in Pennsylvania.[4] As the table suggests, none of the individual income-tax credit programs are currently means tested, and their restrictions on student use are generally weaker than the restrictions placed by the other school choice programs. Appendix A provides some additional information on each state's credit program.

Figure 1 shows total spending each year from 1991 to 1999 on the programs listed in Table 1 (in year 2012 dollars). Here, "spending" refers to (a) credits claimed for individual- and corporate-income tax school choice programs and (b) voucher spending under the McKay program. The figure shows that at the turn of the century there was a surge in funding for school choice, with total funding reaching nearly half a billion dollars—a reasonably large amount given that these programs focus only on a select group of children in five

[3]The credit is capped at 75% of corporate taxes due. Arizona's corporate income tax rate during the period of its corporate tax credit here was 6.968%; its program also awarded credits dollar-for-dollar. Pennsylvania's program awarded a credit based on either 75% or 90% of a donation depending on whether the donation was a one time or repeated contribution. Pennsylvania's corporate income tax rate during the period of study here was 9.99%. Appendix A has more details on these laws.

[4]There was a second voucher program in Florida that coincided with the McKay program, the A+ Accountability and School Choice Program. This program allowed students at public schools which had been awarded a grade of "F" in two of the prior 4 years to receive a voucher. Economists have studied the impact of this program on public school behavior; see Chakrabarti (2013) for a discussion. While the A+ program had the potential to influence public school behavior in interesting and important ways, the number of actual vouchers awarded under the program was much more limited than either of the programs in Table 1 (partly because of court rulings) and the program was ruled unconstitutional in 2006, at which time only about 1,400 students were participating (including students using the vouchers to attend other public schools) (Center on Education Policy, 2011, although Welner, 2008, says enrollment in 2005-06 was only 734). Given that the program's enrollment prior to 2001 (the year the corporate credit began) was extremely limited (only two schools qualified for vouchers in the first possible year, 1999; and no schools did so in the second year) the baseline specifications below would be unchanged even if A+ vouchers had become much more widespread in later years than they actually did (Greene, 2001).

states.

Figure 2 shows the number of individuals assisted by these laws. To count the number of individuals, the figure sums three things: first, the number of scholarships awarded in each state and year by STOs that qualify for tax-credit status; second, the number of vouchers paid out; and third, the total number of individual tax returns claiming a tax credit under a school-choice program. This figure may not exactly represent the number of students in school-choice policies, as the number of taxpayers claiming individual credits could differ from the number of students benefiting from the tax-exempted expenditures.[5] But the figure nonetheless clearly shows striking growth over the past two decades in the participation of these programs, with over half a million individuals each year towards the more recent years. The jump from 1999 to 2000 reflects the Illinois program's adoption.

While Figures 1 and 2 show that the programs in Table 1 were large in terms of funding and scope, there were several other more limited school choice programs enacted during the time period of available data here, particularly towards the end of the sample. Rhode Island introduced a corporate credit for donations to STOs in 2007, where a dollar of donations resulted in a 75 cent credit (or a 90 cent credit if donations are made in two consecutive years.) But, aside from the fact that the program was adopted nearly at the end of the period of study here, total eligible credits was capped at only $1 million annually.

State voucher programs were also introduced in DC, Georgia, Ohio, and Utah.[6] The District of Columbia Opportunity Scholarship program was launched in 2004 and provided vouchers for K-12 students who qualify for free and reduced-price lunches, although the program had a "relatively small annual budget" (Friedman Foundation, 2013). The Georgia Special Needs Scholarship program, enacted in 2007, allows any student with a disability who was previously enrolled in public school to obtain a voucher to attend private school, although the program's participation was relatively low—about a thousand students annually—for the period studied here. The Ohio Autism Scholarship Program, enacted in 2003, and later the Educational Choice Scholarship Program in 2006, both provide vouchers to students across the state. The former program is limited only to students diagnosed with an autism spectrum disorder and registered in the public school special education system; participation in this program was relatively low during the period of study here. The Educational Choice program provides vouchers to students who attend poorly-performing local public schools (or who attend a charter school but who would be assigned to such a school otherwise). Although participation was relatively small in the early years of the program (and the period of study used here), it has since become a large program with annual expenditures of nearly $60 million (Alliance for School Choice,

[5] The relevant individual-credit programs here limit credits to expenditures on dependents, so that very high discrepancies between credits claimed and students is unlikely. If multiple dependents benefit from a single taxpayer's credit then the figure would underestimate the number of students assisted by these policies.

[6] A voucher program was also considered in Colorado before being invalidated by a court ruling. Colorado has subsequently introduced a pilot voucher program in 2011, but this is after the period of study here.

2012). As these programs were more limited in scope or student availability during the years covered by the data below, our baseline results will exclude these programs. However, we also report estimates that include the effects of these laws; we find that their inclusion does not change our main qualitative results.

One might naturally wonder about the circumstances behind these states adopting large school choice laws before other states. As Table 1 suggests, the policies passed by states here include several relatively large states but feature states from across different parts of the country; the laws were signed both by democratic and republican governors. Moreover, while five states are included, the number of states seriously pursuing or subsequently enacting similar school choice programs is much larger; Huerta and d'Entremont (2007) count at least 40 states considering tax credit programs along the lines of the six programs used here. Further, several states have had attempted passage of school choice programs blocked by court ruling or referenda, including California, Colorado, Florida, Michigan, Vermont, and Washington. Many states have also passed vouchers following the states used here, including states at the very end of the study period here such as the District of Columbia, Georgia, Ohio, Rhode Island, and Utah. Even more recently, Indiana, Louisiana, New Hampshire, North Carolina, Oklahoma, Virginia, and Wisconsin have all enacted legislation for school choice.[7] These recent adopters can be exploited to create a more refined control-group for estimation below—that is, if estimates using "late" adopters as a control group yield highly different estimates than estimates using all non-Table-1 states as a control group, it would raise concerns that there are strong differences in private school markets that are related to (eventual) school-choice adoption. Similar estimates regardless of the control group used would lessen this concern. We explore this below.

But while the large number of states eventually undertaking school choice can allow some investigation into the selectivity of states listed Table 1, one might wonder what determined the *timing* of law passage in different states. In some cases the timing stems in part from political events.[8] But the prominent force in the timing of these laws was perhaps judicial. School choice programs face important legal hurdles, with a foremost obstacle being the historical presence of Blaine Amendments in several state constitutions. In 1875, James Blaine, a congressional representative in Maine, proposed an amendment to the constitution to preclude grants or appropriations to sectarian institutions or organizations.[9] While the amendment was not adopted (falling four votes short of adoption in the Senate), 37 states subsequently adopted "Blaine

[7] Additionally, in May 2013, Senator Marco Rubio (R-FL) introduced a bill to establish a federal tax credit that would be available to both individuals and corporations.

[8] For example, Florida's programs coincided with Jeb Bush's election in 1998 after he narrowly lost his electoral bid in 1994, and the corporate tax credit in Arizona was made law by governor Janet Napolitano in a compromise with the state legislature during state budget negotiations after she had vetoed four such bills earlier (Welner, 2008). Tom Ridge signed Pennsylvania's law after four failed attempts to get vouchers through the legislature (Averett and Wilkerson, 2001), although Adams (2002) refers the adoption of Pennsylvania's credit as "quiet" and "little noticed."

[9] During his career, Blaine also served as Secretary of State, Speaker of the House, a Republican Presidential nominee, and a member of the Senate.

Amendments" to their constitutions.[10] The movement to adopt these amendments was often associated with anti-Catholic sentiment (Huera and d'Entremont, 2007). Further, 29 states include a constitutional clause to prohibit the state from forcing residents to support any ministry (as states sometimes did in Colonial times). Only three states have neither a Blaine Amendment nor a compelled-support-clause.[11]

Blaine Amendments and anti-support clauses have historical roots that far precede the modern school-choice movement; to quote Duncan (2003), "the social and religious contexts in which the State Blaines operate today are far different from those of their origins." But they have proven important and sometimes unpredictable determinants of the legality of school-choice policies today. Court decisions over the validity of these policies have varied, sometimes in ways hard to predict. In Illinois, two separate cases on the legality of the state's tax-credit program yielded six separate decisions before the legality of the program was recognized (Huerta and d'Entremont, 2007). When Arizona enacted the first tax-credit school-choice programs the legislature was in fact uncertain of the program's legality (Werner, 2008), although the legality was ultimate affirmed in the State Supreme Court ruling of *Kotterman V. Killian* in 1999. But state-constitutional clauses on religion have provided limitations on school choice, as discussed in chapter 5 in Werner (2008). In some states where it appears that Blaine Amendment language may preclude school choice, there have been attempts to alter the language of the law.[12] Thus, the process for enacting school choice can involve extended legislative and judicial deliberation, creating uncertainty on the timing of enactment even within adopting states.

The decision of governments and courts to revisit legal provisions that have "slumbered in state constitutions for over a century" (Duncan, 2009) may in part coincide with other efforts to reform public education. This appears to be true in several of the states listed in Table 1. In particular, the establishment of Florida's McKay program coincided with an increase in public school funding for school construction and maintenance, and Pennsylvania's tax-credit program coincided with a program to support educational improvement organizations that provide innovative public school programs. However, if these coincidental laws made public schooling more attractive, they presumably worked *against* the efficacy of laws intending to facilitate student's matriculation to private schools.

Another concern is simply one of reverse causation—growth in private school revenue may allow for private schools to devote lobbying resources towards school choice, so increases in private school revenue lead to greater school choice, not the other way around. Although such a coincidental timing pattern might

[10] The wording of Blaine's proposed amendment was, "No State shall make any law respecting an establishment of religion, or prohibiting the free exercise thereof; and no money raised by taxation in any State for the support of public schools, or derived from any public fund therefor, nor any public lands devoted thereto, shall ever be under the control of any religious sect; nor shall any money so raised or lands so devoted be divided between religious sects or denominations."

[11] The three states are Louisiana, North Carolina, and, unexpectedly, James Blaine's own state of Maine.

[12] For example, in 2013 North Dakota considered a bill (HC3037) to alter the state's constitution to make it easier to permit state support of sectarian education.

be unlikely given the discussion above about the legislative and judicial processes that can accompany school choice reform, we can address these concerns in the empirical part of the paper by including a number of controls for a state's socioeconomic circumstances and the population of school-aged children. Additionally, we discuss empirical tests of this concern below. Before turning to the empirical methodology, however, we next discuss the data used.

## 3. Data

This section discusses the data used; additional details are provided in the appendix. The primary financial data we use come from the Core Financial Files produced by the National Center for Charitable Statistics (NCCS). These files contain information collected from Internal Revenue Service (IRS) Form 990 and Form 990-EZ for 501(c)(3) public charities with at least $25,000 in receipts. Although these organizations are "non profit", they must file tax returns containing detailed financial information. As mentioned in the last section, several states changed their laws in limited ways or very late in our sample (Ohio after 2003; Utah after 2005; Georgia and Rhode Island after 2007; and Louisiana after 2008); we omit these state/year observations from the baseline estimates but include them and their laws changes as a robustness check. The primary financial variable we use is program service revenue, which is revenue earned from activities that form the basis of an organization's exemption from tax. Tuition charged by a private school is a canonical example, although other revenue, such as the sale of school supplies, would also be included in the figure we use. Using a figure that includes all program service revenue, rather than just tuition, is sensible as schools may potentially attempt to obtain school-choice rents by raising both tuition and other student fees.[13] It also reflects true revenue, as opposed to estimating revenue based on "sticker" tuition prices. Our NCCS data use tax returns covering fiscal years from 1991 to 2009. We know of no other large-scale annual dataset that provides this type of financial information for US private schools.[14]

This nonprofit tax-return data includes information for all nonprofits filing such returns, not just schools. To identify private schools in the tax return data, we match the tax return data to information in the Private School Universe Survey (PSS). Conducted biennially by the National Center for Education Statistics, the PSS is intended to be a census of private schools in the United States, but it does not include any financial information about schools.[15] We match the two datasets using information on school name and address. As

[13]Other examples of program service revenue would include admissions fees charged by a museum, or conference registration fees charged by the ASSA for attending its annual meeting.

[14]There are several concepts of "year" that are relevant here. As discussed in the appendix, the NCCS uses several notions of year when compiling the data; our use of the term reflects the fiscal year for which a charity files its tax return. The PSS timing is to October of a given year; we thus match NCCS and PSS data together based on the October in which the relevant fiscal year falls. Since most fiscal years are either calendar years or July-to-June fiscal years, this matching procedure should work well. We discuss a few additional issues with handling the timing of school choice laws more below.

[15]In 1993, the PSS did not ask schools to report pre-school enrollment. Also, religious affiliation is missing for roughly 16%

discussed in the appendix, school names and addresses are only available in the PSS beginning in 2005/06 but are available for all years in the NCCS data. This allows us to match any school in the PSS from 2005 onwards, and then "follow" that school back in time. However, this procedure will miss schools that existed during the period of our sample (e.g., the 1990s) and exited the sample before the 2005/2006 PSS.

There are several steps we can take to address this fact. First, we explored the degree of "churn" in our data to see if there is strong attrition as the sample goes back into the 1990s. Fortunately, this is not the case, as over 70 percent of the observations in our matched sample can be followed back to 1995 and over half can be followed back to 1991. Next, we address this concern by identifying schools in the NCCS using additional information on charities' tax returns. NCCS data include a charity's National Taxonomy of Exempt Entities (NTEE) Classification System code, which organizations use to describe their primary purpose on their tax forms. We identify charities that classify themselves using codes B20 (elementary and secondary schools), B24 (primary and elementary schools), and B25 (secondary and high schools). The benefit of identifying schools in this way is that it avoids the bias towards schools that are open towards the end of the sample period.[16] Below, we show results using the PSS matched schools and then present estimates where additional NCCS-identified schools are added. The estimates are quite similar, suggesting that our matching procedure works well.

The populations of schools in the PSS and NCCS datasets are potentially different; most notably, some religiously affiliated private schools—particularly Catholic Schools—are not required to file tax returns and almost never voluntarily do so. Our sample essentially excludes Catholic schools. Our matched sample contains in an average year about 3,200 schools with about 800,000 students; or about one out of every 9 private schools and one out of every 5 private-school students in a typical year. Appendix Table 2 shows our success rate in matching schools between the tax data and the PSS and shows that our sample consists of schools that are larger and less likely to be religious than the average private school. However, the PSS data contain information on religious affiliation, and about half of all schools in the sample report that they are religiously affiliated.

Nonetheless, one might wonder how the results for our sample of schools might extend to other schools. We will attempt to be careful in drawing strong conclusions for all schools from the results below. But we note that several of our results would be noteworthy even if they pertained to our sample alone. Further, there are several facts that give us optimism that our results might serve at least as a rough guide for the

of schools in 1993. All other years of the PSS include pre-school enrollment data and report religious affiliation for all schools. For the enrollment results below, we interpolate missing 1993 data.

[16]One might ask why we do not simply construct the sample using NTEE codes, dispensing with the PSS entirely. There are three drawbacks of using NTEE codes. First, some schools may not be identified under these codes, as discussed in the appendix. Second, some schools identified in this fashion cannot be matched to PSS data, so we cannot calculate revenue-per-student (the tax return information does not give the number of students in a school), which we use in some estimates below. Finally, the PSS also includes information on religious affiliation.

effects of choice on other schools. First, we can construct the ratio of students enrolled in schools filing a form 990 to all private-school students; if this ratio changes in response to the passage of school choice, it might raise a concern that schools using choice concurrently decide to file. Estimating such a regression using the baseline specification below suggests that the fraction of filing schools does *not* change when choice is introduced; the estimated impact of choice on the fraction of students in filing schools is small, statistically insignificant, and produces a confidence interval ruling out a modest increase (or decrease) in the relative size of filing schools.[17]

Second, we can explore trends in teacher and enrollment populations in our sample and in the PSS overall; evidence of strongly divergent trends would also raise a concern about the generalizability of our sample. Figure 3 shows trends over time in average enrollment (Panel A) and in average number of teachers (Panel B). Clearly the schools in our sample are larger and have more teachers. But the two panels show that in both samples there is a slight decline in enrollment over the period of study and that the number of teachers is reasonably flat. The trends in these characteristics for schools in our sample seem reasonably comparable to the trends for all schools in the PSS.[18]

Third, for the year 2007, we obtained from Arizona school-by-school information on the amount of tax-credit-eligible scholarships claimed; using this information, we can explore whether the schools in our sample receive disproportionate amounts of choice funding. Evidence of this might affect the perceived generalizability of our results. In this particular state and year, the schools in our sample received about 18% of all tax-credit scholarships. We have 60 AZ schools in our sample this year while the PSS lists 334, so that the fraction of schools in our sample ($60/334 = 0.1796$) matches the fraction of school choice funds captured by our sample quite well.

Finally, while we do not have financial information in the PSS, we do have enrollment, and below we can compare the estimated effect of enrollment for our sample to the effect for all schools. The precision of the results (for both samples) can vary based on the construction of our standard errors, but fortunately the estimates are qualitatively similar and suggest that the change in enrollment seen in our sample reflects the change in enrollment for other private schools. We can also use this enrollment-based comparison to mediate our out-of-sample predictions for our financial results and below we do so. We will also use tuition information in the Schools and Staffing Survey to adjust these predictions; doing so suggests that our tax sample has reasonable predictive content for other private schools. In sum then, our sample includes only a

[17] Using a specification matching column 5 of Table 7 below, where the dependent variable is the fraction of students in filing schools (mean of 0.199, standard deviation of 0.10), the coefficient on the School Choice Laws variable is -0.0019 and the state-clustered standard error is 0.007, producing a 95% confidence interval from -0.016 to 0.012.

[18] Since the tax sample is less likely to be religiously affiliated, one might wonder if there are divergent trends in the share of the samples made up by religious schools. Appendix Figure 1 shows that from the mid 1990s on the two samples show similar trends (although again different levels) in the share of schools that are Catholic, religious-but-not-Catholic, or not religiously affiliated.

fraction of private schools and these schools differ in certain observable ways from the typical private school. But we are optimistic that the results below may be informative for other private schools as well.

After constructing our tax-return sample, we then drop several types of observations. First, we drop a small number observations with multiple matches between the PSS and NCCS. We also exclude a few schools that report being located in different states in different years, including any that fail to report a state in any year. We also drop tax returns for schools in outlying US territories, which we cannot match to enrollment records, since the PSS does not cover these territories. We exclude observations that report zero or negative program service revenue or do not report program service revenue on their tax returns. Lastly, and perhaps most notably, the NCCS guidelines themselves note that the tax return dataset sometimes lists very large revenue values; we drop observations where revenue per student figures are extremely high; over \$100,000. This amounts to about 4 percent of the sample. It is possible that a legitimate private school has per-student revenue of over \$100,000 but we expect that most of these observations are erroneous, or may reflect the returns of parent organizations, such as a hospital or university, that offer education or daycare to children. Below we discuss estimates where this \$100,000 cutoff is altered; the coefficients from samples with different cutoffs are typically qualitatively similar to the baseline estimates.

## 4. Specification

This section describes the specification we use for our baseline estimates. As our key policies vary at the state level, we aggregate our sample to the state/year level, and then estimate:[19]

$$y_{sy} = \alpha \text{index}_{sy} + \beta \mathrm{X}_{sy} + \theta_s + \vartheta_y + \mathrm{T}_{sy} + \varepsilon_{sy}$$

where, for the baseline specification, $y_{sy}$ is total private-school revenue in state $s$ in year $y$ in year 2012 dollars, index is an index for school choice laws (described below), X is a matrix of regressors, including controls for state median income, the state unemployment rate, the population density, the fraction of a state's residents that are noncitizens, the fraction born abroad, the fraction of the population that is white ages 6-10, 11-13, and 14-17, and the same age profiles for blacks, Hispanics (of any race), and non-Hispanics of Other Races. These variables are taken from the Bureau of Labor Statics, the Census, and the American Community Survey; see the appendix for details. The term $\theta_s$ represents a set of state dummies, $\vartheta_y$ represents a set of year dummies, and $\mathrm{T}_{sy}$ represents a set of state-specific time trends; both linear and quadratic trends

[19]As discussed in the appendix, the number of fiscal year 1998 NCCS returns is quite small, apparently because of a regulatory change in the dissemination of IRS 990 forms that year. After aggregating the data, we linearly impute revenue figures for fiscal year 1998.

will be used. The term $\varepsilon_{sy}$ is noise.

The index used in the baseline specification simply equals the number of different types of school choice laws present in a state: individual income-tax credits, corporate income tax credits, or vouchers. The index takes on values zero (no school choice), one (one school choice program) or two (two types of school choice program, true in Florida after 2001 and Arizona after 2006). However, estimates will also be presented allowing different types of school choice program to have different effects.

In constructing the index, it can be unclear whether a law enacted in a certain year would first have an impact in the current school year or instead would impact subsequent school years. For our baseline regressions, we create our index based on the year a school choice program was enacted, but, to account for the uncertainty in the timing of donations following enactment, include a dummy variable that equals unity the year a state enacts a school-choice law (and zero all other years). There are a few exceptions to the basic rule of coding a law based on year of enactment. First, Iowa's school choice law was enacted in 1987—before our sample begins—but was expanded in 1996 and again in 1998. State administrative data show a large increase in the program after the 1998 reform; we consequently code Iowa's index as going from zero to one in 1998.[20] Next, Arizona's individual credit was enacted in 1997, but donations were not eligible for a credit until 1998, and use of the program was limited until 1999 (Arizona Department of Revenue, 2008). We code Arizona's first year as 1998.

Finally, while the McKay voucher program began in 1999, it was initially a pilot program; the program saw a rapid expansion in the 2001.[21] We thus code both Florida programs beginning in 2001. All of these coding decisions are made in an attempt to present conservative estimates. That is, the results below suggest weaker effects for individual credits and vouchers. Altering the timing of these laws to their earlier enactments, or simply dropping the year of enactment and the year following enactment from the regressions for all states, yields similar estimates to those shown here.

The specification above can be interpreted as a "difference-in-difference" style estimate. States adopting school choice laws are allowed to differ from other states, and can even have different trends over time given our trend controls. The specifications also account for changing economic and demographic trends across states and over time. However, the number of treatment states (five) is relatively small. As Conley and Taber (2011) note, inference with difference-in-difference style estimates using a small number of treatment groups can be problematic, as estimates can be subject to a finite-sample-bias. They propose an inference technique to account for the small number of treatment groups in the estimation. In the results that follow, we both report conventional state-clustered standard errors, as well as 95-percent confidence intervals (and,

[20]Program expenditures tripled from 1997 to 1998, going from \$3 million to \$9 million (Iowa Department of Revenue, 2012).

[21]Enrollment grew from 970 students in 00/01 to 5,013 in the 01/02 school year (Florida Office of Independent Education & Parental Choice, 2006).

in some cases, $p$ values), using an approach based off of Conley and Taber's method. We discuss the method in the appendix but provide a synopsis here. First, we estimate, using OLS on the full sample, the equation

$$y_{sy} = \beta \mathrm{X}_{sy} + \theta_s + \vartheta_y + \mathrm{T}_{sy} + \varepsilon_{sy}$$

where the index coefficient has been removed, and create residuals $\tilde{\varepsilon}_{sy}$ . We next assign the treatment states' index values to the control states, where the assignment is done at the state level (e.g., the index values for all years in Arizona are assigned to Alabama). After the five true state-index profiles have randomly been assigned to five new states, we regress the new-states' residuals $\tilde{\varepsilon}_{sy}$ on the state-index profiles and record the coefficient. We run 5,000 such regressions to estimate a distribution of coefficients that "should" be zero, but may be nonzero or non-symmetric due to finite sample bias. Using the null-hypothesis that the true coefficient value for the actual-law-change regression is zero, we can "invert" this distribution to construct a 95-percent confidence interval that takes the finite sample bias into account. Fortunately, the confidence intervals produced by this approach are often similar to those from state-clustered standard errors, and are sometimes smaller. But the intervals can be non-symmetric and (especially when the number of treatment states is reduced) become larger than the clustered standard errors would suggest.

Table 2 provides selected means for variables of interest. The first variable, total revenue, reflects total program service revenue added up for all schools in a given state and year; the mean of this variable in the table is the mean of aggregated total revenue averaged over all states and years. The typical estimate in our sample thus records a little over \$200 million in private school revenue, and a little over 16,000 students. "Spending on School Choice" is constructed as before, reflecting vouchers, individual credits and corporate credits. The mean given is for the 61 state/year combinations where school choice spending was greater than zero. If one were to take the total number of individuals assisted by school choice estimated in Figure 2 (which is about 4.3 million across all states and years), one would estimate that the amount of choice-funding-per-user would be \$827 (\$58,342,000 × 61 /4,300,000 ≅ \$827). The table also shows that for a typical cell we have 66 school observations.

# 5. Estimates

## 5A. Baseline Estimates

Before turning to regression estimates, Figure 4 shows the basic result by plotting out total revenue before and after a state imposes a school choice law. We calculate average private school revenue in year 2012 dollars in our treatment states four years before a school law changed, three years before, and so on, up to four years after. The figure shows a clear and persistent jump in total revenue following the law change. Accounting for the pre-existing upward trend before the law change, the size of this jump appears to be about \$30 million to \$40 million in magnitude. We interpret a jump of this size below. It is also noteworthy that there is an absence of any clear jump up or down in the year prior to the laws' enactment (although we can test for this more formally, relative to a control group, below).

Table 3 shows baseline regression results. The dependent variable is total program-service-revenue, in \$1000s, for all schools in a state/year. Coefficients for the school-policy index are shown in each column, below that is the state-clustered standard error and below that the 95% confidence interval as adapted from Conley and Taber (2011). The first column shows that, when an additional school choice policy is introduced (increasing the index's value by 1), total program-service-revenue in our sample increases by about \$30 million, a number roughly comparable to the jump shown Figure 4. This is a little under 15 percent of average revenue as shown in Table 2. Typical school-choice spending is about \$60 million; the estimates indicate that–within our sample alone–this spending raises revenue substantially. We take up the extension of these effects to all private schools momentarily.

The rest of the table explores this result under other specifications. Column 2 removes the right-hand-side controls for state unemployment, income, population noncitizen, and the share of the population in different age and racial groups, but this does not alter the estimates. In column 3, we include only the sample of states that currently have school-choice laws, that had them prior to our sample, or that adopted them after our sample ends. If the policy changes we are studying are from a highly selected group, then this alteration of our control group could lead to different estimates. Fortunately, this is not the case, and comparing the policy changes here to policy changes for states adopting just a few years later does not change the estimates.[22] The next column adds quadratic (rather than just linear) state-specific trends, but the estimate is not sensitive to this change. Column 5 adds in schools identified using the NTEE Classification System code, rather than just crosswalking schools between the PSS and the NCCS datasets. This raises the number of individual school/year observations in the sample from about 61,000 to about 78,000. If the estimates change significantly here, it might raise concerns over the sample selection process of matching PSS and NCCS data. In fact, in this case the result is somewhat larger in magnitude.[23]

Appendix Table 1 presents an additional robustness check of this result; the first column in that appendix table repeats the baseline estimate in Table 1 but adds in five "lead" dummies that equal zero (e.g.) one year before a school-choice law is enacted and zero all other years. The table shows there is no evidence of

[22]In this case, however, the Conley-Taber estimates become non-symmetrical, although clearly still significant.

[23]As mentioned earlier, using different revenue-per-student cutoffs provide similar estimates for the results in Table 3. For example, using the full sample regardless of per-student revenue yields a baseline estimate for total revenue of 40,238 [std err = 22,141]. In contrast, limiting the sample to schools that have less than \$25,000 in revenue per student yields total revenue coefficient of 40,206 [18,016].

pre-existing trends in revenue, revenue per student, or total students in states that passed a school choice law, suggesting that the results here are not driven by reverse-causation or by some other unobservable driver in these states.

Table 4 presents an alternate test of this possibility. As mentioned above, the tax returns include NTEE codes which organizations use to identify their primary purpose. In Table 4 we estimate regressions on total revenue for a variety of nonprofits that we do not expect to respond to school choice. These regressions use the quadratic-trend estimations shown in column 4 of Table 3. (Using the baseline specification in Table 3 produces similar results.) Revenues are again in 1000s of 2012 dollars and as before groups with zero or negative revenues are excluded. If *all* types of nonprofit organizations, such as zoos and aquariums (included in column 2) or labor unions (column 4) saw a surge in revenue that coincided with school choice laws, this would raise a concern that the increase we identify might be driven by some coincidental growth in nonprofits generally. But the findings in Table 4 indicate that this is not the case.

Table 5 explores the effect of school choice revenue across different school choice programs. In particular, all of our observations with a school choice program include either a corporate tax credit or an individual tax credit. Table 5 thus adds to the regressions in Table 3 an additional variable that is simply a dummy which equals unity if a state had an individual-tax-credit school-choice program in effect. The first row of the table thus reflects the three states with corporate credit laws, along with one voucher law (the McKay voucher in Florida). We include the McKay voucher with the corporate credits since the McKay voucher's expansion coincided with the adoption of the corporate credit in Florida—they are collinear. But we consider an extension below that adds additional voucher programs. The individual-credit dummy will indicate how individual-credit laws differ, and the sum of these two coefficients will show the absolute (rather than the relative) effect of the individual credit laws.

The estimates suggest that individual-income-tax programs have a smaller impact on revenue than other school choice programs and that this difference is statistically significant. The first column, which repeats the baseline regression in Table 3 with the new dummy variable added, suggests that the increase is school revenue is much larger for corporate-credit programs than for individual credit programs. However, as noted in Table 2, these programs are also typically smaller (about half of the size) in terms of dollars spent, and later specifications in Table 5 suggest that the impact of individual-credit programs is about half the size of the corporate programs, indicating that the dollar-for-dollar impact of these programs may be comparable. Table 5 thus provides evidence that the dollar-for-dollar effect of individual-credits may be similar to or smaller than other school choice programs. As in Table 3, the results are qualitatively similar across a variety of specifications.

Looking at revenue per student will allow us to see whether schools' increased revenue is derived by increasing enrollment, or by raising tuition & other fees, or both. Table 6 presents results on the incidence of school choice by exploring how choice affects per-student revenue, both for all schools in the sample combined and by type of law. Since these regressions, unlike those in Tables 3, 4, and 5, are per-student, they are weighted by number of students. (Unweighted estimates are similar.) The baseline estimate is statistically insignificant. Its point estimate suggests that schools increase per-student revenue (e.g., by raising tuition) by a little over $200 when a new school-choice program is introduced. Perhaps the closest estimate to this in the literature is in Angrist, Bettinger, Bloom, King, and Kremer (2002), who find that winning a lottery in Bogotá for a voucher worth $190 raised average private-school tuition and fees by $52, so that every dollar of voucher funding raised tuition and fees by about 25 cents, close to what the point estimate here suggests. We note, however, that their research design compares voucher-lottery winners and losers. This approach, while excellent for the focus of their paper, could miss market changes in response to vouchers affecting all students, so that comparability of the estimates is ex ante unclear. The estimate here should be interpreted as reflecting changes in price from several channels, including: schools price discriminating against voucher students, schools raising tuition and fees generally, marginal students entring the private market choosing schools with different prices than the average private-school student, and inframarginal students choosing more expensive private schools.

The next column separates out individual-credit laws, and here the point estimates are quite different: the coefficient suggests that individual-credit laws see an increase in per-student revenue relative to other laws. The standard error for the individual-credit coefficient indicates the differential impact of individual credit laws is statistically significant. Further, a Wald test of the hypothesis that the sum of the two coefficients is zero is rejected at the one percent level, suggesting that individual credits raise prices absolutely (and not just relatively). The implied increase in per student revenue in response to an individual credit program is nearly $1000 per student, which is quite close to our estimate of choice-funding-per-user noted at the end of the prior section. Thus we cannot reject the hypothesis that the incidence of the subsidy falls entirely on families for the corporate and voucher laws (and, further, the Conley Taber confidence interval's upper bound is below our estimate of funding per student, rejecting full incidence on schools), but we *can* reject full incidence on families for individual credits, where the estimates are compatible with schools capturing the entire subsidy. The data are compatible with the incidence of choice falling entirely on schools for one type of law and entirely on families for another type of law.

The next two columns consider similar robustness tests as before (the table omits results adding NCCS-identified schools to the sample as student counts are unavailable for these schools and the number of students is now used in the dependent variable). The results are again similar under these alternate specifications.

Together, Tables 5 and 6 suggest similar dollar-for-dollar effects of choice on revenue across different programs, but the dollar-per-student effects differ markedly, indicating that the laws have different effects on enrollment. Table 7 shows results with total enrollment in private schools in a state and year as the dependent variable; these results confirm the findings in Tables 5 and 6. In column 2, the corporate credit programs and McKay voucher see an increase in student enrollment of about 3,000 students. The average statewide private school enrollment for observations with a corporate credit is a little over 43,000, so the coefficient indicates a reasonably large increase of about 6% in enrollment. This effect is smaller than the simulated prediction of a roughly one-third increase in enrollment from a $1000 voucher in Ferreyra (2007), although her modeling of the voucher is different from here and she assumes full mobility of households.[24] Further, the results for the individual credit laws are strikingly different. Adding the two coefficients in column 4 together would yield a point estimate suggesting that enrollment *fell* following the adoption of an individual-credit program, although this net decrease is imprecise and a Wald test cannot reject the hypothesis that effect of an individual-credit law on student enrollment is zero.[25] Using only future adoptee states as controls does not change this effect.

The last two columns of Table 7 report estimates using total enrollment from the Private School Universe Survey as the dependent variable. The sample size is smaller as the PSS is only biennial. The specification is the same as in columns 2 and 3 except that the year-of-enactment dummy is also set to 1 for the year following enactment (PSS data are only available every other year). The coefficient on corporate credits is a little over 3 times as large in the PSS sample, and the individual credit effect (the difference 2816 - 2238 in column 4 versus 9150 - 6782 in column 6) is a little over 4 times larger for the PSS. These suggest that our tax sample may have slightly larger increases in enrollment than would be the case if the sample were perfectly representative, although the precision of the estimates certainly would allow for the tax sample to be representative. Moreover, the estimates in the last two columns are qualitatively similar to the results using the tax return data, with corporate credits increasing enrollment much more clearly than individual credits. We can use the results of this table to make out-of-sample revenue predictions, an idea we consider next.

## 5B. Effects for All Private Schools

One could use these results to provide estimates for the effect of choice on all private school revenue. Taking revenue as simply $P \times Q$, where the price is per-student revenue and quantity is total enrollment (and is a

[24] See the first row in Table 5A of her paper.

[25] The Tables 6 and 7 estimates are typically similar when including only schools with per-student revenue below $25,000 or when including all schools. The only notable difference is that, when using only schools with less than $25,000 per student revenue in the sample, both individual and corporate tax credits lead to higher tuition rates, with the coefficient in column 1 in Table 6 becoming 0.67 [0.143]. But the results in the second column of Table 7, that there is a positive enrollment effect for corporate credits and no such effect for individual credits, is found in all samples.

function of price), for a small change in private school subsidization rate *sub* one would have the change in revenue as $\frac{\delta P}{\delta sub}Q + P\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$. This formula might best be thought of as appropriate for small changes in a subsidy, but larger changes would require an additional "large effect" adjustment of $\frac{\delta P}{\delta sub}\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$. This gives the revenue equation:

$$\frac{\delta P}{\delta sub}Q + P\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub} + \frac{\delta P}{\delta sub}\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$$

This is illustrated in figure 5; the two shaded areas represent the effect of a small change (where $\Delta Q = \frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$ and $\Delta P = \frac{\delta P}{\delta sub}$), but the unshaded rectangle of area $\Delta P \Delta Q$ will become relevant in calculations as the change in the subsidy grows large.

Moreover, the basic supply/demand model in the figure obviously makes questionable assumptions for the private education market; we note a few of these below but we can first check whether this revenue formula provides a decent within-sample estimate of the change in revenue from the subsidization created from choice. Further, since our estimates earlier provide the total change in revenue-per-student for all students, our use of the formula does *not* impose the assumption in the picture that the subsidy raises prices for all students, nor does our calculation assume that all students are charged the same price.

Beginning with a within-sample use of the formula, we can estimate the incidence on schools, $\frac{\delta P}{\delta sub}$, using column 1 of Table 6 divided by the per-student-funding estimate of \$827, the enrollment change $\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$ using column 1 of Table 7 over the per-student funding estimate, and for $P$ and $Q$ we can use the sample averages for states with choice, which are about \$11,500 and 24,000 respectively (as opposed to using the full sample means in Table 2 or the corporate-credit-sample means mentioned earlier). This yields a calculated revenue effect of:

$$\frac{\delta P}{\delta sub}Q + P\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub} + \frac{\delta P}{\delta sub}\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub} = \left(\frac{218}{827}24000 + 11500\frac{1546}{218}\frac{218}{827} + \frac{1546}{218}\left(\frac{218}{827}\right)^2\right) \cong 27,825$$

which is quite close to the baseline estimate of 28,350.

Turning to estimates for all schools, we can estimate $\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$ using column 5 of Table 6 again divided by full-sample subsidy estimate of \$827; we can use the PSS to calculate average enrollment in our treatment states and we can estimate per-student revenue using average tuition in the Schools and Staffing Survey (which is much smaller and available far fewer years than our sample, but will provide a representative national tuition average); the SASS data suggests that in 1999 (about the midpoint of our sample) that average tuition for all private schools in the country (in year 2012 dollars) was about \$6500. Tuition in the

SASS is based on highest charged tuition; it may overestimate $P$ if discounts are used for some students (although this would still be accurate if the discounts are awarded through funded scholarships) but it may underestimate $P$ as it does not include other fees. We thus use \$6500 as a reasonable benchmark. The only term in the formula we do not have a direct estimate for is the price effect of subsidies for all schools, $\delta P$.

Table 8 reports estimated revenue effects, estimated price elasticity of supply, and estimated dollar for dollar effects for all private schools using the above revenue formula and a variety of assumptions for $\delta P$. We focus on the effects of all schools since the dollar-for-dollar effects are roughly comparable across types of programs. The first row shows the change in revenue using the above formula (with block bootstrapped Conley-Taber-based 95% confidence intervals; their construction is described in the Appendix), the second row shows the assumed change in revenue per student, and the bottom two rows estimate the elasticity of supply and the effect on private school revenue for each dollar of funding spent.

The first column reports estimates under the assumption that price (that is, revenue per student) does not change when choice is introduced, a very strong assumption given that earlier we found evidence that prices changed for some schools; here any revenue increase comes from a quantity increase alone. Column 2 assumes that the effect on per student revenue for all schools is half of the effect we estimate for the tax sample. Column 3 assumes that the incidence of choice in our sample matches the incidence in all schools. Column 4 assumes that the incidence for other schools is twice the incidence in our sample, and the last column assumes full incidence, $\frac{\delta P}{\delta sub} = 1$.

Regardless of the assumption on how school choice affects revenue-per-student; we find that choice clearly leads to large increases in revenue, with *all* estimates suggesting that funding raises revenue on roughly a dollar-for-dollar basis or more. (Figure 5 shows how a greater-than-dollar-for-dollar effect is quite possible, as the rectangle $ABP_DP_S$ could easily be smaller than the area covered by the change in revenue.) A naive calculation of incidence could be made by simply multiplying our tax-sample revenue estimate 27,825 by 5, implying a dollar for dollar effect of $5 \times 27,825/58,342 = 2.38$; which is comparable to several of the numbers in the table. Overall, Table 8 suggests that choice has large impacts on school revenue for all private schools, and that the results from our tax sample provide a reasonable first-order guide for all schools even if the incidence of schools out of our sample diverges widely from what we observe here.

## 5C. Deadweight Loss and the Elasticity of Demand

One could also use our estimates to calculate the deadweight loss of school-choice programs. As a very simple starting point, one could simply estimate a traditional "deadweight loss triangle" created by the subsidy, where the base of the triangle would reflect the increase in student enrollment as a school choice program is enacted, and the height of the triangle would be "per-student" spending on an average school choice program. We could then divide the area of the triangle by the total spent on the average school-choice program, creating a "deadweight-loss-per-dollar spent estimate." (The calculation of the triangle is illustrated in Figure 5.) Such a calculation of course involves several assumptions, including that the market structure would facilitate such a deadweight loss calculation and that our estimates can be extrapolated to other private schools not in the sample (although the revenue estimates in Table 8 suggest that our results seem at least roughly representative on some margins). The latter assumption is implicitly made here when we calculate the height of the triangle as statewide school-choice spending over the statewide number of students assisted. A more rigorous depiction of the market structure would have to take a strong stand on the objective function of private schools, which is beyond the scope of this paper. A simple calculation also ignores any deadweight loss from the collection of government revenue, or any "spillover" benefits from greater private education. Given these caveats, we think presenting a deadweight loss estimate is worthwhile for motivating future work, for adding context to our estimates, and as a starting baseline, but we suggest the estimate be interpreted with care.

Using the baseline estimate (that is, the estimate which treats all laws the same) in Table 7 to calculate deadweight loss in this way yields a deadweight loss per dollar spent of $0.05, with a 95% confidence interval (calculated using the delta method) of -0.019 to 0.12. We know of no similar calculation to put this into perspective, although we might note that the canonical deadweight loss estimates of income taxation in Harberger (1964) for high-income families are similar. The fact that we estimate an (arguably) modest deadweight loss is not surprising, given that the baseline estimate on the student enrollment is not large.

One could also use the coefficients here to construct an estimated elasticity of demand for private school. Such an elasticity might best be interpreted as applying to marginal students responding to the program, and again will be based on the assumption that the estimates in our sample can extend to the population of private schools. For this estimate we can take the increase in enrollment from the baseline estimate in Table 7 for the change in quantity. The change in price is generated from the baseline per-student-revenue estimate in Table 6 minus average per-student choice funding for states with a school choice law. Dividing the change in quantity by the change in price, and then multiplying by the ratio of average price over average enrollment yields an elasticity of -0.52, with a 95% confidence interval of -0.95 to -0.08.[26] This is slightly larger in absolute value than the baseline elasticity of -0.19 in Dynarski, Gruber, and Li (2009). However, as their elasticity might reflect the price responses of families with siblings already in private school, one might interpret our estimate as not surprising if families with siblings in private school are less price sensitive than

[26] The elasticity estimate uses coefficients from two different regressions; we estimate them together using Seemingly Unrelated Regression and use the resulting variance-covariance matrix for estimating the variance of the elasticity when applying the delta method.

marginal families responding to choice.

## 5D. Religious versus Secular Schools

Some critics of school choice laws have expressed concern that they provide funding for religious organizations. While many congregation-affiliated churches will not be in our sample, our sample *does* include a large number of religiously-oriented schools (about half of the sample). Table 9 provides estimates where the sample has been split by religious affiliation: panel A presents results for secular schools, and panel B presents results for religiously-oriented schools.[27]

The panels show broadly similar effects across both types of schools. Both secular and religious schools see increases in revenue in response to school choice laws, and in both cases overall effects on per-student-revenue and total students are imprecise. Both types of schools report larger revenue effects for corporate-credit/voucher programs than for individual-credit programs, and larger effects on student enrollment for corporate-credit/voucher programs. Perhaps the most notable difference is in the revenue-per-student estimates; these results show that religious schools are perhaps more aggressive in raising per-student tuition and fees in response to an individual-credit law than are secular schools. Clearly, the table indicates that both secular and religious schools see an increase in revenue from school-choice programs.

## 5E. Results for Additional Law Changes

Table 10 includes all states and years, adding in states that enacted voucher programs that were more limited either in scope or in the number of years available in the sample. The first two columns show the baseline index result with these extra observations; it is similar although slightly smaller than before. The smaller size of the "index" variable in this column is not surprising, and the index now includes smaller school choice programs that presumably had smaller effects on school revenue.

The next column breaks the index into three dummies: a dummy for voucher programs, corporate credits, and individual credits. The results clearly suggest the largest gains for corporate credits. The next pair of columns shows estimates on revenue per student. These results echo the earlier results in Table 6, where there is clear evidence of higher per-student-revenue from individual tax credits but not other programs. The last two columns show results on total students, and again the estimates match the earlier results, with individual credits seeing no clear increase in students and enrollment gains observed for corporate credit programs.

There is very little effect from the voucher programs in Table 10 (although some evidence of an increase in

[27]Information on religious affiliation is taken from the PSS; schools are asked "Does this school/program have a religious orientation or purpose?"

enrollment). This may be due to the limited scope and observability of these programs in the sample. Or, it could be that the corporate credits are more effective. The baseline estimates include a large voucher program, the McKay voucher program. While its expansion coincided with Florida's corporate tax-credit program, we note that the McKay program, while large, is focused on disabled students. We therefore dropped from the Florida sample schools that identified themselves in the PSS as having a "special education" school. If the estimates are driven by the McKay voucher program, we might then see our estimates being affected by the inclusion of these special education schools. In fact, this is not the case; redoing column 1 in Table 10 without these schools produces a similar estimate (17684, s.e. of 7533). The same is true in column 2; the corporate coefficient becomes 39409 [9854] and the voucher coefficient becomes 4186 [10,376].

In summary, the above estimates suggest that school-choice laws have reasonably large effects on private school revenue, both for secular and for religiously-oriented schools. This finding likely extends to private schools not in our sample as well. However, we find evidence that the mechanisms by which revenue increases are different across types of laws; in particular we find that individual-income-tax credit laws lead to higher tuition with little evidence of any change in student enrollment.

Why do the effects of these laws differ? Here we offer a few possibilities. First, it may be that private schools enjoy a greater degree of market power for students they have already enrolled who then qualify for an individual tax credit. Importantly, individual tax credit laws typically allow families to target funding towards children already in private schools, including their own children, while the McKay voucher and corporate credit laws place restrictions on student eligibility based on income, disability, or a student's prior enrollment. For the case of individual credits, schools with relatively high market power can thus raise their tuition or other fees to capture the rent created by the subsidy. In contrast, the market may be more competitive for disabled students in public school or low income students in public school who subsequently become interested in attending private school after qualifying for a corporate-funded scholarship. If the marginal cost of adding one of these students is low, it is possible that schools may compete for these latter students by increasing enrollment with minimal changes in tuition or other student fees. A difference in market power could thus reconcile the estimates here.

A second, non-exclusive, explanation is based on information. If schools want to cream skim potential entrants, the nature of this skimming might vary between the different laws. Corporate tax credits must be provided through STO organizations, and schools may partner with these organizations to identify and vet attractive students. With individual credits, this screening device is unavailable. If schools are faced with an increase in demand following an individual credit's implementation, and if (for example) family income is positively related to the unobservable characteristics that schools want to select on, then schools may subsequently be able to raise tuition to improve their screening process. Note that in this story—and in the other stories as well—the fact that enrollment does not change in aggregate does not imply that the population of students attending private school is the same after school choice as before. Even if total enrollment is unchanged it could be that school choice allows newly marginal students to "crowd out" other students who are either screened out, or unwilling to pay the higher tuition (or both) following the policy reform.

## Conclusion

This paper investigates how school choice impacts the finances of private schools. Despite the current explosion of state-instituted choice programs, and in particular tax-credits for school choice, we know of very little work that studies these credits, or discusses the impacts of large choice programs on private schools, or that explores the incidence of school choice, or that calculates the deadweight loss of school-choice-based subsidization.

We find that school choice programs raise school revenues, and that the effects are dollar for dollar or larger. There is strong evidence that different types of school choice programs impact schools differently. In particular, corporate tax credits and individual tax credits increase revenue in different ways, with corporate credits raising revenue through increases in student enrollment while individual-income-tax credits raise revenue through increases in per-pupil expenditures. As we note above, these results may suggest that schools view the market for students currently attending public schools as more competitive than is the market for students already enrolled in private school. Alternately, these results might suggest that private schools cream skim students, and that the STO organizations supported by corporate credits allow schools to cream skim students in a way that other choice programs do not.

Even casually informed citizens are likely aware that school choice has raised important church-state concerns, although we know of no rigorous work that considers this topic. Our findings suggests that school choice leads to significant increases in revenue for religious private schools and that these schools may be especially likely to increase revenue through price increases. However, in considering choice overall, our back-of-the-envelope calculations indicate that the deadweight loss of school choice may be reasonably modest in size.

There are several caveats to our findings. First, as noted above, even in cases where our results suggest no change in enrollment in the face of school choice, this does not necessarily mean that the *composition* of students stayed the same after choice was introduced. Our work here does not capture distributional implications of choice, which are certainly of great interest to scholars and policy makers. Additionally, the simulation-based work noted in the introduction often assumes fully mobile households. If household mobility is greater over a long period of time, then the findings here may underestimate the long-term impacts of choice. But even these short-to-medium-run impacts will likely be of interest given the very large number of nascent choice programs being introduced across the country. Extending our results to these programs, or addressing these other caveats, represent excellent areas for future work.

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# Appendix A: Additional Description of State School-Choice Programs

*Arizona*

In 1997, Arizona offered a tax credit to individual taxpayers for donations made to STOs. The credit was initially equal to the amount of the donation made and capped at $500. STOs are required to spend at least 90% of the money they receive on scholarships and may not limit scholarship recipients to only one school. Some details of Arizona's credit have changed over time. Married and unmarried taxpayers were initially eligible for the same $500 credit, but over time, the limit for married taxpayers has gradually increased to $1,000. Since the program's inception, donors have been prohibited from requiring that their gifts be used to fund a scholarship for any of their dependents. However, there is evidence that in fact many parents did ensure that their contributions resulted in scholarship support for their children, either by "suggesting" their child as a recipient or by (e.g.) making a quid-pro-quo agreement with another family where each family would designate a gift towards the other family's child (Reese, 2009; Bland, 2000). In response to such behavior Arizona later (after the period of this study) prohibited donors from designating their gifts for any particular student.

*Florida*

In 2001, Florida established a tax credit for corporate tax payers that made donations to organizations similar to Arizona's STOs. The credit is limited to 75 percent of a taxpayer's liability although taxpayers can carry forward unused claims for up to three years. As Table 1 shows, scholarships awarded by STOs under this program are restricted to low income students. The maximum scholarship awarded ranged from $3,500 in 2002 up to $3,950 in 2009. This program was preceded by the establishment of the McKay Voucher program in 1999; this voucher program provides fairly generous voucher payments to students with disabilities (as defined in Table 1). The maximum possible payment equals the lesser of (a) the amount the student would have "received" in public school or (b) the amount of the private school's tuition and fees, whichever is less. Although Table 1 shows that this program began in 1999, participation in the program was quite limited in the first two years of its inception (the program in 1999 was in fact a pilot program); this is discussed more below.

*Iowa*

Iowa's credit for tuition and textbook expenses was enacted in 1987; it initially offered a non-refundable, credit on the first $1,000 of tuition and textbook expenses to tax filers with adjusted gross income under $45,000 who claimed the standard deduction or a deduction of up to $1,000 to those who itemized. In 1996, the deduction was eliminated, the tax credit was made available to all filers, the match rate was increased from 5% to 10%, and the income restriction was eliminated. In 1998, the match rate was again increased to 25%, and the list of eligible expenditures was expanded.

*Illinois*

In 1999, Illinois adopted a credit providing a non-refundable credit equal to 25% of educational expenses above $250 incurred on behalf of dependent children. The maximum value of the credit is $500. Eligible expenses include private school tuition as well as book and lab fees that may be incurred by students in either public or private schools. Observers have noted that while the program can cover public-school expenses, families with public-school students "receive very little in the form of tax credits...the credit serves primarily as a reward for Illinois parents who send their children to private schools" (People for the American Way, 2003).

*Pennsylvania*

Pennsylvania enacted a corporate credit in 2001; corporations can claim a nonrefundable credit for contributions to STOs (although the state's legislation refers to them simply as "scholarship organizations") up to 75 percent of contributions made, or 90 percent of contribution if a corporation provides "a written commitment" to provide such a scholarship with the same contribution for two consecutive tax years. The overall credit amount is limited by a fixed appropriation which grew from $30 million in 2001 to about $74 million in 2009. A tax credit for any corporation is limited to $300,000 per tax year. During the period of study here, scholarships could be awarded to families with less than $50,000 in income; this figure has changed since the time period of this study.

# Appendix B: Description of Data

## Enrollment and Tax Data

The financial data we use come primarily from the Core Financial Files produced by the National Center for Charitable Statistics (NCCS) from the Internal Revenue Service's (IRS) Return Transaction Files. These files contain financial information collected from Internal Revenue Service Form 990 and Form 990-EZ for 501(c)(3) public charities with at least $25,000 in gross receipts.[28] Many religiously affiliated organizations, such as Catholic schools, are not required to file Form 990 or Form 990-EZ and are not included in these

[28] Beginning in tax year 2010, the filing threshold was $50,000 in gross receipts.

data.[29]

There are two notions of "year" to consider in the Core Files. The first is the fiscal year reported on a tax return; this is the notion of a year that is most relevant for our study. The second is the "circa year," which refers to the year that a particular set of tax returns was assembled by the NCCS for inclusion in the Core data files. The NCCS creates its "circa year" data files at the beginning of each year by collecting financial information from the most recent available tax return for each organization.[30] We combine these files for 1989-2010 to create our sample. The term "circa year" thus refers to the year that a set of tax returns was collected. This may not be the same as the fiscal year since, when the NCCS collects its data and assembles a "circa year" dataset, the most recent available tax record for a nonprofit may not be from the most recent fiscal year, and an organization may thus have the same fiscal year record appear in multiple circa years. In these cases, we keep the record from the most recent circa year and drop older records of the duplicated fiscal year. We match the tax-return data to other datasets using the fiscal year reported on the tax return, rather than the circa year.

Our primary financial variable of interest is "program service revenue." Reported in Part VIII, line 2g of the 2012 version of Form 990, program service revenue is income a non-profit organization generates by charging for the provision its tax-exempt service.[31] Private school tuition is a canonical example of program service revenue, but this variable could also include revenue derived from other sources, such as the provision of food service or the sale of school supplies.

For data on enrollment and other school characteristics, we use the Private School Universe Survey (PSS). Conducted biennially by the National Center for Education Statistics (NCES), the PSS is meant to be a census of private schools in the United States. Response rates are routinely in excess of 90%. The survey covers enrollment, grades offered, number of teachers, religious affiliation, and racial composition, but it does not include any financial information about schools.[32] We next describe the procedure of matching these two datasets.

[29]Although the information on these forms is collected for tax compliance purposes, researchers have concluded that error rates are suitably low for use in academic research. See Pollak and Pettit (1998).

[30]More specifically, a circa year file will include all the returns received by the IRS in a certain calendar year as well as returns of organizations that have filed in the previous two years but do not appear in the present year's files. In more recent years, NCCS has started creating files that are organized by fiscal year rather than circa year. However, these fiscal year files are are not available for the period of our study.

[31]The previous version of Form 990 reported program service revenue in Section I, Line 2. Form 990-EZ also reports program service revenue in Section I, Line 2. The definition of program service revenue does not change across years or forms.

[32]In 1993, the PSS did not ask schools to report pre-school enrollment. Also, religious affiliation is missing for roughly 16% of schools in 1993. All other years of the PSS include pre-school enrollment data and report religious affiliation for all schools.

## Crosswalk and School Identification Procedures

In order to create a dataset that contains financial and enrollment information for the same schools, we create a crosswalk between the NCCS data and the PSS. Although the two datasets do not contain a common identifier, the PSS data contain schools' names beginning in 2005-06 and schools' street addresses beginning in 2007-08. The NCCS data contain names and addresses in all years. Using all years of NCCS data and the three most recent rounds of the PSS, we match names and addresses to create a crosswalk between the two datasets that links an organization's Employer Identification Number (EIN), reported on its tax forms, to its NCES-assigned PIN number. We then use this EIN-PIN correspondence to match tax and enrollment records in all available years of data. This procedure biases our sample toward currently active and long-lived schools.

In constructing our crosswalk, we collect four types of matches: exact address matches (street address, zip code and state), exact name matches within the same zip code, similar names within "zip plus four" area, and exact name matches within the same state, allowing for zip code mismatches. Address-based matches were inspected and names compared manually to ensure accuracy. This procedure generates a small number of cases in which we match a single EIN to multiple PINs or vice versa. We discard all such matches.[33]

In addition to those matched using our crosswalk, we identify schools in the NCCS data using their National Taxonomy of Exempt Entities (NTEE) Classification System codes, which organizations use to describe their primary purpose on their tax forms. We consider schools that classify themselves using codes B20 (elementary and secondary schools), B24 (primary and elementary schools), and B25 (secondary and high schools). We do not consider preschools (code B21), since the scholarships financed by the tax credits we study generally cannot be used to attend preschool. This approach also does not identify any organizations that function as schools but choose to identify themselves using other codes. A Christian high school, for example might choose to classify itself using NTEE code X20, a code used by organizations related to Christianity, instead of B25. Similarly, a school for developmentally disabled children might classify itself using code P82 (developmentally disabled centers) rather than one of the codes we consider. Since organizations can list only one NTEE code on their tax forms, we are unable to identify any schools that do not list schooling as their primary purpose.

[33]One could imagine cases in which one identifier or the other might change for legitimate reasons (e.g. a school could come under new management, or it could reincorporate itself as a tax entity, leading a single PIN to match, appropriately, to multiple EINs, or a single non-profit organization operates multiple schools, leading a single EIN to match to multiple PINs). However, the EIN in the NCCS data and the PIN in the PSS are generally intended to be permanent identifiers, so in most cases they should not change over time.

## Differences between Enrollment and Tax Samples

The PSS and NCCS data are collected with very different goals, and so there are important differences between the types of schools found in each sample. The PSS is intended to include all schools that offer classroom instruction in at least one grade from kindergarten through twelfth grade and are not supported by public money. There are no enrollment, revenue, or religious affiliation requirements for inclusion in the PSS. Since charter schools are supported by public money, they are not covered by the PSS.[34]

The NCCS data describe non-profit organizations generally and only incidentally include schools. Moreover, since filing requirements are designed to apply generally, broad categories of schools are likely to be under-represented in the NCCS data. Most notably, many religious organizations are not required to pay taxes, file tax returns, or even register with the IRS. Many religious organizations also operate private schools. Schools operated by religious organizations, therefore, are less likely to be included in the NCCS data than they are to be included in the PSS, which surveys schools of all religious affiliations. The NCCS data may also miss small schools with very low levels of revenue, since organizations with gross receipts under $25,000 during our analysis period were not required to file Form 990 or Form 990-EZ. The vast majority of schools are likely exceed this threshold, but a small, kindergarten-terminal school, for example, could conceivably fall below the revenue cutoff for inclusion in the NCCS data while still appearing in the PSS, since it offers kindergarten.

These differences between the PSS and NCCS data also imply that the set of schools we are able to match to a tax record will differ from the set of schools we are not able to match. Across all years, matched schools have higher enrollment (both including and excluding preschool) and employ more teachers than unmatched schools. A substantially smaller share of matched schools are affiliated with the Catholic church, and a substantially larger share are nonsectarian. Matched schools have slightly lower rates of affiliation with religions other than Catholicism. Appendix Figure 1 plots the share of matched and unmatched schools that are religious (Catholic or not) or non religious. (Trends for total enrollment and total teachers are given in Figure 3 in the main text.) Although the levels differ, trends are similar across the two groups. These pictures look roughly the same if we produce them using all years of PSS data for schools in our crosswalk instead of just those years in which we match them to tax records.

Appendix Table 1 reports school counts and enrollment totals for the full PSS, crosswalk, and matched samples. As one would expect based on the procedure used to create our crosswalk, the crosswalk and matched samples include a larger share of total schools and total students in more recent years, with the matched sample including more than 20% of all students in 2005 and reaching 25% in 2009.

[34]Charter schools are included in the Common Core of Data, the survey of public schools conducted by the NCES.

## Standardizing Fiscal and Academic Years

The task of matching each school's financial information to its enrollment information for the appropriate year is complicated by the fact that many schools use fiscal years that differ from their academic years. The PSS asks administrators to report enrollment at the beginning of October, and schools often require students to have paid tuition at the beginning of the school year. It is important, therefore, to make sure that the tax records we match to each enrollment record includes the beginning of the academic year.

We address this issue by assigning an academic year to each NCCS observation based on the calendar year in which the October of its fiscal year falls. For example, suppose we observe a tax record for a school with a July1, 2003 – June 30, 2004 fiscal year. The October of this school's fiscal year is in 2003, so we assign this tax record to the 2003-04 academic year. If this school used a January 1, 2003 – December 31, 2003 fiscal year, this tax record would still be assigned to the 2003-04 academic year, because its fiscal year contains October 2003. However, if the school instead used a November 1, 2003 – October 31, 2004 fiscal year, this tax record would be assigned to the 2004-05 academic year.

For the schools in our data, the most commonly used fiscal year runs from July 1 through June 30, with over 60% of tax records in each circa year following this calendar. This means that financial and enrollment information align very well for most of our matched schools. There is a large drop in the number of schools with fiscal-year 1998 tax returns in the data; this drop is driven by a decline in the number of organizations with a fiscal year ending in June and is not a product of our cross walk procedure, but rather is observed in the "raw" Core data. This decline appears to be driven by a change enacted in June of 1999 affecting public disclosure of nonprofit 990 forms. [35]

## Sample Restrictions for School-level Analysis

For schools with matched financial and enrollment records, we construct a measure that approximates tuition by dividing total program service revenue by total enrollment. While we believe this to be the best available method of estimating tuition, it does produce some figures that are implausibly large, reaching into the hundreds of thousands and even millions of dollars. To some extent, seemingly high estimates of tuition arise from the fact that program service revenue includes income from both tuition and other sources, but this

[35] Beginning in June 1999, IRS regulations required all 501(c) organizations except private foundations to provide copies of their tax forms to anyone who requested them. If this disclosure requirement caused some schools with fiscal years ending in June 1999 (corresponding to the 1998-99 academic year) to file their tax returns late that year (that is, after NCCS collected its Core data at the beginning of 2000) before returning to filing on schedule in subsequent years (before the beginning of 2001), the NCCS data collection procedure could have missed these records. The most recent available tax record for these schools at the beginning of 2000 would have corresponded to the 1997-98 academic year, and the most recent available record in 2001 would have corresponded to the 1999-2000 academic year, missing 1998-99. Since NCCS does not update a given circa year Core file as additional records become available, our dataset constructed from those files would be missing any tax records filed late in this manner.

cannot explain tuition estimates of seven figures or more. High tuition estimates could also be a product of matches between a school's enrollment record and the tax return of a parent organization that also receives substantial revenue from other sources. For example, a university could operate a kindergarten-terminal preschool for the children of its staff. If the enrollment record of that preschool matched to the university's tax return, our measure of that preschool's tuition could easily reach tens or hundreds of millions of dollars. Finally, high tuition estimates could simply be a result of outliers in the data. The NCCS data documentation repeatedly notes (and suggest correcting for) the presence of outliers. Many non-profit organizations are small, and the accountants filing these forms may have imperfect knowledge of the relevant rules, so it would not be surprising if some mistakes are made. We address excessively high tuition figures in our baseline estimates by excluding schools with estimated tuition of $100,000 or more.

We apply several other restrictions to produce our school-level analysis sample. In both the tax data and the PSS, we exclude schools that report being located in different states in different years, including any that fail to report a state in any year. We also drop tax returns for schools in outlying US territories, which we cannot match to enrollment records, since the PSS does not cover these territories. Finally, we exclude schools that report zero or negative program service revenue on their tax returns.

## Appendix C: Description of Conley-Taber Confidence Intervals

This appendix provides a synopsis of the method we adopt from Conley and Taber (2011); readers seeking more detail are directed to their paper. Consider a difference in difference model $Y_{jt} = \alpha d_{jt} + X_{jt}^{\prime}\beta + \theta_j + \gamma_t + \eta_{jt}$ , where $d$ is a (possibly non-binary) policy variable, $X$ is a matrix of regressors, and $\theta$ and $\gamma$ are group and time fixed effects. The parameter of interest is $\alpha$. After projecting the variables onto time and group fixed effects and taking residuals, we have $\tilde{Y}_{jt} = \alpha \tilde{d}_{jt} + \tilde{X}'_{jt}\beta + \tilde{\eta}_{jt}$ . The number of groups where $d$ changes value, denoted $N_1$, and the number of time periods $T$ are taken as fixed as the number of groups where $d$ does not change, $N_0$, grows. The integers from $N_1 + 1$ to $N_1 + N_0$ are understood to index the $N_0$ control groups. Under reasonable conditions (see Assumption 1 in Conley and Taber, 2011), it can be shown that as $N_0 \to \infty$, the OLS estimator $\hat{\alpha}$ converges in probability to $\alpha + W$, where , where

$$W = \frac{\sum_{j=1}^{N_1} \sum_{t=1}^{T} \left(d_{jt} - \bar{d}_j\right) \left(\eta_{jt} - \bar{\eta}_j\right)}{\sum_{j=1}^{N_1} \sum_{t=1}^{T} \left(d_{jt} - \bar{d}_j\right)^2}$$

where $\bar{d}_j = \frac{1}{T}\sum_{t=1}^{T} d_{jt}$ and similarly for $\bar{\eta}_j$ . Defining the conditional distribution of $W$ as $\Gamma(w)$; it can be shown that under reasonable conditions a consistent estimator (as $N_0 \to \infty$) of this distribution is

$$\hat{\Gamma}(w) = \left(\frac{1}{N_0}\right)^{N_1} \sum_{l_1=N_1+1}^{N_1+N_0} \cdots \sum_{l_{N_1}=N_1+1}^{N_1+N_0} 1\left(\frac{\sum_{j=1}^{N_1}\sum_{t=1}^{T}\left(d_{jt}-\bar{d}_j\right)\left(\tilde{Y}_{l_j t}-\tilde{X}_{l_j t}'\hat{\beta}\right)}{\sum_{j=1}^{N_1}\sum_{t=1}^{T}\left(d_{jt}-\bar{d}_j\right)^2} < w\right)$$

where $\hat{\beta}$ is the OLS estimator.

As described in the text, first we estimate, using OLS on the full sample, the equation $y_{sy} = \beta \mathrm{X}_{sy} + \theta_s + \vartheta_y + \mathrm{T}_{sy} + \varepsilon_{sy}$, where the index coefficient has been removed, and use this regression to create residuals $\tilde{\varepsilon}_{sy}$. We next randomly assign the treatment states' index values to the control states, where the assignment is done at the state level (e.g., the index values for all years in Arizona are assigned to Alabama). After the five true state-index profiles have randomly been assigned to five new states we regress the residuals of the five new states on the state-index profiles and record the coefficient. We run 5,000 such regressions to estimate a distribution of coefficients.

Using these 5,000 coefficients we can construct a $p$ value by calculating, if $\hat{\alpha} > 0$ ($\hat{\alpha} < 0$ ), $p = 2 \times f$ , where $f$ is the fraction of coefficients greater than (less than) our true estimate $\hat{\alpha}$.[36] We can take the 97.5 percentile value from the distribution of 5,000 and subtract this from $\hat{\alpha}$; this is the lower-bound for our 95 percent confidence interval. We can subtract the 2.5 percentile value of this distribution from $\hat{\alpha}$ to generate the upper bound of the confidence interval.

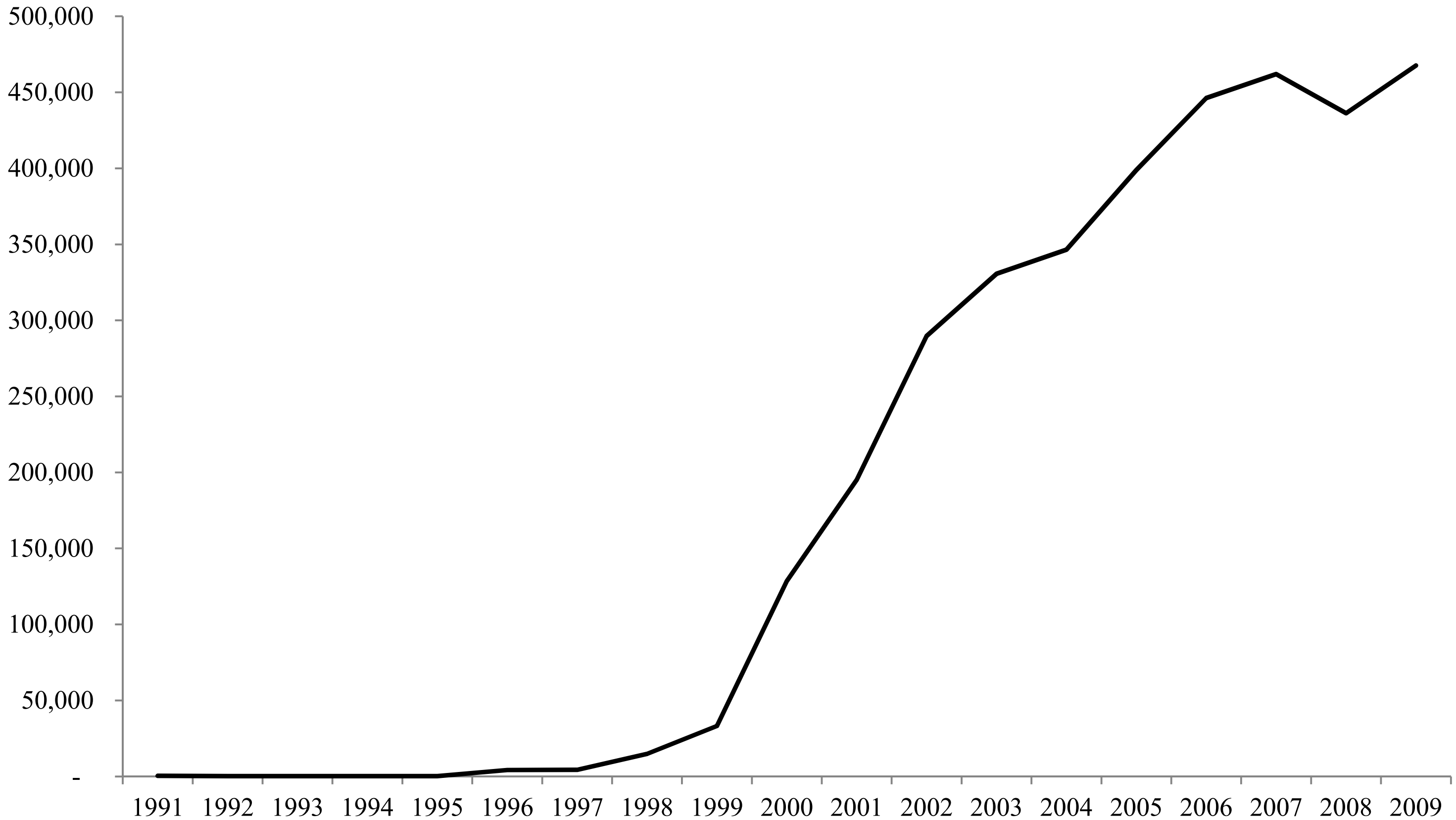
Our estimator $\hat{\Gamma}(w)$ matches the above representation of $\hat{\Gamma}(w)$ except that, rather than estimate the full distribution of $\hat{\Gamma}(w)$, we randomly assign the treatment variable and conduct 5,000 draws for each confidence interval. We are thus drawing 5,000 values of $\hat{\Gamma}(w)$ rather than the full distribution.[37]

For calculating confidence intervals out-of-sample effects on revenue, where the formula is $\frac{\delta P}{\delta sub}Q + P\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub} + \frac{\delta P}{\delta sub}\frac{\delta Q}{\delta P}\frac{\delta P}{\delta sub}$, we estimate a regression where enrollment (i.e. $Q$) is the dependent variable, and then for random-assignment regression $j$ we use the "placebo" coefficient $\tilde{\beta}_j$ to calculate $\Psi(\tilde{\beta}_j) = P\frac{\widetilde{\beta_j}}{\delta P}\frac{\delta P}{\delta sub} + \frac{\delta P}{\delta sub}\frac{\widetilde{\beta_j}}{\delta P}\frac{\delta P}{\delta sub}$, omitting the constant term $\frac{\delta P}{\delta sub}Q$. The resulting distribution $\Psi(\tilde{\beta}_j)$ can then be inverted to construct confidence intervals as above.

[36] In the case where $f$ is greater than $\frac{1}{2}$, we use $2 \times (1-f)$ .

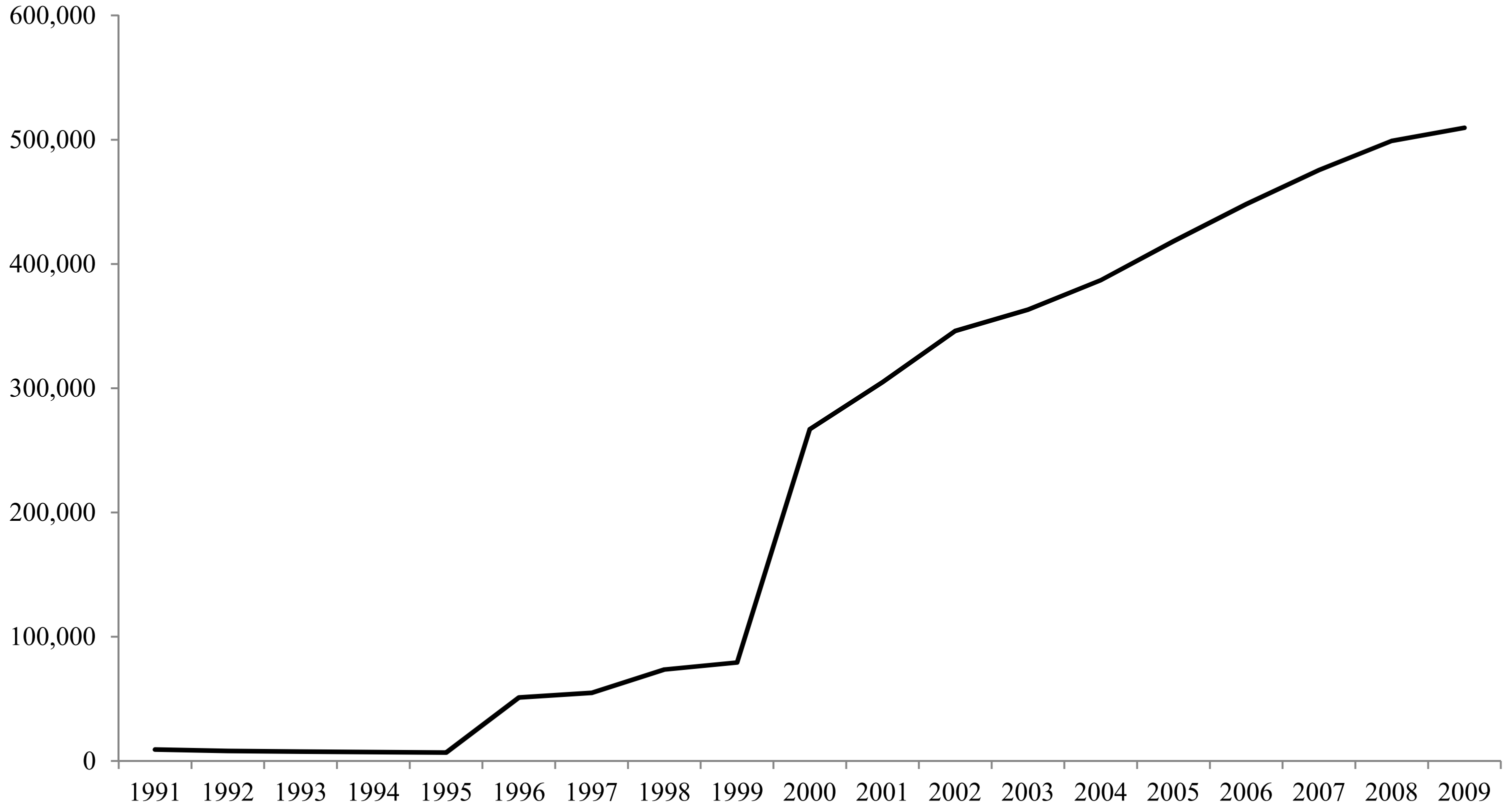
[37] To estimate the full distribution of $\hat{\Gamma}(w)$ here, where $N_0$ is 45 and $N_1$ is 5, would require 184,528,125 regressions for each confidence interval.

**Figure 1: Funding for School Choice, in $1000s**




The figure shows the total amount of funding, in year 2012 dollars, for the statewide school choice funding programs in Arizona, Florida, Illinois, Iowa, and Pennsylvania. See text for more details on the specific programs here.

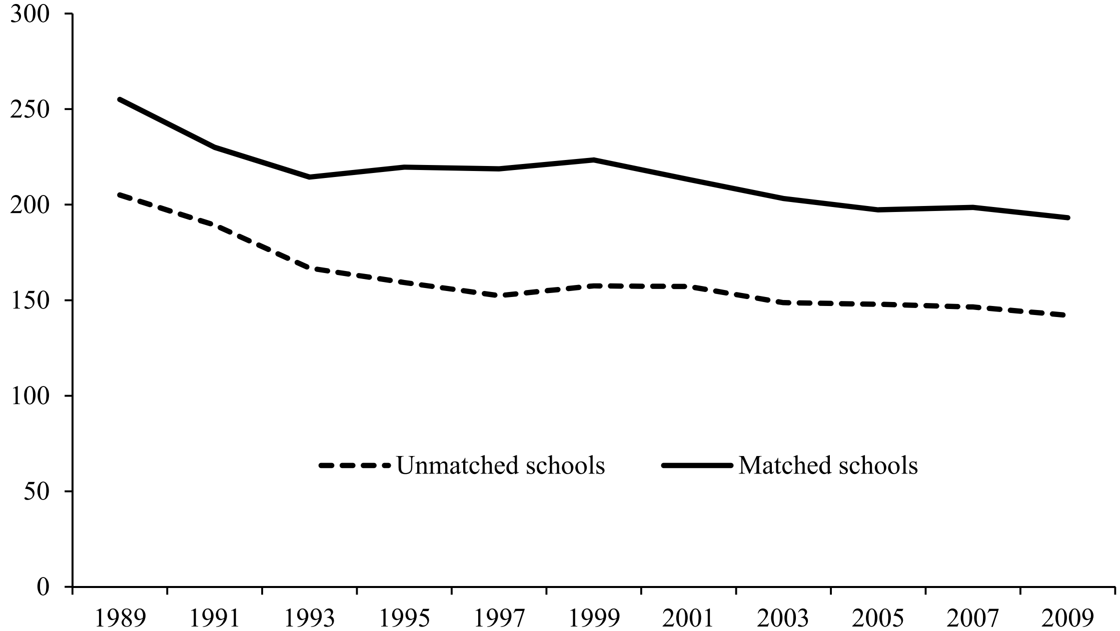
**Figure 2: Total Individuals Assisted By Statewide School Choice Laws**




The figure shows the total number of individuals assisted by the school choice laws enacted in Arizona, Florida, Illinois, Iowa, and Pennsylvania. Specificaly, the figure reports the number of individuals receiving a voucher, a tax-supported scholarship, or claiming a tax-credit for school expenditures (including tuition) in a given year.

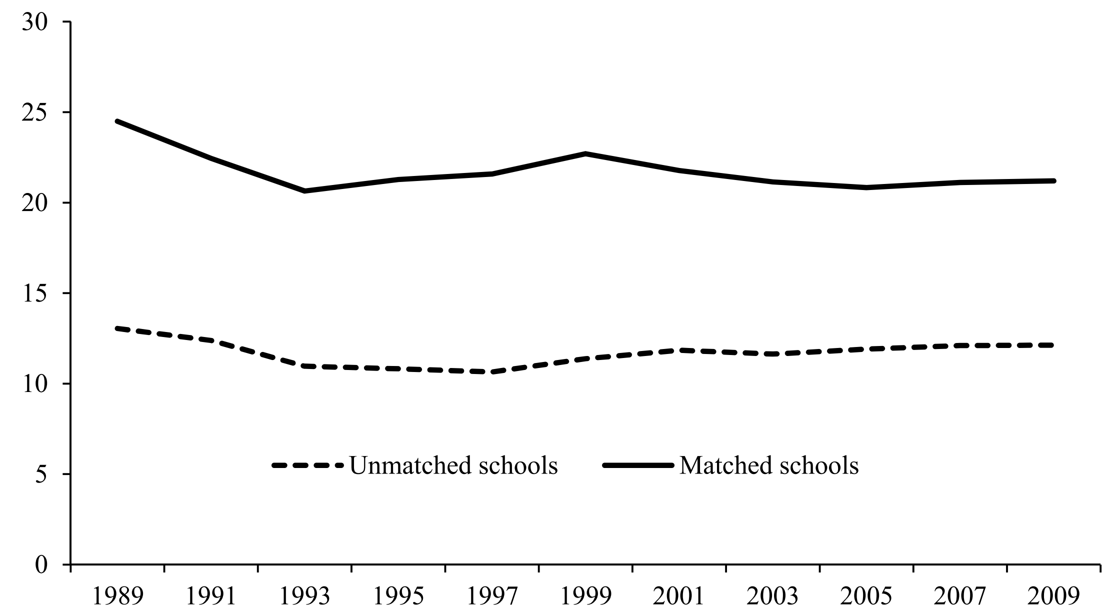
**Figure 3: Trends in the 990 Sample Versus the PSS Sample**

**Panel A: Enrollment**



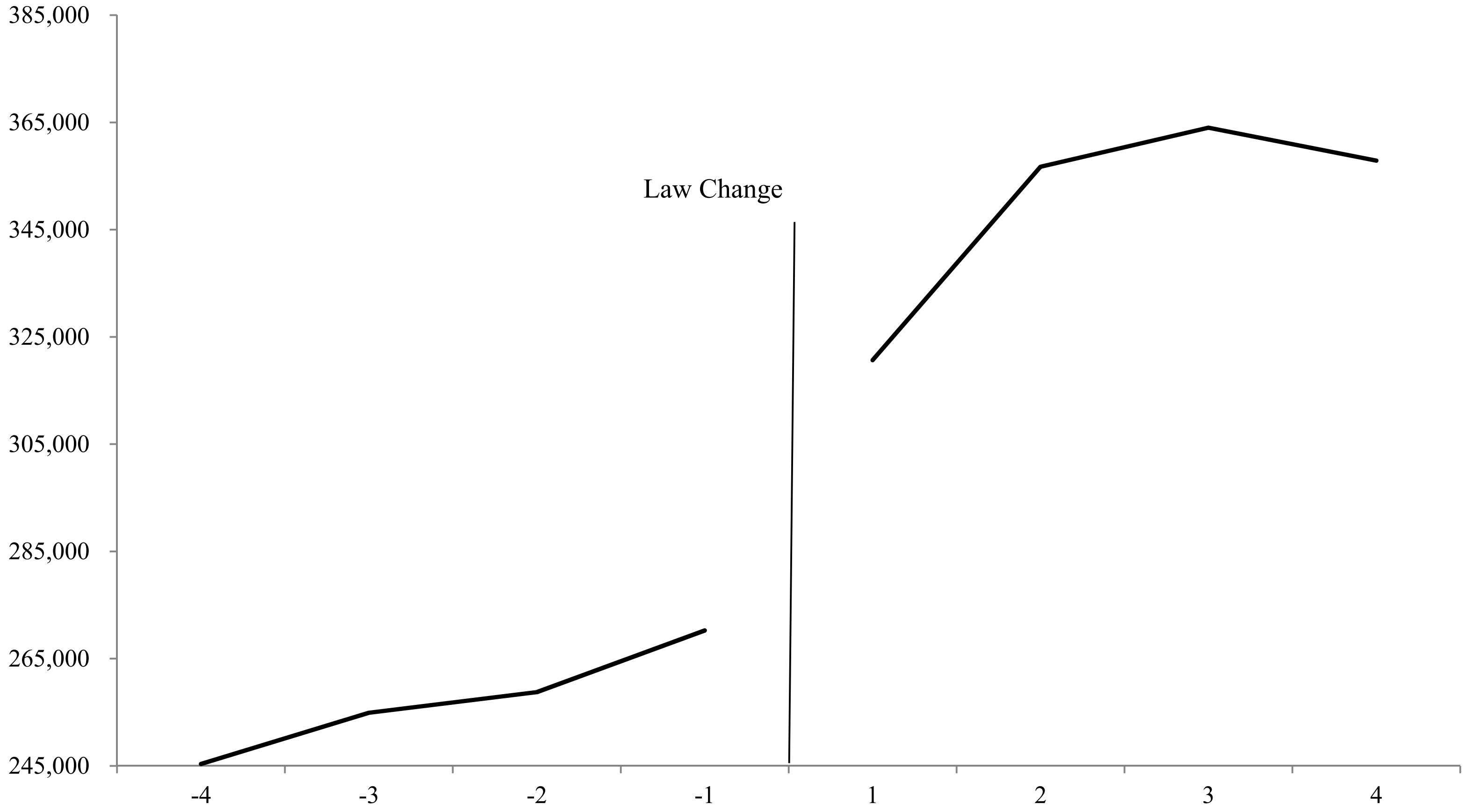

Note: Figure plots mean total enrollment in grades between kindergarten and twelfth grade for all schools in the PSS, including those that offer only a subset of those grades. A school is considered matched in a given academic year if it is linked to its tax record for the fiscal year that includes the October of that academic year.

**Panel B: Teachers**




Note: Figure plots mean number of teachers at all schools in the PSS. A school is considered matched in a given academic year if it is linked to its tax record for the fiscal year that includes the October of that academic year.

**Figure 4: Private School Revenue pre- and post- School Choice Reform**




The figure shows average private school revenue, in 1000s of 2012 dollars for private schools filing 990 tax returns in states which enacted statewide school choice policies during the period of study (Arizona, Florida, Illinois, Iowa, and Pennsylvania). Average revenue across each state is calculate from 4 years before a policy change to 4 years after; the year of the policy change (year “zero”) is omitted from the figure.

**Figure 5: Figure of Basic Revenue Effects and DWL Effects**

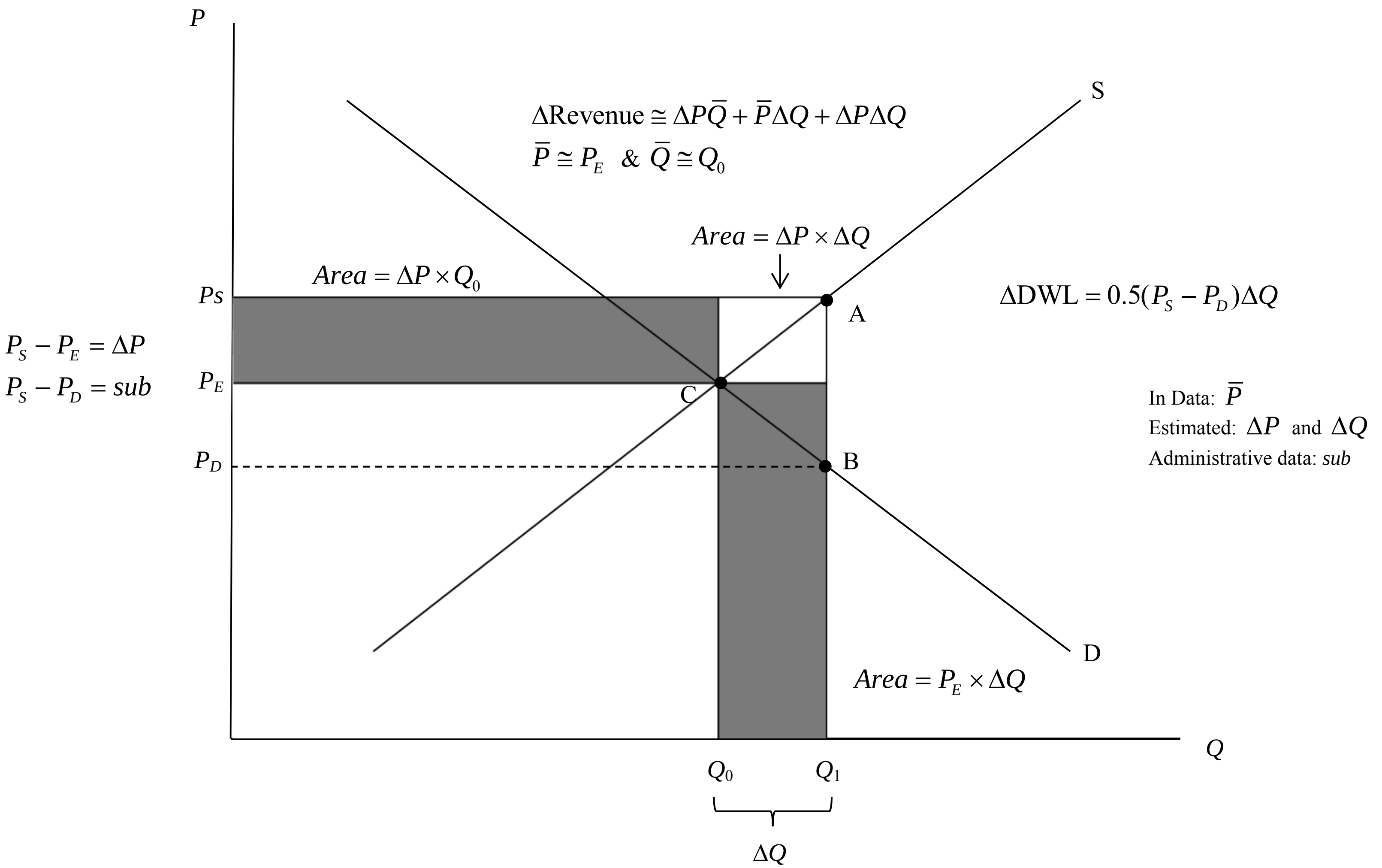


**Table 1: Overview of Main School-Choice Laws Used**

| State | Type of Law | Year Enacted/ Expanded | Means Tested? | Restrictions on Student Use |
|---|---|---|---|---|
| Arizona | Individual Tax Credit | 1997 | No | Original legislation prohibited designated STO contributions towards one's own child, but accounts indicate that this restriction was circumvented (see text in the appendix). |
| Arizona | Corporate Tax Credit | 2006 | Yes | STOs must not restrict donations to one school and corporate donations cannot designate a specific recipient. The income of students receiving scholarships must not exceed 185 percent of the free/reduced-price lunch limit. |
| Florida | Voucher (McKay) | 1999 | No | The McKay voucher program is restricted to students who have been issued an Individualized Education Plan or a 504 Accommodation plan. Student must also have been enrolled and reported for funding by a Florida school district the year prior to applying for a scholarship. Students with parents in the Armed Forces may also be eligible. |
| Florida | Corporate Tax Credit | 2001 | Yes | Eligible students must qualify for free/reduced-price lunch and must have either attended public school the previous year, received a scholarship the previous year, or be entering kindergarten or first grade. |
| Illinois | Individual Tax Credit | 1999 | No | Taxpayer must be the "custodian" of a pupil, under age 21, who is a full-time student of an elementary or secondary school in Illinois. |
| Iowa | Individual Tax Credit | 1987, Expanded 1996/98 and 2006 | Pre-1996: Yes Post-1996: No | Pre 1996: a household income limit was in place and expenses for "extracurricular activities" beyond tuition were ineligible. Both of these restrictions were eliminated starting in 1996. Post 2006 contributions to STOs cannot be used for the direct benefit of any dependent and cannot be designated by the taxpayer. |
| Pennsylvania | Corporate Tax Credit | 2001 | Yes | Family income must be under $50,000; this is adjusted upwards by $10,000 for each dependent in the household. (These numbers have been increased since the period of study here.) |

Sources: the primary source of data for this table was the authored by the Research Department of the Minnesota House of Representatives (2011); this report was verified by using state statutes and other state publications.

**Table 2: Selected Means**

| | Mean [std. dev.] |
|---|---|
| Total Revenue ($1000s) | 210,587<br>[301,016] |
| Total Enrollment | 16,457<br>[18,998] |
| Revenue Per Student | 11,019<br>[5,592] |
| Spending on School Choice ($1000s) | 58,342<br>[65,508] |
| School Choice: Individual Tax Credits (1000s) | 45,373<br>[29255] |
| Number of Schools per State/Year Observation | 66<br>[77] |

All monetary figures are in year 2012 dollars. Spending on school choice equals the total amount of tax credits claimed, and vouchers paid, in any state with a school choice tax credit or voucher program in the sample; there are 61 such state/year observations. The individual tax credit total equals the total amount of individual school-choice tax credits claimed in states with an education school-choice tax credit program; see Table 1. There are 928 state/year observations from 1991 to 2009; although there student data is unavailable for two observations, so that the number of students and revenue per student are based off of 926 observations.

**Table 3:**
**The Effect of Tax-Credit Laws on School Revenue (in $1000s)**

| | Baseline | No RHS | Future-Adoptee States as Controls | Quadratic Trends | Add NCCS-identified Schools |
|---|---|---|---|---|---|
| School Choice Laws | 28,350<br>[7,539]<br>(21,872 ~ 34,850) | 28,425<br>[7,274]<br>(19,243 ~ 34,707) | 28,886<br>[6,920]<br>(25,045 ~ 33,919) | 33,792<br>[9,032]<br>(29,025 ~ 42,022) | 35,258<br>[9,045]<br>(29,836 ~ 43,931) |
| RHS Controls | Yes | No | Yes | Yes | Yes |
| State Quadradic Trends | No | No | No | Yes | Yes |
| State Linear Trends | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes |
| State Dummies | Yes | Yes | Yes | Yes | Yes |
| Observations | 928 | 928 | 346 | 928 | 928 |
| R-squared | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 |

The dependent variable is total program service revenue, in 1000s, for private schools, in year 2012 dollars. The data are aggregated to the state level and cover the years 1991 to 2009. There are 61,219 school/year observations in the baseline regression. The standard errors in brackets are clustered by state; the 95% confidence intervals are calculated based on 5,000 block-bootstrapped replications in an approach adopted from Conley and Taber (2011); see text. The regressor "School Choice Laws" is the number of statewide invididual- or corporate-funded school choice programs in a given state and year. All regressions include dummy variables that equal unity the year a school choice policy was enacted. The right-hand side controls include controls for state median income, the state unemployment rate, the population density, the fraction of a state's residents that are noncitizens, the fraction born abroad, the fraction of the population that is white ages 6-10, 11-13, and 14-17, and the same for blacks, Hispanics (of any race), and non-Hispanics of Other Races. Column 2 omits these extra controls. Colum 3 includes states that adopted a school choice law during or after the period of this sample; these states are AZ, CO, FL, GA, IA, IL, IN, LA, MN, MS, NC, NH, OH, OK, PA, RI, VA, VT, WI. Column 4 adds state-specific quadratic trends. The last column redoes column 4 but adds in schools identified using NCCS information; there are 77,818 school/year observations for this regression. The mean amount of funding for the school choice policies (in year 2012 dollars) in the regression sample is about $58 million.

**Table 4:**
**School Choice and Revenue for Other Types of Nonprofits**

| | Schools | Animal-Related | Crime & Legal-Related | Employment | Agriculture & Nutrition | International |
|---|---|---|---|---|---|---|
| School Choice Laws | 33,792<br>[9,032]<br>(29,025 ~ 42,022) | 2,172<br>[1,854]<br>(486 ~ 5540) | 8,140<br>[5,123]<br>(5,849 ~ 10,808) | -1,850<br>[4,587]<br>(-6,144 ~ 3,019) | -2,277<br>[1,386]<br>(-2,980 ~ -1,450) | 913<br>[4,464]<br>(-2,570 ~ 5,011) |
| RHS Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| State Quad. Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| State Linear Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| State Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 928 | 927 | 929 | 928 | 926 | 875 |
| R-squared | 0.99 | 0.95 | 0.97 | 0.99 | 0.98 | 0.92 |

The figure shows regressions on total revenues in 1000s of year 2012 dollars for several different types of nonprofit organizations. The first column reports estimates using our sample of schools; this column matches column 4 in Table 3. The categorization of nonprofits is done using the National Taxonomy of Exempt Entities Core Codes Classification System (NTEE-CC). The full designation for column 5 is "Food, Agriculture & Nutrition" and for column 7 it is "International, Foreign Affairs & National Security; the headers on the table are slightly shortened to avoid clutter. Examples of Animal-Related nonprofits include zoos, aquariums, and animal-protection groups. Examples of Crime & Legal nonprofits include law enforcement and legal services groups. Examples of Employment nonprofits include Goodwill Industries and labor unions. Examples of Agriculture & Nutrition nonprofits include food banks & pantries. Examples of Housing & Shelter nonprofits include housing search assistance groups and senior citizens housing & retirement communities. Examples of International Nonprofits include international human rights groups and international relief groups.

**Table 5:**

**Individual- and Corporate-Tax-Credit Laws and School Revenue (in $1000s)**

| | Baseline | No RHS | Future-Adoptee States as Controls | Quadratic Trends | Add NCCS-identified Schools |
|---|---|---|---|---|---|
| School Choice Laws | 37,488<br>[7,450]<br>(31,014 ~ 44,042) | 36,520<br>[6,871]<br>(26,881 ~ 42,559) | 32,989<br>[6,642]<br>(28,894 ~ 37,900) | 40,887<br>[10,342]<br>(35,989 ~ 49,580) | 44,132<br>[8,999]<br>(38,698 ~ 53,164) |
| Individual-Tax Credit Laws | -30,091<br>[7,954]<br>(-36,547 ~ -22,283) | -25,732<br>[7,728]<br>(-33,152 ~ -19,356) | -14,859<br>[11,814]<br>(-18,833 ~ -9,291) | -19,337<br>[16,685]<br>(-23,593 ~ -6,470) | -25,231<br>[15,510]<br>(-29,814 ~ -12,375) |
| RHS Controls | Yes | No | Yes | Yes | Yes |
| State Quadradic Trends | No | No | No | Yes | Yes |
| State Linear Trends | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes |
| State Dummies | Yes | Yes | Yes | Yes | Yes |
| Observations | 928 | 928 | 346 | 928 | 928 |
| R-squared | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 |

The dependent variable is total program service revenue, in 1000s, for private schools, in year 2012 dollars. The data are aggregated to the state level and cover the years 1991 to 2009. There are 61,219 school/year observations in the sample. The standard errors in brackets are clustered by state; the 95% confidence intervals are calculated based on 5,000 block-bootstrapped replications in an approach adopted from Conley and Taber (2011); see text. The regressor "School Choice Laws" is the number of statewide invididual- or corporate-funded school choice programs in a given state and year. The "Individual-Tax Credit Laws" coefficient is a dummy that equals unity when a state enacts an individual income tax credit for private school tuition. All regressions include dummy variables that equal unity the year a school choice policy was enacted; see Table 3 for a description of the other right-hand-side controls. Column 2 omits extra RHS controls. Colum 3 includes states that adopted a school choice law during or after the period of this sample; see Table 3. Column 4 adds state-specific quadratic trends; the last column redoes column 4 but adds in schools identified using NCCS information. The mean amount of individual credits claimed in the regression sample (in year 2012 dollars) for states offering individual credits is about $45 million; the mean amount of funding for school choice policies overall for policies included in the regression is about $100 million.

**Table 6:**
**Tax Credits and Per-Student Revenue (in $1000s)**

| | Per-Student Revenue | | Future-Adoptee States as Controls | | Per-Student Revenue Quadratic Trends | |
|---|---|---|---|---|---|---|
| School Choice Laws | 0.218<br>[0.420]<br>(-0.64 ~ 0.55) | 0.116<br>[0.406]<br>(-0.75 ~ 0.47) | 0.269<br>[0.364]<br>(-0.12 ~ 0.59) | 0.125<br>[0.277]<br>(-0.24 ~ 0 .44) | 0.296<br>[0.397]<br>(-0.13 ~ 0.92) | 0.187<br>[0.377]<br>(0.07 ~ 0.84) |
| Individual Tax Credit Law | - | 0.874<br>[0.385]<br>(0.05 ~ 1.33) | - | 1.269<br>[0.423]<br>(0.84 ~ 1.67) | - | 0.992<br>[0.444]<br>(0.86 ~ 2.07) |
| RHS Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| St. Quad. Trnds. | No | No | No | No | Yes | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| State Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 926 | 926 | 346 | 346 | 926 | 926 |
| R-squared | 0.98 | 0.98 | 0.97 | 0.97 | 0.98 | 0.98 |

The dependent variable is total program service revenue over total students for private schools in year 2012 dollars. The data are aggregated to the state level and cover the years 1991 to 2009. The standard errors in brackets are clustered by state; the 95% confidence intervals are calculated based on 5,000 block-bootstrapped replications in an approach adopted from Conley and Taber (2011); see text. The regressor "index" equals zero if a state has no state income-tax credit law, one if a state has either an individual- or a corporate-credit law and two if both (in Arizona after 2006). All regressions include dummy variables that equal unity the year a tax-credit policy was enacted; see Table 3 for a description of the other right-hand-side controls. Since these regressions report means across states and years (unlike the total revenue and student results), the regression are weighted by the number of students (unweighted regressions typically give similar results). The table omits NCCS-identified schools as student counts are unavailable for these schools.

**Table 7:**
**Tax Credits and Enrollment**

| | Baseline | | With Quadratic Trends | | Total Students: PSS Data | |
|---|---|---|---|---|---|---|
| School Choice Laws | 1,546<br>[1,177]<br>(790 ~ 2,060) | 2,741<br>[973]<br>(1,961 ~ 3,222) | 2,091<br>[1,080]<br>(1,721 ~ 2,702) | 2,816<br>[939]<br>(2,455 ~ 3,424) | 6,833<br>[9,940]<br>(2,331 ~ 9,651) | 9,150<br>[12,475]<br>(7,268 ~ 11,718) |
| Individual Tax Credit Law | - | -4,178<br>[1,575]<br>(-4,659 ~ -3,637) | - | -2,238<br>[1,656]<br>(-2,589 ~ -1,261) | - | -6,782<br>[14,388]<br>(-9,144 ~ -3,044) |
| RHS Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| St. Quad. Trnds. | No | No | Yes | Yes | Yes | Yes |
| State Trends | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| State Dummies | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 926 | 926 | 926 | 926 | 486 | 486 |
| R-squared | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 | 0.99 |

The dependent variable is total enrollment (pre school through grade 12) in private schools. The data are aggregated to the state level and cover the years 1991 to 2009. The standard errors in brackets are clustered by state; the 95% confidence intervals are calculated based on 5,000 block-bootstrapped replications in an approach adopted from Conley and Taber (2011); see text. The regressor "index" equals zero if a state has no state income-tax credit law, one if a state has either an individual- or a corporate-credit law and two if both (in Arizona after 2006). All regressions include dummy variables that equal unity the year a tax-credit policy was enacted; see Table 3 for a description of the other right-hand-side controls. Estimating the baseline estimates using the future-law-changers sample produces estimates are similar to those in the table and are omitted for brevity.

**Table 8:**
**Estimated Revenue Impact for All Private Schools in Treatment States**

| | Zero Incidence on Schools | ½ Sample Incidence | Sample Incidence | 2 × Sample Incidence | Full Incidence on Schools |
|---|---|---|---|---|---|
| Change in Revenue (1000s) | 53,705<br>(14,605 ~ 75,434) | 86,469<br>(47,367 ~ 108,198) | 119,277<br>(80,175 ~ 141,007) | 184,760<br>(145,656 ~ 206,490) | 302,288<br>(263,181 ~ 324,020) |
| Δ Revenue per Student | $0 | $109 | $218 | $436 | $827 |
| Elasticity of Supply | - | 1.6 | 0.8 | 0.4 | 0.2 |
| Dollar-for-Dollar Effect of Choice Funding | 0.921 | 1.482 | 2.044 | 3.167 | 5.181 |

The table shows the calculated impact of school choice on school revenue for all private schools. The formula for calculation is given in the text; here the "price" of schools is taken from the Schools and Staffing Survey, the quantity is from total enrollment in all private schools in the treatment states as reported in the Private School Survey, the subsidy amount is from administrative data for all programs, and the change in enrollment is from the earlier regressions on PSS data for all schools. The estimates in the table vary depending upon the assumed incidence on schools, i.e. the change in the per-student revenue from school choice. Column 1 assumes no change, column 2 assumes a change in per-student revenue equal to one half of the change in per-student revenue observed in the tax data, column 3 uses the same change in per-student revenue, column 4 uses twice the change, and column five assumes that the full incidence falls on schools. The bottom row divides the top row estimates by the average spending amount in Table 2: $58,342. The 95% confidence intervals in parentheses are calculated from block bootstrapping in an approach similar to the Conley-Taber based intervals used earlier; see text.

**Table 9: School Choice Funding in Religious and Secular Schools**

Panel A: Secular Schools

| | Total Revenue (in $1000s) | | Revenue Per Student (in $1000s) | | Total Students | |
|---|---|---|---|---|---|---|
| School Choice Laws | 14,236<br>[7,012]<br>(8,031 ~ 18,092) | 20,591<br>[8,640]<br>(14,675 ~ 24,659) | 0.229<br>[0.791]<br>(-1.19 ~ 3.09) | 0.27<br>[0.900]<br>(-1.498 ~ 3.993) | 765<br>[475]<br>(-189 ~ 1,719) | 1,256<br>[396]<br>(776 ~ 1,494) |
| Individual Credit Laws | - | -21,014<br>[8,141]<br>(-28,104 ~ 16,117) | - | -0.166<br>[0.831]<br>(-1.55 ~ 6.58) | - | -1,723<br>[665]<br>(-2,080 ~ -1,432) |

Panel B: Religious Schools

| | Total Revenue (in $1000s) | | Revenue Per Student (in $1000s) | | Total Students | |
|---|---|---|---|---|---|---|
| School Choice Laws | 14,259<br>[3,105]<br>(10,797 ~ 18,240) | 17,069<br>[2,510]<br>(13,731 ~ 20,990) | 0.106<br>[0.255]<br>(-0.26 ~ 1.67) | -0.047<br>[0.167]<br>(-0.42 ~ 1.51) | 821<br>[700]<br>(433 ~ 1,162) | 1,530<br>[585]<br>(1,155 ~ 1,851) |
| Individual Credit Laws | - | -9,218<br>[3,711]<br>(-12,434 ~ 5,056) | - | 1.409<br>[0.254]<br>(1.00 ~ 4.89) | - | -2,473<br>[981]<br>(-2,724 ~ 2,145) |

There are 31,760 secular school/year observations and 30,352 religious school/year observations (a small number of schools that do not report a religious affiliation are not included in the sample). The specifications otherwise match the baseline regressions in Table 3; all regressions include state dummies, year dummies, state linear trends, and right-hand-side covariates. The standard errors in brackets are clustered by state; the 95% confidence intervals are calculated based on 5,000 block-bootstrapped replications in an approach adopted from Conley and Taber (2011); see text.

**Table 10:**
**Regressions Using All States, Years and Laws**

| | Revenue ($1000s) | Revenue ($1000s) | Revenue Per Student ($1000s) | Revenue Per Student ($1000s) | Students | Students |
|---|---|---|---|---|---|---|
| School Choice Laws | 18,632<br>[7,881]<br>(11,786 ~ 25,230) | - | 0.17<br>[0.264]<br>(-0.71 ~ 0.50) | - | 1,317<br>[930]<br>(482 ~ 1,801) | - |
| Corporate-Tax Credit Laws | - | 40,850<br>[10,210]<br>(32,021 ~ 51,302) | - | 0.517<br>[0.597]<br>(-0.96 ~ 0.95) | - | 3,006<br>[1,592]<br>(2,352 ~ 3,955) |
| Individual-Tax Credit Laws | - | 8,359<br>[6,228]<br>(2,195 ~ 16,130) | - | 1.158<br>[0.238]<br>(0.33 ~ 1.59) | - | -1,324<br>[1,166]<br>(-1,800 ~ -836) |
| Voucher Laws | - | 5,341<br>[10,647]<br>(-965 ~ 13,685) | - | -0.39<br>[0.410]<br>(-0.75 ~ -0.09) | - | 1,251<br>[1,607]<br>(705 ~ 2,296) |

These regressions add in school choice policies that were limited in scope or enacted at the very end of the sample period. These policy changes include a corporate tax credit in Rhode Island in 2007, a voucher program in Ohio starting 2003, a voucher program in Georgia in 2007, and a voucher program in Utah in 2005. Columns 1, 3, and 5 show baseline estiamates on the School Choice Laws regressor (where the regressor reflects these law changes as well as the changes used in the main estimates). Columns 2, 4, and 6 present dummy variables for whether a state has a voucher program, an individual tax-credit program, or a corporate tax-credit program. Standard errors clustered by state.

**Appendix Table 1:**
**Baseline Estimates on Total Revenue with Pre-Law-Change Placebo Dummies**

| | Revenue ($1000s) | Revenue Per Student ($1000s) | Students |
|---|---|---|---|
| School Choice Laws | 29,042<br>[10,471] | 0.075<br>[0.270] | 2,318<br>[1,299] |
| One-Year Lead | -6,390<br>[8,960] | -0.168<br>[0.469] | 435<br>[1,239] |
| Two-Year Lead | 454<br>[7,075] | 0.066<br>[0.419] | 249.2<br>[969] |
| Three-Year Lead | 8,961<br>[7,492] | -0.211<br>[0.349] | 1,363<br>[1,132] |
| Four-Year Lead | 10,133<br>[11,880] | -0.375<br>[0.316] | 1,702<br>[1,477] |
| Five-Year Lead | -3,838<br>[3,854] | -0.611<br>[0.348] | 833<br>[808] |

The dependent variable is listed at the top of each column; monetary figures are in year 2012 dollars. The regression specification matches the baseline specification in Table 3, except that "lead" dummies have been added that equal unity a given number of years before a state enacts a tax-credit law. For example, the three-year lead equals unity three years before a state enacts a law (and equals zero all other years). Standard errors clustered by state.

## Appendix Table 2: School Counts and Enrollment Totals by Year

Panel A

| | Number of Schools | | | Total Enrollment | | |
|---|---|---|---|---|---|---|
| | PSS | Crosswalk | Matched | PSS | Crosswalk | Matched |
| 1991 | 23,766 | 3,735 | 2,086 | 5,286,398 | 1,006,057 | 546,289 |
| 1993 | 36,633 | 4,453 | 2,552 | 5,103,811 | 1,067,934 | 597,099 |
| 1995 | 28,622 | 4,960 | 3,007 | 5,918,044 | 1,300,344 | 774,850 |
| 1997 | 30,255 | 5,464 | 3,825 | 5,944,322 | 1,385,114 | 974,820 |
| 1999 | 29,159 | 5,608 | 3,120 | 6,018,280 | 1,459,796 | 853,777 |
| 2001 | 30,812 | 6,352 | 4,008 | 6,319,646 | 1,582,273 | 1,005,443 |
| 2003 | 30,071 | 6,777 | 4,910 | 6,099,221 | 1,649,958 | 1,216,358 |
| 2005 | 29,784 | 7,417 | 5,526 | 6,073,243 | 1,762,806 | 1,336,708 |
| 2007 | 28,450 | 7,437 | 5,698 | 5,910,210 | 1,851,425 | 1,456,768 |
| 2009 | 28,217 | 7,593 | 5,837 | 5,488,491 | 1,783,111 | 1,377,590 |

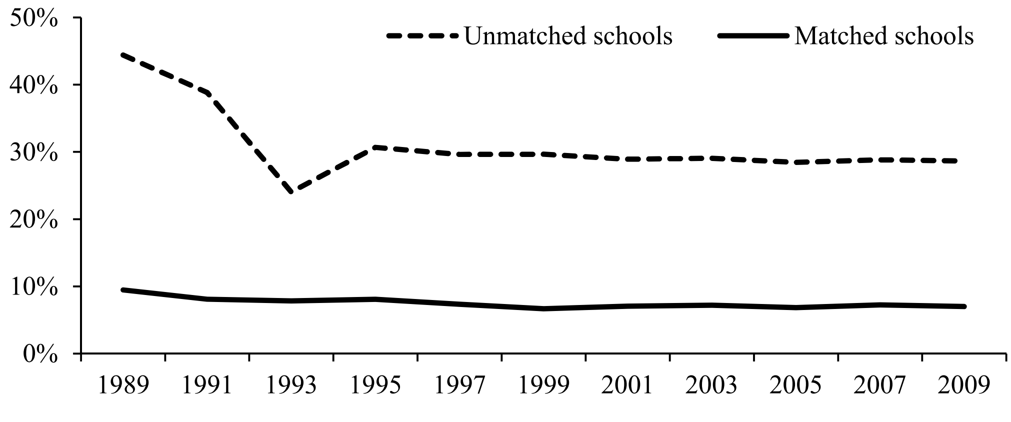
Panel B

| | Number of Catholic Schools | | | Number of Other Religious Schools | | | Number of Nonsectarian Schools | | |
|---|---|---|---|---|---|---|---|---|---|
| | PSS | Crosswalk | Matched | PSS | Crosswalk | Matched | PSS | Crosswalk | Matched |
| 1991 | 8,593 | 542 | 169 | 10,535 | 1,448 | 681 | 4,638 | 1,745 | 1,236 |
| 1993 | 8,397 | 555 | 199 | 15,473 | 1,721 | 820 | 6,912 | 2,113 | 1,497 |
| 1995 | 8,107 | 588 | 242 | 12,237 | 1,919 | 989 | 8,278 | 2,453 | 1,776 |
| 1997 | 8,117 | 598 | 279 | 12,756 | 2,136 | 1,267 | 9,382 | 2,730 | 2,279 |
| 1999 | 7,934 | 597 | 211 | 12,586 | 2,238 | 1,060 | 8,639 | 2,773 | 1,849 |
| 2001 | 8,044 | 632 | 282 | 13,638 | 2,659 | 1,485 | 9,130 | 3,061 | 2,241 |
| 2003 | 7,677 | 638 | 354 | 13,210 | 2,830 | 1,828 | 9,184 | 3,309 | 2,728 |
| 2005 | 7,292 | 672 | 380 | 13,447 | 3,200 | 2,190 | 9,045 | 3,545 | 2,956 |
| 2007 | 6,985 | 676 | 415 | 12,831 | 3,230 | 2,301 | 8,634 | 3,531 | 2,982 |
| 2009 | 6,835 | 667 | 412 | 12,619 | 3,279 | 2,430 | 8,763 | 3,647 | 2,995 |

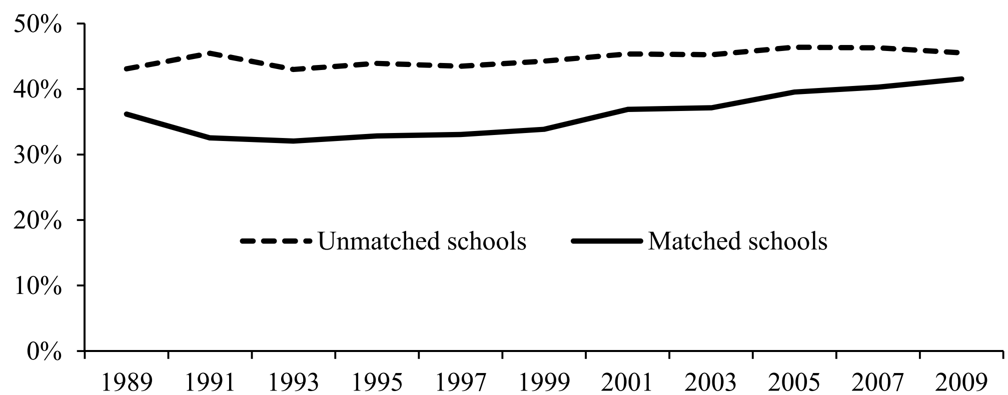
Note: Figures in columns marked PSS are based on the unrestricted Private School Universe Survey sample. Crosswalk columns report statistics using all available years of PSS data for schools for which we match a PSS record to a tax record in at least one year, regardless of whether the match was for the year in question. The Matched columns report statistics based only on PSS records that are actually matched to a tax record in the same year. Total enrollment is calculated using sample weights.

**Appendix Figure 1**

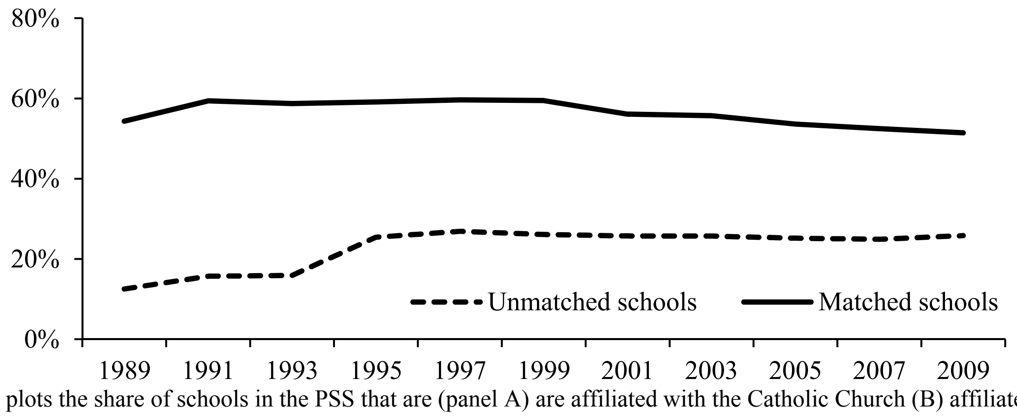
Panel A: Share of Matched and Unmatched Schools Affiliated with the Catholic Church




Panel B: Share of Matched and Unmatched Schools Affiliated with Non-Catholic Religious Organizations




Panel C: Share of Matched and Unmatched Schools Not Religiously Affiliated




Note: Figure plots the share of schools in the PSS that are (panel A) are affiliated with the Catholic Church (B) affiliated with a religion other than Catholicism (C) nonsectarian. Sample weights are not used to produce these values. A school is considered matched in a given academic year if it is linked to its tax record for the fiscal year that includes the October of that academic year.