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The Effects of Universal School Vouchers on Private School Tuition and Enrollment: A National Analysis

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Abstract: Three-quarters of a century after Milton Friedman popularized the idea, universal school vouchers have suddenly become a reality in 17 states since 2021. These new policies promise to be one of the most far-reaching reforms in U.S. education history. We make two contributions to understanding this policy. First, we provide a rich description of the private school sector nationally. We show that private schools vary widely and are much smaller and cheaper than is typically understood. Second, we study the policy's short-term effects on student enrollment and tuition using difference-in-differences analysis comparing the 11 states that adopted universal vouchers between 2021 and 2024 to non-universal voucher states. We find that universal vouchers have started to reshape the schooling market. Private school enrollment has increased by 3-4 percent relative to non-voucher states on average, mainly in schools with low baseline enrollments and non-Catholic religious schools. Vouchers have also likely increased private school tuition by 5-10 percent, primarily in non-religious schools and those that had low-enrollment/low-tuition at baseline. We also discuss the effects of COVID on enrollment and tuition, which complicates the analysis and interpretation of the voucher effects. Finally, we explain why the effects have been small so far and why they are likely to grow as schools and families adjust to the new policy reality.

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I. Introduction

School vouchers have been the subject of small-scale experimentation in the U.S. for roughly a half-century. First popularized by Milton Friedman (1955), the idea involves using government revenue to fund education at essentially all schools that families choose, including religious and other private schools, with minimal regulations. While there is a vast literature on these prior voucher programs (Epple, Romano, & Urquiola, 2017; Shakeel et al., 2021), early U.S. programs were small in scale and often targeted to specific subgroups, e.g., students living in particular cities, low-income students, and students with disabilities. Since 2021, however, 17 Republican-leaning states have, for the first time, adopted universal school vouchers in line with Friedman's original vision.

How has the market responded so far, in terms of private school enrollment and pricing strategies? We address this question using up to 57 percent of all U.S. private schools within a difference-in-differences framework. We focus on 11 states where policies went into effect in 2021 (Indiana and New Hampshire), in 2022 (Arizona, North Carolina, and West Virginia), in 2023 (Florida, Iowa, and Ohio), or in 2024 (Oklahoma, South Carolina, and Utah), where we have at least one year of post-treatment data. Figure 1 summarizes the changes in student eligibility in these states over the past five years.

We compare the short-run equilibrium outcomes in these treated states to 34 non-universal-voucher states with a difference-in-differences (DD) design. Since the treated states started their programs in different years, this is a staggered research design that leads us to use the method proposed by Callaway and Sant'Anna (2021). Also, since the share of eligible students varies over time in measurable ways, we also estimate continuous-treatment versions of our models (Callaway, Goodman-Bacon, and Sant'Anna, 2025; Chaisemartin, D'Haultfœuille, & Vazquez-Bare, 2022).

Our results suggest that universal vouchers have had only small effects so far, increasing private school tuition by 5-10 percent and increasing enrollment by 3-4 percent relative to non-voucher states, 1-4 years after treatment. This analysis builds on prior work that has examined the effects of similar programs in one state, Iowa (Fontana & Jennings, 2023). While our results pertain only to recent statewide programs, we note that they are especially salient given the recent passage of President Trump's *Big Beautiful Bill*, which allows all states to accept funds from a new federal tax credit that operates, in effect, as a national school voucher program.

Our national estimates are based on data from two main sources: (1) the Private School Review (PSR), a website where private schools advertise their schools and post information about their tuition, enrollment, and more; and (2) the National Center for Charitable Statistics (NCCS) at the Urban Institute, which reports total revenues for most non-profit organizations, including many schools. The PSR allows us to reliably track enrollment from 2020 to 2024 for 57 percent of private schools.[1] The PSR also includes usable tuition data for 19 percent of K-12 private schools nationally during this period. The NCCS data are likely more accurate but are available for only 11 percent of private schools and cannot, by themselves, distinguish tuition rates from enrollment levels or tuition revenue from other revenue. While each data source has its weaknesses, the combination of sources provides informative tests about private school responses to vouchers. We also use enrollment data from the more well-known Private School

[1] The denominator in these sample size calculations is the total number of schools listed with any data in the PSR, or 26,423 non-preschool-only schools. The PSR total number of schools is similar to the 29,730 private K-12 schools reported by the U.S. Department of Education.

Survey (PSS) from the U.S. Department of Education, though these data do not include tuition or revenue, which are key variables in this analysis.

While not part of our original purpose, we also find that COVID generated much larger increases in tuition (less so, enrollment) than vouchers have so far. Increased tuition from COVID is not surprising given that private schools experienced higher costs with respect to technology, increased demand from families, and general price inflation. This turns out to be important because state voucher programs were adopted in the midst of, or just after, the pandemic. In some analyses, this appears to keep us from satisfying the parallel trends assumption. COVID also changes the interpretation of our voucher results. Private schools may have been hesitant to significantly increase tuition in response to vouchers, just after they had raised tuition substantially due to COVID. For this reason, we expect our estimated effects to grow over time.

In the theory section, we propose seven additional supply- and demand-side reasons why the transition to a new equilibrium, with universal vouchers, is likely to be slow. For example, there are costs to switching schools and households and schools may be uncertain about whether the programs will continue. This helps explain why the initial effects we observe seem small so far and suggests that larger effects are yet to come.

The theory section also suggests possible dimensions of effect heterogeneity, which we subsequently test. The average positive effects on tuition rates are concentrated in non-religious schools and those that had low enrollment and low tuition at baseline. The limited tuition increases in religious schools are consistent with our theory that religious schools are motivated to "expand their flocks." In addition to the mission or spiritual dimension, religious schools have a financial incentive to keep tuition low to encourage families to belong to churches, which may generate additional revenue in the form of tithing or donations. The voucher effects on private school enrollment are concentrated mainly in schools with low baseline enrollments as well as Protestant and other non-Catholic religious schools. These results align with our theoretical predictions.

Our analysis is motivated partially by two fiscal concerns raised by voucher critics: (a) that private schools will attract substantial additional enrollments and thereby shift revenues away from traditional public and charter schools; and (b) that private schools will respond to the subsidies by raising tuition, extracting rents, and undermining the aim of broadened private school access.[2] This latter potential effect is similar to the long-running debate in higher education about the extent to which government grants and subsidized loans lead colleges to raise their tuition, i.e., the "Bennett Hypothesis." The evidence from higher education on this point is mixed (Cook & Turner, 2025) and, in any event, may not generalize to K-12 schooling.[3] Given the many reasons why the transition is likely to be slow, it is likely too early to judge the extent to which the Bennett Hypothesis is valid in primary and secondary education. However, our estimate of a 5-10 percent increase in tuition suggests that the program's incidence, so far, has accrued almost entirely to households, especially those with higher incomes.

Finally, we provide some of the first nationwide descriptive evidence about the national distribution of private school enrollments and tuition. The simple (unweighted) national *average* of enrollment and tuition across all private schools between 2020 and 2024 is 218 students and

[2] Other concerns arise regarding effects on student outcomes and welfare. Vouchers might shift the focus of education to the private good elements of education and away from the public good side (e.g., citizenship, tolerance for others, and community relations) (Levin, 2002). Even the private goods of schooling might be provided inefficiently because of the market imperfections inherent to schooling markets (e.g., imperfect information and geographic constraints) (Harris, 2024). There is also ongoing debate over voucher effects on common education outcomes like test scores and college-going (Harris, forthcoming; Wolf, forthcoming).

[3] For example, unlike higher education, school vouchers and private schools exist alongside ubiquitous zero-price traditional public schools (TPS), giving private schools a stronger incentive than colleges to keep their prices low.

$13,197, respectively, in the PSR. At one extreme, a non-trivial number of schools, mostly boarding schools, have tuitions above $150,000 per year. These figures fit the stereotype that private schools are expensive and somewhat smaller than publicly funded schools, but still similar in size and structure. However, the skewed distribution masks the fact that the *modal* private school has tuition of only $5,000 annually and a school size of 30 students, only slightly more students than the new microschools that have developed in recent years. Many private schools evidently operate "under the radar" with very few students in church basements, for example. This means the new voucher programs will increase access for a larger number of families than might otherwise have been assumed and will likely increase enrollment in smaller and cheaper schools.

Section II summarizes our theory, including the reasons why the small effects observed so far are predictable. Section III provides more details about the actual voucher policy changes occurring in these states and describes the treatment and comparison states. Section IV describes our data and provides the national descriptive evidence. Section V outlines our econometric framework. In Section VI, we present our results, discuss how these represent short-run effects, propose reasons to expect different long-run effects, and consider program incidence. Section VII concludes.

II. Theory

We start this section with a simple model that assumes profit maximization and no peer effects and then derive predictions about how schools and households will respond. As these assumptions are clearly unrealistic in almost any schooling market, we then relax them and discuss how the predictions change.

II.A. Simple Model

We begin with certain economic assumptions that apply to all households and schools and lead to an initial equilibrium condition before vouchers are introduced. The market moves to a new equilibrium after the policy. This theory is rooted in the work on school location, entry, and exit of Glomm, Harris, and Lo (2004).

Suppose that households maximize utility, which includes consumption and the well-being of their children (e.g., schooling outcomes). All households prefer higher levels of academic quality q (i.e., schools with larger participant effects) but vary in their tastes for a single other output h (e.g., religious instruction). Finally, households prefer living near the schools their children attend, due to the cost of transportation.

Next, suppose there are two types of schools: traditional public schools (TPS) run by the government (non-profit) and for-profit private schools. (We ignore charter schools for simplicity.) Schools of both types can be placed in the product space $\{q, h\}$. TPS do not produce h, while some private schools do (i.e., some are religious). All types of schools produce academic quality q, but this varies within types because of: (a) variation in total input levels; (b) the fact that some schools are trying to produce both types of outputs (h and q) and must split resources between them; and (c) random shocks in the efficiency of resource use. Each of these two outputs has a single known production function that is subject to economies of scale.

In the baseline period, we have only traditional public schools and tuition-paying private schools, without vouchers. Private schools choose to locate where demand is high and where there are few competitors.[4] Households sort themselves, in a Tiebout (1956) sense, into districts

[4] The location process is complex and involved game theoretic considerations that irrelevant to the present analysis.

based on how well their optimal bundles of taxes and services match those actually available, including how well their tastes match the public schooling characteristics and/or the location of private schools. Since all households prefer higher q, and it is a normal good, households sort their locations by income, allowing higher-income households to obtain higher q. In the second period, a universal voucher policy is introduced so that the net price becomes tuition less the voucher amount.[5] We discuss the policy provisions below.

The next section discusses predictions of the long-term effects that arise from this simple model with profit-maximizing private schools and no peer effects. This is followed by a discussion of what happens when we relax these assumptions and allow for non-profit schools as well as peer effects and distinguish short- and long-term effects.

II.B. Predictions of Voucher Effects from the Simple Model

Before proceeding, we clarify the details of the vouchers. They vary according to six main dimensions: (a) form of voucher (lump sum versus marginal price); (b) the size of the voucher; (c) restrictions on household eligibility; (d) restrictions on how the money can be used; (e) restrictions on the types of organizations that can be suppliers; and (f) top-up versus no-top-up in tuition over the voucher amount. Here, we consider only the case that aligns with the new state vouchers that are the subject of this analysis: lump sum, top-up vouchers with universal household eligibility and minimal restrictions over both how funds can be used (within the educational category) and who can be a supplier. For now, we focus on long-run equilibrium responses where fixed costs are irrelevant and everything (private school location and entry, household location, etc.) is flexible. Later, we discuss how this might look different in the short

[5] We borrow this terminology and others below from the higher education literature.

and medium runs. We focus first on the changes in tuition and then discuss the implications for enrollment.

First, on average, we predict that private schools would increase their tuition levels (sticker prices), but in a way that the net price still drops. It is difficult to identify plausible scenarios where this would not occur. Schools could conceivably capture a large share of the voucher amount as rents, but not enough to increase the net price.[6] The remaining predictions focus on effect heterogeneity in school responses.

Second, this type of voucher creates seemingly strong incentives for schools with baseline tuition below the voucher to raise tuition to the voucher level. If the funds can only be used for tuition, it is difficult to imagine any circumstances in which all private schools would not raise tuition to the voucher level. Doing so imposes zero direct costs on households.[7] The current iteration of vouchers, however, does include educational savings accounts (ESAs) that can be used for non-tuition educational expenses (e.g., tutors outside of school). This means that *every* dollar of tuition paid imposes some utility cost on households, even when tuition is less than the voucher. (It might appear that the partial rollover of voucher/ESA funds reinforces this, but this only clear in the elementary grades and the incentives reverse in upper grades.[8])

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[6] An easy way to see this is to draw various types of simple supply and demand graphs that shift demand to the right with the introduction of the voucher. The equilibrium sticker price rises but never more than the voucher amount itself. Even at the extreme, with a vertical (fixed) supply curve, the sticker price rises by the precise amount of the voucher but not more. The incidence of the subsidy depends on the elasticities of supply and demand.

[7] Recently adopted voucher programs include education savings accounts (ESAs) such that families would use some money for tuition and other money for other education-related expenses. This creates an incentive to keep the tuition level down, allowing families to use the funds for other purposes if they wish.

[8] If the funds can roll over, then parents can save and substitute spending across time, making them at least somewhat more reluctant to spend the funds when their children are young and making elementary schools less inclined to raise tuition. However, if families do save some of the funds, then this creates an incentive for high schools to raise tuition *more* than they would without the rollover. In other words, the net effect of the roll over on tuition is likely to be small as the incentives across grades cancel out.

Third, this type of voucher might create incentives for schools with high enrollments to raise tuition more than small schools because: (a) for larger schools, adding students is more likely to increase expenses (e.g., hiring more teachers to create new classes or expanding physical space); and (b) such schools are more likely to be at capacity, so that they cannot expand enrollment without incurring substantial additional costs (e.g., building more classrooms). However, even larger schools might be able to increase enrollments in particular grades without hiring more teachers, and it is difficult to identify the minimum efficient scale, especially given that we cannot observe the size of buildings.

Fourth, as noted above, most private schools are located based on their pre-voucher local demand. The change in equilibrium tuition and enrollment from vouchers would depend on the elasticity of supply and demand. In a perfectly competitive market, the demand curve facing each firm would be horizontal. More realistically, especially given the geography of schooling and role of transportation costs, demand would be downward sloping, but the elasticity might vary depending on the availability of substitutes and household income.[9] So, we expect larger tuition changes when there are fewer schooling options nearby and when the baseline price is low relative to household income. To the former point, when there are more private schools nearby, especially those in similar parts of the product space, the firm's elasticity of demand will be higher and private schools will increase tuition to a lesser extent.[10] Regarding the price/income

[9] Whether goods are necessities also drive the elasticity, but this is true for almost everyone and we are focused here on factors that lead to heterogeneity. Similarly, religiosity affects the level of demand, but it is not obvious that it affects the elasticity.

[10] We searched the industrial organization literature for estimates of the own-price elasticity of demand for similar services. Also, note that private schools have to pay attention to potential new entrants as well, not just the pre-existing ones noted in the text.

ratio, when the price of an expensive good increases, the negative effect on utility from the loss of other consumption is greater.[11]

Population religiosity might also be an important factor. It certainly shapes the level of demand for private schools. Some religious families might see religious schooling specifically as a necessity, which would reduce the elasticity of demand. This factor may be intertwined with income if income and religion are correlated.

Finally, we consider the implications of the above changes in tuition for enrollment. The first prediction above was that, under any plausible scenario, the net price will drop. Therefore, so long as education is a normal good with downward-sloping demand, equilibrium enrollment would increase.

II.C. Relaxing the For-Profit and No-Peer-Effect Assumptions

We started with a simple model assuming profit maximization and no peer effects. But all schools face peer effects, and TPS and private schools are rarely profit-maximizing in practice. Below, we consider the implications of these realities.

When governments and non-profits operate schools, it is common for researchers to assume *revenue* maximization as an alternative goal. For small schools, this does not seem to influence the previous prediction because profit maximization and revenue maximization are essentially the same thing under the above conditions (i.e., no capacity constraints); the marginal cost of adding students is approximately zero near the original equilibrium. For schools with capacity constraints, revenue maximization would lead to incentives similar to smaller non-profit

[11] The discussion here assumes a single local household income. However, we have to distinguish the households already sending their children to private schools from those who were not doing so pre-voucher. Schools have an incentive to remain attractive to their current customers, but also to draw in more inframarginal households that would not choose private schools without vouchers but would once the policy is introduced.

schools. As noted above, the main differences between small and large schools are on the cost side.

Even revenue maximization is too simplistic an objective for private schools. Many are religious and have an interest in "expanding the flock" of followers, regardless of revenue. Religious private schools are also often tied to churches, so that the revenue of interest is actually that of the whole church. When more students attend the school, this may increase attendance in church services and, indirectly, church donations. Finally, most private schools see themselves as having a charitable mission and may face criticism if they "took advantage" of the voucher to increase tuition to raise the salaries of staff and leadership.[12]

Peer effects also complicate the analysis of revenue because bringing in certain students may affect the demand of other students (and not just because of changes in class size). Parents clearly pay attention to demographics when choosing schools (Glazerman and Dotter, 2017). Indeed, the "prestige" of private schools often goes hand in hand with their selectivity, and parents are willing to pay for prestige.[13] Since the marginal students most likely to enter private schools will likely have "less desirable" characteristics, this may lead schools, especially those with capacity constraints, to increase tuition more than they would otherwise. In other words, while the input cost of adding more students might be low, the revenue loss from the drop in prestige would reduce net revenue. Schools may be particularly concerned about how prestige affects their long-term revenue, causing them to be hesitant or unwilling to participate in voucher programs out of fear of losing reputation. Also note that private schools, unlike TPS, can choose

[12] One specific reason for this is that parents often serve as board members of private schools. Those same boards set tuition rates. These parents have a direct incentive to keep tuition increases low and are likely to face pressure from other parents.

[13] See DeAngelis et al. (2019) for evidence that private school leaders strongly value their ability to select students.

who enrolls, which may allow them to select on student characteristics without pricing out certain families.

These practical realities require some reconsideration of the predictions of the simple model. The first conclusion, about the average changes in tuition, is no longer as clear; we still expect private schools to raise their tuition, but to a lesser extent. All the above discussion of non-profits points in this direction (e.g., that religious private schools want to expand their flocks and do not want to be seen as taking advantage). However, the predictions regarding effect heterogeneity still seem to hold. Other things equal, we still expect private schools with low tuition at baseline to increase tuition more, private schools with lower baseline enrollments to increase tuition less, and small, private religious schools to increase tuition less. Private schools located near others, both geographically and in the product space, will also increase tuition less.[14,15]

Later, we discuss why the adjustment to a new equilibrium is likely to be slower than with a similarly sized (net) price drop with other goods and services. This is largely because of the unique features of schooling as well as the fact that the price drop comes from a government subsidy. While there are good reasons to expect the long-term effects to be large, forecasting long-run effects is always more difficult, especially with major changes such as universal school vouchers.

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[14] An additional consideration is the possibility of collusion. The law does not distinguish for-profits from non-profits.

[15] The above discussion also raises an additional question: Why are private schools almost universally non-profit to begin with? We suggest three related answers: (a) for-profits require positive accounting profits and, if non-profits can operate efficiently, it may be difficult for them to compete when they have to allocate some resources to profits; and (b) the personal commitment many educators have for their work may indeed make non-profits more effective without a profit motive; and (c) even if (b) is false, parents may perceive that it is true, thus reducing perceived quality of for-profit schools and, consequently, parental demand.

The discussion also points to key dimensions along which voucher effects on enrollment and tuition might vary, e.g., that tuition increases are likely to be more pronounced in private schools with low baseline tuition. We return to this in our discussion of the effect heterogeneity analysis.

III. The New Universal School Voucher Policies

Voucher policies can be designed in many different ways. We expect the effect of vouchers on tuition and enrollment to be proportional to: (a) the share of *students* eligible to receive them; (b) the share of *schools* eligible to participate[16]; (c) the size of the per-student voucher funding; and (d) the degree of regulation. Note that regulation might shape the share of eligible schools that choose to participate in voucher programs (DeAngelis, Burke, & Wolf, 2019). Since most of the programs have few limits on school eligibility and minimal regulations (and the latter are, in any event, difficult to quantify), we focus on student eligibility and the voucher amount.

As an example, in Arizona, all students in the state became eligible for vouchers of $7,500 per year. Likewise, almost all private schools are eligible to participate, provided they meet minimal accreditation requirements. Participating private schools are not required to collect data about student outcomes in anything resembling the fashion of TPS, and the government does not hold them accountable for the results they do make available. Private schools can also "top off" the voucher and set tuition above (or below) the voucher amount, as they see fit. The policies in the other universal voucher states are similar.

[16] Some states, such as Tennessee, restrict to larger and more established private schools. (We thank Sean Corcoran for making this observation.) At this point, we have no way of clearly distinguishing eligible from ineligible schools, but it does not appear that many schools are excluded nationally.

Most of the new universal voucher programs also include what advocates call "education savings accounts" (ESAs), which allow families to use funds not only for private school tuition but also for other education services, such as tutoring, computers, and extracurricular activities. While the ESA name is a misnomer,[17] this means the new policies are more than just vouchers, what some call "neo-vouchers" or "super-vouchers." The ESA element could influence how private schools respond in their tuition pricing because an increase in tuition takes away other ways that households might use the same funds. However, we expect this effect to be small since the scope for using the funds aside from tuition is limited.

Figure 1 lists the states that have adopted what are commonly called "universal vouchers" and shows how student eligibility phased in over time. Each cell shows the percentage of students eligible for vouchers, by year. This is important for the methods that follow because we estimate voucher effects using both dichotomous and continuous measures of student eligibility share as our measure of treatment. While we only present eligibility by year for our dichotomously treated states, we collect this information for all states to establish which are appropriate controls and for use in the continuous treatment analyses. We include programs narrowly defined as vouchers, as well as ESAs, individual K-12 tax credits and deductions, refundable tax credits, and tax-credit scholarships. Regardless of program type, we are concerned with the award amount and the percentage of students in a state who are eligible. (See details in Appendix Table 1.)

We calculated the percentage of eligible students using data from EdChoice, a group that advocates on behalf of school vouchers. EdChoice has reported the percentage of students eligible for each type of voucher program in each state since at least 2020. We used the Wayback

[17] These education savings accounts usually create no mechanism for "savings" on the part of taxpayers/consumers.

Machine to take snapshots of their website in each year of the analysis. Next, we restricted the data collection to programs with voucher levels consistently above $2,000 per student. As noted above, we do not expect small voucher programs to have much effect on our tuition and enrollment outcomes or the market generally.

A small share of the EdChoice eligibility data appeared inaccurate or at least inconsistent with our intended use; the organization's reported eligibility rates seemed much higher than was plausible given the specified eligibility criteria. This happened exclusively with tax credit scholarship programs. For tax credit scholarships only, EdChoice reports the percentage of students who are *income eligible* for the program rather than eligible overall (i.e., some tax credit scholarships have generous income eligibility but low budget caps that effectively hamstring overall eligibility). For this reason, we exclude tax credit scholarship programs with low budget caps. A further complication is that most states with any voucher program have many of them, and the students eligible for one program are also potentially eligible for others, so we cannot just add up eligibility across programs. In our robustness checks, we therefore calculated student eligibility in several different ways.

One of our statewide eligibility calculations takes the midpoint between the eligibility summed across programs (capped at 100%) and the maximum eligibility across all the programs. This method implicitly assumes that about half the students eligible for one program are eligible for another as well. This yields a rough estimate of the eligibility share for at least medium-sized vouchers (>$2,000) for each state and year. The resulting eligibility shares are shown in Figure 1.

Our threshold for the dichotomous treatment, or what we call "universal eligibility," is based on when a voucher program reaches an income eligibility cutoff of 250 percent of the federal poverty line. In most states, this roughly corresponds to about half of students being eligible. In all states, the dichotomous switch from untreated to treated is accompanied by a significant expansion in program eligibility. The year at which this threshold is reached is shaded in gray. Importantly, the dichotomous treatment timing is based not on the EdChoice data, but our own investigation of the policies (the percentage of students living <250% of the poverty line is not shown in the figure). This approach aligns with the fact that all states that phased in their recent universal voucher/ESA programs did so based on this type of income threshold. Despite using different data sources (EdChoice percent eligible versus poverty thresholds) and different methods (dichotomous versus continuous treatment), the results turn out to be very similar.

We include 34 control states where no universal voucher program was passed before October 2024 (our latest round of data collection).[18] Some of these states do have smaller voucher programs targeted to specific populations of students, which stay constant over the study period. We also omit two states, Tennessee and Wisconsin, from the analysis (i.e., they are neither control nor treatment) because they had large-scale, though geographically constrained, voucher programs throughout the period of our analysis.[19] More details on all of the nation's voucher-like programs are in Appendix Table 1.[20]

This discussion highlights the difficulty of determining precisely how many students are eligible for vouchers by year, a key factor in establishing the policy treatment dosage. This suggests that we should implement methods relying on both dichotomous treatment definition

[18] Alabama, Arkansas, Louisiana, and Wyoming are excluded because these states adopted universal vouchers before we completed data collection, so their data might have been influenced by anticipation effects. Idaho and Texas passed universal vouchers after our last data collection; we include these two states as controls because it is less likely that their outcomes were affected by anticipation. The control states include Washington, DC, which has a well-known voucher program, but that program has very low enrollment and did not change meaningfully during the panel.

[19] Ohio also has a geographically constrained voucher in Cleveland, but take-up is much lower than the program in the comparably sized Milwaukee.

[20] Appendix Table 1 shows the policy changes for all states that have any type of voucher program. The table also describes the timing of the policies (if and when it became universal) as well as key components of the policy design and the size of the voucher in relation to the average per-pupil public school funding in the state.

and continuous treatment. The former allows for comparisons between states with "high dosage" (i.e., states with large voucher funding per student and near-universal eligibility) and "low dosage" states that have no program with either high funding levels or high eligibility rates in any year. A limitation of this dichotomous approach is the gradual increase in dosage in some treated states that gets ignored; this might affect the pre-trends and attenuate the estimated treatment effects toward zero. Using continuous treatment allows us to leverage all the variation, but with the limitation that it relies on smaller changes in dosage and that we can only capture dosage with considerable error, for the reasons noted.

IV. Data

The prior section discusses how we created the treatment variable. This section focuses on our data sources primarily for the dependent variables, as well as some limited covariates.

IV.A. Private School Review

Private School Review (PSR) is an advertising website where U.S. private schools can post school information aimed at prospective families. Each PSR webpage gives information about a particular private school (i.e., one PSR webpage for each school), including fields for enrollment, tuition, the percentage of students on financial aid, and average financial aid; however, not every school includes information for all fields. Additionally, many other fields are present for most schools (Table 1).

In November of 2023, we scraped every individual PSR webpage (~ 36,500), which gives us data for the 2023-24 academic year. We also scraped every individual PSR webpage (~ 37,500) in October of 2024, which gives us data for the 2024-25 academic year. To get historical data, we scraped previous iterations of individual PSR webpages using the Wayback Machine, which is an internet archive attempting to capture screenshots of the internet over time. The Wayback Machine stores webpages' code and attempts to take a new capture when that code changes, though this does not necessarily happen as soon as the change is made. Any change in any of the dozens of fields could trigger a new screenshot. Using PSR webpage URLs gathered directly from the PSR, we scraped all previous iterations of PSR webpages going back to 2015. Linking observations based on their PSR webpage URL allows us to create a panel of private school data spanning the 2015-16 to 2024-25 academic years.

Given the above description of the Wayback Machine, we assume, through the 2022-23 academic year, that data from a school's PSR webpage is valid up until the webpage changes. To avoid relying too heavily on this assumption, we restrict the analytic sample to frequent updaters (at least 3 PSR webpage updates 2015-16 through 2022-23 academic years).[21] From 2022-23 academic year forward, this assumption is no longer necessary because we captured the data directly from the website through scraping.[22]

To ensure that schools have an early enough baseline tuition and that their PSR updates cover the entire period of analysis, we define complete cases as having a non-missing tuition observation in or before the 2020-2021 school year. We omit earlier years to avoid reducing the number of complete cases that we rely on as the main sample for analysis (though results are not much affected if we do not restrict to complete cases tuition) to keep the estimating sample consistent. This process yields a sample of just under 16,000 frequently updating private schools,

[21] A potential problem that could arise with this method is that, to the extent that changes in the websites are driven by changes in tuition, this could select on the dependent variable. Specifically, we might expect that schools raising their tuitions would be more likely to change their websites in ways that trigger a website "update" in the Wayback Machine. However, since our results yield smaller effects from the PSR data, this does not seem to be a primary concern.

[22] It might appear that this creates a change in the method of data collection in the middle of the panel, but the data from the Wayback Machine are from the same websites, and we use balanced panels (i.e., no school enters the sample in 2022-23).

with nearly 5,000, or 19 percent of the nation's private schools, being complete cases for tuition.[23]

IV.B. Private School Survey (PSS)

The PSS is a survey administered every other year by the U.S. Department of Education. As it is a survey, and schools are not required to participate, not all schools respond. The response rate in most years is about 75 percent. We use a version of the PSS embedded within the National Longitudinal School Database (NLSD) (Carroll et al., 2023).

The PSS does not ask for tuition information, but it does ask for enrollment. This is a useful supplement to the PSR enrollment figures, given the issues noted above with schools updating their websites and the smaller sample size of the PSR. It is also helpful for imputing the tuition rate in the NCCS data, as discussed below.

IV.C. National Center for Charitable Statistics (NCCS)

The National Center for Charitable Statistics (NCCS), housed within the Urban Institute, includes tax information for non-profit organizations from Form 990 and Form 990EZ filers. Our sample covers filings from 2017 to 2022.

We identify which non-profit organizations are schools in three ways. First, the NCCS has included a flag for whether the organization is a school or college.[24] Second, the NCCS includes codes from the National Taxonomy of Exempt Entities (NTEE) codes that also indicate

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[23] These figures represent the number of unique schools. In other parts of the study (e.g., Table 3), we report the number of school-by-year observations. Also, we remove from the sample those schools that only serve pre-K grades as voucher programs only apply to K-12 schooling. Additionally, we manually cleaned approximately 2,500 tuition observations as they did not report a single tuition level and instead left a more complicated discussion in text notes on the PSR site. Where multiple tuition rates were reported, we took an average.

[24] This flag appears to be very incomplete when we examine the other two approaches. Also, note that the NCCS school indicator first started in 2012. The NCCS has been expanding this school indicator, so significant coverage only begins in 2019. We use the more recent flags, combined with the employer identification number to impute the school indicator going back to earlier periods.

school status.[25] Third, we match NCCS filers to the PSS based on exact organizational name and exact address/state/zip combinations.[26] We explain below how we use these indicators.

In our analysis, we start by examining effects on total revenue, but we are mainly interested in tuition rates, so we approximate this by dividing NCCS revenue by enrollment, the latter of which is only available in the PSS and PSR. This requires creating three samples of schools: all known NCCS K-12 schools, NCCS/PSS matches, and NCCS/PSR matches. The "all known NCCS K-12 schools" sample includes all NCCS/PSS matches (note that this includes both filers flagged as a school by the NCCS and not flagged as a school by the NCCS) and filers with the requisite NTEE codes. The NCCS/PSS sample, as the name implies, is a subset that only includes exact matches between the NCCS and PSS. A small share of schools in these initial samples are excluded for other reasons.[27] Again, we use these data to create balanced panels, so we restrict to filers with non-missing revenue observations over the period 2019-2021.[28]

Using these Form 990 filings to measure revenue from the academic year is also problematic because tax and academic years do not necessarily coincide. NCCS filings are aligned with tax years, which can be any 12-month period, not necessarily aligning with academic years. To deal with this, we impose assumptions about the schedule of tuition payments of private schools. In particular, we assume a "Pre-Pay" schedule where payments are

[25] The NTEE codes are: B20 (elementary and secondary schools), B24 (primary and elementary schools), and B25 (secondary and high schools)
[26] Complicating this, organizational names and addresses only exist for NCCS filings through 2019.
[27] We exclude a few hundred NCCS filers that matched to more than one PSS school across periods. While such matches could be legitimate, this complicates the calculation of school-specific revenue. We also exclude schools whose state changes over time. Finally, we exclude any filer whose NCCS or PSS ID changes over time (NCCS and PSS deal with organizational name/address changes differently).
[28] Years refer to fall academic years (e.g., 2019 is August 2019 to August 2020) unless specifically referencing tax years.

made uniformly April-August preceding the fall academic year. One reason is that this is the only method that allows us to have non-missing observations in the fall 2022 academic year.[29]

In some of the analysis that follows, we study total revenue from the NCCS, which, under certain assumptions, is the tuition rate (net price) multiplied by enrollment. However, we are trying to isolate voucher effects on these two variables separately. In the PSR (see above), we can do that directly, using the posted tuition rates. With the NCCS, we divide total revenue by enrollment using data from the PSS. Since the enrollment data are sometimes missing, we pursue two enrollment imputation strategies.[30]

One of the main sources of revenue for schools, beyond tuition, is donations. If vouchers have no effect on donations, which seems plausible, this would slightly attenuate our estimates, which are modeled as percentage changes in revenue (see above regarding the asinh transformation). Vouchers could also increase donations, given the income effect on parents (the primary beneficiaries of vouchers), or decrease donations, given the improved financial position of private schools, which might lead donors to see less of a need for their support. In the latter case, the decline in donations would offset any increase in tuition in the net revenue calculation. Whatever the direction of the effect, we expect its size to be small because the vast majority of private school revenue comes from tuition (Hillen, 2023).[31]

[29] In Appendix A, we provide additional details about alternative methods, which we label "Installment" and "Tax Year" methods. For both "Installment" and "Tax Year," including observations in 2022 would require filings from 2023 to cover the 2022 fall academic year as it spans August 2022 - May 2023 (installment assumes payments are made during the academic year and tax year assumes filings refer to the previous academic year).

[30] The first, carry forward ("CF"), refers to imputing enrollment forward, i.e., enrollment is assumed to be constant until the next non-missing observations. Second, we linearly extrapolate between fall academic years ("Lin.").

[31] We could only find information about independent schools, which tend to have higher tuition rates.

IV.D. Nonresponse and Frequency Weights

We are ultimately interested in the degree to which vouchers affect the national population of private schools. The representativeness of our sample is difficult to establish because there is no national census of private schools with which to compare any sample, and we have a limited number of variables even for the schools that do appear in the data. The best we can do is compare the largest sample, the 57 percent of schools in the PSR frequent updaters sample with enrollment data (the closest we have to a "population"), to the analytic sample. The average enrollment in this large sample is 214 students. This compares with an average enrollment of 290 students in the PSR analytic sample and 159-169 students in the NCCS analytic samples.[32] In other words, the PSR analytic sample schools appear larger than the population, and the NCCS analytic sample is smaller than the population.

These discrepancies between the samples and population do not necessarily mean our estimated voucher effects are not representative, but that is a possible concern. To address this, we created and applied nonresponse weights as a robustness check (these are not used in the main analyses). We created weights focusing on the most widely available and important school characteristic: enrollment size. In short, we: (1) placed all PSR schools with enrollment data in 4-5 bins based on their enrollments[33]; (2) counted the number of schools in each bin with non-missing enrollment, by state; (3) counted the number of schools from step (2) *in the analytic sample* (i.e., with tuition information available), by state; (4) divided the number from the second

[32] Appendix Table 6 provides more detailed comparisons of the two NCCS samples, one of which uses enrollment from the PSS and the other of which the PSR enrollment. The means and standard deviations for enrollment are very similar for these matched PSS and PSR schools (Appendix Table 6).
[33] In the 5-bin version, we used 0-50, 51-100, 101-200, 201-300, and >300 students. In the 4-bin version, we used 0-100, 101-200, 201-300, and >300. This creates a maximum of 225 (180) state-by-enrollment with 5 (4) bins. The larger the number of bins, the greater the chance that the weights are slightly off because some cells in some states will be empty. On the other hand, larger bins also mean grouping more schools that are unlike one another. We end up with only six (four) empty cells with 5 (4) bins. These bins refer to 2021 enrollment.

step by the number from the third step to obtain non-response weights. In effect, when we apply these non-response weights, we attach a higher weight to (non-missing) school observations in the analytic sample, in proportion to the number of missing schools in each bin. As with many other parts of our analysis, the non-response weights have a limited influence on our results; therefore, we show them only in the appendices.

Our estimates are also designed to treat each school equally, i.e., the estimated voucher effects are the effects on the average *school*. However, we might also be interested in how this affects the average private *household*. This requires the use of frequency weights, also based on school enrollment.

In short, our main estimates use no weights, and we employ the above non-response and frequency weights to provide additional information about voucher effects.

IV.E. Descriptive Statistics: Enrollment and Tuition

Our main outcomes are enrollment and posted tuition, as well as the financial aid that schools offer. As in higher education, government subsidies and financial aid create a discrepancy between the sticker price and the actual net price. Schools might, for example, reduce their aid levels, knowing that the voucher now makes the school much more accessible to everyone. Or schools might change their pricing strategies entirely and lean more heavily into price differentiation through varied financial aid packages.

Figure 2 shows histograms of enrollment and tuition from the PSR analytic sample (we provide histograms for the entire PSR population in the appendix), which are heavily right-skewed. To our knowledge, this information has never been available before. The fact that the modal school has 30 students and tuition of roughly $5,000 might be surprising. The lack of data on private schools, which the current study is intended to address, may mean there is some misunderstanding about the sector as a whole, including the size, design, and location of private schools.

Table 1 shows the descriptive statistics for the PSR analytic sample of private schools.[34] Table 2 shows the differences in means by treated (universal voucher) and non-treated states. The treatment group has lower tuition, lower aid levels, fewer teachers with advanced degrees, and offers fewer extracurricular activities, but more sports.

V. Econometric Framework

V.A. Difference-in-Differences

The purpose of our analysis is to identify the causal effect of the introduction of school vouchers on private school tuition, enrollment, and financial aid. We compare these outcomes in states that introduced universal vouchers during the 2021-2024 period to states that never had a substantial voucher program during this period. We estimate this effect following an event study difference-in-differences (DD) specification that allows for time-varying treatment effects. The two-way fixed effects (TWFE) version is represented by:

$$y_{pst} = \sum_{\tau=-q}^{-2} \theta_\tau \mathrm{D}_{s\tau} + \sum_{\tau=0}^{m} \theta_\tau D_{s\tau} + \beta \mathrm{X}_{pst} + \upsilon_t + \gamma_p + \varepsilon_{st} \qquad (4)$$

where y_{pst} represents the school enrollment, tuition, and other variables of private school p in state *s* at time *t*. The terms υ_t and γ_p, are time and school fixed effects, respectively, so we are comparing each private school to itself over time. (While the policy is at the state level, we cannot also include state fixed effects because schools do not move across states.) The variable $D_{s\tau}$ takes on a value of 1 if the observation's period t is τ years away from the universal voucher

[34] The tuition numbers are in nominal dollars throughout the paper, in part because inflation cancels out in the DD-style estimation.

introduction in state s. We are interested in θ_τ, which represents lead and lag effects of adoption up to q years before the initial adoption year and m years after. Our omitted period is $\tau = -1$, representing the year before the expansion.

We generally estimate without controls (X_{pst}) because of a high degree of missing data (see the bottom panel of Table 1), which would substantially change the samples. ε_{st} is an error term that accommodates clustering at the state level. Also, we do not apply either non-response or frequency weights in the main analyses.

Since states adopted universal voucher policies in different years, this is a staggered DD empirical strategy. Therefore, we use the modified version of TWFE DD developed by Callaway and Sant'Anna (2021) (CSDID). CSDID is one of the most flexible staggered DD estimators, which we pair with state-by-state TWFE estimations.[35]

For the continuous treatment method, we use the methods proposed by Callaway and Sant'Anna (CS, 2025) and Chaisemartin, D'Haultfoeuille, and Vasquz-Bare (CHV, 2022) as well as a continuous TWFE DD outlined in equation (5). This equation is quite similar to (4) except that we replace the dichotomous treatment indicator $D_{s\tau}$ with the continuous measure of voucher eligibility VE_{st}, which is on a 0-1 scale so that the parameter estimates can be more directly compared with the dichotomous DD. These new methods cannot be easily shown in an equation. Instead, to highlight the main distinction, in the treatment variable, we show a modified version of equation (4).

$$y_{pst} = \vartheta VE_{st} + \beta X_{pst} + \upsilon_t + \gamma_s + \varepsilon_{st} \tag{5}$$

[35] As in other parts of the analysis, we only use states as controls that are not treated at any point during the panel period. In the CSDID setting, this is discussed and recommended by Baker et al. (2022).

We mainly report the results from the dichotomous version (equation 4). Neither the dichotomous nor continuous method has a clear advantage over the other in this context (see Data section). Also, the results turn out to be quite similar. Finally, we only apply the continuous DD method to the PSR data because the NCCS analysis includes only the 2021-22 treated cohort, so there is far less variation in treatment dose than in the PSR analysis.

The tuition and enrollment data are both highly right-skewed. This opens up the possibility that small changes in outliers could have a disproportionate impact on the results. Therefore, for all main results, we re-estimate using the inverse hyperbolic sine (asinh) of the dependent variable; the interpretation is the same as the natural log but is defined at zero, which is an issue for the financial aid variables. Since the asinh is scale-dependent, we also re-estimate with Poisson regression as a robustness check.

V.B. Threats to Identification

Our main identifying assumption is that there are no unobserved shocks affecting the market outcomes that are also correlated with the timing and location of voucher adoption, i.e., the two sets of states would have followed the same trends in the absence of the adoption of universal vouchers.

COVID introduces a potential violation of this assumption as it generated large spikes in tuition across the country in the years just prior to universal voucher adoption. While the pandemic affected all states, it affected different states in different ways that are difficult to model. As we will show, the TWFE estimates generally do not pass parallel pre-trend tests, though, visually, the effects appear larger than the pre-trend differentials, which gives us some confidence in the treatment effect estimates. The pre-trend assumption also appears more plausible in the CSDID.

In addition, we pursue a synthetic difference-in-differences (SDID) empirical strategy following Arkhangelsky et al. (2021) and Clarke et al. (2024). SDID is a strategy similar to synthetic control, and it creates a weighted average of the comparison observations that matches the treated pre-trend, within a DD framework. SDID also permits estimation with staggered treatment adoption, but note that it is a different process than with CSDID. We provide SDID estimates and event studies for all main outcomes in the appendix.

It is also possible that other education policies might have changed at the same time vouchers were adopted. The most obvious example is, again, related to COVID. The decision in some states and districts to close traditional public and charter schools during COVID may have affected the relative attractiveness of, and enrollment in, schools in each sector. Another, non-COVID possibility arises with the murder of George Floyd in May of 2020. This spurred protests and the emergence of the Black Lives Matter movement, which also affected school operations and instruction in some states more than others. To address this possibility, we re-estimate using only other politically "red" states as controls.

VI. Results

VI.A. State-by-State Simple Trends (from PSR)

We started by graphing the simple trends in outcomes for the country as a whole and in each state. Figure 3(a) shows that private school enrollments have been largely flat before and after COVID. However, private school tuition, shown in Figure 3(b), has increased substantially. The first break in the trend coincided with the fall of 2020 and the first full semester under the COVID pandemic. Tuition rates likely increased because of the higher costs of offering education under these conditions, combined with increased demand. We see another slight upward shift in the slope in 2023-24, which, in theory, could reflect the introduction of vouchers in some states. However, this could also simply reflect ongoing national inflation that affected all goods and services to varying degrees. Looking at panel (d), which separates the trends by treatment status, it appears that the tuition increases are similar in voucher and non-voucher states. In percentage terms, tuition increased slightly more in the treated states (24 percent) than in the control states (18 percent).

These figures also reinforce that the control and treatment states were fairly different at baseline. In addition to the baseline tuition levels, the initial treated states are more rural and have lower incomes and costs of living. (See also Table 2.) These figures also seem to suggest some degree of parallel trends, though the formal tests we report later reject parallel trends, partially because our tests are at the school level and cover the entire country, so this is a powerful test, and it is easy to reject the null.

To visualize how these effects varied by state, we report in Appendix Figure 1(a)-1(e) descriptive trends that show the change in tuition and enrollment and the changes in the timing of changes in voucher policies, for each state separately.

Every state shows the same basic pattern of flat enrollment trends and increasing tuition. notably, even control states. The tuition increases are sharpest between the start of COVID and just before voucher adoption. In some cases, the onset of vouchers is associated with an increase in the slope of the tuition trend.[36] Others show a decrease in the tuition slope, while the remainder are smooth through the policy adoption. This reinforces the earlier national figures

[36] One advantage of the Fontana and Jennings (2023) paper is that they can leverage tuition data by grade and the fact that the Iowa policy treated different grades differently. This avoids the COVID problem. On the other hand, it really amounts to answering a different question, about price differentiation within schools.

suggesting that the voucher effects on both enrollment and tuition have probably been small so far. We test this more formally in what follows.

VI.B. Average Treatment on Treated (ATT) Effects

In each section below, we focus mainly on the ATTs from the dichotomous DD as these seem somewhat easier to interpret. As we will show, the shift to continuous treatment DD yields qualitatively similar results. Since the data sources are so different, we also organize the results by source, reporting the results first for tuition and enrollment for the PSR, including robustness checks, and then doing the same for the NCCS. The main robustness checks include: (a) re-estimating without the 2020-21 school year to reduce the influence of COVID; (b) re-estimating with SDID because of issues with non-parallel pre-trends in the simple DD; (c) re-estimating using the continuous treatment DD to use all the potential policy variation and avoid policy phase-in influence on the pre-trends; (d) re-estimating with Poisson regression to address the scale dependence of the asinh transformation; (d) re-estimating with Winsorizing and other sample restrictions[37] to address possible errors in the data and sensitivity to outliers; and (e) re-estimating adding frequency and non-response weights.

VI.B.1. ATTs on Tuition and Pricing (PSR Data)

We begin in Table 3 by reporting the national average treatment effects from the CSDID and state-specific effects from the DD using the PSR data. The latter results help to show whether the responses are robust across states and whether the average effect across states is driven by outliers. We also restrict to schools that exist in the data in all years and that frequently

[37] We restrict to observations whose dependent variables are in a reasonable range, i.e., no zero values for tuition, revenue, or other outcomes and no changes of more than 50% (in absolute value) in a single year. Winsorizing is applied as a robustness check after these restrictions are applied.

update their websites (what we call, frequent updaters). For all the PSR estimates, we use data from (fall) 2020-2024, except where we drop 2020 to address the influence of COVID.

For the country as a whole, the results show no effect on tuition. The point estimate in the top row is almost exactly zero, and the standard error rules out effects larger than +2.1 percent.[38] To see how families experienced this, we re-estimated with frequency weights and found a similar effect. The results are very similar when dropping 2020-21, the first COVID year (second set of rows in Table 3), and Winsorizing the data (in the appendix).

Our estimates sometimes do not pass a parallel trends test (see test results in the bottom two rows of the national estimates in Table 3). This is likely because: (a) the test is very powerful with these data; (b) control and treatment states have different baseline levels that could be related to their longer-term trends; and (c) COVID affected private schools—especially their tuition—in all the states, but perhaps not to the same degree. Visually, the COVID effect is much larger than the voucher effect and this throws off the pre-trends.

Another way to investigate the sensitivity of estimates with respect to pre-trend violations is to follow Rambuchan and Roth (2022) in constructing 95 percent confidence intervals for estimates depending on the allowable level of post-treatment violation in parallel trends. For instance, we could construct a confidence interval for the tuition estimate, imposing that the violation in parallel trends after treatment is no more than twice the largest violation before treatment (M=2). By comparison, the existing estimates assume that M=0.

Comparing multiple levels of allowable post-treatment violation in parallel trends allows us to determine the highest level of post-treatment parallel trends violation (as a proportion of the largest pre-treatment violation) that the results are robust to. For the 2020+ CSDID results, the

[38] We use the baseline treatment group means because of the substantial baseline difference mean levels.

null tuition result is robust to all tested allowable violations (M=0.5 to M=2) while the other three significant results are robust only up to M=0.5. The 2021+ CSDID results, which give about the same point estimates as 2020+, also yield a robust null for the effect on tuition, but the remaining three significant results are now robust up to M=0.75.

To further address the pre-trends issue, we also estimated synthetic differences-in-differences (SDID), a new method that marries the estimation of DD with synthetic control groups (Arkhangelsky et al., 2019; Pailanir et al., 2023). (This method requires aggregation to the level of treatment, i.e., the state, and the estimates are therefore much less precise.) These estimates, shown in the appendix, also show very similar results to the main Table 3 but with a smaller enrollment coefficient. The national ATT for tuition is, again, almost exactly zero.

To address the possibility that COVID responses might be creating the pre-trend issues, we re-estimated using only states that voted for President Trump in 2016 and 2020.[39] We obtain similar results even when using methods where the parallel trend assumption seems more plausible. We also re-estimated the same model as in Table 3, but applying the non-response weights discussed in the Data section. The point estimates are slightly larger (more positive) and the pre-trend concerns are less significant.

Overall, the results are highly robust to a wide variety of data and methodological variations and assumptions. While we cannot rule out violations of the identifying assumptions, most of the steps we take suggest that the bias is modest and does not alter our overall conclusions.

[39] Restricting to control states that voted for Trump in 2016 and in 2020 (AK, ID, KS, KY, MS, MO, MT, NE, ND, SD, and TX) does not substantively change any national estimates.

Table 3 also shows variation across states. These range from a 3.4 percent tuition reduction in Florida to a 4.7 percent increase in Arizona.[40] We might have expected the effects to be larger in the early-adopting states, where there have been more post-treatment years during which tuition and enrollment could change. However, the number of post-treatment periods does not appear to be a significant factor. Winsorizing reduces this range to between -3 percent in Oklahoma and +2.3 percent in South Carolina. (The Arizona and Florida results are closer to zero in that case.) Also, note that while we find negative tuition effects in some states, this is in comparison to the increasing trend in tuition in control states. All states have experienced a tuition increase over the period.

Also, recall that some states phased in vouchers, yet we assign treatment discretely at the point it becomes universal. Table 4 reports the results from the continuous treatment DD (still using the PSR data), using the four methods for calculating the student eligibility share (see section IV and table notes). For tuition, all of the estimates remain insignificant, and almost all the point estimates are less than one percentage point (in absolute value).

Tuition is only one potential part of private schools' pricing strategy. As the higher education sector shows clearly, there can be large differences between the sticker price and net price. While there is no large federal program of loans and grants for private K-12 schools, 33 percent of private school students receive some form of aid (in the analytic sample) and the average aid, among those who received any, is \$5,630 - \$7,790 per student (see Table 2). This itself is an important finding, given the paucity of prior evidence on private school pricing behavior and the role of financial aid.

[40] Since the estimation differs between the national and state-by-state estimates, we also re-estimated the national estimates using the TWFE DD and found results similar to the CSDID.

The right-hand columns of Tables 3 and 4 report the effects on the percentage of students receiving aid and the average aid (among those receiving any). Table 3 reports slight positive effects on both outcomes. We also see that pattern in the TWFE and CSDID results in Table 4, though the estimates are smaller/negative with the first CHV method for both of these outcomes.

VI.B.2. Effects on Enrollment (PSR)

In this study, we are interested in understanding the fiscal implications of universal voucher programs for households, states, and school districts. Pricing is one part of that. Enrollment is the other.

The results in Table 3 suggest that universal vouchers have increased the average enrollment by 3.5 percent. This seems small given the quite substantial decline in net price experienced by households, though this is predictable based on the theory in section II. Specifically, we expect enrollment to change slowly due to capacity constraints, admission requirements, and the fact that the vast majority of students are already in a school, and there are strong frictions to changing schools.

In the robustness checks, with continuous DD, the estimates for enrollment are very similar to the dichotomous DD, ranging from +0.024 to +0.045 (see Table 4). The midpoint of this range, +0.035, is almost exactly what we reported in Table 3. Half of these estimates are precisely estimated. The biggest change is that Winsorizing reduces the effect on student enrollment drops from +3.5 to +2.1 percent.

More surprising is that we see small *reductions* in private school enrollment in Utah and West Virginia, though the effect is only significant in West Virginia. This does not appear to be due to a problem with parallel trends. Looking at the state-by-state descriptive trends in Appendix Figures 1(c) and 1(d), we can see small declines in enrollment in these states—and not in the other states. This also does not appear to be a bounce-back from COVID because the new, post-voucher enrollment levels are actually below pre-COVID levels. One possible explanation is that some other shock occurred in these states that offset the voucher effect. For example, West Virginia is one of the only voucher/ESA states that has seen substantial declines in its school-age population in recent years, though it is unclear whether such shocks coincided with voucher adoption.

Overall, based on these PSR results, we see no effect of universal vouchers on tuition and only a small increase in enrollment.

VI.B.3. ATTs on School Revenue (NCCS Data)

Table 5 shows the DD estimates on total revenue from the NCCS, allocating the funding under the “Pre-Pay” assumption discussed in Section III for the years 2019-2021. Because these data are not as recent, the results only include Indiana and New Hampshire as treated states, which introduced vouchers in the same year (i.e., it is not a staggered design). Given the issues with parallel trends noted above and the fact that COVID spans a larger proportion of this estimation, we also report the SDID alongside the DD.

The first two columns of Table 5 focus on total revenue, but the results are sensitive to the use of DD and SDID. The former shows effects in the range of 8-17 percent (one of the three is precise), while the SDID estimates become close to zero. (Note that the results were more robust when we switched from the DD to SDID with the PSR data above, possibly because COVID played a larger part in NCCS estimation.)

The last four columns of Table 5 focus on per-pupil revenue, which is meant to approximate the PSR tuition figures in Table 3. We divide total revenue for each school by the enrollment reported in the PSS. Since this requires non-missing enrollment data, the estimates involve a smaller sample than in the first row (NCCS/PSS matches and NCCS/PSR matches). Here, we see a more consistent picture with tuition increasing by 5-10 percent.[41] In Table 3, with the PSR tuition, we saw essentially zero effect in these two states. However, again, recall the change in the sample of schools within these states. The NCCS analytic sample includes smaller schools disproportionately while the PSR analytic sample includes many larger schools.

If total revenue is tuition multiplied by enrollment, then the above estimates point to a consistent and fairly narrow range of possibilities. A revenue increase of 10 percent, combined with an enrollment effect of +3.5 percent, implies a tuition effect of about 6 percent. This is larger than what we saw in Table 3 with the PSR, where there was essentially no effect on tuition. The coefficients on revenue and per-student revenue are smaller when we Winsorize and make other sample restrictions, but larger when we use Poisson regression.

We identify two potential reasons for the discrepancies between the PSR and NCCS. The first is data quality. It seems reasonable to assume that the NCCS revenue data have limited error; these are accounting data reported to the IRS, so errors could be costly. On the other hand, the NCCS data require assumptions about the assignment of tax year funding to academic years and may involve non-tuition revenue that is of less interest, so there are data quality issues in both sources. Also, Fontana and Jennings (2023) found numerous errors in the PSR data in their Iowa analysis when they compared the PSR with data from websites and other more direct sources. The direction of these biases is unclear.

Second, even if the data are accurate, the results might be different because the sample of schools is different between the two data sets. As noted, the NCCS (PSR) samples have lower

[41] Only one estimate is out of this range. See the far-right middle row, which shows a near-zero effect.

(higher) school enrollments than the largest sample we could identify. Also, note that our theory suggests that school size is a key factor differentiating school responses to vouchers.

On this point, it is important to underscore that the PSR is an *advertising* website meant to attract families and increase enrollment. Schools that list themselves on the PSR may be revealing that they have excess enrollment capacity. This may be especially pronounced in schools that fill their PSR webpage with advertising information (webpages that include tuition often include other low-coverage variables, such as financial aid offerings) and those that value keeping their advertising information up to date (frequent updaters). By using the PSR analytic sample, we may be using a sample of private schools that are particularly interested in and capable of increasing their enrollment at low or zero cost. As discussed in the theory section, this would dampen the effects on tuition. There is some evidence of this as the PSR analytic sample enrollment estimate is 3.5 percent (Table 3) and the PSR whole enrollment sample (does not restrict to complete cases tuition) enrollment estimate is 2.1 percent (Appendix Table 5). The PSR sample, therefore, appears to be a slightly selected sub-sample.

The analysis that follows informs our understanding of these issues, clarifies the reasons for this variance, and narrows the reasonable range further.

VI.B.4. Comparison with Fontana and Jennings

Fontana and Jennings (2023) also studied the effects of universal vouchers on tuition, focusing on Iowa and comparing with Nebraska. They found a 10 percent increase in tuition in their analysis. Our national estimate is lower, near zero, but we also estimated the effects differently: (a) they used only one treatment state and one comparison state; (b) they were able to collect the data manually and likely with greater accuracy; (c) they have a different sample of schools within each state; (d) we have one additional year of data comparing Iowa to all the non-voucher states; and (e) their observations are at the school-grade level.

To make more direct comparisons, in Table 6, we re-estimated using PSR data only through 2023 and restricting only to Iowa and Nebraska, as they did. In this specification, the estimate turns negative; the coefficients are in the range of -1.3 to -1.9 percent. However, including 2024 results in estimates ranging from +1.4 to +2.7 percent. One possible reason the 2024 results may more closely align with Fontana and Jennings (2023) is that the PSR data, as opposed to their manually collected data, may show tuition changes with a one-year lag. We also explored whether the choice of Nebraska as a comparison state was important, but this does not seem to be the case.

The remaining discrepancy is that the PSR and their manually collected data yield different samples of schools and different tuition responses. Our sample includes 37/180 Iowa schools and 22/233 Nebraska schools. The Fontana and Jennings (2023) paper appears to include slightly more schools—106 in total—across the two states (denominator: 413 schools). There is no way to know how well these lists overlap with each other or compare with the population.

Perhaps the most important issue, for purposes of our national analysis, is how Iowa generalizes to other states. Iowa and Nebraska are unusual states with very low baseline tuition rates (~$6,000 average tuition for Iowa and ~$5,000 for Nebraska). These are less than half the national average and well below other voucher states.[42] Also, as noted in the theory section, we strongly expect larger tuition increases in states like Iowa that have low tuition—schools have almost nothing to lose by raising tuition to the level of the voucher. The Iowa voucher, at $7,400, is well above the state's actual tuition average—more so than any other state. This is important

[42] This can be seen in Table 3. The simple state average tuition rates in the 11 voucher states is $10,328.

because the elasticity of demand for schools between $6,000 and $7,400 after the voucher policy is likely close to zero. Our effect heterogeneity analysis, explained further below, reinforces that low-baseline-tuition schools increased their tuition more. We also find that smaller schools increased their tuition more, even controlling for tuition, and Iowa average school size is a third lower than the national average. For this reason, we expect a larger tuition effect in Iowa compared with the larger group of voucher states.

VI.B.5. Summary of Effects

The full range of estimates in the above analysis suggest voucher effects on tuition in the range of 0-13 percent. We initially reported a simple tuition increase that was 6 percent higher in the universal voucher states. Our dichotomous and continuous treatment DD estimates with the PSR data, in contrast, suggest a precise null effect, including negative tuition effects in a handful of states. This is likely because of sample differences and pre-trend issues. The PSR analytic sample of schools may be selected in ways that predict lower tuition effects. Jennings and Fontana (2023) estimate an effect of 10 percent, but only for Iowa, which has very low tuition relative to the voucher amount. This suggests that our PSR estimates are biased downward.

In contrast, our SDID estimates from the NCCS data suggest effects typically around 5 percent, while the DD estimates on per-pupil revenue with the same data are in the 6-10 percent range. The estimates are somewhat larger, up to 13 percent, when we use total revenue.[43] But the NCCS analysis focuses on smaller schools where we expect larger effects.

Based on all this evidence, we conclude that universal vouchers have probably increased tuition by the narrower range of 5-10 percent. This range excludes the null tuition effects from the PSR because we view those as underestimates, based on our direct comparison with Fontana

[43] Above, we reported a 17 percent increase in revenue. Here, we reduce that by the aggregate change in enrollment (3.5 percent) to arrive at 13 percent.

and Jenning's (2023) Iowa results. Likewise, their 10 percent tuition increase in Iowa is likely at the upper end of the realistic national range, given that this state's schools have lower-than-average baseline tuition rates (again, see the theory in Section II). Given all of this, the 13 percent estimate from the NCCS is also likely biased upwards.

We also emphasize that COVID is a likely contributor to the pre-trend issues and changes the interpretation of the estimates. The effects likely would have been larger if universal vouchers had been adopted outside of the pandemic, which led schools to raise tuition substantially before vouchers were considered.

VI.B.6. Incidence

Who benefits most from the program? Given the small changes in enrollment, it is fairly obvious that voucher/ESA funds primarily benefit families whose children already attended, or would have attended, private schools. Other evidence reinforces this (Klinenberg et al., 2024).[44]

We can get more specific about who benefits. A simple interpretation of the 5-10 percent increase in tuition is that 90-95 percent of the rents are going to households. This is arguably a lower bound because, to the extent that "money matters" in producing schooling outcomes, the extra spending by schools should benefit households, too.

We are also interested in incidence by household type. Data provided by Murnane et al. (2018), as of 2014, suggest that the following shares of households send their children to private schools: 0.57 (high-income), 0.27 (middle-income), and 0.15 (low-income).[45] Voucher incidence

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[44] We cannot directly observe whether the students attending private school before vouchers were the same as those who attended afterwards with these data, but, given the strong frictions involved in switching schools (see next section), and the sharp reduction in price from vouchers encouraging past private school enrollees to continue, the likelihood is very high that the post-treatment private school households (voucher recipients) heavily overlaps the counterfactual private household population.

[45] Klinenberg et al. (2024) show that, in the recent voucher program in Arizona, the share of people in the highest-decile neighborhoods using vouchers is more than three times higher than the share in the lowest-decile neighborhoods.

by income likely deviates from these shares because: (a) some state programs still have a small degree of income targeting; (b) the vouchers are ultimately paid from taxes that fall more on middle- and high-income households; (c) low-income households are more likely to attend the low-tuition schools that were more likely to raise tuition and shift rents to schools; and (d) higher-income households might be more aware of the policy and able to manage the administrative burdens involved. Points (a) and (b) shift the benefits/incidence toward low/middle-income households, while (c) and (d) shift the incidence toward higher-income households. We also note that the small degree of income-targeting is almost certain to fade over time, as the clear intent is to make these programs completely universal. The incidence is also affected by the time horizon of the analysis, which we discuss further below.

VI.B.7. Discussion: Short, Medium, and Long Run

In Section II, we posed theories pertaining to the potential long-run effects of vouchers. However, our analysis includes data only from the short run, in which there are numerous market frictions. This could explain why, despite a 50-80 percent decline in price, we only see a 5-10 percent increase in tuition and a 3-4 percent increase in enrollment.

To better understand this, it is helpful to define three timeframes: (a) the *short run* when everything but tuition and enrollment are fixed; (b) the *medium run* when household and school locations, curricula and other elements of school design are flexible but everything else is fixed (e.g., no new school entrants); and (c) the *long run* when all the costs can vary and all decisions can change. We expect the short-run effects to be smaller than the long-run results for seven reasons:

1. *It takes time for families to become aware of new voucher programs and the new options they make available*. Most parents will likely become aware quickly that they might be

eligible for a voucher/ESA, but it will take time for them to clarify eligibility, learn how the program works, and explore newly available schooling options. Knowledge about schools spreads largely from word of mouth, and many public school parents likely know few families who have sent their children to private schools.

2. *Families with children currently in school will likely hesitate to make changes midstream*. Many families make their housing choices to gain access to particular public schools. For this and other reasons, a substantial majority of families in public and charter schools are already happy with their schools (Education Next, 2022). Also, families do not wish to switch schools, except at transition grades, because this upends child and parent social relationships and has negative effects on student achievement. We therefore expect larger effects at kindergarten and transition grades.
3. *Especially with peer effects, some families will wait to move until after other families have switched schools*. As others have observed in the context of the racial segregation literature, if parents prefer schools with certain kinds of students, then, when one student switches to a private school, this increases the chances that others who prefer that type of student will follow suit and switch to a private school, too. However, this takes time as parents must observe families moving and the impact on the schooling environment before making their own moves. In other words, we can expect a slow cascading effect.
4. *Many families still cannot afford private schools even with vouchers.* Recall that the recent voucher programs allow private schools to "top up" tuition, i.e., to charge more than the voucher amount. This is similar to the point we made earlier about the role of price and household income in shaping the elasticity of demand. When the price is too high relative to income, the utility cost may be too great to justify the expense.

5. *Parents and schools are subject to inertia.* It is human nature to continue doing things as they have done before. For example, parents have ingrained norms and expectations that incline them toward certain schooling sectors. Many adults who went to public schools think they will send their children to public schools, too—and so on for other school sectors. These norms and expectations are likely stronger than typical consumer product decisions because of the central role that schooling plays in children's (and parents') lives and social development. This means that even future parents and students might change their decisions slowly. Likewise, many private schools first opened decades, or even centuries, ago and are likely to continue their older ways.
6. *To the extent that parents are willing to alter their schooling plans, changes in the supply of private schools will have to happen first and may also be slow to arise*. This is because: (a) elite private schools are often the ones in highest demand, but are at capacity and have no room to add more students; (b) elite private schools want to maintain their elite status by selecting their preferred students (DeAngelis et al., 2019) and have little incentive to add students; and (c) the broader set of private schools was designed to serve a certain, tuition-paying population, so schools will have to adjust—their curricula, marketing, building capacity, and so on—to attract and accommodate more families.
7. *Private schools might be reluctant to change given uncertainty about the future of voucher programs*. These new voucher programs have been controversial in many states. They are also expensive and strain government budgets in ways that call into question their sustainability. Private schools might be reluctant to invest in physical space, hire more teachers, or increase salaries if they think the program is temporary. This is part of the larger credible commitment problem that faces all government programs. Parents, too,

might wait to participate until they are sure the funds will be available throughout their children's schooling lives.

We argue that there are all plausible explanations for the relatively modest size change in tuition and enrollment in these first few years.

These differences across the short/medium/long-run effects also affect incidence. In the long run, we expect more marginal households to shift into the private schools, and these are likely to be disproportionately in the low/middle-income categories, relative to the pre-voucher overall shares (see prior section). This depends somewhat on future school pricing strategies, especially as the top-up provision may encourage larger tuition increases that keep low-income households in public and charter schools. Still, while the short-term benefits largely accrue to high-income households, we expect this will shift somewhat in the other direction over time.

VI.C. Effect Heterogeneity

In section II, we discussed theory and explained, among other things, why we expect the effects to vary across schools. To test these hypotheses, Table 7 reports interaction terms from the DD method and various (pre-treatment) covariates that we theorized might be related to how private schools respond to vouchers.

These results suggest that private school enrollment gains are concentrated mainly in schools with low baseline enrollment, and in Protestant and non-Christian religious schools (Jewish, Islamic, unspecified, and so on)—loosely speaking in the "church basement" schools. Additionally, newer schools that may have more excess capacity or are more willing to add students saw enrollment gains.

In contrast, vouchers have increased tuition primarily in *non-religious* schools and those that had low-enrollment/low-tuition at baseline. This is consistent with the theory that religious schools are more interested in, and succeeding in, using vouchers to “expand their flock” of followers.

VII. Conclusion

This study makes several contributions to the literature. First, we produce a more complete descriptive picture of the private school sector. The federal Private School Survey (PSS) includes only data on those who respond and does not include tuition information. Our data, which combine the PSS, PSR, and NCCS provide the first comprehensive dataset on this topic, which allows us to provide a clear portrait of the private school sector in the United States.

Even before COVID and vouchers, the private school universe was quite diverse. It may be surprising just how many very small, inexpensive schools exist. The “long tail” of the distribution also highlights the chasm between small schools spending only a few thousand dollars per pupil and boarding schools that charge upwards of $150,000 per year. The modal private school has 30 students and charges about $5,000 per year.

In mostly predictable ways, the effects of universal school vouchers have been modest so far. We find that universal school vouchers have increased tuition in voucher states by 5-10 percent and increased enrollment by 3-4 percent so far. Regardless, so far, the lion’s share of universal voucher awards seems to go towards families with students already in private school, likely making the tax and subsidy system less progressive.

Universal vouchers promise to substantially reshape this education landscape over time, including shifts in enrollment from public to private schools. The impediments to change, noted in the theory section, apply only in the short run. Vouchers dramatically reduce the net price for most parents by 50-80 percent. Moreover, with minimal regulation, it will become difficult for traditional public and charter schools to compete. Private schools are allowed to select the students they prefer and teach religion, neither of which is allowed in public schools. This will also give incentives to charter schools to convert to private schools. If they can receive the same funds, then they can operate more freely without regulation and government accountability.

Our work is also related to other works in progress using similar data. For example, in ongoing work, we are examining the effects of vouchers on: (a) enrollment in traditional public and charter schools; (b) the quality of TPS and charter schools that are induced to exit and/or are located nearby the private schools that are accepting vouchers; (c) the investments TPS are making in their physical capital; and (d) the entry and exit of schools across all sectors. In addition, we are engaging in qualitative analysis using interviews with leaders across sectors in voucher states to understand how vouchers are shaping decisions across sectors, in ways that will not show in quantitative data for many years.

Given that the effects have been small in the short run, our work also offers a basis for continuing to track voucher effects in the years and decades to come. This will be important to both the states that have already adopted vouchers and to those that are considering it. Our ongoing work will be informative about where these new markets are headed in the years ahead. More generally, these measures provide a window into understanding how this new, vastly different type of schooling market is likely to function.

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Table 1: Descriptive Statistics For PSR Analytic Sample (Without Imputation)

	Mean/SD	Min	Max	N
Dependent Variables				
Total Students	294.55	3	6,319	19,827
	(306.88)			
Yearly Tuition	13,400.65	0	166,500	19,013
	(14,380.24)			
Avg. Financial Aid	7,369.66	100	61,450	6,628
	(9,891.04)			
% on Financial Aid	32.90	0	100	8,475
	(21.71)			
Other Variables				
Total Teachers	29.60	1	610	20,009
	(32.93)			
Student:Teacher	12.03	0.21	668	19,644
	(13.37)			
Avg. Class Size	15.74	1	140	17,304
	(5.97)			
% of Teachers with Adv. Degrees	55.86	0	100	10,657
	(26.44)			
% Students of Color	32.61	0	100	18,218
	(27.64)			
Has High School	0.48	0	1	23,645
	(0.50)			
Has Middle School	0.72	0	1	23,645
	(0.45)			
Has Elementary School	0.72	0	1	23,645
	(0.45)			
Year Founded	1,961.98	1645	2020	18,678
	(42.14)			
Total Extracurriculars	10.99	1	104	12,977
	(10.69)			
Total Sports	8.19	1	54	14,758
	(5.32)			
APs Offered	12.30	1	38	5,868
	(7.68)			

Notes: Data come from the PSR frequent updaters dataset (at least 3 Wayback Machine updates 2016-2022). This sample reflects the analytic sample for estimation, except that variables are not imputed forward, which includes all observations from 2020 to 2024 for schools whose first tuition observation is from 2020 or earlier. Note that this is intended to provide a snapshot of the data we collected directly from the PSR, without imputing any data. However, values for the dependent variables are only marginally affected by using their imputed-forward versions. States with pending universal voucher programs enacted before October 2024 are excluded from this sample. Additionally, Tennessee and Wisconsin are excluded because they have geographically targeted programs that are small on a statewide basis but large in a local sense. Note that tuition and average financial aid are nominal values and not inflation-adjusted. The "Other Variables" are not used in the causal analysis, but we include them as part of our larger goal of describing the private school market.

Table 2: Means, Standard Deviations, and Non-Missing Data Rates For Treatment and Control States From PSR Analytic Sample

	Treated States	Control States	Difference in Means t-test
Dependent Variables			
Total Students	302.08	292.22	9.86
	82.93	84.14	(1.92)
Yearly Tuition	10,328.40	14,348.55	-4,020.15***
	79.56	80.68	(-16.48)
Avg. Financial Aid	5,630.25	7,790.44	-2,160.19***
	26.03	28.93	(-7.43)
% on Financial Aid	38.33	31.40	6.93***
	32.46	36.90	(12.20)
Other Variables			
Total Teachers	28.87	29.83	-0.96
	84.15	84.77	(-1.76)
Student:Teacher	12.04	12.02	0.01
	82.54	83.36	(0.06)
Avg. Class Size	15.62	15.78	-0.16
	70.63	73.98	(-1.50)
% of Teachers with Adv. Degrees	49.23	57.93	-8.70***
	45.06	45.07	(-14.62)
% Students of Color	29.47	33.58	-4.11***
	76.18	77.32	(-8.53)
Has High School	0.54	0.47	0.07***
	100.00	100.00	(8.98)
Has Middle School	0.78	0.70	0.08***
	100.00	100.00	(11.91)
Has Elementary School	0.78	0.70	0.08***
	100.00	100.00	(11.25)
Year Founded	1,970.12	1,959.49	10.63***
	77.62	79.42	(14.68)
Total Extracurriculars	9.90	11.32	-1.42***
	53.72	55.25	(-6.40)
Total Sports	8.21	8.19	0.02
	60.07	63.15	(0.21)
APs Offered	12.09	12.36	-0.26
	24.72	24.85	(-1.12)

Notes: For the "Treated States" and "Control States" columns, top entries are means and bottom entries are the percent of non-missing observations for each variable. In the "Difference in Means t-test" column, top entries are the difference in means and bottom entries are t-statistics. The data and sample construction are otherwise identical to Table 1.

Table 3: Average Treatment on Treated (ATT) Effects of Vouchers - Dichotomous Treatment (PSR Data)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+	-0.0026	0.0345***	0.0087*	0.0247*
(se)	(0.0116)	(0.0071)	(0.0035)	(0.0108)
P.T. Mean	9,580.46	297.40	0.36	5,073.54
N	23,645	23,325	12,930	8,120
Pretrend χ^2	33.09	128.67	36.87	34.23
Pretrend p	0.000	0.000	0.000	0.000
CSDID 2021+	-0.0019	0.0391***	0.0108*	0.0316**
	(0.0141)	(0.0067)	(0.0041)	(0.0115)
	9,477.21	301.81	0.36	4,441.60
	18,840	18,184	10,084	6,316
	3.03	31.20	1.90	17.64
	0.387	0.000	0.388	0.001
Treated 2021-22				
IN DD Coef.	-0.009**	0.009	0.009***	-0.013**
	(0.003)	(0.004)	(0.001)	(0.005)
	7,673.27	312.48	0.49	14,699.75
	18,345	18,100	10,325	6,510
NH DD Coef.	-0.003	0.024***	-0.006***	0.017**
	(0.003)	(0.004)	(0.001)	(0.005)
	21,413.34	194.90	0.35	18,566.65
	18,270	18,025	10,260	6,455
Treated 2022-23				
AZ DD Coef.	0.047***	0.060***	0.012***	0.043***
	(0.003)	(0.005)	(0.001)	(0.004)
	10,375.64	240.73	0.49	6,409.38
	18,470	18,210	10,385	6,530
Pretrend F	20.48	35.42	2.48	37.40
Pretrend p	0.000	0.000	0.124	0.000
NC DD Coef.	0.007*	0.059***	0.004***	0.028***
	(0.003)	(0.005)	(0.001)	(0.004)
	10,627.06	316.03	0.25	5,528.25
	18,795	18,545	10,520	6,640
	47.06	86.22	4.25	0.00
	0.000	0.000	0.047	0.951
WV DD Coef.	-0.022***	-0.059***	-0.005***	-0.034***
	(0.003)	(0.005)	(0.001)	(0.004)
	6,186.50	160.67	0.16	503.75
	18,100	17,855	10,145	6,390
	23.95	31.05	3.61	6.84
	0.000	0.000	0.066	0.013

Table 3 (Cont.): Average Treatment on Treated (ATT) Effects of Vouchers - Dichotomous Treatment (PSR Data)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Treated 2023-24				
FL DD Coef.	-0.034***	0.046***	0.007***	0.005
(se)	(0.004)	(0.006)	(0.001)	(0.005)
P.T. Mean	9,276.77	309.45	0.39	4,832.32
N	19,980	19,700	11,035	6,915
Pretrend F	1.80	5.24	8.62	15.62
Pretrend p	0.188	0.028	0.006	0.000
IA DD Coef.	0.019***	0.057***	-0.004***	0.020***
	(0.004)	(0.006)	(0.001)	(0.005)
	5,506.01	198.49	0.40	1,285.42
	18,195	17,950	10,200	6,410
	2.44	14.87	5.65	584.51
	0.127	0.000	0.023	0.000
OH DD Coef.	-0.001	0.030***	0.019***	0.079***
	(0.004)	(0.006)	(0.001)	(0.005)
	8,246.58	328.26	0.42	3,761.16
	18,860	18,610	10,620	6,660
	2.90	0.06	4.82	1.50
	0.097	0.815	0.035	0.229
Treated 2024-25				
OK DD Coef.	-0.017***	0.023**	0.009***	-0.030***
	(0.004)	(0.007)	(0.002)	(0.006)
	6,789.62	344.34	0.21	2,525.96
	18,170	17,920	10,225	6,440
	4.16	1.48	8.21	42.74
	0.049	0.232	0.007	0.000
SC DD Coef.	0.009*	0.081***	-0.005**	-0.033***
	(0.004)	(0.007)	(0.002)	(0.006)
	9,329.49	280.39	0.26	3,295.31
	18,405	18,160	10,310	6,460
	61.63	119.43	3.44	23.82
	0.000	0.000	0.072	0.00002
UT DD Coef.	-0.008*	-0.012	-0.003	-0.025***
	(0.004)	(0.007)	(0.002)	(0.006)
	18,957.59	251.60	0.23	7,317.38
	18,155	17,910	10,205	6,410
	19.11	2.83	50.69	57.37
	0.000	0.102	0.000	0.000

Notes Entries are (in order): coefficients, standard errors, level pre-treatment means for treated states, the number of observations, pretrend statistic (χ^2 for CSDID and F for DD), and pretrend p-value. CSDID estimations use both never-treated and not-yet-treated states as controls. The first two rows give CSDID estimates using all treated states from the remainder of the table over two time periods: 2020-2024 and 2021-2024. Each subsequent row gives the TWFE DD coefficients for all outcomes, with only one treated state (abbreviation of treated state comes before "DD Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect. We exclude states with other current or pending universal voucher programs enacted before October 2024. Additionally, Tennessee and Wisconsin are excluded because they have geographically targeted programs that are small on a statewide basis but large in a local sense. Data comes from the PSR frequent updaters impute-forward dataset (at least 3 Wayback Machine updates 2016-2022). For all specifications, only complete case tuition observations (first tuition observation is from 2020 or earlier) are used, and a balanced panel is used for each outcome. Tuition and average financial aid are nominal values and are not inflation-adjusted. Event studies are provided for the first row of estimates in Figure 4.

Table 4: Average Treatment on Treated (ATT) Effects of Vouchers - Continuous Treatment (PSR Data)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Highest Eligibility				
TWFE DD Coef.	-0.009	0.045***	0.009**	0.025
	(0.013)	(0.009)	(0.003)	(0.015)
CH Total Effect	0.006	0.024	-0.007	-0.041
	(0.016)	(0.044)	(0.005)	(0.035)
CH Total Effect (Rounded to 10%)	-0.009	0.022	0.007	0.024
	(0.018)	(0.019)	(0.017)	(0.036)
CS ATT	-0.006	0.035***	0.007**	0.024**
	(0.012)	(0.006)	(0.002)	(0.010)
Partial Overlap Eligibility				
TWFE DD Coef.	-0.009	0.044***	0.009**	0.024
	(0.013)	(0.008)	(0.003)	(0.015)
CH Total Effect	0.006	0.024	-0.007	-0.041
	(0.016)	(0.044)	(0.005)	(0.035)
CH Total Effect (Rounded to 10%)	0.007	0.021	-0.009	-0.016
	(0.019)	(0.030)	(0.008)	(0.030)
CS ATT	-0.006	0.035***	0.007**	0.024**
	(0.010)	(0.007)	(0.003)	(0.009)
Voucher and ESA Highest Eligibility				
TWFE DD Coef.	-0.009	0.045***	0.009**	0.027
	(0.014)	(0.009)	(0.003)	(0.015)
CH Total Effect	0.002	0.035	-0.006	-0.035
	(0.012)	(0.035)	(0.004)	(0.025)
CH Total Effect (Rounded to 10%)	0.001	0.030	0.010	0.033
	(0.021)	(0.019)	(0.015)	(0.035)
CS ATT	-0.006	0.035***	0.007**	0.026*
	(0.012)	(0.006)	(0.002)	(0.013)
Voucher and ESA Partial Overlap Eligibility				
TWFE DD Coef.	-0.009	0.045***	0.009**	0.026
	(0.013)	(0.009)	(0.003)	(0.015)
CH Total Effect	0.002	0.035	-0.006	-0.035
	(0.012)	(0.035)	(0.004)	(0.025)
CH Total Effect (Rounded to 10%)	0.020	0.037	-0.002	0.013
	(0.022)	(0.024)	(0.006)	(0.029)
CS ATT	-0.006	0.035***	0.007**	0.026*
	(0.016)	(0.006)	(0.002)	(0.011)
N	23,645	23,325	12,930	8,120

Notes: Entries are coefficients with standard errors beneath. Continuous treatment is constructed as explained in the text and represents a 0-1 share of eligible students. The number of observations is consistent with columns/outcomes as shown in the final row of the table, but note that CH estimates are identified using the number of switchers . "Highest Eligibility" refers to using the maximum eligibility among existing school choice programs within a state as treatment, and "Partial Overlap Eligibility" refers to using the average of the maximum eligibility and the sum of eligibilities (to allow for partial overlap between programs). The top two panels restrict to school choice programs that have awards consistently over $2,000 and reasonable budget caps. The bottom two panels restrict further to only voucher or ESA programs. Coefficients are either from TWFE, de Chaisemartin et al. (2022) (CH), or Callaway et al. (2025) (CS) estimates. CH identifies estimates using the number of switchers at each level of baseline treatment, so we also provide a specification (rounded to 10%) where eligibility is rounded to the nearest 10%. This increases the number of switchers and control units at each initial dose (including at a dose of 0). For CS estimates, treatment starts during the first large jump in eligibility (if no such jump but non-zero eligibility, then treatment starts in 2020) and is the average of eligibility after the treatment start date (CS does not allow treatment dose to vary). Note that CH and TWFE estimates allow dose to vary over time, but only CH doesn't impose a treatment start time. Standard errors are clustered at the state level for TWFE and CH estimates, but no clustering option currently exists for CS. For each outcome, a balanced panel is used. The sample and data construction are otherwise identical to Table 3.

Table 5: Average Treatment on Treated (ATT) Effects of Vouchers - Dichotomous Treatment (NCCS Data)

	asinh(Revenue) DD	asinh(Revenue) SDID	asinh(PS Revenue) DD CF	asinh(PS Revenue) SDID CF	asinh(PS Revenue) DD Lin.	asinh(PS Revenue) SDID Lin.
All Known NCCS Schools						
DD or SDID Coef. 19-21	0.084	-0.003				
(se)	(0.108)	(0.309)				
P.T. Mean	2,918,968	3,534,172				
N	8,820	117				
Pretrend F	0.09					
Pretrend p	0.769					
NCCS/PSS Matches						
DD or SDID Coef. 19-21	0.145*	-0.006	0.104**	0.051	0.099**	-0.010
	(0.059)	(0.077)	(0.032)	(0.199)	(0.033)	(0.089)
	3,878,076	4,665,092	3,878,076	4,665,092	3,878,076	4,665,092
	2,556	108	2,556	108	2,556	108
	14.38		23.82		21.53	
	0.001		0.000		0.000	
NCCS/PSR Matches						
DD or SDID Coef. 19-21	0.173	0.042	0.061	0.049	0.070	0.049
	(0.108)	(0.109)	(0.076)	(0.095)	(0.079)	(0.105)
	4,615,737	4,418,613	171,295	163,108	171,567	163,395
	1,320	105	1,320	105	1,320	105
	6.79		6.28		5.75	
	0.014		0.017		0.022	

Notes: The only treated units are Indiana and New Hampshire (all other treated states from Table 3 are excluded). From the top, entries are DD or SDID coefficient estimates, standard errors, level pre-treatment means for treated units, the number of observations, pretrend F-statistics, and pretrend p-values. SDID estimates require aggregation to the state level, which is why the number of observations decreases. The outcome for this regression is either asinh(school-level total program service revenue) or asinh(per-student school-level total program service revenue), taken from the National Center for Charitable Statistics (NCCS), and treatment is the implementation of a universal school voucher program (see Figure 1 for treatment years). In order to estimate effects on per-student revenue ("PS Revenue"), we need enrollment data, which is why per-student revenue estimates are only available for observations with a PSS match (enrollment data comes from the PSS). In all cases, per-student revenue is calculated based on NCES PSS enrollment. NCES PSS enrollment is available every other year, so we must either assume that enrollment stays the same between observations ("CF") or that it changes linearly between periods ("Lin."). "All NCCS K-12 Schools" refers to all NCCS observations with an organizational name and/or address match in the NCES Private School Survey (PSS) as well as additional NCCS observations identified with K-12-specific NTEE codes (B20, B24, B25). "NCCS/PSS Matches" refer to all NCCS observations with an organizational name and/or address match in the PSS. "NCCS/PSR Matches refer to all NCCS observations with a match in the Private School Review sample (notice that this is a subset of NCCS/PSS matches as NCCS/PSR matches were found via a PSR/PSS crosswalk). We estimate over the period 2019-2021 as we want to keep periods consistent between PSR and NCCS estimations. Standard errors are clustered at the state level. The control states are the same as Table 3.

Table 6: Average Treatment on Treated (ATT) for Iowa (Comparing with Fontana and Jennings)

	(1) F&J	(2) Our F&J 1	(3) Our F&J 2	(4) (2) with 24	(5) (3) with 24	(6) Unweighted	(7) (6) with 24
Years	21-23	21-23	21-23	21-24	21-24	21-23	21-24
Weight Type	Unclear	pweight	pweight	pweight	pweight	None	None
Weighting	School Enr.	22 School Enr.	22 IA DoE	22 School Enr.	22 IA DoE	N/A	N/A
Coefficient	0.10	-0.013	-0.018	0.027	0.014	-0.019	0.014
	(2.5)	(-0.79)	(-0.80)	(0.81)	(0.34)	(-1.06)	(0.39)
Observations	738	177	177	236	236	177	236

Notes: Entries are coefficients with t-statistics below. Data come from the PSR frequent updaters dataset using complete cases 2020 observations (first tuition observation comes in or before 2020). Only IA and NE are used for these results, and standard errors are unclustered in all PSR estimates. The outcome is asinh(yearly tuition), "Years" refer to Fall school years (e.g., 22 is the 2022-2023 school year), and the treatment year is 2023 in all specifications. The first column, "F&J," is the estimate (with school FEs, weighted on school enrollment, and standard errors clustered at the school-grade level) taken directly from Table 2 of the Fontana and Jennings (2024) paper. Note that observations in the Fontana and Jennings specification are at the school-grade level rather than at the school level, as we use. All other specifications use PSR imputed forward tuition, the same as is used in the rest of the PSR analysis. It is unclear how Fontana and Jennings weight their estimation by school enrollment, so we provide estimates with two different non-response weights for 2022 school enrollment. "22 School Enr." refers to calculating weights by taking the number of schools in each enrollment bin (0-100, 101-200, 201-300, and 300+) in the 2022 PSR population sample and dividing by the number of schools in each enrollment bin from the tuition analytic sample (bin calculations at both levels are done within IA and NE separately). "22 School Enr." weights assume that the PSR enrollment sample in 2022 (57% of national private schools) is representative of the true enrollment distribution in each state. Note that "22 School Enr." weights are similar to the non-response weights found in Appendix Table D8, except that 2022 enrollment (last pre-treatment year for IA) is used rather than 2021 enrollment. "22 IA DoE." refers to calculating weights by taking the number of schools in each enrollment bin (0-100, 101-200, 201-300, and 300+) in the 2022-23 Iowa Department of Education's certified nonpublic enrollment sample and dividing by the number of schools in each enrollment bin (bin calculations at both levels are done within IA and NE separately). Note that "22 IA DoE" weights assume that IA and NE have the same number of schools and enrollment bin distributions, which isn't necessarily true (NE does not publicly report nonpublic enrollment by school). Mean for yearly tuition in Iowa before 2023 is $4,941.95. Note that tuition is a nominal value and is not inflation-adjusted.

Table 7: Effect Heterogeneity - Dichotomous Treatment (PSR Data)

Panel A: CSDID and Staggered TWFE DD Estimates

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+	-0.0026	0.0345***	0.0087*	0.0247*
(se)	(0.0116)	(0.0071)	(0.0035)	(0.0108)
TWFE DD Coef.	-0.009	0.040***	0.008**	0.022
	(0.012)	(0.007)	(0.003)	(0.013)
P.T. Mean	9,580.46	297.40	0.36	5,073.45
N	23,645	23,325	12,930	8,120

Panel B: TWFE DD Heterogeneity Estimates

	DDH asinh(Tuition) Coefs	DDH asinh(Students) Coefs	DDH % on Aid Coefs	DDH asinh(Avg. Aid) Coefs
post*treat	-0.017**	0.019	0.004	0.025***
	(0.006)	(0.012)	(0.006)	(0.007)
2021 Tuition $<$ 9000	0.027*	-0.016	0.006	-0.018
	(0.011)	(0.011)	(0.004)	(0.018)
Baseline Students $<$ 100	0.034*	0.027*	0.006	0.032
	(0.017)	(0.011)	(0.010)	(0.032)
Catholic	-0.003	-0.021*	0.003	0.019
	(0.009)	(0.010)	(0.006)	(0.017)
Protestant	-0.032	0.045*	-0.002	-0.003
	(0.023)	(0.018)	(0.008)	(0.018)
Other Religion	-0.032*	0.072*	0.020	0.003
	(0.013)	(0.034)	(0.017)	(0.065)
Founded After 2000	-0.019	0.046**	-0.011	-0.037
	(0.019)	(0.014)	(0.006)	(0.022)
P.T. Mean	9,808.44	312.82	0.36	5,140.95
N	20,405	20,265	11,725	7,390

Notes: In panel A, for each column's outcome, entries are staggered DD estimates (CSDID or TWFE) followed by their standard errors. CSDID estimates use never-treated and not-yet-treated as controls. The agreement between CSDID and TWFE DD estimates helps to justify the TWFE DD heterogeneity analysis in panel B. In panel B, each column represents a single TWFE DD heterogeneity regression for that column's outcome, where the first row with post*treat gives the usual DD coefficient, and each subsequent row gives the coefficients for post*treat interacted with that row's indicator. "P.T. Mean" is the mean over all pre-treatment periods for treated states, though those periods are different depending on the treatment cohort. Sample and data construction are otherwise identical to Table 3.

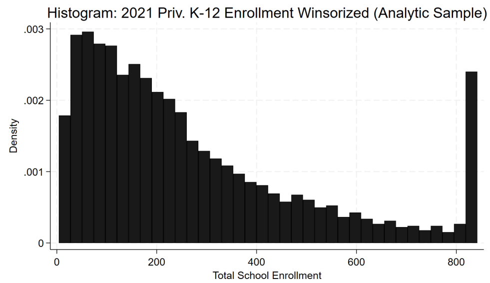
Figure 1: Statewide Student Eligibility Share and Treatment Status for Treated States, by State and Year

State	Academic Year	Max Elig. (All)	Partial Overlap Elig. (All)	Max Elig. (V + ESA)	Partial Overlap Elig. (V + ESA)
AZ	2020	60%	78%	23%	23%
AZ	2021	60%	78%	23%	23%
AZ	2022	100%	100%	100%	100%
AZ	2023	100%	100%	100%	100%
AZ	2024	100%	100%	100%	100%
FL	2020	56%	78%	56%	78%
FL	2021	62%	81%	62%	81%
FL	2022	62%	81%	62%	81%
FL	2023	100%	100%	100%	100%
FL	2024	100%	100%	100%	100%
IA	2020	0%	0%	0%	0%
IA	2021	0%	0%	0%	0%
IA	2022	0%	0%	0%	0%
IA	2023	94%	94%	94%	94%
IA	2024	94%	94%	94%	94%
IN	2020	47%	47%	47%	47%
IN	2021	79%	79%	79%	79%
IN	2022	77%	84%	77%	84%
IN	2023	98%	99%	98%	99%
IN	2024	100%	100%	100%	100%
NC	2020	43%	43%	43%	43%
NC	2021	43%	43%	43%	43%
NC	2022	62%	67%	62%	67%
NC	2023	81%	86%	81%	86%
NC	2024	100%	100%	100%	100%
NH	2020	0.5%	0.5%	0.5%	0.5%
NH	2021	30%	30%	30%	30%
NH	2022	31%	31%	31%	31%
NH	2023	30%	30%	30%	30%
NH	2024	48%	48%	48%	48%
OH	2020	34%	56%	34%	49%
OH	2021	40%	62%	40%	55%
OH	2022	40%	63%	40%	56%
OH	2023	96%	98%	96%	98%
OH	2024	100%	100%	100%	100%
OK	2020	15%	15%	15%	15%
OK	2021	15%	15%	15%	15%
OK	2022	18%	18%	18%	18%
OK	2023	18%	18%	18%	18%
OK	2024	100%	100%	17%	17%
SC	2020	13%	19%	0%	0%
SC	2021	13%	19%	0%	0%
SC	2022	13%	20%	0%	0%
SC	2023	13%	20%	0%	0%
SC	2024	71%	84%	71%	71%
UT	2020	12%	18%	12%	18%
UT	2021	12%	18%	12%	18%
UT	2022	13%	20%	13%	20%
UT	2023	13%	20%	13%	20%
UT	2024	100%	100%	100%	100%
WV	2020	0%	0%	0%	0%
WV	2021	0%	0%	0%	0%
WV	2022	94%	94%	94%	94%
WV	2023	93%	93%	93%	93%
WV	2024	93%	93%	93%	93%

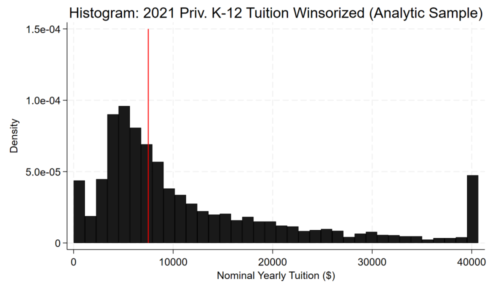
Notes: To construct this table, we collect the percentage of students eligible for any school choice program in these states from EdChoice between 2020 and 2024. These programs include vouchers, ESAs, tax-credit ESAs, individual K–12 tax credits & deductions, refundable tax credits, and tax-credit scholarships. We restrict to school choice programs that have awards consistently over $2,000 and reasonable budget caps. Many states have multiple programs, with several offering more than one voucher or ESA program. We use two rules to determine statewide eligibility when there are multiple programs in a state: the *maximum* eligibility of any program ("Max Elig.") and the *average* of the maximum eligibility and the sum of eligibilities ("Partial Overlap Elig."). Note that if the sum of eligibilities $> 100\%$, then the average is between the maximum and 100%. The maximum eligibility rule assumes that all programs have the same eligible populations, and the average eligibility rule assumes only partial overlap between eligible populations. We present statewide student eligibility share for all programs ("All") and for exclusively voucher and ESA programs ("V + ESA"). Highlighted rows indicate when each state is considered treated with a universal voucher in dichotomous treatment specifications.

Figure 2: Histograms of National Enrollment and Tuition using PSR Analytic Sample

(a) Enrollment



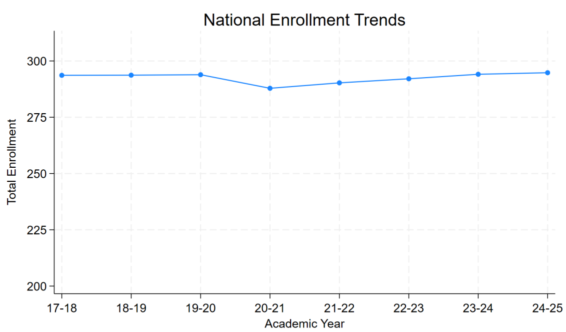
(b) Tuition ($)



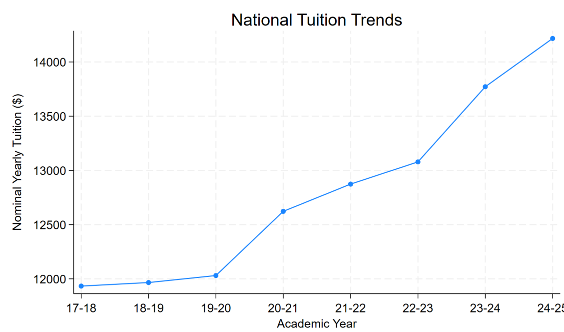
NOTES: This sample represents the PSR analytic sample and includes schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) and have their first non-missing tuition observation in or before 2020 (complete case tuition). States with pending universal voucher programs enacted before October 2024 are excluded from these figures. Additionally, Tennessee and Wisconsin are also excluded from these figures due to the nature of their voucher programs. We use 2021 as a baseline as it is the first year that universal vouchers were implemented. Note that tuition is a nominal value and is not inflation-adjusted. Histograms for the entire sample of frequent updaters (no complete case tuition restriction) and the entire 2024 PSR sample (no frequent updater or complete case tuition restrictions) are presented in Appendix C. Note that the range in densities (y-axis) is different between these sets of histograms. Our analytic sample seems to include a disproportionately large number of high-enrollment schools and a small number of low-enrollment schools.

Figure 3: Enrollment and Tuition Trends using PSR Analytic Sample

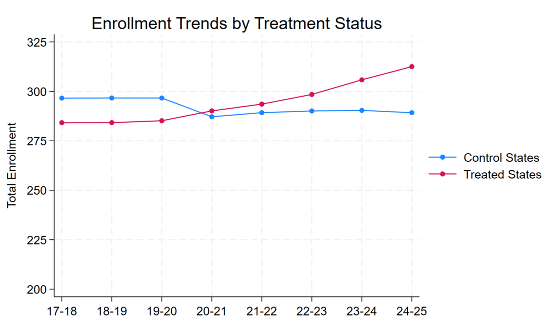
(a) National Enrollment



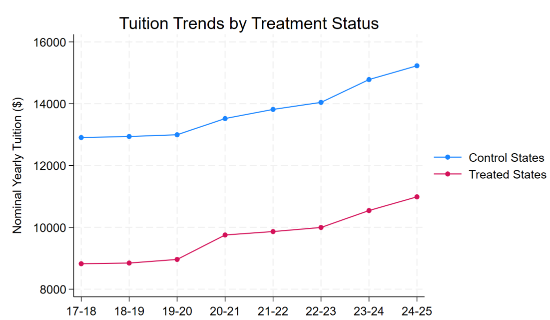
(b) National Tuition



(c) Enrollment Disaggregated



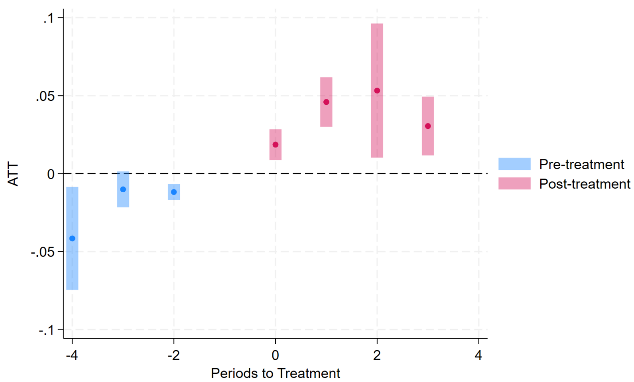
(d) Tuition Disaggregated



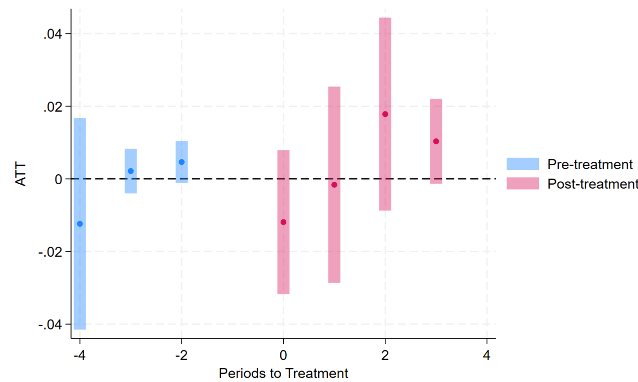
NOTES: See notes to Figure 2.

Figure 4: Event Studies of National Voucher Effects from PSR Data (CSDID Method with Long Gaps)

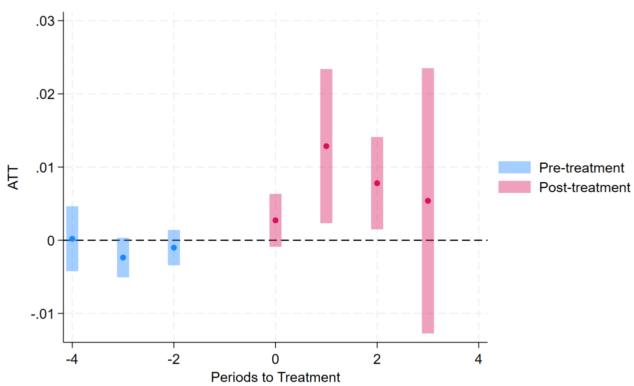
(a) asinh(Total Enrollment)



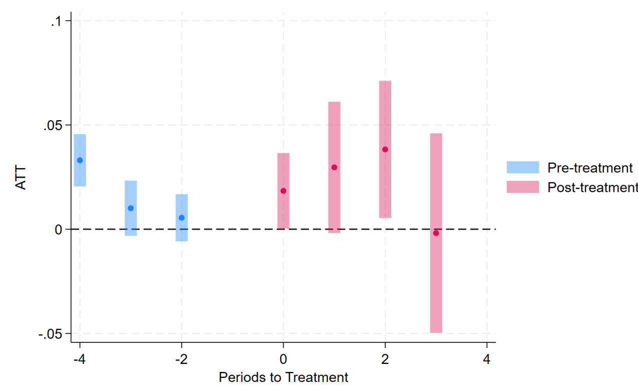
(b) asinh(Nominal Yearly Tuition) ($)



(c) % on Financial Aid



(d) asinh(Nominal Average Financial Aid) ($)



NOTES: These figures reflect CSDID event study estimates using schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters). Additionally, included schools must have their first non-missing tuition observation before 2021 (complete case tuition). All treated states from Table 3 are included in this staggered CSDID estimation. Note that tuition and average financial aid are in nominal values and are not inflation-adjusted. These event studies use 'long gaps" where all periods are compared to the last pre-treatment period (standard event study comparisons). Event studies using short gaps are presented in the appendix. These event studies correspond to the estimates in the first row of Table 3.

Contents

Appendices

A Details on States and Their Policies

A.1 List of Every Voucher-Like Policy by State and Further Description

Table A1 lists all national voucher-like policies with program characteristics and statistics from EdChoice's "School Choice in America Dashboard." The table was last updated in June 2024 and may not contain the most up-to-date data on all policies; however, it provides an informative starting point, as this information is not readily available across all national programs.

We split these policies into four groups based on the data from EdChoice and our own research into specific policies. Blue indicates the programs we use for estimation throughout the paper, which consist of universal programs that went into effect between the 2021 and 2024 academic years. There are 13 such policies in 11 states. Green indicates programs that are universal but take effect after the 2024 academic year, of which there are three policies in three states. Orange indicates programs that we do not consider universal but that EdChoice labels as universal. There are four of these policies in as many states, and we opt not to label them as universal for two main reasons. First, while EdChoice generally gives the percentage of students eligible for a program overall (calculated by EdChoice), they only provide the percentage of *income-eligible* students for tax-credit scholarships. This causes the percentage eligible for tax-credit scholarships to be overstated. Second, these programs tend to have a low average funding, and do not remain consistently above $2,000 throughout the period 2020-2024 (while not shown, we also compiled the same data from EdChoice each year 2020-2024 using the Wayback Machine). Additionally, we exclude Alaska's hybrid program because it targets homeschooling and requires an Independent Learning Plan to receive funds. Additionally, Alaska's program remains constant throughout the study period and since 2014, making it a suitable control regardless. Finally, unhighlighted programs indicate voucher-like programs that exist, but do not have universal eligibility.

For our empirical analyses, we exclude states with universal voucher programs going into effect after the 2024 academic year (Green: Alabama, Arkansas, and Louisiana) as they may experience anticipatory effects. We also exclude Tennessee, Wisconsin, and Wyoming. Tennessee and Wisconsin have large voucher-like policies that are geographically isolated. Tennessee's Education Savings Account Program serves students in Chattanooga, Memphis, and Nashville, while Wisconsin has programs specifically for Milwaukee and Racine. As these programs make it difficult to determine eligibility and are not part of the new wave of universal voucher policies, we exclude them from analysis. Wyoming's Steamboat Legacy Scholarship Act began as the Wyoming Education Savings Account Program, which was enacted in 2024 and targeted low-income students, but it was expanded in early 2025 to become universal that same year, though this is currently being fought in the state's court system. Wyoming is an unusual case, and we exclude it from analysis due to the tumult in its policy's creation and specific language in the Wyoming Constitution that prohibits state monies going to individuals "except for the necessary support of the poor" (Hill, 2005), which has delayed the passage of voucher-like policy over fears of unconstitutionality (which were evidently not unfounded). Specifically, the governor of Wyoming partially vetoed a similar universal program in 2024, citing concerns about constitutionality, but reversed his opinion in 2025, passing the universal program and praising it as a "remarkable achievement for Wyoming" (Klingsporn, 2025). While other states have similar language in their constitutions (Hill, 2005), and Wyoming is not the only state to have had its program challenged on constitutional grounds (Wagner, 2025), it is the only universal program that was passed and subsequently challenged before its enactment. For these reasons, we consider Wyoming a special case that may also have experienced anticipatory effects; therefore, we exclude it from our analysis. Note that the new Texas and Idaho universal programs are not included in Table A1, as these programs are their first school-choice legislation and were passed several months into 2025 (Table A1 last modified in June 2024). We include Texas and Idaho as controls, as their policies (in any form) were passed *after* our last round of data collection in October 2024. Therefore, any data we have from either state should still serve as clean controls.

For the programs we identify as universal, we researched the policies independently of EdChoice to determine when a program became universal. This is important as we use EdChoice's eligibility in our continuous DD estimations, but rely on our own research to identify the timing of universality in our dichotomous DD estimations. Additionally, many of these universal programs expanded to universal coverage over time, so the year launched is not necessarily the year of universal coverage. Note that the *only* column in Table A1 that does not come from EdChoice is "Year Universal," which comes from our own research. We label a program as universal when income eligibility rises above 200% - 250% of the FPL without a budget or enrollment cap that severely hampers eligibility. We do not exclude programs with prior public school enrollment requirements, as the vast majority of students in a state attend public school, and we are interested in enrollment effects, which will definitionally have to come from public-to-private switchers. Arizona, Florida, New Hampshire, North Carolina, Oklahoma, Ohio, and Utah did not have prior public school requirements in the year we count their programs as universal. Indiana, Iowa, South Carolina, and West Virginia had prior public school requirements in the year we count their programs as universal; however, note that Indiana's and Iowa's requirements were only applicable to families with incomes above a certain threshold. These prior public school requirements tend to disappear within several years of adoption, so we should expect take-up of these programs to increase over time. Note that South Carolina has the most restrictive enrollment cap for its program, which limits the program to nearly 20% of the state's private school enrollment (switchers only) and increases to 25% in subsequent years. It would take a substantial enrollment effect to induce that amount of public-to-private switching, so we do not consider this enrollment cap to be binding.

Appendix Table A1: Voucher Policy Table

State	Program Type	Program Name	Enacted	Launched	Year Universal	Participation	Participation Rate	Eligibility	Eligibility Rate	Average Funding	Public Funding	Schools	Universal
Alabama	Individual Tax Credit/Deduction	Accountability Act of 2013 Parent-Taxpayer Refundable Tax Credits	2013	2013		50	TBD	96,927	11%	3910	33%	N/A	N/A
Alabama	Tax-Credit Scholarship	Education Scholarship Program	2013	2013		3,579	2%	439,734	52%	7320	62%	N/A	N/A
Alabama	Education Savings Account	Creating Hope and Opportunity for Our Students' Education (CHOOSE) Act of 2024	2024	2025	2025	Not Yet Launched	Not Yet Launched	–	100%	Not Yet Launched	0%	Not Yet Launched	Eligibility
Alaska	Hybrid	Correspondence Study and Student Allotment Program	–	2014	**Homeschool-focused, Stable 2020-24**	N/A	TBD	–	–	N/A	90%	N/A	Eligibility
Arizona	Education Savings Account	Empowerment Scholarship Accounts	2011	2011	2022	83,082	TBD	1,265,358	100%	9572	90%	404	Full
Arizona	Tax-Credit Scholarship	"Switcher" Individual Income Tax Credit Scholarship Program	2012	2012		21,241	2%	1,112,721	88%	1710	16%	346	N/A
Arizona	Tax-Credit Scholarship	Lexie's Law for Disabled and Displaced Students Tax Credit Scholarship Program	2009	2009		1168	1%	149,495	12%	3844	37%	137	N/A
Arizona	Tax-Credit Scholarship	Low-Income Corporate Income Tax Credit Scholarship Program	2006	2006		29,582	TBD	793,379	63%	3834	37%	291	N/A
Arizona	Tax-Credit Scholarship	Original Individual Income Tax Credit Scholarship Program	1997	1997	**Low average funding, Stable 2020-24**	23,826	2%	1,265,358	100%	2093	20%	350	Eligibility
Arkansas	Education Savings Account	Arkansas Children's Educational Freedom Account Program	2023	2024	2025	14,297	1%	525,951	100%	6856	56%	94	Full
Arkansas	Tax-Credit Scholarship	Philanthropic Investment in Arkansas Kids Scholarship Program	2021	2022		409	TBD	241,937	46%	7618	46%	Not Yet Launched	N/A
Florida	Education Savings Account	Family Empowerment Scholarship for Educational Options Program	2019	2019	2023	220,974	7%	3,372,584	100%	8100	72%	1,960	Full
Florida	Education Savings Account	Family Empowerment Scholarship for Students with Unique Abilities (ESA)	2014	2014		122,051	7%	393,228	12%	10000	90%	1,972	N/A
Florida	Tax-Credit ESA	Florida Tax Credit Scholarship Program	2001	2001	2023	106,442	7%	3,372,584	100%	8100	69%	1,990	Eligibility
Georgia	Education Savings Account	The Promise Scholarship Act	2024	2025		Not Yet Launched	TBD	–	–	Not Yet Launched	44%	Not Yet Launched	N/A
Georgia	Tax-Credit Scholarship	Qualified Education Expense Tax Credit	2008	2008		21,849	1%	1,701,885	89%	4624	34%	N/A	N/A
Georgia	Voucher	Georgia Special Needs Scholarship Program	2007	2007		5,864	2%	220,202	11%	6821	50%	281	N/A
Illinois	Individual Tax Credit/Deduction	Tax Credits for Educational Expenses	1999	2000		210,224	TBD	2,029,756	100%	322	2%	N/A	N/A
Indiana	Education Savings Account	Education Scholarship Account Program	2021	2022		862	<1%	174,063	15%	11601	54%	N/A	N/A
Indiana	Individual Tax Credit/Deduction	Private School/Homeschool Deduction	2011	2011		57,628	TBD	171,143	13%	1840	17%	N/A	N/A
Indiana	Tax-Credit Scholarship	School Scholarship Tax Credit	2009	2010		11,405	3%	1,149,405	98%	2054	18%	381	N/A
Indiana	Voucher	Choice Scholarship Program	2011	2011	2021	75,269	8%	1,149,405	98%	6264	51%	357	N/A
Iowa	Education Savings Account	Education Savings Account Program	2023	2023	2023	27,862	TBD	566,488	100%	7826	59%	N/A	Eligibility
Iowa	Individual Tax Credit/Deduction	Tuition and Textbook Tax Credit	1987	1987	**Low average funding, stable 2020-24**	113,947	TBD	566,488	100%	222	1%	N/A	Eligibility
Iowa	Tax-Credit Scholarship	School Tuition Organization Tax Credit	2006	2006		10,239	7%	385,212	68%	2062	15%	143	N/A
Kansas	Tax-Credit Scholarship	Tax Credit for Low-Income Students Scholarship Program	2014	2015		1,340	TBD	162,920	32%	3199	23%	88	N/A

Notes: Data from this table comes mainly from the "School Choice in America Dashboard" from Edchoice (last modified June 2024). Not every voucher program is universal, and all are included here. Blue programs represent policies in our analytic sample (11 states), green programs represent policies that go into effect in 2025 (3 states, excluded from analysis), and orange programs represent policies that Edchoice labels as universal but we do not consider universal. Purple programs have a note explaining why we don't consider them universal under "Year Universal." We consider a voucher program to be universal if the income eligibility threshold is greater than 200% - 250% of the Federal Poverty Line or Federal Free and Reduced Price Lunch, which is why "Year Universal" is not necessarily the year a program was enacted (many programs stage up eligibility over time).

Appendix Table A1 (Continued): Voucher Policy Table

State	Program Type	Program Name	Enacted	Launched	Year Universal	Participation	Participation Rate	Eligibility	Eligibility Rate	Average Funding	Public Funding	Schools	Universal
Louisiana	Individual Tax Credit/Deduction	Elementary and Secondary School Tuition Deduction	2008	2008		63,355	TBD	102,439	13%	269	45%	N/A	N/A
Louisiana	Education Savings Account	Giving All True Opportunity to Rise (LA GATOR) Scholarship Program	2024	2025	2025	Not Yet Launched	Not Yet Launched	–	100%	Not Yet Launched	N/A		Eligibility
Louisiana	Tax-Credit Scholarship	Tuition Donation Credit Program	2012	2012		3,316	TBD	407,927	53%	4225	28%	191	N/A
Louisiana	Voucher	School Choice Program for Certain Students with Exceptionalities	2010	2011		441	TBD	29,436	10%	4604	31%	21	N/A
Louisiana	Voucher	Student Scholarships for Educational Excellence Program	2008	2008		5,415	TBD	287,208	37%	6886	46%	125	N/A
Maine	Voucher	Town Tuitioning Program	1873	1873		4,149	100%	4,701	2%	13268	78%	31	N/A
Maryland	Voucher	Broadening Options and Opportunities for Students Today (BOOST) Program	2016	2016		2,942	TBD	242,996	24%	2937	16%	154	N/A
Minnesota	Individual Tax Credit/Deduction	Education Deduction	1955	1955		125,481	TBD	950,343	100%	1427	9%	N/A	N/A
Minnesota	Individual Tax Credit/Deduction	K–12 Education Credit	1997	1998		44,825	TBD	285,103	30%	341	2%	N/A	N/A
Mississippi	Education Savings Account	Equal Opportunity for Students with Special Needs Program	2015	2015		345	1%	32,478	7%	7829	71%	90	N/A
Mississippi	Voucher	Mississippi Dyslexia Therapy Scholarship for Students with Dyslexia Program	2012	2012		246	1%	19,033	4%	6695	60%	5	N/A
Mississippi	Voucher	Nate Rogers Scholarship for Students with Disabilities Program	2013	2013		10	TBD	13,796	3%	6063	55%	1	N/A
Missouri	Tax-Credit ESA	Missouri Empowerment Scholarship Accounts Program	2021	2021		1,997	TBD	804,287	79%	6375	50%	N/A	N/A
Montana	Education Savings Account	Montana Special Needs Equal Opportunity Education Savings Account Program	2023	2024		N/A	TBD	20,292	12%	N/A	N/A	Not Yet Launched	N/A
Montana	Tax-Credit Scholarship	Tax Credits for Contributions to Student Scholarship Organizations	2015	2015	**Low average funding, Stable 2020-24**	1,671	–	165,578	100%	2190	16%	73	Eligibility
Nevada	Tax-Credit Scholarship	Nevada Educational Choice Scholarship Program	2015	2015		1,827	TBD	307,677	58%	5263	47%	156	N/A
New Hampshire	Education Savings Account	Education Freedom Account Program	2021	2021	2021	5,600	8%	83,766	45%	5100	25%	N/A	N/A
New Hampshire	Tax-Credit Scholarship	Education Tax Credit Program	2012	2013		827	3%	70,022	38%	3045	15%	68	N/A
New Hampshire	Voucher	Town Tuitioning Program	2017	2017		17	TBD	1,857	<1%	14000	88%	4	N/A
North Carolina	Education Savings Account	Education Student Accounts (ESA+)	2021	2022		3,566	TBD	189,710	10%	11846	98%	328	N/A
North Carolina	Voucher	Opportunity Scholarships	2013	2014	2022	37,329	5%	1,768,527	100%	5701	47%	544	Eligibility
Ohio	Individual Tax Credit/Deduction	K–12 Home Education Tax Credit	2021	2021		Not Yet Launched	Not Yet Launched	1,835,912	100%	250	2%	Not Yet Launched	N/A
Ohio	Individual Tax Credit/Deduction	K–12 Nonchartered Private School Tax Credit	2021	2021		1305	Not Yet Launched	1,835,912	100%	549	7%	N/A	N/A
Ohio	Tax-Credit Scholarship	Ohio Tax-Credit Scholarship Program	2021	2021		N/A	TBD	1,835,912	100%	N/A	N/A	N/A	N/A
Ohio	Voucher	Autism Scholarship Program	2003	2004		5,205	15%	35,285	2%	29639	194%	279	N/A
Ohio	Voucher	Cleveland Scholarship Program	1995	1996		8,045	12%	59,409	100%	6092	42%	58	N/A
Ohio	Voucher	Educational Choice Scholarship Program	2005	2006	2024	41,303	7%	1,835,912	100%	6036	40%	462	Eligibility

Notes: Data from this table comes mainly from the "School Choice in America Dashboard" from Edchoice (last modified June 2024). Not every voucher program is universal, and all are included here. Blue programs represent policies in our analytic sample (11 states), green programs represent policies that go into effect in 2025 (3 states, excluded from analysis), and orange programs represent policies that Edchoice labels as universal but we do not consider universal. Orange programs have a note explaining why we don't consider them universal under "Year Universal." We consider a voucher program to be universal if the income eligibility threshold is greater than 200% - 250% of the Federal Poverty Line or Federal Free and Reduced Price Lunch, which is why "Year Universal" is not necessarily the year a program was enacted (many programs stage up eligibility over time).

Appendix Table A1 (Continued): Voucher Policy Table

State	Program Type	Program Name	Enacted	Launched	Year Universal	Participation	Participation Rate	Eligibility	Eligibility Rate	Average Funding	Public Funding	Schools	Universal
Ohio	Voucher	Educational Choice Expansion Scholarship (EdChoice) Program	2013	2013	2023	88,238	TBD	1,762,476	96%	5512	36%	511	N/A
Ohio	Voucher	Jon Peterson Special Needs Scholarship Program	2011	2012		8,650	3%	256,912	14%	10801	72%	428	N/A
Oklahoma	Refundable Tax Credit	Oklahoma Parental Choice Tax Credit Act	2023	2024	2024	30,060	TBD	748,961	100%	3051	N/A	N/A	Eligibility
Oklahoma	Tax-Credit Scholarship	Oklahoma Equal Opportunity Education Scholarships	2011	2013		3,222	TBD	656,090	88%	2695	25%	106	N/A
Oklahoma	Voucher	Lindsey Nicole Henry Scholarships for Students with Disabilities	2010	2010		1,256	1%	115,890	15%	8083	80%	73	N/A
Pennsylvania	Tax-Credit Scholarship	Educational Improvement Tax Credit Program	2001	2001		54,241	5%	1,408,653	73%	2583	14%	N/A	N/A
Pennsylvania	Tax-Credit Scholarship	Opportunity Scholarship Tax Credit Program	2012	2012		23,430	10%	157,811	8%	1853	10%	N/A	N/A
Puerto Rico	Voucher	Free School Selection Program	2018	2019		878	TBD	229,484	62%	2276	37%	65	N/A
Rhode Island	Tax-Credit Scholarship	Tax Credits for Contributions to Scholarship Organizations	2006	2007		478	1%	61,645	41%	3009	15%	51	N/A
South Carolina	Education Savings Account	South Carolina Education Scholarship Trust Fund Program	2023	2024	2024	Not Yet Launched	Not Yet Launched	358,287	71%	6000	47%	Not Yet Launched	N/A
South Carolina	Individual Tax Credit/Deduction	Refundable Educational Credit for Exceptional Needs Children	2015	2015		624	TBD	105,519	12%	8013	62%	N/A	N/A
South Carolina	Tax-Credit Scholarship	Educational Credit for Exceptional Needs Children Fund	2013	2014		1,460	1%	105,519	12%	2400	19%	102	N/A
South Dakota	Tax-Credit Scholarship	Partners in Education Tax Credit Program	2016	2016		1671	4%	77,596	48%	1972	17%	45	N/A
Tennessee	Education Savings Account	Education Savings Account Program	2019	2021		3578	TBD	826,086	76%	9329	80%	N/A	N/A
Tennessee	Education Savings Account	Individualized Education Account Program	2015	2017		692	TBD	26,101	2%	10709	91%	29	N/A
Utah	Education Savings Account	Utah Fits All Scholarship Program	2023	2024	2024	10,000	TBD	709,787	100%	8000	84%	N/A	Eligibility
Utah	Tax-Credit ESA	Carson Smith Opportunity Scholarship	2024	2024		782	–	–	12%	1891	21%	93	N/A
Utah	Voucher	Carson Smith Opportunity Scholarship Program (Legacy)	2005	2005		1191	TBD	84,143	12%	1891	21%	93	N/A
Vermont	Voucher	Vermont – Town Tuitioning Program	1869	1869		3,541	100%	3,673	4%	16488	66%	140	N/A
Virginia	Tax-Credit Scholarship	Education Improvement Scholarships Tax Credits Program	2012	2013		5,274	TBD	592,751	43%	2918	19%	192	N/A
Washington, D.C.	Voucher	D.C. Opportunity Scholarship Program	2004	2004		1,311	12%	29,236	27%	12967	46%	37	N/A
West Virginia	Education Savings Account	Hope Scholarship Program	2021	2022	2022	11,000	1%	245,626	100%	4299	34%	145	Full
Wisconsin	Individual Tax Credit/Deduction	K–12 Private School Tuition Deduction	2013	2014		31,860	25%	134,527	14%	5407	40%	N/A	N/A
Wisconsin	Voucher	Milwaukee Parental Choice Program	1990	1990		29,732	64%	76,223	80%	10539	72%	136	N/A
Wisconsin	Voucher	Parental Choice Program (Statewide)	2013	2013		21,638	14%	244,532	30%	10501	72%	344	N/A
Wisconsin	Voucher	Parental Private School Choice Program (Racine)	2011	2011		4,185	TBD	16,225	66%	10585	73%	36	N/A
Wisconsin	Voucher	Special Needs Scholarship Program	2015	2016		3,068	<1%	116,182	12%	15065	103%	179	N/A
Wyoming	Education Savings Account	Steamboat Legacy Scholarship Act	2024	2025		Not Yet Launched	Not Yet Launched	19,818	21%	6000	32%	–	N/A

Notes: Data from this table comes mainly from the "School Choice in America Dashboard" from Edchoice (last modified June 2024). Not every voucher program is universal, and all are included here. Blue programs represent policies in our analytic sample (11 states), green programs represent policies that go into effect in 2025 (3 states, excluded from analysis), and orange programs represent policies that Edchoice labels as universal but we do not consider universal. Purple programs have a note explaining why we don't consider them universal under "Year Universal." We consider a voucher program to be universal if the income eligibility threshold is greater than 200% - 250% of the Federal Poverty Line or Federal Free and Reduced Price Lunch, which is why "Year Universal" is not necessarily the year a program was enacted (many programs stage up eligibility over time).

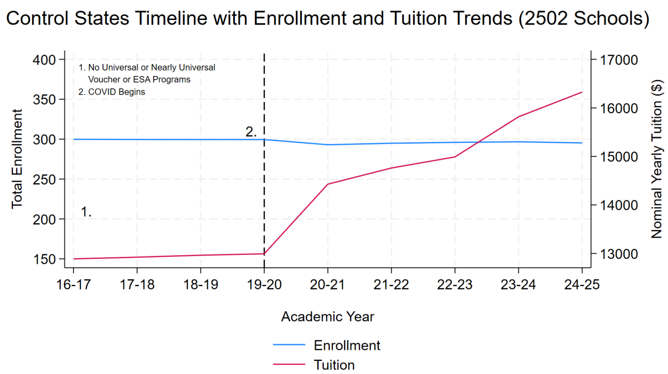
A.2 Simple Trends in Enrollment and Tuition by State

Appendix figures A1a - A2e show simple trends in enrollment and tuition by state. We include graphs for each treated state, organized by treatment year, as well as for all control states combined. These graphs indicate the onset of COVID-19 and treatment years, and also provide brief descriptions of the school-choice environments prior to the universal program.

Constructing these figures requires a choice between using a consistent panel of schools across the period 2016-2024 and reflecting our analytic sample. This is because our analytic sample imposes complete cases tuition 2020-2024 to keep our estimating sample large (going further back continuously cuts down on the number of schools for which we have a full panel in tuition). We opt to use a consistent panel of schools in the graphs below so that trends are not affected by changes in the sample composition over time. For our period of interest (2020-2024), the trends when using our analytic sample are only marginally different. The only major caveat is that these graphs include far less schools than appear in our analytic sample (though trends are similar 2020-2024 regardless of which sample we use). For example, Figure A1a includes 2,502 schools in control states, and our analytic sample includes 3,606 schools in control states.

As described in Section 3, tuition has been rising everywhere (treated and control) since COVID, and enrollment has largely remained flat. When interpreting graphs for individual treated states, remember that all estimations make DD comparisons between treated states and control states, which also see marked increases in tuition throughout 2020-2024. This helps explain why visually large effects on individual treated graphs that may coincide with treatment do not necessarily correspond to large state DD estimates. Note that no group has experienced a decline in tuition since COVID.

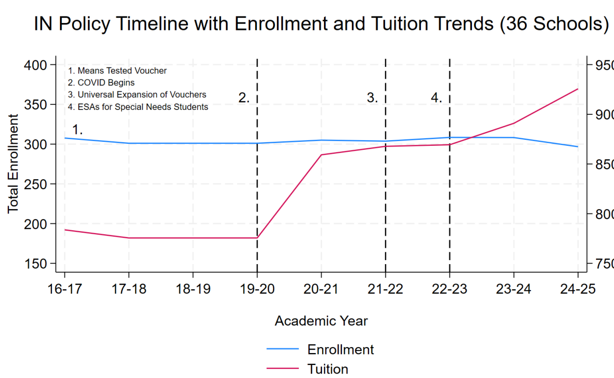
Appendix Figure A1a: Policy Timeline for Control States



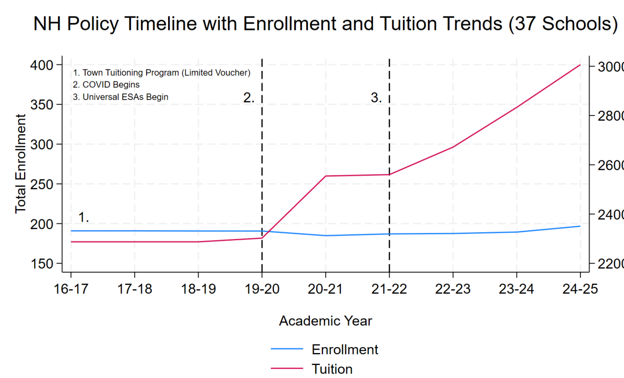
NOTES: Control states include Alaska, California, Colorado, Connecticut, Delaware, District Of Columbia, Georgia, Hawaii, Idaho, Illinois, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Montana, Nebraska, Nevada, New Jersey, New Mexico, New York, North Dakota, Oregon, Pennsylvania, Rhode Island, South Dakota, Texas, Vermont, Virginia, and Washington. This figure reflects schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) with their first non-missing tuition observation before 2018 (complete case tuition 2017). Note that this is NOT the same restriction we make for the analytic sample (for the analytic, we use complete case 2020). We do this to maintain the same sample throughout the timelines. Therefore, the number of schools listed in each timeline is fewer than those included in the analytic sample, however, the trend 2020-2024 is only marginally different when including all schools in the analytic sample (not shown). Note that tuition is in nominal values and is not inflation adjusted.
SOURCE: Private School Review 2015-2024.

Appendix Figure A1b: Policy Timelines for States Treated 2021-22

(a) Indiana



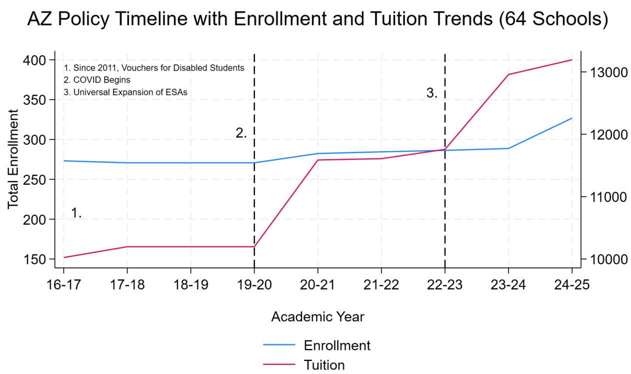
(b) New Hampshire



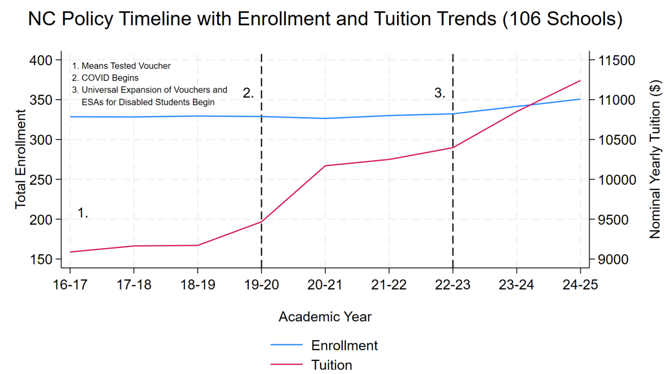
NOTES: These figures reflect schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) with their first non-missing tuition observation before 2018 (complete case tuition 2017). Note that this is NOT the same restriction we make for the analytic sample (for the analytic, we use complete case 2020). We do this to maintain the same sample throughout the timelines. Therefore, the number of schools listed in each timeline is fewer than those included in the analytic sample, however, the trend 2020-2024 is only marginally different when including all schools in the analytic sample (not shown). Note that tuition is in nominal values and is not inflation adjusted.
SOURCE: Private School Review 2015-2024.

Appendix Figure A1c: Policy Timelines for States Treated 2022-23

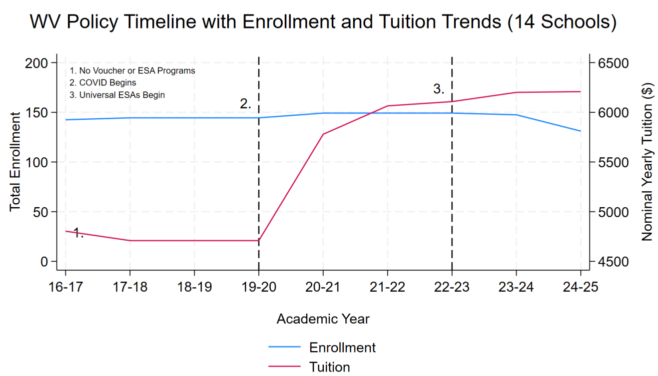
(a) Arizona



(b) North Carolina



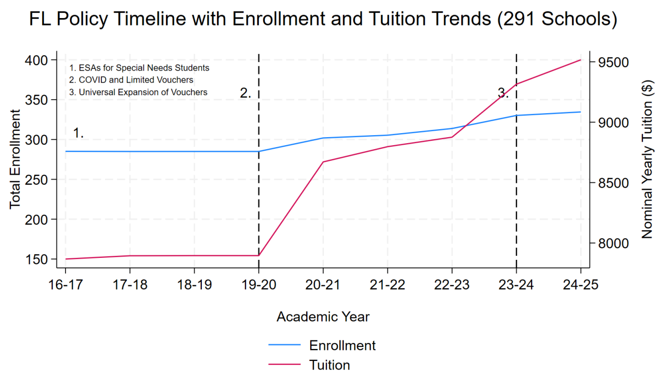
(c) West Virginia



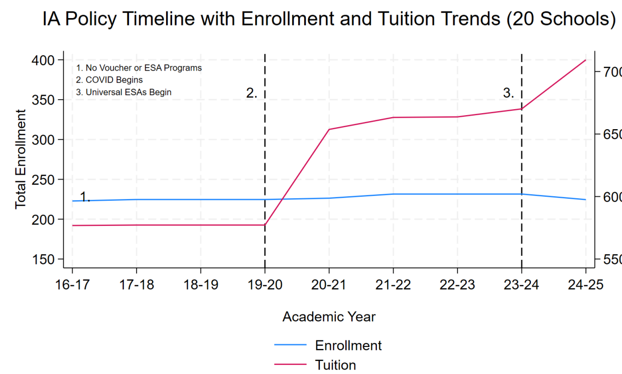
NOTES: These figures reflect schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) with their first non-missing tuition observation before 2018 (complete case tuition 2017). Note that this is NOT the same restriction we make for the analytic sample (for the analytic, we use complete case 2020). We do this to maintain the same sample throughout the timelines. Therefore, the number of schools listed in each timeline is fewer than those included in the analytic sample, however, the trend 2020-2024 is only marginally different when including all schools in the analytic sample (not shown). Note that tuition is in nominal values and is not inflation adjusted.
SOURCE: Private School Review 2015-2024.

Appendix Figure A1d: Policy Timelines for States Treated 2023-24

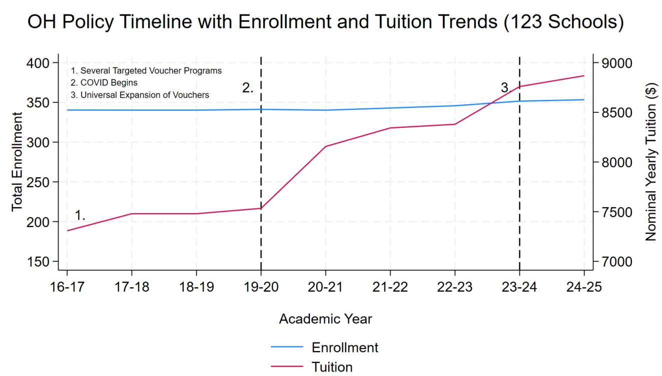
(a) Florida



(b) Iowa



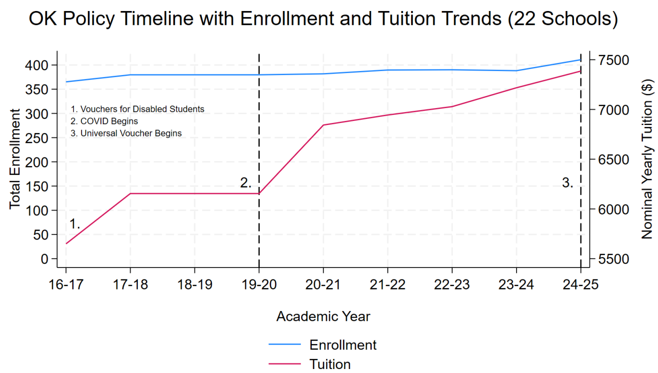
(c) Ohio



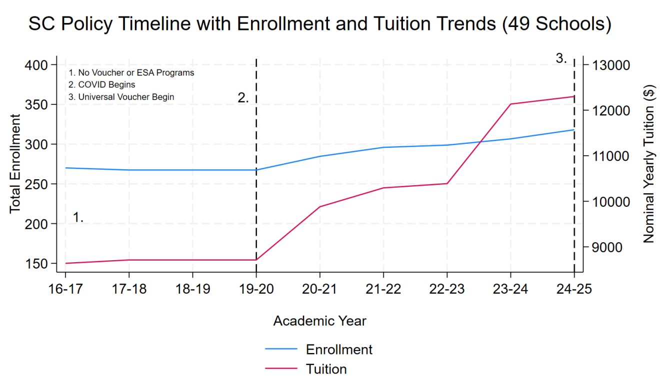
NOTES: These figures reflect schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) with their first non-missing tuition observation before 2018 (complete case tuition 2017). Note that this is NOT the same restriction we make for the analytic sample (for the analytic, we use complete case 2020). We do this to maintain the same sample throughout the timelines. Therefore, the number of schools listed in each timeline is fewer than those included in the analytic sample, however, the trend 2020-2024 is only marginally different when including all schools in the analytic sample (not shown). Note that tuition is in nominal values and is not inflation adjusted.
SOURCE: Private School Review 2015-2024.

Appendix Figure A1e: Policy Timelines for States Treated 2024-25

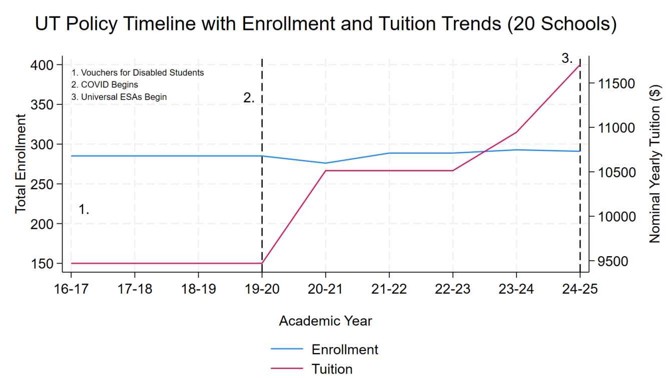
(a) Oklahoma



(b) South Carolina



(c) Utah



NOTES: These figures reflect schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters) with their first non-missing tuition observation before 2018 (complete case tuition 2017). Note that this is NOT the same restriction we make for the analytic sample (for the analytic, we use complete case 2020). We do this to maintain the same sample throughout the timelines. Therefore, the number of schools listed in each timeline is fewer than those included in the analytic sample, however, the trend 2020-2024 is only marginally different when including all schools in the analytic sample (not shown). Note that tuition is in nominal values and is not inflation adjusted.
SOURCE: Private School Review 2015-2024.

B Details on Data Collection and Coding

B.1 NCCS Panel Creation and Payment Schedule Information

The National Center for Charitable Statistics (NCCS), housed within the Urban Institute, includes tax information for non-profit organizations from Form 990 and Form 990EZ filers. Our sample covers filings from 2016 to 2022, although the estimation period is 2019 to 2021. While the NCCS data is likely reported with less error than the PSR, several assumptions must be levied to obtain a panel of total program service revenue aligned with academic years for identifiable K-12 private schools. The first challenge is to identify K-12 private schools among NCCS filers, and the second challenge is to determine a reasonable method for assigning revenue based on tax years to academic years.

We identify K-12 private schools in two ways: matching NCCS filers to schools in the PSS and identifying NCCS filers with NTEE codes that are specific to K-12 schools (B20 indicates elementary and secondary schools, B24 indicates primary and elementary schools, and B25 indicates secondary and high schools). We match NCCS filers to PSS schools based on exact matches for organizational name and zip code, or exact matches for organizational state, address, and zip code. For NCCS filers who have this information, the process is straightforward. We consider an NCCS filer to have a match in the PSS if any 2019 NCCS filing has a match either on exact organizational name and zip code, or exact organizational state, address, and zip code with a PSS school from the 17-18, 19-20, or 21-22 academic years surveys (PSS is released every other year). The NCCS provides an Employer Identification Number (EIN2), which allows us to track filers over time if we identify them as a private school among 2019 NCCS filers. We use 2019 as it is the start of our estimation period, and organizational name and address are not available in subsequent years in the NCCS.

One complication to this process is that if the NCCS labels a filer as a school, through its school indicator, then the filer's organizational name and address are suppressed. The NCCS school indicator has been used since 2012; however, its coverage was very low until 2019, when its use spiked significantly. Ideally, we would rely on the NCCS school indicator to identify schools, but the indicator identifies preschools, K-12 schools, and colleges, with no way to distinguish between groups (we only want to capture K-12 private schools). However, since the NCCS school indicator has low coverage before 2019, and EIN2 allows us to track NCCS filers across years, we can still determine the organizational name and full address for some of these filers. We first collect the EIN2 for each filer that the NCCS indicates as a school 2019-2022. Then, we follow those filers back in time to their 2016 through 2018 NCCS filings, where they may not be indicated as a school (and therefore have organizational name and full address). We then use these NCCS filers' organizational names and full addresses from before they were indicated as a school to match NCCS filers to the PSS in the same way as before.

This process leaves us with two groups: NCCS/PSS matches (both labeled as a school by the NCCS or not) and NCCS filers with K-12-specific NTEE codes. These two groups make up our "All Known NCCS Schools" sample. Note that the NTEE codes allow us to identify many schools that the NCCS/PSS matching could not. Requiring exact matches for NCCS/PSS matches may be stringent, but we are most concerned about minimizing error in identifying K-12 private schools. NCCS/PSS matches make up our second sample. We then construct a third sample, NCCS/PSR matches, which is a subset of the NCCS/PSS matches, as we matched them through existing crosswalks between the PSR analytic sample and the PSS (created with the help of the REACH center). This results in 2,940 "All Known NCCS Schools," 852 "NCCS/PSS Matches," and 440 "NCCS/PSR Matches" in their respective analytic samples. For reference, the analytic sample in PSR national 2020+ estimations contains 4,729 schools. Note that we require enrollment to estimate effects on per-student revenue (our proxy for tuition), which restricts these estimations to the "NCCS/PSS Matches" and "NCCS/PSR Matches" samples. The disagreement between PSR and NCCS tuition results is likely related to differences in samples (see Section IV.D).

After constructing these samples, we must determine a method to assign revenue to academic years when filings cover tax years that do not necessarily coincide. To do this, we must first assume that all revenue comes from tuition payments. It is unclear what revenue is included in NCCS filings, but tuition payments likely make up a large percentage of total revenue. We consider three revenue allocation schemes: "Pre-Pay" assumes tuition payments are made uniformly April-August preceding the Fall academic year, "Installment" assumes that tuition payments are made uniformly August-May of the Fall academic year, and "Tax Year" assumes that an NCCS filing includes the previous Fall academic year's tuition payments. Table B1 and Figure B1 give more information about these revenue allocation schemes. Note that the "Tax Year" schedule is somewhere between the "Pre-Pay" and "Installment" schedules. We choose to focus on the "Pre-Pay" schedule as it is the only schedule that produces reasonable or consistent estimates, but all three were explored. Additionally, "Pre-Pay" likely explains how tuition is paid for a significant number of families, as it represents paying for a school year in the summer before it begins, and should capture down payments and early payments for families that do pay in installments. We allocate revenue to academic years based on the number of "Pre-Pay" months for each academic year a tax filing intersects (see Figure B1 for examples).

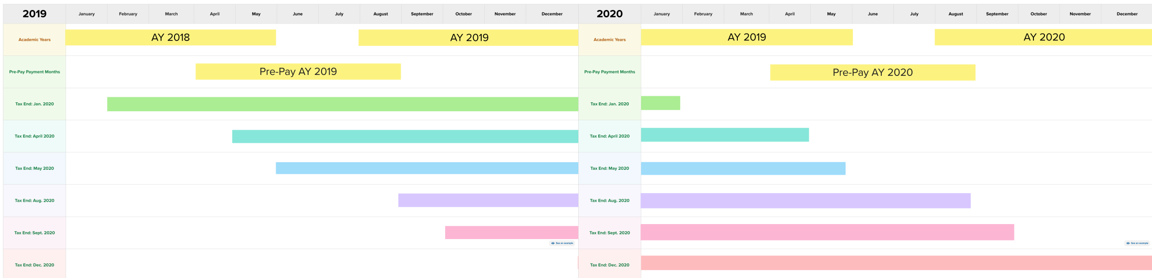
Appendix Table B1: NCCS Revenue Allocation For Tax Year “t” by Payment Schedule

Tax End Month (Year t)	Pre-Pay	Installment	Tax Year
January	AY t-1	Split t-2/t-1	AY t-1
February	AY t-1	Split t-2/t-1	AY t-1
March	AY t-1	Split t-2/t-1	AY t-1
April	Split t-1/t	Split t-2/t-1	AY t-1
May	Split t-1/t	AY t-1	AY t-1
June	Split t-1/t	AY t-1	AY t-1
July	Split t-1/t	AY t-1	AY t-1
August	AY t	Split t-1/t	AY t-1
September	AY t	Split t-1/t	AY t-1
October	AY t	Split t-1/t	AY t-1
November	AY t	Split t-1/t	AY t-1
December	AY t	Split t-1/t	AY t-1

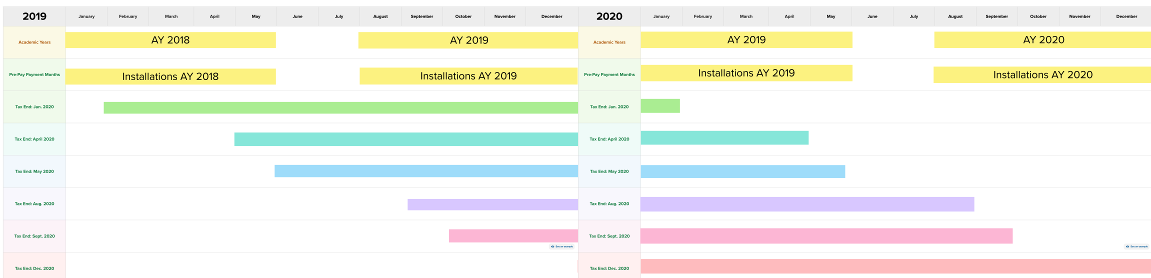
Notes: This table describes how NCCS total program service revenue is allocated to academic years depending on the month the tax year ends. Tax years give revenue over a 12-month period, so they may intersect with different payment windows depending on the payment schedule assumption. The leftmost column gives different months that tax year “t” could end. Entries show which academic year total program service revenue should be allocated based on each payment schedule and tax end month. Cells containing ”AY” indicate that those filings would only intersect one academic year’s payment window (given the payment schedule assumption). Cells containing ”Split” indicate that those filings would intersect two academic years’ payment windows (given the payment schedule assumption). Revenue is split into different academic years by the proportion of payment months covered from each academic year. Notice that “AY t” only appears for the “Pre-Pay” schedule, which shows why effects in 2022 are only estimable for “Pre-Pay” as the last NCCS tax year is 2022.

Appendix Figure B1: NCCS Payment Schedules

(a) Pre-Pay



(b) Installment



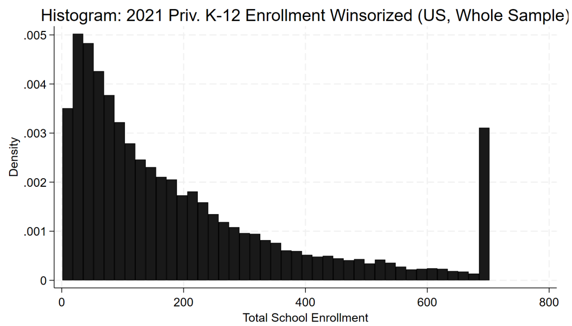
NOTES: These figures reflect the assumed academic year payment schedules for NCCS estimations. "Pre-Pay" assumes that all payments are made uniformly April-August preceding the academic year. "Installment" assumes that payments are made uniformly August-May. The last schedule, "Tax Year" is not shown here as "Tax Year" schedule assumes that an NCCS filing refers to the academic year before the filing year (should be in between the "Pre-Pay" and "Installment" schedules).

C Additional Descriptive Statistics

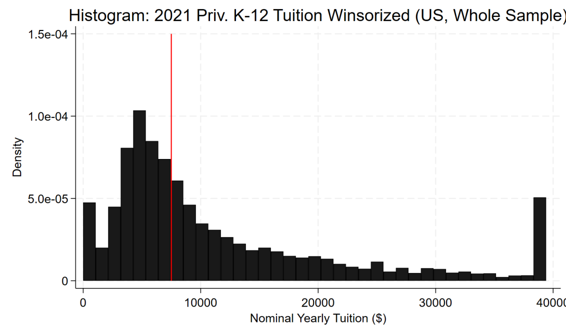
C.1 All Frequent Updaters in 2021

Appendix Figure C1: Histograms: National Enrollment and Tuition (Whole Sample of Frequent Updaters)

(a) Enrollment



(b) Tuition ($)

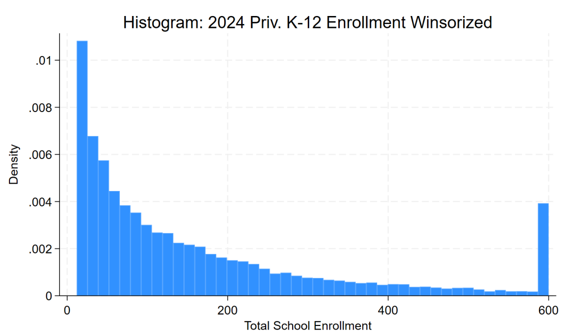


NOTES: This sample includes schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters). No tuition or enrollment restrictions were levied in this case, and these histograms reflect the national distributions in 2021 for frequent updaters. We use 2021 as the base year, as it is the first year that universal vouchers were implemented. All treated states from Table 3 are included in these figures. States with pending universal voucher programs are excluded from these figures. Additionally, Tennessee, Wisconsin, and Wyoming are also excluded from these figures due to the nature of their voucher programs. Note that tuition is in nominal values and is not inflation-adjusted.
SOURCE: Private School Review 2015-2024.

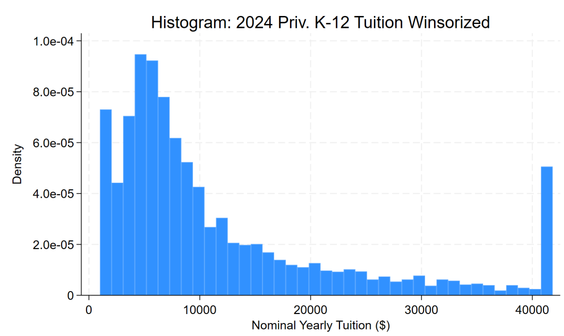
C.2 All Private Schools in 2024

Appendix Figure C2: Histograms: National Enrollment and Tuition (All 2024 Open K-12 PSR Schools)

(a) Enrollment



(b) Tuition ($)



NOTES: This sample includes all schools that report either tuition or enrollment in 2024 only. Both enrollment and tuition are Winsorized at the 5% and 95% levels. Notice that this sample does not rely on frequent updaters or any temporal restriction we have used in the paper to this point. These histograms aim to provide a snapshot of the entire private school sector in 2024 as captured by the PSR.
SOURCE: Private School Review 2024.

Appendix Table C1: Listed Religion of All Open K-12 Private Schools in 2024

Religion	Frequency	Percent of Schools Reporting Religion
African Methodist Episcopal	21	0.07
Amish	992	3.31
Anglican	1	0
Assembly of God	250	0.83
Baptist	1,828	6.09
Brethren	53	0.18
Calvinist	73	0.24
Catholic	6,334	21.11
Christian	4,232	14.1
Church of Christ	109	0.36
Church of God	75	0.25
Church of God in Christ	20	0.07
Church of the Nazarene	60	0.2
Disciples of Christ	12	0.04
Episcopal	371	1.24
Evangelical Lutheran Church in America (formerly AELC, ALC, or LCA)	122	0.41
Friends	70	0.23
Greek Orthodox	28	0.09
Islamic	294	0.98
Jewish	935	3.12
Latter Day Saints	14	0.05
Lutheran Church Missouri Synod	971	3.24
Mennonite	390	1.3
Methodist	279	0.93
Non-Sectarian	10,186	33.94
Other	579	1.93
Other Lutheran	77	0.26
Pentecostal	305	1.02
Presbyterian	217	0.72
Seventh Day Adventist	774	2.58
Wisconsin Evangelical Lutheran Synod	338	1.13
Total	30,010	100

Notes: This sample includes all schools that report religion in 2024 only. Notice that this sample does not rely on frequent updaters or any temporal restriction we have used in the paper to this point. This table aims to provide a snapshot of the entire private school sector in 2024 as captured by the PSR.

SOURCE: Private School Review 2024.

D Robustness Checks

D.1 Synthetic DD Results for PSR

Appendix Table D1: Synthetic DD Results (all asinh outcomes by Treated State, Complete Cases Tuition)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Staggered SDID Coef.	0.005	0.009	0.005	0.012
(se)	(0.010)	(0.017)	(0.004)	(0.014)
P.T. Mean	10,098.62	274.15	0.31	4,839.32
N	225	225	225	225
Treated 2021-22				
IN SDID Coef.	-0.005	-0.003	0.012	-0.015
	(0.022)	(0.034)	(0.010)	(0.036)
	7,673.27	312.48	0.49	14,699.75
	175	175	175	175
NH SDID Coef.	0.002	0.012	-0.003	0.016
	(0.022)	(0.035)	(0.008)	(0.038)
	21,413.34	194.90	0.35	18,566.65
	175	175	175	175
Treated 2022-23				
AZ SDID Coef.	0.054**	0.044	0.013	0.063**
	(0.020)	(0.043)	(0.008)	(0.024)
	10,375.64	237.83	0.49	6,409.38
	175	175	175	175
NC SDID Coef.	0.018	0.040	0.007	0.034
	(0.021)	(0.038)	(0.006)	(0.027)
	10,627.06	316.03	0.25	5,528.25
	175	175	175	175
WV SDID Coef.	-0.014	-0.065	-0.003	-0.022
	(0.023)	(0.042)	(0.005)	(0.029)
	6,186.50	160.67	0.16	503.75
	175	175	175	175

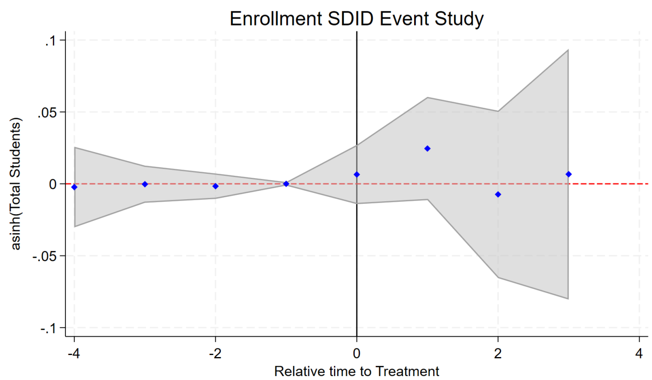
Appendix Table D1 (Continued): Synthetic DD Results (all asinh outcomes by Treated State, Complete Cases Tuition)

	ln(Tuition)	ln(Students)	ln(% on Aid)	ln(Avg. Aid)
Treated 2023-24				
FL SDID Coef.	-0.032	0.023	0.006	0.026
(se)	(0.029)	(0.055)	(0.006)	(0.039)
P.T. Mean	9,276.77	309.45	0.39	4,832.32
N	175	175	175	175
IA SDID Coef.	0.021	0.039	-0.002	-0.045
	(0.028)	(0.069)	(0.005)	(0.031)
	5,506.01	198.49	0.40	1,285.42
	175	175	175	175
OH SDID Coef.	0.002	0.010	0.019**	0.087*
	(0.031)	(0.045)	(0.006)	(0.038)
	8,246.58	328.26	0.42	3,761.16
	175	175	175	175
Treated 2024-25				
OK SDID Coef.	-0.016	0.012	0.013	-0.007
	(0.045)	(0.047)	(0.007)	(0.054)
	6,789.62	344.34	0.21	2,525.96
	175	175	175	175
SC SDID Coef.	-0.002	0.022	-0.002	-0.019
	(0.023)	(0.047)	(0.010)	(0.060)
	9,329.49	280.39	0.26	3,295.31
	175	175	175	175
UT SDID Coef.	-0.007	-0.032	-0.010	-0.001
	(0.042)	(0.058)	(0.009)	(0.044)
	18,957.59	251.60	0.23	7,317.38
	175	175	175	175

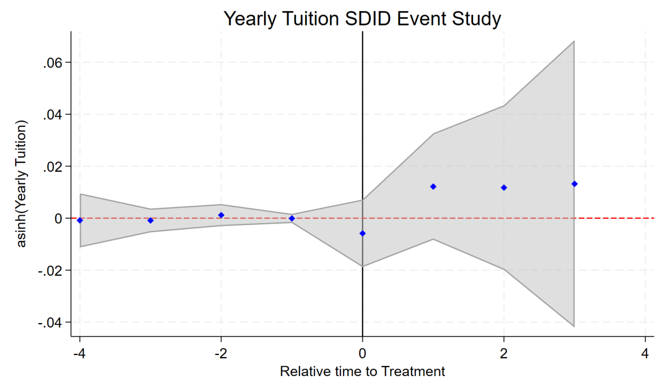
Notes "SDID" refers to synthetic difference-in-differences. Note that using SDID specifications requires us to aggregate data to the treatment level (state level), which decreases the number of observations. Entries are coefficients, standard errors, the level pre-treatment mean for treated states, and the number of observations. SDID estimations follow Arkhangelsky et al. (2021), and staggered SDID estimations use only never-treated as controls. Standard errors are calculated using placebos. The first row gives staggered SDID estimates using all treated states from Table 3a. Each subsequent row gives the SDID coefficients for all outcomes counting only 1 state as treated (abbreviation of treated state comes before "SDID Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". Estimations use academic years after 2019 and exclude states with other current or pending universal voucher programs. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only balanced panels with complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies for the first row of estimates are provided.

Appendix Figure D1: National Synthetic DiD Event Studies for All asinh Outcomes

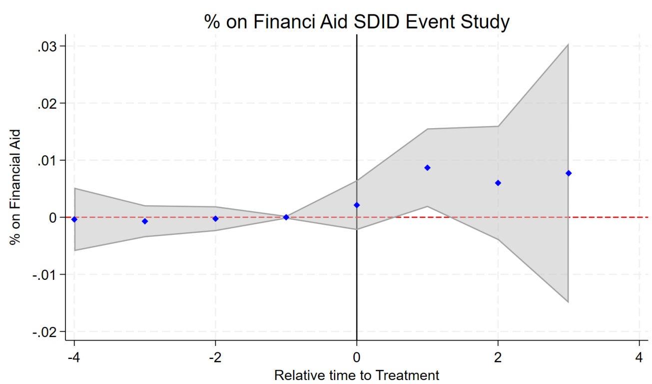
(a) Total Enrollment



(b) Nominal Yearly Tuition ($)

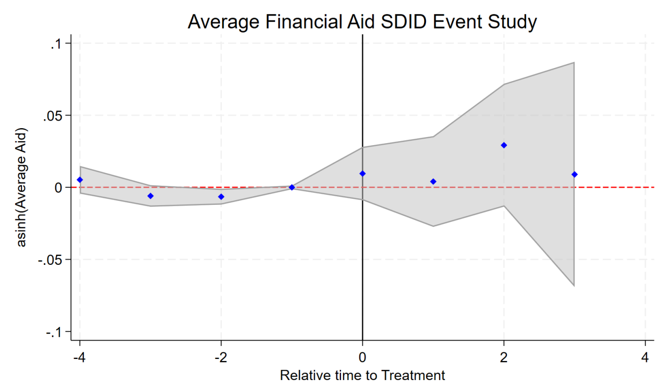


(c) % on Financial Aid



(d) Nominal Average Financial Aid ($)

=



NOTES: These figures reflect SDID event study estimates using schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters). Additionally, included schools must have their first non-missing tuition observation before 2021 (complete case tuition). All treated states from Table 3 are included in this staggered DD estimation. Note that tuition and average financial aid are in nominal values and are not inflation adjusted. Time “0” refers to the first treatment period. These event studies correspond to the estimates from the first row of Appendix Table 2.

SOURCE: Private School Review 2015-2024.

D.2 Using Poisson Regression instead of asinh

Appendix Table D2: State Poisson DD ATE Results (All Level Outcomes by Treated State, Complete Cases Tuition)

	Tuition	Students	% on Aid	Avg. Aid
Staggered TWFE DD Coef.	-0.004	0.040***	0.018**	0.013
(se)	(0.007)	(0.008)	(0.006)	(0.014)
P.T. Mean	9,580.46	297.40	0.36	5,073.45
N	23,620	23,325	12,930	8,120
Treated 2021-22				
IN DD Coef.	-0.023***	0.003	0.012***	-0.046***
	(0.004)	(0.004)	(0.004)	(0.005)
	7,673.27	312.48	0.49	14,699.75
	18,320	18,100	10,325	6,510
NH DD Coef.	0.007	0.026***	-0.020***	0.008
	(0.004)	(0.004)	(0.004)	(0.005)
	21,413.34	194.90	0.35	18,566.65
	18,245	18,025	10,260	6,455
Treated 2022-23				
AZ DD Coef.	0.018***	0.052***	0.018***	0.031***
	(0.004)	(0.005)	(0.003)	(0.006)
	10,375.64	240.73	0.49	6,409.38
	18,445	18,210	10,385	6,530
NC DD Coef.	-0.020***	0.044***	0.020***	0.036***
	(0.004)	(0.005)	(0.003)	(0.006)
	10,627.06	316.03	0.25	5,528.25
	18,770	18,545	10,520	6,640
WV DD Coef.	-0.034***	-0.018***	-0.016***	-0.054***
	(0.004)	(0.005)	(0.003)	(0.006)
	6,186.50	160.67	0.16	503.75
	18,075	17,855	10,145	6,390

Appendix Table D2 (Continued): State Poisson DD ATE Results (All Level Outcomes by Treated State, Complete Cases Tuition)

	Tuition	Students	% on Aid	Avg. Aid
Treated 2023-24				
FL DD Coef.	0.003	0.054***	0.015***	-0.003
(se)	(0.005)	(0.006)	(0.003)	(0.008)
P.T. Mean	9,276.77	309.45	0.39	4,832.32
N	19,955	19,700	11,035	6,915
IA DD Coef.	-0.002	0.019***	-0.014***	0.132***
	(0.005)	(0.006)	(0.003)	(0.008)
	5,506.01	198.49	0.40	1,285.42
	18,170	17,950	10,200	6,410
OH DD Coef.	-0.026***	0.031***	0.039***	0.083***
	(0.005)	(0.006)	(0.003)	(0.008)
	8,246.58	328.26	0.42	3,761.16
	18,835	18,610	10,620	6,660
Treated 2024-25				
OK DD Coef.	-0.028***	0.047***	0.053***	-0.025**
	(0.006)	(0.006)	(0.005)	(0.008)
	6,789.62	344.34	0.21	2,525.96
	18,145	17,920	10,225	6,440
SC DD Coef.	0.042***	0.072***	-0.012*	-0.050***
	(0.006)	(0.006)	(0.005)	(0.008)
	9,329.49	280.39	0.26	3,295.31
	18,380	18,160	10,310	6,460
UT DD Coef.	-0.000	0.013*	-0.003	0.012
	(0.006)	(0.006)	(0.005)	(0.008)
	18,957.59	251.60	0.23	7,317.38
	18,130	17,910	10,205	6,410

Notes Entries are coefficients, standard errors, level pre-treatment mean for treated states, and number of observations. Standard errors are clustered at the state level. Coefficients come from the TWFE DD specification, but with a Poisson regression (ppmlhdfe). Observation numbers may be slightly less than in Table 3a due to how ppmlhdfe deals with separated values. Each row gives the TWFE DD coefficients for all outcomes counting only 1 state as treated (abbreviation of treated state comes before "DD Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". Estimations use academic years after 2019 and exclude states with other current or pending universal voucher programs. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies are provided for the first row of estimates.

Appendix Table D3: State Poisson DD ATE Results (All Level Outcomes by Treated State, Complete Cases Tuition)
Later Treated States with Only 1 Pre-Period

	Tuition	Students	% on Aid	Avg. Aid
Treated 2022-23				
AZ DD Coef.	0.024***	0.051***	0.018***	0.043***
(se)	(0.004)	(0.005)	(0.003)	(0.006)
P.T. Mean	10,418.61	241.83	0.49	6,385.94
N	14,756	14,568	8,308	5,224
NC DD Coef.	-0.005	0.042***	0.023***	0.029***
	(0.004)	(0.005)	(0.003)	(0.006)
	10,577.40	317.96	0.25	5,615.56
	15,016	14,836	8,416	5,312
WV DD Coef.	-0.041***	-0.014**	-0.014***	-0.045***
	(0.004)	(0.005)	(0.003)	(0.006)
	6,297.61	160.67	0.16	503.75
	14,460	14,284	8,116	5,112
Treated 2023-24				
FL DD Coef.	0.007	0.039***	0.010***	0.009
	(0.005)	(0.004)	(0.003)	(0.007)
	9,407.01	315.65	0.39	4,834.76
	11,973	11,820	6,621	4,149
IA DD Coef.	0.008	0.016***	-0.009***	-0.006
	(0.005)	(0.004)	(0.003)	(0.007)
	5,549.08	200.00	0.40	1,493.75
	10,902	10,770	6,120	3,846
OH DD Coef.	-0.014**	0.025***	0.039***	0.071***
	(0.005)	(0.004)	(0.003)	(0.007)
	8,297.96	331.97	0.42	3,853.69
	11,301	11,166	6,372	3,996
Treated 2024-25				
OK DD Coef.	-0.013***	0.043***	0.059***	0.001
	(0.003)	(0.003)	(0.005)	(0.005)
	7,047.34	347.06	0.21	2,549.57
	7,258	7,168	4,090	2,576
SC DD Coef.	-0.015***	0.037***	-0.008	-0.035***
	(0.003)	(0.003)	(0.005)	(0.005)
	10,398.58	291.53	0.26	3,363.44
	7,352	7,264	4,124	2,584
UT DD Coef.	0.032***	-0.002	-0.022***	0.047***
	(0.003)	(0.003)	(0.005)	(0.005)
	19,328.97	256.41	0.23	7,317.38
	7,252	7,164	4,082	2,564

Notes Entries are coefficients, standard errors, level pre-treatment mean for treated states, and number of observations. Standard errors are clustered at the state level. Coefficients come from the TWFE DD specification, but with a Poisson regression (ppmlhdfe). Observation numbers may be slightly less than in Table 3b due to how ppmlhdfe deals with separated values. Estimates are similar to the final 3 panels of Appendix Table 7a/7a (Continued) but use only one preperiod. Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". States with other current or pending universal voucher programs are excluded in each estimation. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted.

Appendix Table D4: Total Program Service Revenue Poisson DD Results for Pre-Pay Schedule

	Revenue DD	Per-Student Revenue (CF) DD	Per-Student Revenue (Lin.) DD
All Known NCCS Schools			
DD Coef. 19-21	0.014		
(se)	(0.031)		
P.T. Mean	3,676,776		
N	7,101		
NCCS/PSS Matches			
DD Coef. 19-21	0.032	0.198***	0.154***
	(0.031)	(0.060)	(0.048)
	4,790,565	127,736	129,307
	2,196	2,196	2,196
NCCS/PSR Matches			
DD Coef. 19-21	0.093***	0.239*	0.191**
	(0.015)	(0.105)	(0.068)
	5,846,600	216,973	217,318
	1,149	1,149	1,149

Notes: From the top, entries are DD or SDID coefficient estimates, standard errors, level pre-treatment mean for treated units, and number of observations. Coefficients come from the TWFE DD specification, but with a Poisson regression (ppmlhdfe). Observation numbers may be slightly less than in Table 4 due to how ppmlhdfe deals with separated values. The outcome for this regression is either school-level total program service revenue or per-student school-level total program service revenue, taken from the National Center for Charitable Statistics (NCCS), and treatment is the implementation of a universal school voucher program ($>$ 200% FPL or FRPL). In order to estimate on per-student revenue ("PS Revenue"), we need enrollment data, which is why per-student revenue estimates are only available for observations with a PSS match (enrollment data comes from the PSS). In all cases, per-student revenue is calculated based on NCES PSS enrollment. NCES PSS enrollment is available every other year, so we must either assume that enrollment stays the same between observations ("CF") or that it changes linearly between periods ("Lin."). "All NCCS K-12 Schools" refers to all NCCS observations with an organizational name and/or address match in the NCES Private School Survey (PSS) as well as additional NCCS observations identified with K-12-specific NTEE codes (B20, B24, B25). "NCCS/PSS Matches" refer to all NCCS observations with an organizational name and/or address match in the PSS. "NCCS/PSR Matches refer to all NCCS observations with a match in the Private School Review sample (notice that this is a subset of NCCS/PSS matches as NCCS/PSR matches were found via PSS ppin). We estimate over the period 2019-2021 as we want to keep periods consistent between PSR and NCCS estimations, but the latest full NCCS dataset is for 2021, so we expand to 2019. States with other pending universal voucher programs are excluded. Standard errors are clustered at the state level. Note that the only treated units in these estimations are Indiana and New Hampshire. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted.
SOURCE: Private School Review 2015-2024, National Center for Charitable Statistics 2019-2021, and National Center for Education Statistics Private School Survey 2019-2021.

D.3 Adding State-Specific Linear Trends

Appendix Table D5: State DD ATE Results with State-Specific Linear Time Trends (All Level Outcomes by Treated State, Complete Cases Tuition)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Staggered TWFE DD Coef.	-0.015	0.009	-0.000	0.013
(se)	(0.008)	(0.008)	(0.001)	(0.010)
P.T. Mean	9,580.46	297.40	0.36	5,073.45
N	23,645	23,325	12,930	8,120
Treated 2021-22				
IN DD Coef.	-0.022***	-0.023***	-0.001	0.004
	(0.003)	(0.003)	(0.001)	(0.005)
	7,673.27	312.48	0.49	14,699.75
	18,345	18,100	10,325	6,510
NH DD Coef.	-0.019***	0.014***	0.000	-0.040***
	(0.003)	(0.003)	(0.001)	(0.005)
	21,413.34	194.90	0.35	18,566.65
	18,270	18,025	10,260	6,455
Treated 2022-23				
AZ DD Coef.	0.035***	-0.043***	0.005**	-0.008*
	(0.003)	(0.004)	(0.001)	(0.004)
	10,375.64	240.73	0.49	6,409.38
	18,470	18,210	10,385	6,530
Pretrend F	4.04	1.53	0.27	4.53
Pretrend p	0.04455	0.21606	0.60613	0.03334
NC DD Coef.	-0.015***	-0.025***	0.001	-0.010*
	(0.003)	(0.004)	(0.001)	(0.004)
	10,627.06	316.03	0.25	5,528.25
	18,795	18,545	10,520	6,640
	6.31	2.56	0.11	0.84
	0.01201	0.10945	0.74324	0.36064
WV DD Coef.	-0.014***	0.091***	0.004**	0.013***
	(0.003)	(0.004)	(0.001)	(0.004)
	6,186.50	160.67	0.16	503.75
	18,100	17,855	10,145	6,390
	0.17	2.40	8.49	5.41
	0.67584	0.12122	0.00357	0.02005

Appendix Table D5 (Continued): State DD ATE Results with State-Specific Linear Time Trends (All Level Outcomes by Treated State, Complete Cases Tuition)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Treated 2023-24				
FL DD Coef.	-0.033***	0.026***	-0.001*	0.027***
	(0.003)	(0.004)	(0.001)	(0.005)
P.T. Mean	9,276.77	309.45	0.39	4,832.32
N	19,980	19,700	11,035	6,915
Pretrend F	0.72	3.37	1.60	0.01
Pretrend p	0.39469	0.06658	0.20552	0.92142
IA DD Coef.	-0.025***	0.030***	0.001*	0.026***
	(0.003)	(0.004)	(0.001)	(0.005)
	5,506.01	198.49	0.40	1,285.42
	18,195	17,950	10,200	6,410
	5.20	1.05	0.93	1.08
	0.02266	0.30648	0.33523	0.29935
OH DD Coef.	0.007*	0.027***	-0.002**	0.071***
	(0.003)	(0.004)	(0.001)	(0.005)
	8,246.58	328.26	0.42	3,761.16
	18,860	18,610	10,620	6,660
	0.23	0.26	5.39	0.82
	0.63525	0.61205	0.02027	0.36578
Treated 2024-25				
OK DD Coef.	-0.010**	0.017***	0.012***	-0.001
	(0.003)	(0.004)	(0.002)	(0.004)
	6,789.62	344.34	0.21	2,525.96
	18,170	17,920	10,225	6,440
SC DD Coef.	-0.019***	0.029***	-0.003	-0.012**
	(0.003)	(0.004)	(0.002)	(0.004)
	9,329.49	280.39	0.26	3,295.31
	18,405	18,160	10,310	6,460
UT DD Coef.	0.007*	-0.020***	-0.011***	0.007
	(0.003)	(0.004)	(0.002)	(0.004)
	18,957.59	251.60	0.23	7,317.38
	18,155	17,910	10,205	6,410

Notes Entries are coefficients, standard errors, level pre-treatment mean for treated states, and number of observations. Standard errors are clustered at the state level with degrees of freedom adjusted as in Bell and McCaffrey (2002). The first row gives staggered TWFE DD estimates with state-specific linear time trends using all treated states from the remainder of Table 3a. Each subsequent row gives the TWFE DD coefficients with state-specific linear time trends for all outcomes counting only 1 state as treated (abbreviation of treated state comes before "DD Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". Estimations use academic years after 2019 and exclude states with other current or pending universal voucher programs. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies are provided for the first row of estimates.

D.4 Frequency Weights And Results with No Tuition Restrictions

Appendix Table D6: Robustness of Estimates to Majority K-12 Schools and Enrollment Frequency Weights)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+ Base Coef.	-0.0026	0.0345***	0.0087*	0.0247*
(se)	(0.0116)	(0.0071)	(0.0035)	(0.0108)
CSDID 2020+ Majority K-12 Coef.	-0.0017	0.0335***	0.0088*	0.0247*
	(0.0090)	(0.0071)	(0.0035)	(0.0108)
TWFE DD Base Coef.	-0.009	0.040***	0.008**	0.022
	(0.012)	(0.007)	(0.003)	(0.013)
TWFE DD Enrollment-Weighted Coef.	-0.001	0.038***	0.006	0.013
	(0.003)	(0.007)	(0.004)	(0.008)

Notes: Entries are coefficients followed by their standard errors. Standard errors are clustered at the state level. "Base" estimates come from the CSDID and TWFE specifications found in Panel A of Table 5 and represent the baseline specifications. "Majority K-12" refers to restricting the sample to schools with the majority of their grades being K-12 (any school that begins in preschool is required to at least have grades K-2nd). Note that exclusive preschools are already excluded from all estimations. Our base estimates can be thought of as a school-level effect as the unit of analysis is the school. To get at more of a student-level effect, we use frequency weights with school enrollment in the estimation (we turn to the TWFE DD Base specification here as frequency weights are incompatible with CSDID). The agreement between all of these estimates helps show that early-childhood-focused and small schools are not driving the effect of interest. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies are provided for the first row of estimates.

Appendix Table D7: ATE Estimates for Entire Sample (No Tuition Restriction on Non-Tuition Outcomes)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+ Coef.	-0.0026	0.0211***	0.0067*	0.0199
(se)	(0.0116)	(0.0052)	(0.0032)	(0.0108)
N	23,645	69,810	15,390	8,880
SDID 2020+ Coef.	0.0046	0.0047	0.0039	0.0074
	(0.0097)	(0.0062)	(0.0028)	(0.0116)
	225	225	225	225

Notes: Entries are either CSDID or SDID coefficients, standard errors, and number of observations. Standard errors are clustered at the state level for CSDID estimates and are calculated with placebos in the SDID estimates. SDID estimates require aggregation to the state level (same level as treatment). All other PSR estimations in this paper keep the analytic sample consistent, requiring all included schools to be frequent updaters and have their first non-missing tuition observation in or before 2020. Here, we do not make the same restriction, and the sample for each outcome only requires they have valid entries for their outcome only 2020-2024 (i.e., asinh(Students) estimates only require schools have non-missing enrollment data without levying any restriction on their tuition data). Note that the first column recreates estimates from Table 3a and Appendix Table 2 by construction. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies are provided for the first row of estimates.

D.5 Adding Non-Response Weights

Table D8: Average Treatment on Treated (ATT) Effects of Vouchers - Dichotomous Treatment with Non-Response Weights (PSR Data)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+	0.0007	0.0348***	0.0077**	0.0255
(se)	(0.0113)	(0.0084)	(0.0030)	(0.0136)
P.T.Mean	9,580.46	297.40	0.36	5,073.45
N	23,645	23,325	12,930	8,120
Pretrend χ^2	27.93	42.17	8.55	43.92
Pretrend p	0.00010	0.00000	0.20019	0.00000
CSDID 2021+	0.0016	0.0414***	0.0087*	0.0344*
	(0.0146)	(0.0077)	(0.0035)	(0.0162)
	9,477.21	301.81	0.36	4,441.60
	18,440	18,184	10,084	6,316
	3.50	18.84	1.52	9.62
	0.32073	0.00030	0.67676	0.02205
Treated 2021-22				
IN DD Coef.	0.001	0.011*	0.009***	-0.003
	(0.004)	(0.005)	(0.001)	(0.005)
	7,673.27	312.48	0.49	14,699.75
	18,345	18,100	10,325	6,510
NH DD Coef.	-0.009*	0.015**	-0.005***	-0.009
	(0.004)	(0.005)	(0.001)	(0.005)
	21,413.34	194.90	0.35	18,566.65
	18,270	18,025	10,260	6,455
Treated 2022-23				
AZ DD Coef.	0.061***	0.077***	0.009***	0.041***
	(0.004)	(0.006)	(0.001)	(0.005)
	10,375.64	240.73	0.49	6,409.38
	18,470	18,210	10,385	6,530
Pretrend F	11.00	17.63	7.71	20.30
	0.00218	0.00018	0.00888	0.00007
NC DD Coef.	0.011**	0.068***	0.005***	0.059***
	(0.004)	(0.006)	(0.001)	(0.005)
	10,627.06	316.03	0.25	5,528.25
	18,795	18,545	10,520	6,640
	21.31	132.54	16.27	43.92
	0.00005	0.00000	0.00029	0.00000
WV DD Coef.	-0.022***	-0.058***	-0.004***	-0.027***
	(0.004)	(0.006)	(0.001)	(0.005)
	6,186.50	160.67	0.16	503.75
	18,100	17,855	10,145	6,390
	14.53	12.97	1.71	4.62
	0.00055	0.00100	0.20033	0.03883

Table D8 (Cont.): Average Treatment on Treated (ATT) Effects of Vouchers - Dichotomous Treatment with Non-Response Weights (PSR Data)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Treated 2023-24				
FL DD Coef.	-0.030***	0.047***	0.008***	0.004
(se)	(0.004)	(0.007)	(0.001)	(0.006)
P.T. Mean	9,276.77	309.45	0.39	4,832.32
N	19,980	19,700	11,035	6,915
Pretrend F	1.36	0.81	9.36	9.93
Pretrend p	0.25151	0.37559	0.00431	0.00339
IA DD Coef.	0.026***	0.053***	-0.003**	0.021***
	(0.004)	(0.007)	(0.001)	(0.006)
	5,506.01	198.49	0.40	1,285.42
	18,195	17,950	10,200	6,410
	0.02	19.75	3.03	604.89
	0.89562	0.00009	0.09071	0.00000
OH DD Coef.	0.003	0.033***	0.029***	0.126***
	(0.004)	(0.007)	(0.001)	(0.006)
	8,246.58	328.26	0.42	3,761.16
	18,860	18,610	10,620	6,660
	2.59	0.08	0.17	13.93
	0.11684	0.78340	0.68685	0.00069
Treated 2024-25				
OK DD Coef.	-0.016***	0.023**	0.006***	-0.021***
	(0.004)	(0.008)	(0.001)	(0.006)
	6,789.62	344.34	0.21	2,525.96
	18,170	17,920	10,225	6,440
	1.27	0.13	1.68	21.44
	0.26733	0.71703	0.20327	0.00005
SC DD Coef.	0.023***	0.070***	-0.003*	-0.025***
	(0.004)	(0.008)	(0.001)	(0.006)
	9,329.49	280.39	0.26	3,295.31
	18,405	18,160	10,310	6,460
	49.85	48.68	49.27	11.51
	0.00000	0.00000	0.00000	0.00177
UT DD Coef.	-0.018***	-0.014	-0.003	-0.026***
	(0.004)	(0.008)	(0.001)	(0.006)
	18,957.59	251.60	0.23	7,317.38
	18,155	17,910	10,205	6,410
	24.38	1.30	10.62	29.71
	0.00002	0.26129	0.00255	0.00000

Notes Entries are coefficients, standard errors, level pre-treatment means for treated states, the number of observations, pretrend statistic (χ^2 for CSDID and F for DD), and pretrend p-value. Weights are pweights and are calculated by dividing the number of schools in each enrollment bin (0-100, 101-200, 201-300, and 300+) in the PSR population sample (excluding other u-vouchers, TN, and WI) by the number of schools in each enrollment bin from each outcome's analytic sample (bin calculations at both levels are done within states). CSDID estimations use both never-treated and not-yet-treated units as controls. The first two rows give CSDID estimates using all treated states from the remainder of the table over two time periods: 2020-2024 and 2021-2024. Each subsequent row gives the TWFE DD coefficients for all outcomes, counting only one state as treated (abbreviation of treated state comes before "DD Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". Estimations use academic years after 2019 and exclude states with other current or pending universal voucher programs enacted before October 2024. Additionally, Tennessee and Wisconsin are excluded from the sample due to the nature of their voucher programs (geographically constrained). Data comes from the PSR frequent updaters impute forward dataset (at least 3 Wayback Machine updates 2016-2022). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Note that tuition and average financial aid are nominal values and are not inflation-adjusted.

D.6 Restricting Sample to Reasonable Changes and No Observations of Zero

Table D9: CSDID and DD ATE Results (All asinh Outcomes by Treated State, Complete Cases Tuition, Winsorized by State (5%, 95%), Reasonable Ranges)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
CSDID 2020+	-0.0036	0.0206***	0.0029*	0.0048
(se)	(0.0034)	(0.0063)	(0.0014)	(0.0042)
P.T. Mean	9,453.59	305.59	0.37	5,097.96
N	22,510	21,750	12,240	7,615
Pretrend χ^2	33.21	55.84	82.22	30.95
Pretrend p	0.00001	0.00000	0.00000	0.00003
CSDID 2021+	-0.0026	0.0252***	0.0035*	0.0067
	(0.0038)	(0.0058)	(0.0017)	(0.0046)
	9,299.88	308.95	0.36	4,442.51
	17,556	16,960	9,540	5,924
	18.22	49.13	30.64	1.77
	0.00040	0.00000	0.00000	0.62091
Treated 2021-22				
IN DD Coef.	-0.013***	0.003	0.004***	-0.008***
	(0.002)	(0.003)	(0.001)	(0.002)
	7,911.25	313.58	0.49	15,133.07
	17,510	16,920	9,795	6,140
NH DD Coef.	-0.001	-0.004	-0.003***	0.008***
	(0.002)	(0.003)	(0.001)	(0.002)
	21,255.87	208.73	0.35	18,488.00
	17,445	16,820	9,740	6,080
Treated 2022-23				
AZ DD Coef.	0.002	0.028***	0.006***	0.020***
	(0.002)	(0.003)	(0.001)	(0.002)
	9,973.53	250.83	0.50	6,876.79
	17,625	16,985	9,850	6,145
Pretrend F	26.84	22.40	8.41	35.82
Pretrend p	0.00001	0.00004	0.00650	0.00000
NC DD Coef.	-0.000	0.044***	0.001	0.010***
	(0.002)	(0.003)	(0.001)	(0.002)
	10,852.71	335.64	0.26	5,523.45
	17,935	17,310	9,965	6,245
	52.70	91.76	0.06	100.13
	0.00000	0.00000	0.81341	0.00000
WV DD Coef.	-0.019***	-0.036***	-0.001	-0.012***
	(0.002)	(0.003)	(0.001)	(0.002)
	6,462.18	169.53	0.16	503.75
	17,280	16,680	9,625	6,025
	195.46	26.64	1.26	14.61
	0.00000	0.00001	0.26995	0.00054

Table D9 (Continued): CSDID and DD ATE Results (All asinh Outcomes by Treated State, Complete Cases Tuition, Winsorized by State (5%, 95%), Reasonable Ranges)

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Treated 2023-24				
FL DD Coef.	-0.010***	0.037***	0.002***	0.005*
	(0.003)	(0.004)	(0.001)	(0.002)
P.T. Mean	9,243.90	317.18	0.39	4,820.48
N	19,030	18,375	10,465	6,525
Pretrend F	1.57	16.24	4.47	4.69
Pretrend p	0.21924	0.00030	0.04200	0.03750
IA DD Coef.	-0.016***	0.036***	-0.001	-0.013***
	(0.003)	(0.004)	(0.001)	(0.002)
	5,713.23	207.45	0.40	1,135.71
	17,355	16,770	9,680	6,040
	0.03	48.37	2.27	23.05
	0.85703	0.00000	0.14150	0.00003
OH DD Coef.	0.006*	0.009*	0.007***	-0.010***
	(0.003)	(0.004)	(0.001)	(0.002)
	8,211.75	334.42	0.44	3,717.52
	18,010	17,400	10,065	6,250
	1.11	2.79	2.62	30.61
	0.29934	0.10400	0.11473	0.00000
Treated 2024-25				
OK DD Coef.	-0.030***	-0.010*	-0.002*	-0.003
	(0.002)	(0.004)	(0.001)	(0.002)
	6,892.43	341.73	0.21	2,525.96
	17,345	16,735	9,695	6,075
	5.74	0.01	6.47	51.30
	0.02222	0.93060	0.01567	0.00000
SC DD Coef.	0.023***	0.069***	0.001	-0.004
	(0.002)	(0.004)	(0.001)	(0.002)
	7,939.48	283.78	0.26	3,295.31
	17,585	16,970	9,780	6,095
	269.92	157.37	19.47	0.00
	0.00000	0.00000	0.00010	0.99077
UT DD Coef.	-0.005*	-0.001	0.000	-0.013***
	(0.002)	(0.004)	(0.001)	(0.002)
	19,115.54	260.09	0.23	7,317.38
	17,335	16,735	9,680	6,045
	13.30	3.40	6.00	51.30
	0.00088	0.07409	0.01965	0.00000

Notes Entries are coefficients, standard errors, level pre-treatment mean for treated states, and number of observations. CSDID estimations use both never-treated and not-yet-treated units as controls. Standard errors are clustered at the state level. For the estimating sample, all outcomes are winsorized within states to the 5% and 95% levels, and "reasonable ranges" refers to excluding schools that list tuition of 0 or an annual change in outcome greater than +/- 50%. The first two rows give CSDID estimates using all treated states from the remainder of Table 3a. Each subsequent row gives the TWFE DD coefficients for all outcomes counting only 1 state as treated (abbreviation of treated state comes before "DD Coef."). Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". Estimations use academic years after 2019 and exclude states with other current or pending universal voucher programs. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted. Event studies are provided for the first row of estimates.

Table D10: Total Program Service Revenue DD and SDID Results for Pre-Pay Schedule (Winsorized by State (5%, 95%), Reasonable Range)

	asinh(Revenue) DD	asinh(Revenue) SDID	asinh(PS Revenue) DD CF	asinh(PS Revenue) SDID CF	asinh(PS Revenue) DD Lin.	asinh(PS Revenue) SDID Lin.
All Known NCCS Schools						
DD or SDID Coef. 19-21	0.015	0.005				
(se)	(0.021)	(0.037)				
P.T. Mean	4,598,365	5,071,788				
N	5,238	114				
Pretrend F	0.27					
Pretrend p	0.60406					
NCCS/PSS Matches						
DD or SDID Coef. 19-21	0.040	-0.012	0.038**	-0.034	0.036**	-0.033
	(0.027)	(0.055)	(0.013)	(0.110)	(0.013)	(0.071)
	4,070,274	4,471,854	32,345	27,934	33,686	27,503
	1,908	108	1,908	108	1,908	108
	47.21		47.21		18.14	
	0.00000		0.00000		0.00015	
NCCS/PSR Matches						
DD or SDID Coef. 19-21	0.020	0.012	-0.035	-0.020	-0.024	-0.005
	(0.078)	(0.082)	(0.080)	(0.093)	(0.077)	(0.088)
	4,545,033	3,920,064	30,656	22,600	31,067	22,127
	1,026	105	1,026	105	1,026	105
	0.04		0.04		0.10	
	0.85201		0.85200		0.74981	

Notes: From the top, entries are DD or SDID coefficient estimates, standard errors, level pre-treatment mean for treated units, number of observations, pretrend F-statistics, and pretrend p-values. Standard errors are clustered at the state level for DD estimates and calculated using placebos for SDID estimates. SDID estimates require aggregation to the state level, which is why the number of observations decreases (revenue and students averaged to the state level and then per-student revenue calculated and asinh applied). The outcome for this regression is either asinh(school-level total program service revenue) or asinh(per-student school-level total program service revenue), taken from the National Center for Charitable Statistics (NCCS), and treatment is the implementation of a universal school voucher program ($>$ 200% FPL or FRPL). Total revenue is winsorized to the 5% and 95% levels (did not winsorize per-student revenue directly, only through total revenue). "Reasonable Range" refers to the restriction in all estimations that revenue not be 0 in any year and that the change in revenue (in either direction) can be no more than 50% between years. Additionally, for per-student revenue, we restrict to observations with less than $500,000 in per-student revenue. To estimate effects on per-student revenue ("PS Revenue"), we need enrollment data, which is why per-student revenue estimates are only available for observations with a PSS match (enrollment data comes from the PSS). In all cases, per-student revenue is calculated based on NCES PSS enrollment. NCES PSS enrollment is available every other year, so we must either assume that enrollment stays the same between observations ("CF") or that it changes linearly between periods ("Lin."). "All NCCS K-12 Schools" refers to all NCCS observations with an organizational name and/or address match in the NCES Private School Survey (PSS) as well as additional NCCS observations identified with K-12-specific NTEE codes (B20, B24, B25). "NCCS/PSS Matches" refer to all NCCS observations with an organizational name and/or address match in the PSS. "NCCS/PSR Matches refer to all NCCS observations with a match in the Private School Review sample (notice that this is a subset of NCCS/PSS matches as NCCS/PSR matches were found via PSS ppin). We estimate over the period 2019-2021 as we want to keep periods consistent between PSR and NCCS estimations, but the latest full NCCS dataset is for 2021, so we expand to 2019. States with other pending universal voucher programs are excluded. Standard errors are clustered at the state level. Note that the only treated units in these estimations are Indiana and New Hampshire. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted.

SOURCE: Private School Review 2015-2024, National Center for Charitable Statistics 2019-2021, and National Center for Education Statistics Private School Survey 2019-2021.

D.7 Only Including One Pre-Treatment Period

Table 3b: CSDID and DD ATE Results (all asinh outcomes by Treated State, Complete Cases Tuition)
Later Treated States with Only 1 Pre-Period

	asinh(Tuition)	asinh(Students)	% on Aid	asinh(Avg. Aid)
Treated 2022-23				
AZ DD Coef.	0.052***	0.055***	0.011***	0.055***
(se)	(0.003)	(0.005)	(0.001)	(0.004)
P.T. Mean	10,418.61	241.83	0.49	6,385.94
N	14,776	14,568	8,308	5,224
NC DD Coef.	0.014***	0.052***	0.005***	0.028***
	(0.003)	(0.005)	(0.001)	(0.004)
	10,577.40	317.96	0.25	5,615.56
	15,036	14,836	8,416	5,312
WV DD Coef.	-0.017***	-0.054***	-0.004***	-0.029***
	(0.003)	(0.005)	(0.001)	(0.004)
	6,297.61	160.67	0.16	503.75
	14,480	14,284	8,116	5,112
Treated 2023-24				
FL DD Coef.	-0.033***	0.040***	0.005***	0.015**
	(0.003)	(0.005)	(0.001)	(0.005)
	9,407.01	315.65	0.39	4,834.76
	11,988	11,820	6,621	4,149
IA DD Coef.	0.023***	0.052***	-0.003**	-0.054***
	(0.003)	(0.005)	(0.001)	(0.005)
	5,549.08	200.00	0.40	1,493.75
	10,917	10,770	6,120	3,846
OH DD Coef.	0.004	0.028***	0.018***	0.079***
	(0.003)	(0.005)	(0.001)	(0.005)
	8,297.96	331.97	0.42	3,853.69
	11,316	11,166	6,372	3,996
Treated 2024-25				
OK DD Coef.	-0.013***	0.022***	0.010***	-0.008*
	(0.002)	(0.004)	(0.002)	(0.004)
	7,047.34	347.06	0.21	2,549.57
	7,268	7,168	4,090	2,576
SC DD Coef.	-0.005	0.044***	-0.004*	-0.019***
	(0.002)	(0.004)	(0.002)	(0.004)
	10,398.58	291.53	0.26	3,363.44
	7,362	7,264	4,124	2,584
UT DD Coef.	-0.002	-0.027***	-0.007***	-0.002
	(0.002)	(0.004)	(0.002)	(0.004)
	19,328.97	256.41	0.23	7,317.38
	7,262	7,164	4,082	2,564

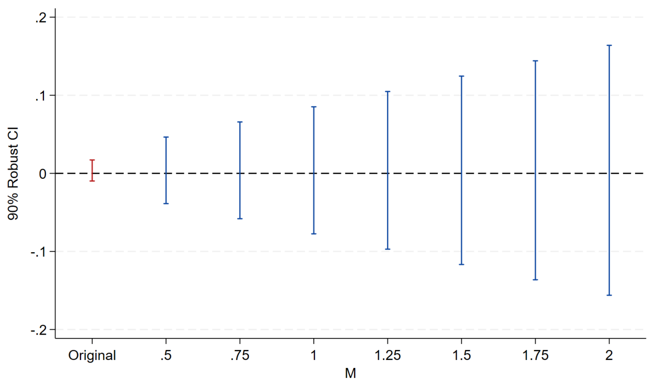
Notes Entries are coefficients, standard errors, level pre-treatment mean for treated states, and number of observations. Standard errors are clustered at the state level. Estimates are similar to the final 3 panels of Table 3a/3a (Continued) but use only one preperiod. Treated observations are those in each treated state after their universal voucher policy came into effect, which is denoted with "Treated ...". States with other current or pending universal voucher programs are excluded in each estimation. Tennessee, Wisconsin, and Wyoming are also excluded from the analysis due to the nature of their voucher programs. Data comes from the frequent updaters impute forward dataset, which includes data 2016-2024 for a panel of about 16k schools (preschools only and closed schools are excluded). For all specifications, only complete case tuition observations (first comes in or before 2020) are used with no restriction on baseline tuition. Frequent updaters had at least 3 updates 2015-2022 and infrequent schools did not. Note that tuition and average financial aid are nominal values and are not inflation adjusted.

E Additional Analysis of Pre-Trends in CSDID

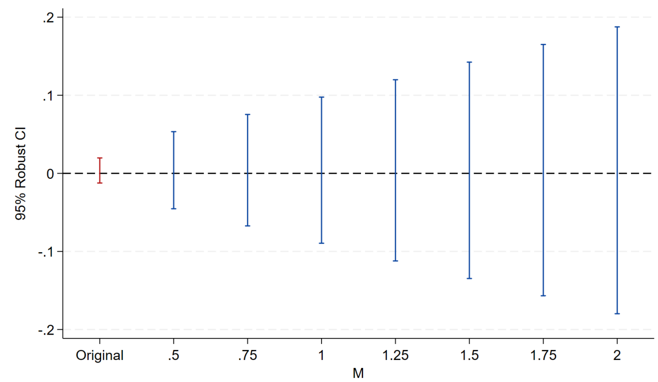
E.1 Tuition CSDID Estimates

Appendix Figure E1: Sensitivity Analyses Using Relative Magnitude Restrictions for 2020+ and 2021+ National CSDID Tuition Estimates

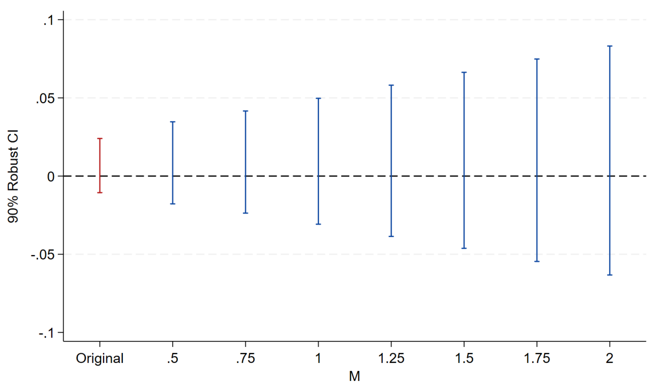
(a) Tuition CSDID 2020+ (90% CI)



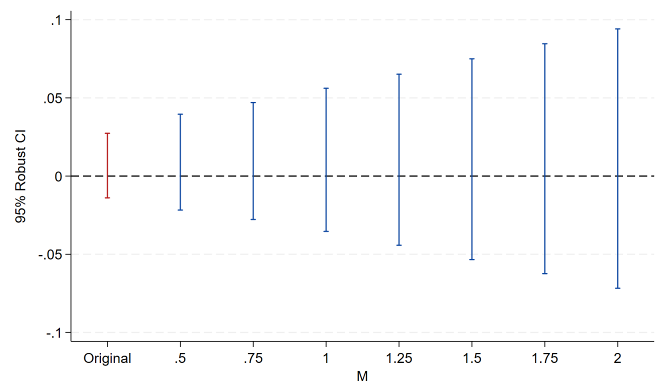
(b) Tuition CSDID 2020+ (95% CI)



(c) Tuition CSDID 2021+ (90% CI)



(d) Tuition CSDID 2021+ (95% CI)

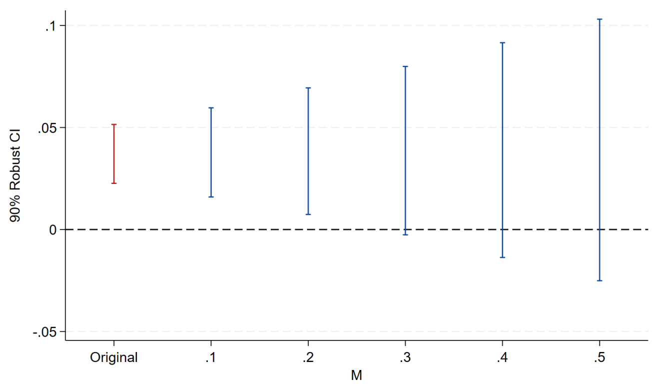


NOTES: These figures reflect sensitivity analyses using relative magnitude restrictions as described in Rambuchan and Roth (2022). M represents the proportion of the maximum pre-treatment violation in parallel trends that is allowed in the post-treatment period. M = 0 implies that the counterfactual difference in trends is exactly linear (i.e., parallel trends hold), and larger values of M allow for more non-linearity. The plots show the 90% or 95% confidence intervals for our national CSDID tuition results (first two rows of Table 3). Since COVID plays such a large part in the pre-period of our estimations, we provide plots for 2020+ and 2021+ specifications (excluding 2020 may be helpful to avoid COVID). Note that these figures use event study estimates from the "Long2" version of CSDID (Figure 4).
SOURCE: Private School Review 2015-2024.

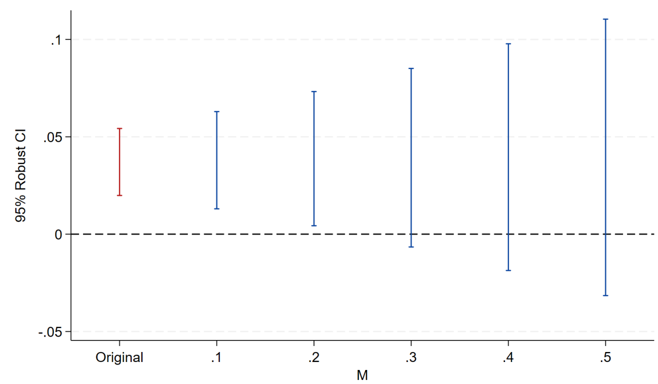
E.2 Enrollment CSDID Estimates

Appendix Figure E2: Sensitivity Analyses Using Relative Magnitude Restrictions for 2020+ and 2021+ National CSDID Enrollment Estimates

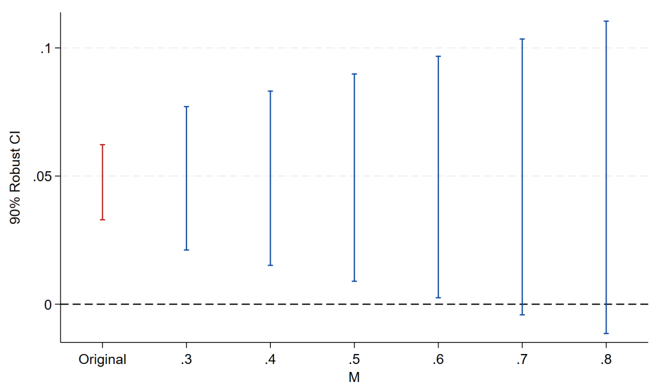
(a) Enrollment CSDID 2020+ (90% CI)



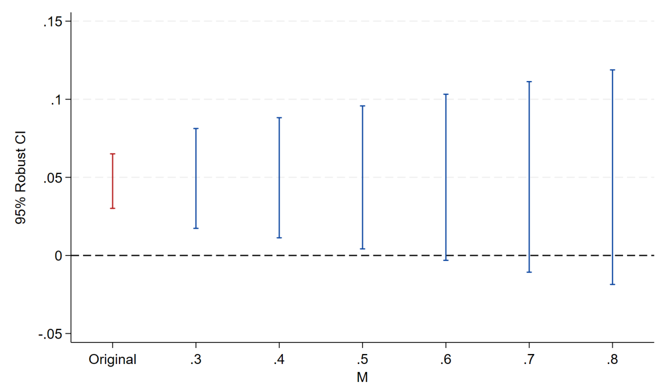
(b) Enrollment CSDID 2020+ (95% CI)



(c) Enrollment CSDID 2021+ (90% CI)



(d) Enrollment CSDID 2021+ (95% CI)

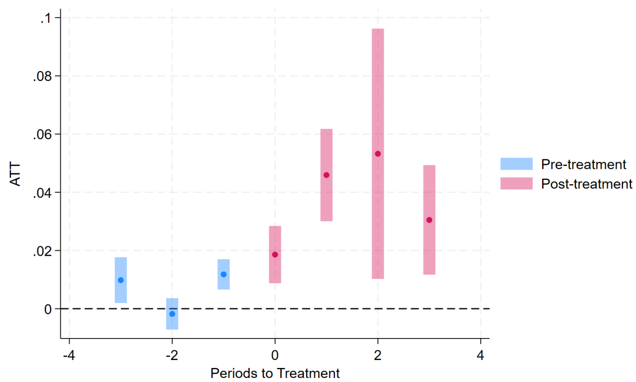


NOTES: These figures reflect sensitivity analyses using relative magnitude restrictions as described in Rambuchan and Roth (2022). M represents the proportion of the maximum pre-treatment violation in parallel trends that is allowed in the post-treatment period. M = 0 implies that the counterfactual difference in trends is exactly linear (i.e., parallel trends hold), and larger values of M allow for more non-linearity. The plots show the 90% or 95% confidence intervals for our national CSDID enrollment results (first two rows of Table 3). Since COVID plays such a large part in the pre-period of our estimations, we provide plots for 2020+ and 2021+ specifications (excluding 2020 may be helpful to avoid COVID). Note that these figures use event study estimates from the “Long2” version of CSDID (Figure 4).
SOURCE: Private School Review 2015-2024.

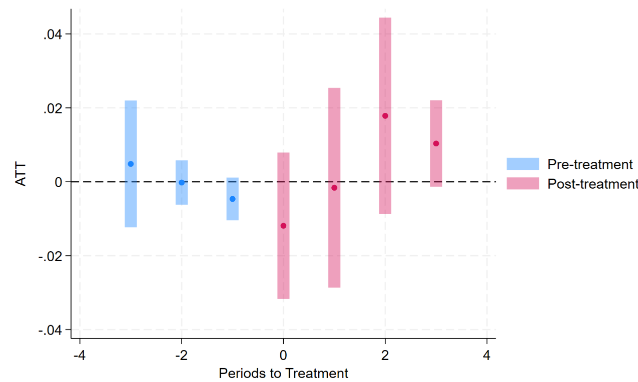
E.3 "Short Gaps" CSDID Event Studies

Appendix Figure E3: National CSDID Event Studies for all asinh outcomes (Default version of CSDID)

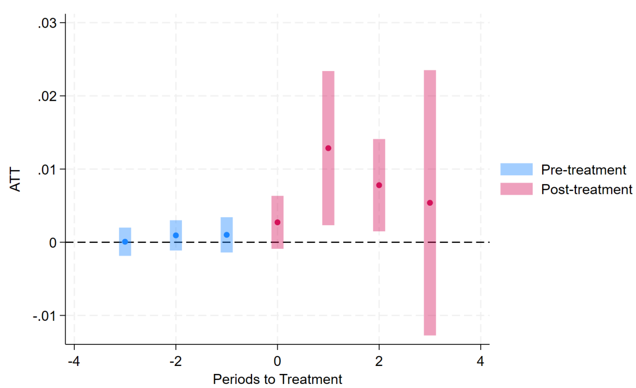
(a) asinh(Total Enrollment)



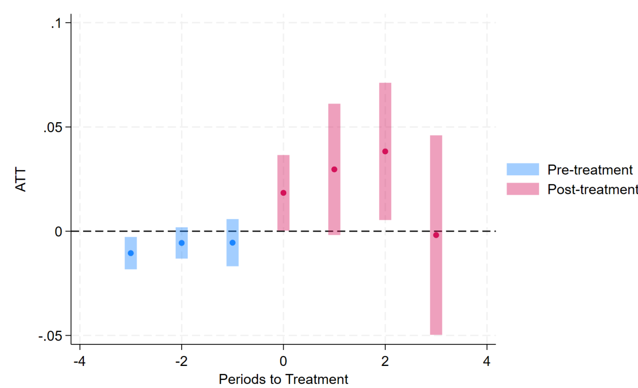
(b) asinh(Nominal Yearly Tuition) ($)



(c) % on Financial Aid



(d) asinh(Nominal Average Financial Aid) ($)

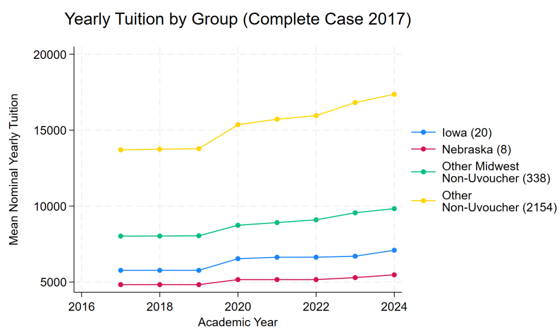


NOTES: These figures reflect CSDID event study estimates using schools that updated their Private School Review Webpage at least 3 times 2016-2022 (frequent updaters). Additionally, included schools must have their first non-missing tuition observation before 2021 (complete case tuition). All treated states from Table 3 are included in this staggered DD estimation. Note that tuition and average financial aid are in nominal values and are not inflation-adjusted. Default version of CSDID compares each period to the previous period (NOT to the last pre-treatment period), known as "short gaps". These event studies correspond to the estimates in the first row of Table 3a.
SOURCE: Private School Review 2015-2024.

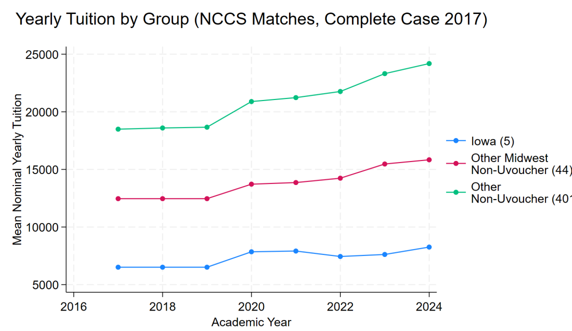
F Additional Analysis of Iowa

Appendix Figure 3: Comparing IA, NE, Other Midwest Control States, and All Other Controls: PSR Tuition and NCCS Revenue (Total and Per-Student)

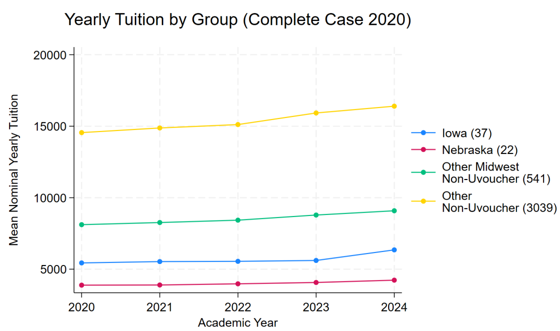
(a) PSR



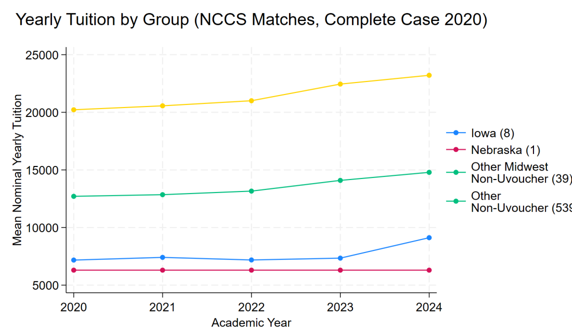
(b) PSR



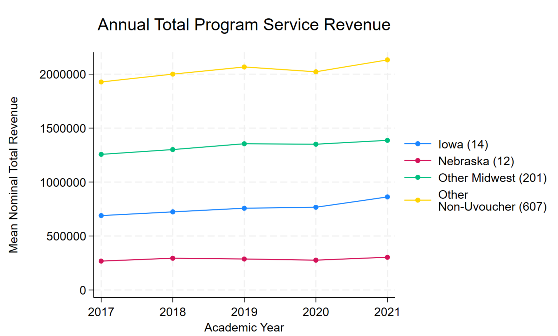
(c) PSR



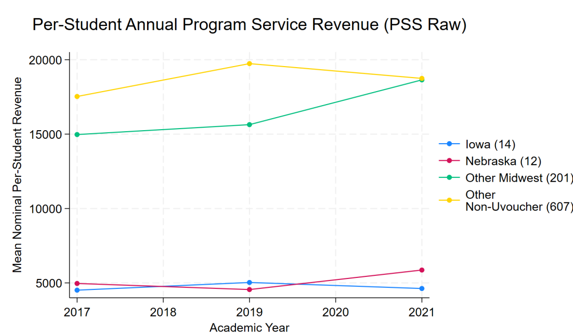
(d) PSR



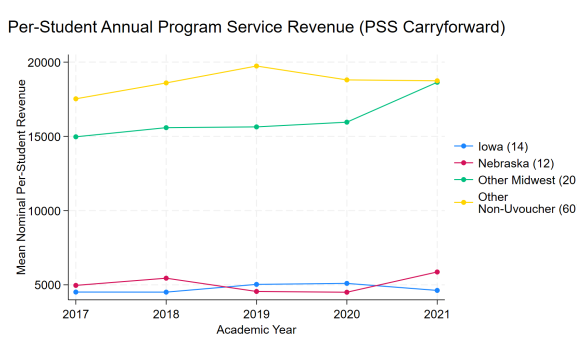
(e) NCCS



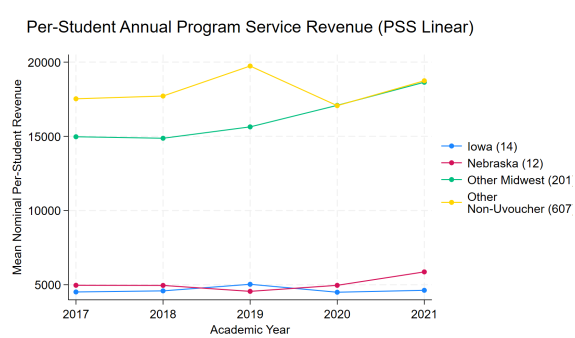
(f) NCCS



(g) NCCS



(h) NCCS



NOTES: Panels (a) - (d) compare PSR yearly nominal tuition between groups. Panel (a) reflects all PSR observations with first tuition observation coming in or before 2017, (b) reflects the same sample restricted to NCCS matches (matched through NCES ppin), (c) reflects all PSR observations with first tuition observation in or before 2020 (analytic sample), and (d) reflects the same sample restricted to NCCS matches (through NCES ppin). Notice that panel (b) does not have a trend line for Nebraska, which results from there being no complete case 2017 NCCS/PSR match in that state. Panels (d) - (g) show the same group comparison for NCCS total and per-student program service revenue (aligned on the "Pre-Pay" assumption). No tuition or revenue restrictions were used in any panels. Per-student revenue is calculated using PSS enrollment. "Raw" refers to using the unchanged PSS enrollment (3 observations: 2017, 2019, 2021), "carryforward" refers to assuming PSS enrollment is unchanged between observations, and "linear" refers to linear interpolation between enrollment observations. The number of schools in each group is reflected in the legend.