
A WIN-WIN SOLUTION

The Empirical Evidence
on School Choice

FOURTH EDITION

Greg **Forster**, Ph.D.

MAY 2016

**Friedman
Foundation**

*For
Educational
Choice*

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Table of Contents

Executive Summary	1
Introduction	3
Choice in Education	3
Why Science Matters—the “Gold Standard” and Other Methods.....	4
The Method of This Report	6
Criteria for Study Inclusion or Exclusion.....	6
Defining a “Study”	7
Search Method.....	7
Part I: Academic Outcomes of Choice Participants	9
The Importance of Random Assignment—The Gold Standard	10
What the Gold-Standard Studies Show	11
Part II: Academic Outcomes of Public Schools.....	15
What the Studies Show	16
Part III: Fiscal Impact on Taxpayers and Public Schools	20
Measuring Fiscal Impact	21
What the Studies Show	22
Part IV: Racial Segregation in Schools.....	25
Measuring Racial Segregation	26
What the Studies Show	27
Part V: Civic Values and Practices	29
Measuring Civic Values and Practices.....	30
What the Studies Show	30
Conclusion: Remarks on School Choice	33
Universal Choice Could Deliver and Education Revolution.....	34
Yes, Choice Improves Schools.....	34
Choice Could Work Much Better If We Let It.....	34
Only Universal School Choice Can Sustain Dramatic Change.....	35
References.....	37
Academic Outcomes of Participants.....	37
Academic Outcomes of Public Schools	37
Fiscal Impact on Taxpayers and Public Schools.....	39
Racial Segregation	39
Civic Values and Practices.....	40
Notes	41
About the Author.....	41

List of Tables

- Table 1:** Empirical Studies on School Choice 2
- Table 2:** Academic Outcomes of Choice Participants..... 14
- Table 3:** Academic Outcomes of Public Schools 19
- Table 4:** Fiscal Impact on Taxpayers and Public Schools..... 24
- Table 5:** Racial Segregation 28
- Table 6:** Civic Values and Practices..... 32

Executive Summary

This report surveys the empirical research on private school choice programs. It provides a thorough overview of what the research has found on five key topics:

- Academic outcomes of choice participants
- Academic outcomes of public schools
- Fiscal impact on taxpayers and public schools
- Racial segregation in schools
- Civic values and practices

The evidence points clearly in one direction. Opponents frequently claim school choice does not benefit participants, hurts public schools, costs taxpayers, facilitates segregation, and even undermines democracy. However, the empirical evidence shows that choice improves academic outcomes for participants and public schools, saves taxpayer money, moves students into more integrated classrooms, and strengthens the shared civic values and practices essential to American democracy. A few outlier cases that do not fit this pattern may get a disproportionate amount of attention, but the research consensus in favor of school choice as a general policy is clear and consistent.

The results are not difficult to explain. School choice improves academic outcomes for participants and public schools by allowing students to find the schools that best match their needs and by introducing healthy competition that keeps schools mission-focused. It saves money by eliminating administrative bloat and rewarding good stewardship of resources. It breaks down the barriers of residential segregation, drawing students together from diverse communities. And it strengthens democracy by accommodating diversity, de-politicizing the curriculum, and allowing schools the freedom to sustain the strong institutional cultures that are necessary to cultivate democratic virtues, such as honesty, diligence, achievement, responsibility, service to others, civic participation, and respect for the rights of others.

The size of the benefit provided by existing school choice programs is sometimes large, but is usually more modest. This is not surprising because the programs themselves are modest—curtailed by strict limits on the students they can serve, the resources they provide, and the freedom to innovate. Only a universal educational choice program, accessible to all students, is likely to deliver the kind of dramatic improvement American schools need in all five of these important areas.

Key findings:

- Eighteen empirical studies have examined academic outcomes for school choice participants using random assignment, the gold standard of social science. Of those, 14 find choice improves student outcomes: six find all students benefit and eight find some benefit and some are not visibly affected. Two studies find no visible effect, and two studies find Louisiana’s voucher program—where most of the eligible private schools were scared away from the program by an expectation of hostile future action from regulators—had a negative effect.
- Thirty-three empirical studies (including all methods) have examined school choice’s effect on students’ academic outcomes in public schools. Of those, 31 find choice improved public schools. One finds no visible effect. One finds a negative effect.
- Twenty-eight empirical studies have examined school choice’s fiscal impact on taxpayers and public schools. Of these, 25 find school choice programs save money. Three find the programs they study are revenue neutral. No empirical study has found a negative fiscal impact.
- Ten empirical studies have examined school choice and racial segregation in schools. Of those, nine find school choice moves students from more segregated schools into less segregated schools, and one finds no net effect on segregation. No empirical study has found that choice increases racial segregation.

- Eleven empirical studies have examined school choice’s effect on civic values and practices, such as respect for the rights of others and civic knowledge. Of those, eight find school choice improves civic values and practices. Three find no visible effect from school choice. No empirical study has found that school choice has a negative effect on civic values and practices.

TABLE 1 Empirical Studies on School Choice

	Any Positive Effect	No Visible Effect	Any Negative Effect
Academic Outcomes of Choice Participants	14	2	2
Academic Outcomes of Public Schools	31	1	1
Fiscal Impact on Taxpayers and Public Schools	25	3	0
Racial Segregation in Schools	9	1	0
Civic Values and Practices	8	3	0

Note: Shows the number of empirical studies with each type of finding. The first row includes all studies using random-assignment methods. Other rows include all studies using all types of methods.

Introduction

School choice programs allow parents to decide what schools their children attend using the public funds dedicated to their children’s educations. Some “school choice” programs are limited to a choice among government-owned schools and thus provide a “choice” of options that are all ultimately controlled by the same entity. Private school choice—the subject of this report—gives parents the option of selecting a private school. Such programs are among the most prominent and successful reforms in the education field. There are now 61 such school choice programs in 30 states and Washington, D.C. More than 399,000 students use these programs to attend private schools.¹

The most well-known form of school choice is school vouchers, which give parents the ability to redirect their children’s education funding to a participating private school for tuition support. More recently, education savings accounts have introduced an innovation to the school voucher model, allowing parents to use redirected funds for other educational services and expenses in addition to tuition costs. This further incentivizes good financial stewardship because parents can select educational providers for price as well as for quality. An alternative approach, tax-credit scholarships, gives donors a tax credit if they donate money to nonprofits that provide private school scholarships. Finally, some programs give parents a direct tax credit or deduction that reimburses them for a certain amount of private school costs.

One of the most important questions about school choice is how it affects academic outcomes, both for the students who use it and in public schools at large. Defenders of the government school monopoly claim that choice does no good for the students who use it and harms public schools by “draining money” or by “creaming students”—that is, skimming off the best students who rise to the top and would be most attractive to private schools. School choice proponents, on the other hand, argue that choice improves academic outcomes both for the participating students and for public schools. They say choice saves money for public school budgets,

rather than “draining” money, and sends all types of students to private schools rather than “creaming.” They also point to the benefits of allowing each student to find the right school and the healthy incentives created by competition.

School choice raises other important policy questions. Opponents portray school choice as a cost to taxpayers, while supporters say school choice saves money for taxpayers through improved stewardship of resources. Opponents of choice frequently have charged that it will exacerbate racial segregation in schools (which is already at epidemic levels in the government monopoly system) whereas supporters say choice is a tool for breaking down segregation. And opponents argue that private schools will not teach students the civic values and practices upon which democracy depends, such as respect for the rights of others and civic knowledge. Meanwhile, supporters say choice strengthens those same democratic values and practices.

A large body of empirical evidence examines these questions using scientific methods. Twenty years ago, before this body of evidence existed, there was some excuse for making policy based on speculation, anecdotal observation, and intuition. Today, the effects of these programs are known, and there is no longer any excuse for policymakers and opinion leaders to be ignorant of the facts.

This report reviews the available empirical studies on the five policy areas described above. For participant effects, a large body of studies using the “gold standard” method of random assignment is available, so this report reviews that evidence. For the other questions, it reviews all available empirical studies using any quantitative method. It also discusses the most important methodological issues confronted by research on this subject, and some of the larger implications of what the research finds.

Choice in Education

Americans expect and demand the right to select their own goods and services in every area other than

K–12 education, including everything from food, housing, clothing, transportation, and medical care to magazines, haircuts, dry cleaning, and video games. If the government tried to assign people to live in certain neighborhoods or shop at certain grocery stores, Americans would howl in protest. They even expect and demand choice when it comes to education outside of K–12 schools—everywhere from colleges to trade schools to tutoring services. But when it comes to K–12 education, the American idea that people should have stewardship over their own lives and choose for themselves rather than have government dictate what they receive is not embodied in public policy.

The arguments typically used to defend this lack of choice are empirically false or poorly reasoned. For example, teachers' unions claim that choice "drains money" from public schools. But how would Americans respond if they were told that from now on they would have to receive all their medical care from a doctor assigned to them by the government, rather than from their current family doctor, on grounds that their choice to seek care from their own doctor "drains money" from the budget of some other doctor chosen by the government?

Meanwhile, the idea that school choice might improve public schools is dismissed as ideological claptrap. In fact, the empirical evidence consistently shows it is the case, and the reasons are not hard to explain. One reason choice would improve public schools is that it allows parents to find the right particular school for each individual child. Every child is unique and has unique educational needs.

But probably the most important reason school choice would improve public schools is because it gives parents a meaningful way to hold schools accountable for performance. Under the current system, if a school is not doing a good job, the only ways to get a better school—purchase private schooling or move to a new neighborhood—are expensive and impractical.

The current school system is especially unjust to low-income and disadvantaged families. As a government monopoly, the system is most likely to provide good services to, and be responsive to the concerns of,

politically powerful parents, which means wealthier, better-educated, and (let's face it) whiter parents. Poor and otherwise disadvantaged families too often get the least attention from the system. And they are the least likely to have the means to seek private schooling or move. Seventy percent of black workers, for example, make less than \$50,000 per year, compared to 52 percent of white workers.² Indeed, a decreased ability to exit the system only reinforces the system's tendency to deliver poor services. They are captive clientele.

Thus, in the absence of parental choice, schools lack the healthy, natural environment of client empowerment that is essential to producing better performance in most other service institutions. Hospitals know they must do a good job or lose patients. Professionals like doctors and lawyers must provide good services or lose clients. Stores must provide good value or lose customers. This system is so critical to keeping institutions mission-focused that we take it completely for granted—everywhere but in K–12 schooling.

It is widely agreed that monopolies generally provide poor quality because nothing bad will happen to them if they do not serve their clients well. When they get bad service, customers say, "I'll take my business elsewhere," because they know that is what will prompt better service. They do this to nonprofit institutions the same way they do it to businesses, because they know it is not profit that creates better performance; it is client choice.

The failure of education policy to embrace the American principle that people should have stewardship over their own lives and make their own choices is a great hindrance to reform. One way opinion leaders can rectify this problem is by making the public aware of the large body of empirical research that examines how choice affects participants, public schools, and the civic community at large.

Why Science Matters—the "Gold Standard" and Other Methods

There is no such thing as a "scientifically right"

education policy. Science cannot identify what education policy is most fitting to the intrinsic nature of the human person, or most aligned with America's ideals of freedom and democratic self-rule. To answer those questions, one needs other kinds of knowledge—knowledge about the nature of human life, the meaning of freedom and democracy, and the historic self-understanding of the American people.³

However, abstract ideas and history are not by themselves an adequate basis for public policy. The public hears competing claims about the real-world effects of education policy in the concrete world of the here and now. The public wants, and rightly so, to know which claims are true and which are false. That is an empirical question. Addressing such questions is the special right and duty of science.⁴

When evaluating the effectiveness of an education policy, it is especially important to rely on empirical research of high scientific quality. The student outcomes that education reforms are designed to promote are affected by many different influences, including demographic factors (income, race, family structure, etc.), school factors (type of school, teacher quality, etc.), and intangibles, such as the level of enthusiasm parents and teachers invest in a child's education. The job of social science is to disentangle the influence exercised by each of these factors as well as possible with the available evidence.

A study that uses good research methods can overcome those problems and provide reliable information about what is influencing student outcomes. But if scientific procedures are not rigorously followed, or if people make judgments without first examining the science, it is easy to draw the wrong conclusions about what factors cause what outcomes.

The gold standard for empirical science is the method known as "random assignment," in which subjects are randomly divided into a treatment group that will receive the treatment being studied (such as school choice) and a control group that will not receive it. Because the two groups are separated only by a random lottery, they are likely to be very similar in

every respect other than the treatment. See the next section of this report for more about the importance of this method, and its results in studies of choice.

Though it may be the best kind of research, the gold standard of random assignment is not the only kind of research worth considering. Where it is not possible to conduct a random-assignment study, other kinds of research methods can produce useful information that sheds light on important policy questions.

The next best research method is to track year-to-year changes in outcomes for individual students, especially if it is possible to track them as they switch in and out of schools of choice.

Tracking individual students over time removes from the analysis most, though not all, of the influence of unmeasured factors. If a student is advantaged in a way that is not measurable, that advantage will typically be present in the student's outcomes for both year one and year two of the study. Thus, the change in outcomes between year one and year two will mostly be from other factors. Removing the influence of unmeasured factors allows the analysis to isolate the effect of the factors that are being measured, such as the offer of school choice or the decision to switch in or out of a choice school. A moderate disadvantage of this method is that some unmeasured factors influencing outcomes may change over time without those changes being tracked.

If it is not possible to track individual students, good research still can be conducted by tracking year-to-year changes in individual schools. By tracking year-to-year changes we are tracking whether students are learning over time, rather than how much they knew when they walked into school. And we are removing much (though not all) of the influence of demographic characteristics. It is reasonable to expect that the unmeasured advantages of the students in a given school will be similar from year to year. If a school had highly advantaged students last year, it probably will still have highly advantaged students this year. Mobility among the student population will create some change in

student characteristics from year to year, but not so much that we cannot learn anything from school-level studies.

This report is the fourth edition in a series, and the earlier editions contain methodological discussions that may be of interest to the reader of this report. The first edition was published in 2009.⁵ That report included only the research on how school choice affects public schools; its discussion of the methodological issues involved in that research was much more detailed than the overview provided here. The second edition was published in 2011.⁶ That version added the research on participant effects and also provided more detail on the methodological issues involved in those studies than is included here. The third edition was published in 2013 and added reviews of the research on the fiscal, racial, and civic impact of school choice.⁷ Readers seeking more extended methodological discussions of the studies reviewed here may wish to consult those editions.

Previous editions bore the title *A Win-Win Solution* because the evidence on academic effects showed positive results both for participants and for public schools. Starting with the third edition, we have looked beyond those two constituencies to consider three ways in which school choice affects the democratic polity. Thus, school choice is not merely a win-win, but actually a “win-win-win” solution.

The Method of This Report

This edition brings our research review on the five covered topics up to date as of February 25, 2016. The body of research on school choice is very widely and actively discussed. The community of professional social scientists focused on school choice research is large but not too large for easy communication, and all of the research has occurred since the rise of the internet. Though the possibility of a study being overlooked can never be ruled out, discovering the available research is much less difficult in this field than in most other fields of social science. (The exception to this is fiscal analyses, which might

plausibly have been conducted by economic researchers or government agencies not connected to the education research community.)

Criteria for Study Inclusion or Exclusion

We did not include studies of programs outside the U.S. in this review because the education systems of other countries work very differently than those of the U.S., especially in the area of school choice. We did not include case studies and other qualitative studies, whose purpose is to help us learn to ask the right questions rather than to reach broadly generalizable answers; we included only studies using quantitative data. We also did not include statistical modeling that examines what would happen on certain hypothetical assumptions; we only include studies of what has actually happened as a result of school choice programs. We did not include studies that are not publicly available, because scientific validity depends upon transparency and the opportunity for researchers to critique one another’s work. And we excluded studies of private schooling generally, and of “choice” opportunities that are limited to government-owned schools (charter schools, magnet schools, district choice, etc.).

However, to avoid the possibility of “cherry picking,” we included all empirical, quantitative studies of U.S. private school choice programs, within the specific limits of each of our five reviews. We did not exclude studies simply because they used methods we found inadequate or objectionable. We did this to avoid the risk of selectivity; if we excluded every study that used a method we found objectionable, we could bias the review. Hence, this review includes three studies—one finding no visible effect on academic outcomes of participants, one finding a positive effect on academic outcomes in public schools, and one finding no effect on civic values and practices—using methods of highly questionable value. These studies and their shortcomings are discussed in the text below.

In addition, three of the five topical reviews included

in this report excluded some studies based on method, not because we judged the methods to be wrong, but to appropriately limit the scope of our review. These limitations are discussed in more detail in the relevant sections; for convenience, they are summarized here. The review on academic outcomes of choice participants is limited to “gold standard” random-assignment studies, because where a large body of such studies exists, it ought to be given priority. The review of studies on racial segregation excludes methods that do not measure schools against an appropriate measurement of racial segregation. The review of studies on civic values and practices excludes merely descriptive studies, because they do not address a relevant policy question.

Defining a “Study”

This review counts analyses as separate “studies” if they examine separate school choice programs or if they use different sets of analytical models to examine their data. Analyses examining separate programs should be considered as separate studies because the programs are different; every school choice program is unique. If the same researcher finds that a program in New York has a positive effect and also that a program in Washington, D.C. has a positive effect, she has produced two different findings, not the same finding twice. Similarly, analyses that use different statistical models should be considered as separate studies because they are asking different questions of the data. Replication is the essence of science; scientific methods will invariably produce occasional false positives and false negatives, which is why we wait until something has been studied multiple times before we consider conclusions firmly supported. One part of this replication process is the gradual improvement the analytical models we bring to any data set, as the scientific community raises new questions and develops new methodological insight. It is therefore not at all unusual for the same researcher to go back to the same data set and analyze it in new and different ways, even with the same basic research question in mind (e.g. “Did this program have a positive effect on math scores?”). The

differences between the analytical models are sometimes subtle, but subtle differences can be extremely important. Thus, re-analyses of the same data using new models (even if the differences seem small) must be counted as multiple studies. If we tried to distinguish which changes of statistical models are substantial enough to be worth counting as new studies, and which are minor enough to be considered trivial variations on the same study, we would introduce an almost unlimited opportunity for “cherry picking” selectivity.

To add further protection against “cherry picking,” we classify studies by whether their various analytical models produced any positive result or any negative result, rather than picking and choosing (or allowing the studies’ authors to pick and choose) which of a study’s analyses “really count” and which don’t. A study typically includes multiple analytical models—sometimes many of them, occasionally even more than 100. Selecting (or allowing the studies’ authors to select) among these models would make it too easy to ignore findings that fail to confirm our (or the authors’) biases.⁸ Following scientific convention, we classify analyses that do not achieve statistical significance as having found “no visible effect.”

Search Method

Most of the studies added to this report since the previous edition came to the author’s attention informally, either through his own ongoing work in the school choice research field or as a result of others in the field bringing these studies to his attention. (It is difficult to work in this field and not be aware of new studies as they come out!)

However, to help ensure the review was comprehensive, the author conducted two formal searches. The first was a search of the Education Resources Information Center (ERIC) database. Four searches were conducted using the search terms “school choice,” “voucher,” “tax credit,” and “education savings account.”⁹ Since the previous edition of this report brought the review up to date through January

2013, we searched for all relevant studies published in 2013 or later.¹⁰ The second search was of the Journal Storage (JSTOR) database. We again conducted four searches, using the same search terms, for all relevant studies published in 2013 or later.¹¹

PART I

Academic Outcomes of Choice Participants

There have been 18 studies using random-assignment methods to examine how school choice affects the academic outcomes of participants. This body of evidence shows that school choice benefits students. Fourteen studies find positive effects on school choice participants: six find choice had a positive effect across all students participating and another eight find choice had a positive effect on some student groups and no visible impact on other students. Two studies find no visible effect from choice. Two studies on Louisiana’s voucher program find that it had a negative effect.

The Importance of Random Assignment—the Gold Standard

When examining academic effects of school choice on participants, this report focuses on studies using random-assignment methods. These studies separate subjects into “treatment” and “control” groups randomly. Random assignment generates high confidence that factors other than the one being studied—the “treatment”—are not influencing the results.

The special value of random-assignment research is that it removes not only the influence of observable factors, such as demographics, but also the vast array of unobservable factors that researchers know influence education, but cannot directly measure. For example, researchers agree widely that one of the key drivers of student outcomes is parental motivation. Parents who highly value the education of their children are an important positive influence on outcomes. Random assignment assures high confidence that differences in factors such as this are not influencing research results.

Unfortunately, it usually is not possible to conduct random-assignment research on education policy. However, school choice has been one of the rare exceptions. When there are more applicants for a choice program than there are available slots, a random lottery is often used to determine who may participate. This creates a naturally occurring

random-assignment research design. Students who win the lottery and are offered choice can be compared to students who lose the lottery and were not offered choice. If any systematic (i.e., non-random) differences between the outcomes of the two groups are observed, those differences can be attributed to the offer of choice because nothing separates the groups but the offer of choice and randomness.

One limitation of random-assignment research is the randomization process itself; a random-assignment study can be only as good as the randomization used to create the treatment and control groups. In most cases, where random assignment has been used to study school choice programs, the randomization occurs at the level of the program itself, because the program is oversubscribed. This presents no difficulties. However, in some cases the randomization happens at the level of the individual school—the treatment and control groups are created not because the program as such is oversubscribed, but because some schools are. In those cases, the effect being studied is not the effect of the program as such but the effect of those particular schools upon the students who choose those schools. Attrition is another limitation of random assignment. For instance, if significant numbers of students drop out of the program (or the study!) over time, it becomes more difficult for results to achieve standard levels of statistical significance, making it less likely the study will achieve a positive or negative finding and more likely it will find no visible effect.

Nonetheless, because random assignment is so preferable to other methods, it should be given priority whenever a large body of random-assignment studies exists. It would make no sense to ignore the difference between the proven reliability of gold-standard studies and studies that are more methodologically limited. Moreover, identifying all the non-gold-standard research that has been done on this question over the years would be too cumbersome to do here. A great deal of empirical research has compared public and private schools’ effects on test scores using methods other than random assignment. This research question actually

goes all the way back to the origins of modern education science, with the well-known *Coleman Report* of 1966 and James Coleman's follow-up research. It is worth noting, however, that most of the studies that rise to a reasonable level of scientific quality have found in favor of private schools.¹²

What the Gold-Standard Studies Show

The last edition of this report counted 12 random-assignment studies of academic effects of school choice on participants. Readers seeking descriptions of those studies should consult previous editions of this report. Five additional studies have been published or come to our attention since then. Moreover, a publication that previous editions of this report discussed, but did not recognize as a study, is now recognized as a study. These 18 studies collectively show a positive effect from school choice.

Among the 12 studies reviewed in the previous *A Win-Win Solution* report, six find a positive effect for all students. Five find a positive effect for some student groups (black students in some studies; students leaving especially low-performing public schools in others) with no visible impact on other groups. Probably the most plausible hypothesis to explain the studies finding benefits for some groups but not others is that student groups that were served more poorly in their public schools stood to gain more from the opportunity to choose a new school.

The remaining study, a reanalysis of data from a previous study, finds no visible impact from choice. However, the authors Alan Krueger and Pei Zhu introduced two methodological changes that undermine the study's validity. First, they changed the way students were classified by race in a way that violates the rules of social science. When student self-identification is absent, the generally accepted method is to use the race of the mother; Krueger and Zhu classified a student as black if either parent was black. This method is indefensible because it is asymmetrical, treating "black" as a dominant racial

trait. A student with a black father and a Hispanic mother is arbitrarily labeled black by Krueger and Zhu, but would typically be at least as likely to consider herself Hispanic or biracial. Krueger and Zhu also added students with significant missing data to their data set and failed to control for the students' baseline scores (a standard step in scientific education analysis). Unsurprisingly, through these manipulations they were able to lower the variable for statistical significance below the conventional threshold for recognizing a finding.¹³

Paul Peterson and William Howell, the authors of the original study, have demonstrated how badly distorted Krueger and Zhu's findings are. They published a series of 120 reanalyses of their data set, each using a different set of specifications. These analyses demonstrated that the positive finding for black students is robust across numerous different assumptions about racial identification. Howell and Peterson show that the positive effect disappears only if the analysis incorporates Krueger and Zhu's exact combination of arbitrary racial redefinition, students with missing data, and exclusion of baseline scores. If we leave out any two of these three, the results become positive.¹⁴ The Krueger-Zhu statistical model must be regarded as discredited.

Peterson and Howell's publication exposing the deficiencies of Krueger and Zhu's analysis was discussed in earlier editions of this report but was not classified as a study. However, it meets all of this report's criteria for being counted as a study. Therefore, it is counted here as a study finding a positive effect on some participating students.

Of the five random-assignment studies that have been released or come to our attention since the previous edition of this report, two find a positive effect for some participating students and no negative effects for any student group. Matthew Chingos and Peterson published a new analysis of data from a privately funded New York City voucher program in the late 1990s, which they had analyzed using different methods in a previous study.¹⁵ The new analysis included a more sophisticated array of

tests for subgroup effects and other factors. It finds that the program had no visible effect on college enrollment or attainment rates for all students, but did have positive effects on those rates both for minority students in general and for black students in particular. The new analysis also found that the program had a positive effect on college enrollment and attainment rates for only one other subgroup the authors examined: children of mothers born in the U.S. The size of the effects varied according to different statistical models used by the authors. In the simplest model, positive effects are six percentage points on college enrollment rates and five percentage points on bachelor's degree attainment for black students and five percentage points on enrollment and three percentage points on attainment for children of U.S.-born mothers. No negative effects on any student group were found.¹⁶

Analyzing the same New York City program, Marianne Bitler and three co-authors find no visible effect on participants as a whole. They also divide students into quintiles based on student achievement and find no visible effects on academic outcomes in any quintile. They also replicate the racial group analyses of both Peterson and Howell and Krueger and Zhu, discussed previously, using a different method of accounting for missing data. When they replicate Peterson and Howell's statistical model, they find a positive effect for black students; when they replicate Krueger and Zhu's, they find no visible effect.¹⁷

In their discussion, Bitler and her co-authors go to great lengths to give reasons why they would prefer to think that the program had no impact. However, this recitation of their personal preferences does not negate the empirical findings they report. Because they report positive findings among their results, we count this study as having found a positive effect for some participating students. This is consistent with the practice we have followed in classifying all studies in this research review—where multiple statistical models are reported, this report includes them all, to avoid accusations of selectivity (“cherry picking”).

That said, it is all the more appropriate to count the study as having a positive finding given that the Krueger and Zhu statistical model must be regarded as discredited for the reasons described above. If one were to be selective among this report's findings, one would have much stronger grounds for selecting its replication of Peterson and Howell's model and disregarding its replication of Krueger and Zhu's.

One random-assignment study has come to our attention since the previous edition of this report was published. A study by Eric Bettinger and Robert Slonim published in 2006 examined a privately funded school voucher program in Toledo, Ohio. The study's primary topic was the effect of the program on altruistic behavior, but it also contained an analysis of participants' math scores. It found no visible effect from the program.¹⁸

Two random-assignment studies published since the previous edition of this report, both examining the Louisiana Scholarship Program (LSP), find a negative effect on academic outcomes for participating students. The first study was conducted by Atila Abdulkadiroglu, Parag Pathak, and Christopher Walters. It finds that in the first year, the voucher program had a negative effect on participants' math, reading, science, and social studies scores. The negative effect on math scores was 0.4 standard deviations—a large effect, in this case equal to a 50 percent increase in a student's chance of receiving a failing grade.¹⁹ The second study, by Jonathan Mills and Patrick Wolf, finds that the program had a negative effect on math and no visible effect on reading in its first two years. The negative effect in math was 0.34 standard deviations over two years.²⁰

The most likely explanation for this anomalous finding is low private school participation in the program due to poor program design and fear of future action from hostile regulators. In sharp contrast to other choice programs, only a small minority of eligible private schools in Louisiana participate in the voucher program. Less than one-third of Louisiana private schools chose to participate in the program in its first year.²¹ Survey research finds

that fear of “future regulations” was the number one reason cited by private schools choosing not to participate.²²

Among other regulatory burdens, participating schools must administer the state test and can be removed from the program if their scores are too low. They are also subject to inspections by public school officials while tests are being administered. However, in addition to these existing regulations, we should also consider the survey results showing that private school leaders were worried about future regulations. The key issue may not have been the poor initial design of the program so much as a lack of trust that even these undesirable terms of participation would continue to be honored once schools entered the program. Moreover, an attempt by the U.S. Department of Justice to gain extensive regulatory control over participating schools through a strained reading of desegregation law, even though it was eventually struck down in court, would also have been noticed by high-quality Louisiana private schools that might have been interested in participating.²³

Schools choosing to join and remain within a choice program under such adverse conditions are likely to be the worst performing schools. Abdulkadiroglu, Pathak and, Walters note that “survey data show that LSP-eligible private schools experience rapid enrollment declines prior to entering the program, indicating that the LSP may attract private schools struggling to maintain enrollment.”²⁴ Where a program is unattractive to private schools, only schools desperate for cash flow can be expected to enter the program. And those schools will usually be the worst schools. If a private school is strapped for cash, that is a sign parents have decided it is not a good deal.

Other possible explanations have been offered to explain the negative finding. Though they are less likely to be primary factors, they are worth noting.

Lack of alignment between private school curricula and state tests is a possible factor. Schools not being ready to serve more challenging, disadvantaged

student populations is another—although it would be difficult to explain why this was a program-busting problem in Louisiana but not in New York, Milwaukee, Washington, D.C., Cleveland or any other school choice program. One hypothesis that is very unlikely to find empirical validation is the possibility that gains in participating private schools were outshone by even greater gains among public school students. As Abdulkadiroglu, Pathak, and Walters note, the students who lost the voucher lottery ended up in low-performing public schools.²⁵

TABLE 2 Academic Outcomes of Choice Participants

Location	Author	Year	Results			
			Any Positive Effect		No Visible Effect	Any Negative Effect
			All Students	Some Students		
Louisiana	Mills & Wolf	2016				X
Louisiana	Abdulkadiroglu et. al	2016				X
New York	Chingos & Peterson	2015		X		
New York	Bitler et. al.	2015		X		
New York	Chingos & Peterson	2013		X		
D.C.	Wolf et. al.	2013	X			
New York	Jin et. al.	2010		X		
Charlotte	Cowen	2008	X			
Toledo	Bettinger & Slonim	2006			X	
New York	Howell & Peterson	2004		X		
New York	Krueger & Zhu	2004			X	
New York	Barnard et. al.	2003		X		
New York	Howell & Peterson			X		
D.C.	Howell & Peterson	2002	X			
Dayton	Howell & Peterson			X		
Charlotte	Greene	2001	X			
Milwaukee	Greene et. al.	1998	X			
Milwaukee	Rouse	1998	X			

Note: This table shows all empirical studies using random-assignment methods.

PART II

Academic Outcomes of Public Schools

Thirty-three empirical studies have been conducted on how school choice programs affect academic outcomes in public schools. Of these studies, 31 find that choice improves academic outcomes at public schools. One of the remaining studies finds that choice has no visible impact on public schools, and one finds a negative impact.²⁶

For academic outcomes of public schools, this report looks at empirical studies using all methods. It is not possible to conduct random-assignment research on how choice affects public schools. Random assignment is possible only in studies of participants because of the naturally occurring opportunity to conduct a random lottery when there are more applicants to a choice program than there are slots available. There is no naturally occurring equivalent that would permit random-assignment research methods in studying the effects of choice on public schools. We must therefore turn to other evidence.

Fortunately, this question has been studied only more recently and the amount of evidence is manageable. The body of research is also of good methodological quality, increasingly so over time. The last decade has seen major improvements in the quality of studies on this question, to the point where some approach the confidence level of gold-standard random assignment.

It is also important to bear in mind that these studies are examining a different kind of question from those analyzing the effect on participants. The absence of random assignment is not as great a problem here. There is no act of parental choice that must be overcome methodologically. “Choosers” and “non-choosers” are not being compared; all the relevant students are non-choosers. The only comparison is between schools exposed to choice and schools not exposed, which is usually an easier methodological barrier to overcome.

What the Studies Show

There had been 23 studies of academic effects on public schools when the last edition of this report

was published in 2013. Readers seeking a descriptive overview of those studies should consult previous editions of this report. Ten additional studies have been published or have come to our attention since then. Of those 33 studies, 31 find school choice improves public schools, one finds no visible effect, and one finds a negative effect.

Seven studies examine Milwaukee’s voucher program, and all seven find a positive effect on public schools. Milwaukee’s voucher program is available to all students who meet an income restriction, so eligible students are spread across the city. Research methods in Milwaukee therefore cannot compare “public schools that are not affected by vouchers” and “public schools that are affected by vouchers.” Typically, they compare public schools with more students eligible for vouchers to public schools with fewer students eligible for vouchers. In two cases, researchers used the number of nearby private schools participating in the voucher program, or the number of available seats in such schools, as a measurement of voucher impact. All of these methods are like testing the effectiveness of a medicine by comparing the effects of a large dose to the effects of a small dose, rather than to the effects of not taking it at all – the research will tend to make the effect of vouchers look smaller than it really is. But it is the best that can be done given the absence of a better control.

One of these studies has come to our attention since the last edition of this report. In an unpublished dissertation, Nicholas Mader confirms the longstanding research finding that exposure to the voucher program produces moderate academic gains in Milwaukee public schools. He measures changes in the intensity of competitive pressure on a public school by measuring the “classroom capacity” (i.e. number of empty seats, by grade level) in nearby private schools. This is a problematic measure insofar as private schools have some capacity to increase or decrease the number of “seats” they provide in response to demand. Mader finds a positive relationship between increased capacity in nearby private schools and public school performance.²⁷

Fourteen empirical studies have been conducted on how two Florida voucher programs and one Florida tax-credit scholarship program have affected academic outcomes at public schools. Twelve find choice has improved Florida public schools and one finds a negative effect. One of the two voucher programs made all students at underperforming schools eligible for vouchers, so researchers were able to measure the effect of vouchers in two ways: 1) by comparing performance at the same school before and after voucher eligibility and 2) by comparing very similar schools that were just over or just under the threshold for voucher eligibility. Researchers also were able to measure the effect of “voucher threat” at low-performing schools that were in danger of becoming eligible for vouchers. For the other two programs (a voucher program for students with special needs and a tax-credit scholarship program for low-income students) researchers used methods similar to those used in the Milwaukee studies.

Three of those studies were published since the last edition of this report. One is a new analysis by David Figlio and Cassandra Hart of data they had analyzed in an earlier study. That study had used several different measurements of the presence of private school competition to examine the impact of Florida’s tax-credit scholarship program.²⁸ Among the refinements introduced in the new version of the study, they use the number of nearby houses of worship as a proxy measurement for private school competition, alongside more conventional methods of measurement—distance to the nearest private school, number of private schools within a given radius of the public school, number of different types of private schools within a given radius, and number of “slots per grade” (similar to Mader’s “classroom capacity,” above). The new study finds a positive effect on public schools in both reading and math for all five measures of private school competition, though the effect was much smaller for slots per grade and houses of worship than for the other three measures.²⁹

Another study that offers a new analysis of data previously examined by the same researchers was published by Cecilia Rouse, Jane Hannaway, Dan

Goldhaber, and Figlio. In a previous study, they found low-performing schools improved when their students could become eligible for vouchers if the schools didn’t improve.³⁰ In the new study, they find that this accountability pressure is specifically connected to changes in schools’ institutional practices and that these changes are, in turn, connected to test score improvements.³¹

In the third study published since the last edition of this report, Daniel Bowen and Julie Trivitt examine how schools were affected after the voucher program targeting failing schools was ended in 2006. They found that the designation of schools as “failing” had a positive effect (a so-called “stigma effect” motivating schools to improve in order to get rid of the failing grade) and that the removal of voucher eligibility from these schools did not reduce this effect, indicating vouchers were not producing a positive effect. Public schools saw no visible change in math scores, and Bowen and Trivitt found the removal of vouchers increased reading scores—a negative finding for vouchers.³²

The negative finding is hard to explain given that nine other studies find a positive effect from the same voucher program, including one study that focuses (as this one does) on how schools were affected by the removal of the vouchers in 2006.³³ Bowen and Trivitt write: “Despite the exhaustive data available, we are not currently able to explain the negative effect of the threat on reading performance definitively.”³⁴ A change in the state’s system for grading schools did require Bowen and Trivitt to make judgment calls about how to measure school exposure to the stigma effect, but there is no obvious reason to think their method deficient.

Twelve studies have been conducted on the effect of school choice programs in other places (Maine, Vermont, Ohio, Louisiana, Indiana, Washington, D.C., and San Antonio, Texas). Eleven of those studies find choice improves public schools’ academic outcomes, and one finds that it made no visible difference. The outlier study finding no effect is a study of the federal voucher program in Washington, D.C. This

is the nation's only school choice program with a "hold harmless" provision that allocates additional money to the public school system to "compensate" for the loss of students. The provision is intended to insulate the public school system from the competitive effects of school choice. Thus, the absence of a visible effect in this study does not detract from the research consensus in favor of a positive effect on public schools.

Six of these studies have been published, or come to our attention, since the last edition of this report. Three examine a privately funded program offering vouchers to all students in the Edgewood school district, near San Antonio, Texas, starting in 1998. John Diamond finds the percentage of Edgewood public school students passing state tests increased at a higher rate after the program was introduced, and Edgewood's graduation rate increased faster relative to the state as a whole.³⁵ Notably, these are very rough, low-quality methods of analysis, and readers would be unwise to rely much on these findings. Nonetheless, this study is included in this review to avoid "cherry picking" selectivity.

Two other studies of this program used better methods. In the first of these studies, John Merrifield and Nathan Gray find test scores and graduation rates improved at a faster rate in Edgewood than in two selected sets of control districts after the introduction of the program.³⁶ The second, conducted by Gray, Merrifield and Kerry Adzima, confirmed this finding using more sophisticated statistical methods and a different set of control variables.³⁷ This is a better method, although still not as good as the methods used in most studies of school choice programs' effect on public schools. Because all students in the district are eligible, designing a good study to examine its effects is a challenge.

Two studies that examine school choice programs in Louisiana and Indiana were published in a paper by Anna Egalite. She uses a variety of different ways to measure exposure to private school competition—1) distance to the nearest private school participating in the voucher program, 2) number of such schools

within a given distance, 3) number of different types of such schools within a given distance, and 4) an index that measures nearby private school types. She finds that in Louisiana, higher levels of competition on the last three of these measures increased both math and English scores at public schools exposed to vouchers. In Indiana, she finds no visible effect on math scores, but a positive effect on English scores from the last measure (the index of types of schools) and in some cases, depending on the distance from the school used, from the third measure (the number of types of schools).³⁸

Finally, Egalite published another study on how the Louisiana voucher program affects public schools. This study uses the same four measurements of exposure to private school competition, but analyzing them with a more complex statistical model. In this study, she finds positive effects in math when using two of her measurements—the number of private schools in a given radius and the number of types of private schools in a given radius. In contrast to Egalite's first Louisiana study, both positive findings remained when either a larger or smaller radius was used. However, she finds no visible effect on English scores.³⁹

As the first studies on how school choice affects public schools emerged, some speculated that the improvements they found in public schools might be caused by other factors besides a positive impact from school choice. Those alternate theories included a statistical phenomenon known as "regression to the mean," and the possibility that the worst students were leaving public schools. Subsequent research rigorously tested these alternative hypotheses and found them to be unsupported. These theories were extensively discussed in the original 2009 edition of this report; readers seeking a review of them should consult that edition. Notably, Bowen and Trivitt's Florida study is the first empirical study ever to test one of these alternative hypotheses and find in favor of it. In this case, they tested the "stigma effect" hypothesis, which holds that improvement in schools labeled "failing" and eligible for vouchers is due to the stigma of the failing label rather than the effects

of school choice. However, seven other empirical studies have also specifically tested for the presence of a stigma effect (in Florida and Ohio) and found that this effect either did not exist or was not large enough to explain away the school choice effect. Also, stigma cannot explain the positive findings for Milwaukee, Florida’s two other programs, or the century-old “town tuitioning” voucher systems in Maine and Vermont.⁴⁰

TABLE 3 Academic Outcomes of Public Schools

Location	Author	Year	Results		
			Any Positive Effect	No Visible Effect	Any Negative Effect
Louisiana	Egalite	2016	X		
Louisiana	Egalite	2014	X		
Indiana	Egalite	2014	X		
Florida	Figlio & Hart	2014	X		
Florida	Bowen & Trivitt	2014			X
San Antonio	Gray et. al.	2014	X		
Florida	Rouse et. al.	2013	X		
Florida	Chakrabarti	2013	X		
Florida	Figlio & Hart	2011	X		
Florida	Winters & Greene	2011	X		
Ohio	Carr	2011	X		
Milwaukee	Mader	2010	X		
Milwaukee	Greene & Marsh	2009	X		
San Antonio	Merrifield & Gray	2009	X		
Ohio	Forster	2008	X		
Florida	Forster	2008	X		
Milwaukee	Chakrabarti	2008	X		
Florida	Chakrabarti	2008	X		
Milwaukee	Chakrabarti	2008	X		
Florida	Rouse et. al.	2007	X		
Milwaukee	Carnoy et. al.	2007	X		
San Antonio	Diamond	2007	X		
D.C.	Greene & Winters	2007		X	
Florida	Figlio & Rouse	2006	X		
Florida	West & Peterson	2006	X		
Florida	Greene & Winters	2004	X		
Florida	Chakrabarti	2004	X		
Milwaukee	Greene & Forster	2002	X		
San Antonio	Greene & Forster	2002	X		
Maine	Hammons	2002	X		
Vermont	Hammons	2002	X		
Milwaukee	Hoxby	2001	X		
Florida	Greene	2001	X		

Note: This table shows all empirical studies using all methods.

PART III

Fiscal Impact on Taxpayers and Public Schools

There have been 28 empirical studies examining the fiscal impact of school choice on taxpayers and public schools. Of those, 25 find school choice programs save money, and three find the programs studied are revenue neutral.

Measuring Fiscal Impact

This report covers all empirical studies of actual school choice programs. It does not cover analyses that predict fiscal impact using economic modeling; such analyses are not empirical studies. Analyses of this kind are familiar to policymakers and opinion leaders through the widespread use of legislative “notes” and comparable analyses to predict the impact of legislation. These analyses are legitimate and important, as legislators and the public must have some basis on which to evaluate specific legislative proposals, and empirical data from the future are, unfortunately, not available. However, for the general purpose of evaluating the effect of school choice, empirical research on actual program effects is far preferable to modeling.

School choice affects the finances of taxpayers and public schools. Because public schools are government agencies, both represent fiscal impact on the public. Fiscal studies are therefore classified here based on the total effect they find on taxpayers and public schools. Additionally, this method is appropriate because some studies report the total effect on both without differentiating between effect on taxpayers and effect on public schools.

To some extent, effect on taxpayers and effect on public schools are interchangeable. Money saved in state budgets is money that can be used for increased funding of public schools, and money saved in school budgets is money taxpayers can recoup through corresponding funding cuts. This interchangeability is greatly mitigated in practice because states have many budget priorities other than public schools, and funding of public schools is not strongly responsive to improved efficiencies in school budgeting. In the text below, those categories are treated as two distinct loci

of savings. But a program’s fiscal impact on the public includes its effect on both, so each study’s results are classified based on the total effect.

With regard to taxpayers, the public spends money on schools at all three levels of government—federal (10 percent), state (45 percent), and local (45 percent)—but direct fiscal impact on taxpayers usually occurs only at the state level.⁴¹ School choice does not have much immediate impact on federal and local taxpayer funding for schools, because funding at those levels is not highly sensitive to changes in school enrollment. Federal funding mainly flows through Title I for low-income students and special education programs. Title I allocates funds based on the demographics of the school district, and federal special education spending was reformed in 1997 specifically to disconnect funding formulas from enrollment levels (to avoid creating a financial incentive to place students in special education). Meanwhile, local funding typically comes from property taxes. Small amounts of federal and local funding may vary with enrollment, but they are too complex and too small as a percentage of education spending to be worth tracking.⁴²

By contrast, school choice has a major effect on state funding. Spending on schools has been migrating toward the state level over time, to the point where education is now a very large portion of most state budgets. Education spending sometimes even makes up a majority of the state general fund.⁴³ This change has been driven in large part by concerns over equity in funding across districts. Because of those concerns, almost every state funds schools based on their enrollment levels, allocating a base amount per student to each district (usually with some adjustments for local conditions). Most states have two major systems for funding schools: a “formula funding” system that distributes the majority of spending based on number of students and a separate fund for capital expenses, such as building costs.

As a result, school choice creates savings and costs for state budgets. When a student uses school choice to leave public school for a private school, the state

must cover that student's cost to the choice program, but it also spends less on public schools by an amount equal to one student's worth of funding. For example, if the share of public school funding that comes out of the state budget is \$5,000 per student, and the state offers vouchers equal to \$4,000 per student, a typical student using the program will save state taxpayers \$1,000. For each student entering the voucher program from a public school, the state's spending on vouchers will go up by \$4,000, but its spending on public schools will go down by \$5,000. Thus, students who leave public schools because of the school choice program create savings for the state budget. The net effect of the program is equal to the amount of savings from students leaving public schools minus the total cost of the program. This total cost includes costs associated with the small number of voucher participants who would have self-financed private education even in the absence of the program, generating costs but no savings.

There is always some variation within these broad parameters. Some students will not use the full voucher amount, reducing the program cost. Savings in public school spending also will vary from student to student as a result of state funding formulas that adjust spending somewhat based on local conditions in each district.

Another issue is the fiscal impact of school choice on public schools. Here, as with state budgets, there are two sides to the ledger. When a student leaves a public school using a choice program, the school loses all the costs associated with educating that student but not all the funding. As has been noted already, federal and local education spending does not vary much with enrollment, so those funds stay when students leave. That means public schools are left with more money to serve the students who remain.

An empirical study on schools nationwide confirms this. Benjamin Scafidi examines school finances in every state and finds that out of a total of \$12,450 spent per student in 2008–09, only 64 percent (\$7,967) is made up of variable costs that change with the

number of students enrolled.⁴⁴ This means school choice programs produce financial windfalls for local schools if they redirect less than that amount per student.

Though local taxpayers might not immediately benefit much because property tax levels are not sensitive to enrollment changes, local schools would benefit a great deal because they would have more money to spend per student. This is one possible explanation for the positive impact choice has on public school outcomes. Of course, it is possible that as school choice generates large savings for local schools, local governments may eventually take notice and recover some of those savings for taxpayers by lowering property taxes.

What the Studies Show

The previous edition of this report counted six empirical studies on the fiscal effects of school choice. However, one of those “studies” actually includes 12 separate analyses of 12 separate programs, so—applying the same standard that we have consistently applied to academic effects studies in every edition of this report—it ought to have been counted as 12 studies rather than as one.⁴⁵ This would have produced a total of 17 fiscal studies in the previous edition. Since then, 11 more studies have been published; 10 are included in a single report that separately analyzes the fiscal effects of 10 school choice programs. Of these 28 studies, 25 find that school choice programs save money, and three find the programs studied to be revenue neutral.

The publication that studies 12 programs, released in 2007, is a comprehensive review of the fiscal effects of all school choice programs in existence from 1990 to 2006. Two of the programs it studies are century-old “town tuitioning” programs in Maine and Vermont, which were created to cover private school tuition in small towns that decided not to create their own public schools when the government education monopoly was first created. Because of the unique design of these programs, they are not

only revenue neutral for states (which direct 100 percent of state education spending to the program) but generate no savings for local public schools (as the affected towns have no public schools). A third program, a voucher for students with special needs in Utah, is found to be revenue-neutral for the state, which directs 100 percent of its spending for those students into the program. Its impact on public schools was not assessed due to lack of necessary data on local special education spending.

The other nine programs in this study are all found to have a net positive fiscal effect. The three tax-credit scholarship programs studied are all found to cost money at the state level, on the assumption that state legislatures do not reduce public school funding to offset lost tax revenue from the tax credits, but these costs are outweighed by savings for public schools. However, this balance is reversed in the Milwaukee voucher program, whose legislation—over the objection of local school choice advocates—includes a transfer of funding from local schools to the state that is unnecessary for the operation of the program. This “funding flaw,” as it is locally known, creates a negative fiscal impact on local public schools, but is outweighed by savings at the state level (including funds seized by the funding flaw). Fortunately, the funding flaw is now in the process of being phased out. Unfortunately, the phase-out is so gradual that it will not be complete until 2025. The other five programs are either positive or neutral at both the state and local school levels, and all have an overall positive fiscal effect.⁴⁶

Since the previous edition of this report, another large-scale evaluation of multiple programs has been published. A 2014 study by Jeff Spalding examines the fiscal effect of ten school choice programs from 1990 through 2011. Unlike the 2007 review, this study does not differentiate between savings for state budgets and local school budgets. It simply subtracts the per-student cost of providing the program from the per-student reduction in educational costs, yielding a total savings figure without examining how savings were distributed between state and school budgets. It finds that all 10 programs save money; the grand total of

savings from those 10 programs was \$1.7 billion from 1990 through 2011.⁴⁷

Another study of a single program has come to our attention since the previous edition of this report. Merrifield and Gray include in their study of the privately funded program in San Antonio, Texas, an analysis of how the program affected taxpayers. Because the program was privately funded, this analysis explores a different set of questions than the rest of the research in this field. Merrifield and Gray find the program attracted families to the Edgewood school district, where they would become eligible for vouchers. The increase in the number and value of houses, including about a \$6,500 rise in the value of the average house, delivered a \$10.6 million benefit to local taxpayers.⁴⁸

TABLE 4 Fiscal Impact on Taxpayers and Public Schools

Location	Author	Year	Results		
			Positive Effect	No Visible Effect	Negative Effect
D.C.	Spalding	2014	X		
Florida	Spalding	2014	X		
Florida	Spalding	2014	X		
Georgia	Spalding	2014	X		
Louisiana	Spalding	2014	X		
Cleveland	Spalding	2014	X		
Ohio	Spalding	2014	X		
Ohio	Spalding	2014	X		
Utah	Spalding	2014	X		
Milwaukee	Spalding	2014	X		
D.C.	Wolf & McShane	2013	X		
Florida	LOEDR*	2012	X		
Milwaukee	Costrell	2010	X		
San Antonio	Merrifield & Gray	2009	X		
Florida	OPPAGA**	2008	X		
Vermont	Aud	2007		X	
Maine	Aud	2007		X	
Milwaukee	Aud	2007	X		
Cleveland	Aud	2007	X		
Arizona	Aud	2007	X		
Florida	Aud	2007	X		
Florida	Aud	2007	X		
Pennsylvania	Aud	2007	X		
Florida	Aud	2007	X		
D.C.	Aud	2007	X		
Ohio	Aud	2007	X		
Utah	Aud	2007		X	
D.C.	Aud & Michos	2006	X		

Note: This table shows all empirical studies using all methods; the total fiscal effect of school choice programs is referenced.

*LOEDR stands for Legislative Office of Economic and Demographic Research (State of Florida).

**OPPAGA stands for Office of Program Policy Analysis and Government Accountability (State of Florida).

PART IV

Racial Segregation in Schools

There have been 10 studies using valid empirical methods to examine school choice and racial segregation in schools. Nine of those studies find school choice moves students into less racially segregated classrooms. The remaining study finds school choice has no visible effect on racial segregation. None finds choice increases racial segregation.

Public schools have been growing more racially segregated for some time. Paradoxically, this is happening even as residential segregation has declined.⁴⁹ Understandably, racial segregation in schools is an increasing concern. The issue of school choice and racial segregation involves a number of interlocking societal concerns. Public schools are intractably segregated by race, mostly because students are assigned to schools based on where they live. School choice has the potential to break down those residential barriers. Even so, many people have difficulty giving the evidence on this question a hearing. Space does not permit a discussion of the issues here, but they are reviewed in an earlier report entitled *Freedom from Racial Barriers*, and interested readers can consult that publication.⁵⁰

Measuring Racial Segregation

Unfortunately, most research on school segregation is compromised by inadequate definitions of segregation. Researchers typically use the racial makeup of a larger administrative unit—such as a school district, a municipality, or a system of private schools—as the standard against which segregation in individual schools is measured. This problem is present, for example, in the way segregation measures such as the Index of Dissimilarity, the Index of Exposure, and the Gini Index are commonly used. All this approach really does is measure the evenness of the racial distribution within the chosen administrative unit. It ignores any segregation caused by the structure of the administrative unit itself. Much of the segregation in the public school system occurs because school districts and municipal boundaries themselves are segregated, so studies using this approach effectively mask the real level of segregation.

Jay Greene provides an instructive example that shows how this problem undermines the validity of such measures of segregation. In studies using the prevailing method, a school that is 98 percent white is considered perfectly integrated if it is in a school district that also is 98 percent white. The school receives this perfect score even if the 98-percent-white school district is right next door to another district that is 98 percent minority. Clearly, this should be considered segregation, but the prevailing method masks segregation when it occurs at the district level. Greene issues a concise verdict on what studies like this really are saying: “The schools are well integrated, given that they are horribly segregated.”⁵¹

A segregation study in the recent lawsuit challenging Louisiana’s voucher program is a case in point. Christine Rossell finds that the program reduced racial segregation, using the Relative Exposure Index, which “standardizes the IEm index—the proportion white in the average minority child’s school—by the proportion white in the district.” In other words, the index Rossell relies upon does not, in fact, measure how racially segregated schools are. It measures how evenly each school district’s level of racial segregation is distributed across its schools. A highly segregated district in which the experience of segregation was evenly distributed across all its schools would look good on this measure. Rossell gives the game away when she repeatedly refers to the Relative Exposure Index as a measure of “racial balance.” She calls it this because it is not a measure of racial segregation. Unfortunately, Rossell speaks all too truly when she comments that “this index has been used in many cases, including every school desegregation case I have been involved in.”⁵²

Another common problem in the existing research on school segregation is the failure to compare similar grade levels. Elementary schools tend to be more segregated than secondary schools because they draw from a smaller geographic area. Private schools are more likely than public schools to be elementary schools, so a comparison of all public schools and all private schools will create a false impression of greater segregation in private schools.

To get an accurate picture of segregation levels, researchers must compare elementary schools to elementary schools and secondary schools to secondary schools. It also is important not to compare student populations composed of only prekindergarten or kindergarten students, as access to and voluntary participation in these grade levels is very uneven.

This report reviews all available studies using empirical methods that do not fall afoul of the problems described above. The best way to measure segregation is by comparing schools to the racial composition of the larger metropolitan area in which they are situated. By looking at the whole metropolitan area rather than a particular administrative unit, such as a school district, researchers can detect levels of segregation that most studies miss. A second-best way employed by some studies is to measure the occurrence of racial homogeneity. For example, measuring the percentage of schools that are more than 90 percent white or more than 90 percent minority.

In many cases, the available evidence on school segregation is only descriptive rather than causal. Where researchers have access to data on individual school choice users matched to their public and private schools before and after the introduction of a school choice program, they can examine causal relationships in the relevant variables. In other cases, researchers can only describe the segregation levels in affected public and private schools; they cannot examine to what extent the choice program, rather than other factors, is causing the levels to be what they are.

However, the descriptive evidence available in those cases is enough to show the impact choice has on the school environments of participating students. They can tell us whether school choice programs are moving students into schools that are more segregated or less segregated than their assigned public schools at a given moment in time. While we cannot draw causal conclusions when we only have a “snapshot” of a single moment, the snapshot

is still an accurate snapshot; it describes what is happening at that moment. These studies also provide a baseline against which popular descriptive claims can be evaluated. Widespread claims that private schools participating in choice programs are heavily segregated should be examined against this evidence.

What the Studies Show

Ten empirical studies have examined segregation levels in public schools and choice-participating private schools without falling afoul of the methodological problems described previously. Eight of those studies were included in the previous edition of this report, published in 2013. One of those studies, the only one able to use individual student data to examine causal effects, finds the Milwaukee voucher program was having no visible effect on segregation levels in the period studied.⁵³ This was after the program had already been in existence for a decade. It is possible that the program had an effect on segregation during its first decade and then produced a stable equilibrium, but in the absence of the necessary historical data we cannot know.

The remaining seven studies, using descriptive data, find school choice moves students from more segregated public schools into less segregated private schools. Three studies of the voucher program in Milwaukee used measures of racial homogeneity and a fourth compared schools to the population of the surrounding metropolitan area. Two studies of the voucher program in Cleveland compared schools to the surrounding metropolitan area. Additionally, one of them used a measure of racial homogeneity. A study of the voucher program in Washington, D.C. also used both types of measures.

The two empirical studies that have been published since the previous edition of this report both examine the Louisiana voucher program. Egalite and Mills use individual student data, which were available for 841 of the participating students, to perform a

causal analysis of the effect of the program on racial segregation in 2012–13. Using the population of the surrounding metropolitan area as their standard of comparison, they find that the student transfers from public to private schools under the voucher program reduce segregation in both the public schools and the private schools. That is, these transfers move both public and private schools closer to the racial composition of the surrounding metropolitan area.⁵⁴

A second study by Egalite, Mills, and Wolf runs a similar analysis on a data set including 1,741 students. That analysis finds 82 percent of student transfers

reduce segregation in affected public schools, while 18 percent increase it, indicating a large net positive effect. On the other hand, 45 percent of transfers reduce segregation in affected private schools, while 55 percent increase it, indicating a much smaller net negative effect. The overall effect on racial segregation in schools is therefore positive. They also conduct a separate examination of the 34 Louisiana school districts under federal desegregation orders. In those districts, 75 percent of voucher transfers reduce segregation in public schools, while no visible effect was found on private schools.⁵⁵

TABLE 5 Racial Segregation

Location	Author	Year	Results		
			Positive Effect	No Visible Effect	Negative Effect
Louisiana	Egalite et. al.	2016	X		
Louisiana	Egalite & Mills	2014	X		
Milwaukee	Greene et. al.	2010		X	
Milwaukee	Forster	2006	X		
Cleveland	Forster	2006	X		
D.C.	Greene & Winters	2005	X		
Milwaukee	Fuller & Greiveldinger	2002	X		
Milwaukee	Fuller & Mitchell	2000	X		
Milwaukee	Fuller & Mitchell	1999	X		
Cleveland	Greene	1999	X		

Note: This table shows all empirical studies using all methods; the total effect on segregation in all schools is referenced.

PART V

Civic Values and Practices

There have been 11 empirical studies examining how school choice affects civic values and practices, such as toleration for the rights of others. Eight of those studies find school choice has a positive effect on these civic concerns. The remaining three studies find school choice has no visible effect on them. None finds choice has a negative effect on these values and practices.

Measuring Civic Values and Practices

Research on how education affects civic values and practices has measured a wide range of variables, including tolerance for the rights of others, civic knowledge, civic participation, volunteerism, social capital, civic skills, and patriotism. Wolf’s 2008 article “Civics Exam,” the largest existing review of the research comparing public and private schools on these issues, finds the research overwhelmingly points to either no difference or a positive effect from private schooling on these measures—even in studies that use methods to compensate for the “selection bias” of families selecting into private schools. Readers interested in a thorough overview of that research should consult Wolf’s systematic review.⁵⁶

This report looks only at empirical studies of school choice programs, as opposed to the broader universe of studies that compare public and private schooling generally. These studies tend to be methodologically superior, as better ways of accounting for selection bias are often available with choice programs. In four cases, gold-standard random-assignment methods were employed.

This report examines all empirical studies of civic values and practices using all methods, not just the random-assignment studies. This is not the practice we followed for studies of academic effects. There are a smaller number of random-assignment studies available for civic values and practices than for academic effects, and it is dangerous to rely on too small a universe of studies. Only two random-assignment studies of civic values and practices have been published since 2002, one of which was

a re-analysis of old data. In other words, we do not have grounds to expect a significant body of random-assignment studies will be built up over time, as has already occurred for studies on academic effects.

One study examining a privately funded voucher program in San Francisco has been excluded from this review because it is only a descriptive comparison of the voucher-using and non-voucher-using populations. That study finds no visible difference in tolerance for the rights of others between the two populations. However, as a descriptive analysis, this cannot explain much about whether the voucher had an effect, as distinct from other factors.⁵⁷ It was appropriate to include descriptive studies in the review of research on racial segregation because descriptive information about the racial makeup of schools provides insight into an important question: What effect has school choice had on students’ school environments? If choice is moving students from more segregated schools to less segregated schools (or vice versa), it is highly desirable to know that. Here, the descriptive information does not contribute to the relevant policy questions.

The most widely studied topic in this field is tolerance for the rights of others. Researchers generally use the same method to measure this topic, with only small variations. Students are asked to identify their least-liked group of people. Students typically name a variety of groups ranging from neo-Nazis and the KKK to those who disagree with them on passionate political issues (for example, pro-lifers name pro-choicers and pro-choicers name pro-lifers) to disliked religious minorities such as evangelical Christians. Students are then asked a battery of questions on whether their least-liked group should be permitted to have or do certain things. Examples include voting, organizing a march, or having a book in the library sympathetic to their point of view.

What the Studies Show

Of the 11 studies on this topic, seven were included in the previous edition of this report, published in 2013.

Three new studies have been published since then, and one has come to our attention. Of those 11 studies, eight find school choice improves civic values and practices, and three find no visible effect. None finds school choice has a negative effect.

Four of the studies included in the previous edition of this report use random-assignment methods. Of those, one finds students using vouchers to attend private schools are more likely to show tolerance for the rights of others they dislike. A second confirms this positive finding for tolerance, while finding no visible difference in civic knowledge. A third finds no visible difference for tolerance. A fourth finds no visible difference for tolerance or civic knowledge.

Of the three non-random-assignment studies included in the previous edition of this report, one finds school choice students are more tolerant of the rights of those they dislike, but finds no visible effect on civic knowledge. The other two find parents of children participating in school choice programs are 1) more likely to be actively involved in their children's schools, parent-teacher organizations, and other education groups and 2) more likely to see a connection between education and the civic institutions of society, to say that their children are learning how government works, and to be involved in civic activities themselves.

Bettinger and Slonim's 2006 random-assignment study of a privately funded school choice program in Toledo focused on altruistic behaviors. Students and parents participated in six different experiments, during which they had to decide how much out of a given amount of money to keep for themselves and how much to share with a charity or peer. The study finds the voucher program increased the amount of money participating students donated to charities. No effect was found on students' donations to peers or on parents' donations.⁵⁸

In one of the three new studies published since the previous edition of this report, David Fleming, William Mitchell, and Michael McNally find students participating in the Milwaukee voucher program have

modestly higher levels of political tolerance, civic skills, future political participation, and volunteering when compared to public school students. When religious private schools are separated from other private schools, they find the positive effect is significantly stronger in religious schools.⁵⁹

In a second new study, Mills and four co-authors conducted a phone survey of applicants to the Louisiana voucher program, including those who were and those who were not ultimately offered vouchers. They found that among those responding to the survey, there was no visible difference between those who were and were not offered a voucher in respect for the rights of others or in several other non-cognitive outcomes, such as grit, locus of control, and self-esteem. However, diagnostic analysis raised questions about the precision of their survey instrument, and the study was also undermined by a low response rate—only 11 percent. The study authors rightly caution us not to attribute too much weight to these results. As with other studies described above, we include this study in obedience to our rule of including even methodologically challenged studies, to avoid the possibility of “cherry-picking.”⁶⁰

In the final new study, Corey DeAngelis and Wolf examine the impact of Milwaukee's voucher program on students' criminal records. They used seven measurements: 1) whether students were accused of any crime, 2) whether they were convicted of any crime, 3) whether they were convicted of a felony, 4) whether they were convicted of a misdemeanor, 5) whether they were convicted of a traffic-related offence, 6) whether they were convicted of a theft-related offence or 7) whether they were convicted of a drug-related offence. Their sample included students who were in eighth or ninth grade in 2006 and examined criminal outcomes as of 2015, when the students would be 22–25 years old. Matching participating students to non-participating students with similar demographic characteristics and test scores then comparing the two, they found that the voucher program decreased participants' criminal activity, especially for males. The longer students remained in the voucher program, the more this

positive finding was visible across multiple measures of criminal outcomes. Males who remained in the program throughout high school had better outcomes than their public-school peers on all seven measurements. For example, this group had a 79 percent reduction in felonies (relative to the total incidence of felonies) due to vouchers, a 93 percent reduction in drug offenses, and an 87 percent reduction in theft.⁶¹

TABLE 6 Civic Values and Practices

Location	Author	Year	Results		
			Any Positive Effect	No Visible Effect	Any Negative Effect
Milwaukee	DeAngelis & Wolf	2016	X		
Louisiana	Mills et. al.	2016		X	
Milwaukee	Fleming et. al.	2014	X		
Nationwide	Campbell	2013	X		
Milwaukee	Fleming	2012	X		
Milwaukee	Fleming	2011	X		
Toledo	Bettinger & Slonim	2006	X		
D.C.	Howell & Peterson	2002		X	
Nationwide	Campbell	2002	X		
Nationwide	Peterson & Campbell	2001		X	
D.C.	Wolf et. al.	2001	X		

Note: This table shows all empirical studies using all methods.

CONCLUSION

Remarks on School Choice

Universal Choice Could Deliver an Education Revolution

Critics of school choice often ask: If school choice is so great, why are the public school systems in cities and states with choice still showing little to no overall improvement? Milwaukee public schools were widely dysfunctional in 1990 when the voucher program was enacted, and they remain widely dysfunctional today. There has been no “Milwaukee Miracle.”

But the absence of a dramatic “miracle” is not a valid reason to conclude that choice is not helping. The government monopoly school system is so tenaciously resistant to change that it is unreasonable to expect miraculous results from any education reform.

Yes, Choice Improves Schools

Countless factors affect the overall performance of a school system. Some of those factors, such as political policymaking, can change quickly and dramatically. Others, such as demographic factors, are highly stable.

As a result, the overall performance of a school system can never, by itself, provide a reliable guide to whether any one factor, such as school choice, is having a positive effect. If a man with asthma starts taking a new medication and at the same time takes up smoking, his overall health and ability to breathe may not improve, but this has no bearing on the question of whether the medicine is helping.

The only way to know whether choice is having a positive impact is to conduct empirical research using high-quality scientific methods. That is the whole purpose of using scientific methods—to isolate the effect of choice from the effects of all the other factors that influence academic outcomes, so effect can be measured accurately.

Given the remarkably consistent findings of the research, it is clear school choice is having a positive effect. It is wrong to say choice must be doing no good simply because a lot of public schools are still

“failing” standardized tests. Claims that choice “does not work” directly contradict a clear consensus in the scientific evidence.

Choice Could Work Much Better... If We Let It

And yet, though it might be unreasonable to expect miracles, there is still an urgent need for larger improvements than choice is now delivering. Are the results of today’s programs the best that school choice can do? Or is it reasonable to expect more?

The positive effect of school choice programs identified in the empirical research is sometimes large, but it is more often modest in size. That is hardly surprising given that existing choice programs are also modest in size. If modest programs produce modest benefits, is the logical conclusion to deny that these programs have any benefit and give up on them? Or is the logical conclusion to expand them—and protect them from the kinds of overregulation experienced in Louisiana—until they are able to have a bigger effect?

Existing school choice programs are formally hindered in a number of ways, such as:

- limits on the number of students they may serve,
- limits on the types of students they may serve,
- limits on the purchasing power they are allowed to provide,
- limits on families’ ability to supplement that purchasing power, and
- limits on how students may be admitted to participating schools.

An earlier report, *The Greenfield School Revolution and School Choice*, discusses the significance of these limitations in more detail.⁶² And as the case of Louisiana shows, informal threats to choice programs also cannot be overlooked.

Only Universal School Choice Can Sustain Dramatic Change

Ultimately, the only way to make school reform work on a large scale is to break the government monopoly on schooling. The monopoly is not just one powerful obstacle to reform among many; it is what makes all the many obstacles as powerful as they are. The monopoly ensures that no meaningful accountability for performance can occur, except in rare cases as a result of Herculean efforts. The monopoly empowers a dense cluster of rapacious special interests resisting efforts to improve schools.

The monopoly creates an environment where the urgent need for change cannot be made a tangible part of the daily cultural life of the school. Institutional culture in the existing system is hostile, not just to this or that reform, but also to reform in general, because the monopoly excludes the only institutional basis for making the need for change seem plausible and legitimate: the prospect of losing the institution's client base and the funding that goes with it.

When any institution has a captive client base, support for innovation vanishes. Reform requires people and institutions to do uncomfortable new things, and change will not occur until discomfort with the status quo becomes greater than the discomfort of the change. An institution with captive clients can continue to function into the foreseeable future more or less as it always has, without change. Why not just continue doing things in the way that feels comfortable and natural?

Worst of all, the monopoly pushes out educational entrepreneurs who can reinvent schools from the ground up. Only a thriving marketplace that allows entrepreneurs to get the support they need by serving their clients better can produce sustainable innovation.

In any field of human endeavor—whether education, medicine, politics, art, religion, manufacturing, or anything else—entrepreneurs who want to strike out

in new directions and do things radically differently need a client base. There need to be people who will benefit from the new direction and support it. And that client base must be robust on three dimensions: size, strength, and suffrage. There must be enough supporters; they must have enough ability to provide support; and they must have enough freedom to decide for themselves what to support.

The government school monopoly crowds out this client base. School choice has the potential to solve this problem by providing enough families (size) with enough dollars (strength) and enough choice (suffrage) to support educational entrepreneurs. Unfortunately, existing school choice programs fall short on all three dimensions. Only universal choice can open the door to the full-fledged revolution in schooling America needs in the new century.

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7. Greg Forster, *A Win-Win Solution: The Empirical Evidence on School Choice*, 3rd ed. (Indianapolis: Friedman Foundation for Educational Choice, 2013), <http://www.edchoice.org/wp-content/uploads/2015/07/2013-4-A-Win-Win-Solution-WEB.pdf>.
8. Another option would be to consider each analytical model a separate “study” and simply report them in the aggregate as hundreds of studies. But this would give undue weight to research that runs numerous analyses; a study using a hundred analyses (for example, to test the robustness of a finding across many analytic specifications) does not have twenty times more evidentiary weight than a study using five analyses. It would also defeat the purpose of this report, which is to make the results of the research easily understandable.
9. Specifically, we ran the searches using the following four constructions: +“school choice,” +voucher, +“tax credit” and +“education savings account.” Tests showed that plurals did not change search results (e.g. “voucher” and “vouchers” yielded identical results).
10. Specifically, we conducted each search multiple times, adding a date qualifier each time. We used `pubdate:2013`, `pubdate:2014`, `pubdate:2015` and `pubdate:2016` in turn.
11. Specifically, we ran the searches for “articles” or “books” published from 2013 through 2016 using the following four constructions: +“school choice,” +voucher, +vouchers, +“tax credit,” +“tax credits,” +“education savings account” and +“education savings accounts.” Plural search terms yielded different results so we ran both singular and plural versions of search terms.
12. See Paul E. Peterson, “A Courageous Look at the American High School,” *Education Next* 10, no. 2 (Spring 2010), pp. 25-33, http://educationnext.org/files/ednext_20102_24.pdf.
13. Alan Krueger and Pei Zhu, “Another Look at the New York City School Voucher Experiment,” *American Behavioral Scientist* 47, no. 5 (Jan. 2004), pp. 658-98, doi:10.1177/0002764203260152.
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19. Atila Abdulkadiroglu, Parag A. Pathak, and Christopher R. Walters, “School Vouchers and Student Achievement: First-Year Evidence from the Louisiana Scholarship Program,” NBER Working Paper 21839 (Cambridge, MA: National Bureau of Economic Research, 2015), <http://www.nber.org/papers/w21839>.
20. Jonathan N. Mills and Patrick J. Wolf, *The Effects of the Louisiana Scholarship Program on Student Achievement after Two Years*, Louisiana Scholarship Program Evaluation Report 1 (Fayetteville: Univ. of Ark., School Choice Demonstration Project; New Orleans: Tulane Univ., Education Research Alliance, 2016), <http://educationresearchalliancenola.org/files/publications/Report-1-LSP-Y2-Achievement.pdf>.
21. *Ibid.*, p. 38.
22. Brian Kisida, Patrick J. Wolf, and Evan Rhinesmith, *Views from Private Schools* (Washington, DC: American Enterprise Institute, 2015), <https://www.aei.org/wp-content/uploads/2015/01/Views-from-Private-Schools-7.pdf>.
23. See Lindsey M. Burke and Jonathan Butcher, “School-Voucher Rules Trip Up Student Success in Louisiana,” *National Review Online*, Jan. 6, 2016, <http://www.nationalreview.com/article/429320/school-vouchers-threatened-doj-over-regulation>.
24. Abdulkadiroglu, Pathak, and Walters, “School Vouchers,”

abstract. LSP stands for Louisiana Scholarship Program.

25. Ibid. Granted, Louisiana’s underperforming public schools could – in theory – have suddenly started making huge gains, and this remarkable phenomenon could – in theory – have been completely overlooked by everybody right up to the point where it was needed to explain away the empirical findings of the research on Louisiana’s voucher program, whereupon it was suddenly discovered. Evaluating the plausibility of this hypothesis is left as an exercise for the reader. For discussion of these alternative theories, see the articles and blog posts cited in Jason Bedrick, “The Folly of Overregulating School Choice: A Response to Critics,” *Cato at Liberty* (blog), Jan. 8, 2016, <http://www.cato.org/blog/folly-overregulating-school-choice-response-critics>.

26. This report’s count of studies examining public school effects differs somewhat from that in Patrick J. Wolf and Anna J. Egalite, *Pursuing Innovation: How Can Educational Choice Transform K–12 Education in the U.S.?* (Indianapolis: Friedman Foundation for Educational Choice, 2016), <http://www.edchoice.org/wp-content/uploads/2016/05/2016-4-Pursuing-Innovation-WEB.pdf>. The main reason for the difference is a stricter screen used by Wolf and Egalite, which excludes studies included in this report. Wolf and Egalite are not just reviewing what the research finds but conducting an integrative evaluation of the state of knowledge in the field. A stricter screen is appropriate to their integrative purpose just as a more inclusive screen is appropriate to the descriptive purpose of this report. (Integrative and evaluative comments are offered in the text of this report, but they are not its main purpose.) Also, Wolf and Egalite have stated to the author of this report that one study included here (the Bowen and Trivitt study in Florida) came out too late to be included in their report.

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34. Bowen and Trivitt, “Stigma without Sanctions,” p. 11.

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37. Nathan L. Gray, John D. Merrifield, and Kerry A. Adzima, “A Private Universal Voucher Program’s Effects on Traditional Public Schools,” *Journal of Economics and Finance* 39, no. 1 (Dec. 2014), pp. 1–26, doi:10.1007/s12197-014-9309-z.

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39. Egalite, *The Competitive Effects of the Louisiana Scholarship Program on Public School Performance*, Louisiana Scholarship Program Evaluation Report 4 (Fayetteville: Univ. of Ark., School Choice Demonstration Project; New Orleans: Tulane Univ., Education Research Alliance, 2016), <http://educationresearchalliancenola.org/files/publications/Report-4-LSP-Competitive-Effects.pdf>

40. For details see the discussions in the 2009 and 2011 editions of this report, as well as Matthew Carr’s study of Ohio’s EdChoice program, discussed in the 2013 edition of this report.

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45. See note 42 above.

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47. Jeff Spalding, *The School Voucher Audit: Do Publicly Funded Private School Choice Programs Save Money?* (Indianapolis: Friedman Foundation for Educational Choice, 2014), <http://www.edchoice.org/wp-content/uploads/2015/07/The-School-Voucher-Audit-Do-Publicly-Funded-Private-School-Choice-Programs-Save-Money.pdf>.
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52. Christine H. Rossell, *A Report on the Effect of the 2012-13 Scholarship Program on Racial Imbalance in Louisiana School Districts Under a Desegregation Court Order as of 2012-13*, special report prepared in the case of *Brumfield v. Dodd*, Nov. 7, 2013, <http://images.politico.com/global/2013/11/07/lavoucheraff2.pdf>.
53. Jay P. Greene, Jonathan N. Mills, and Stuart Buck, *The Milwaukee Parental Choice Program’s Effect on School Integration*, SCDP Milwaukee Evaluation Report 20 (Fayetteville: Univ. of Ark., Dept. of Education Reform, School Choice Demonstration Project, 2010), <http://www.uaedreform.org/downloads/2010/04/report-20-the-milwaukee-parental-choice-programs-effect-on-school-integration.pdf>.
54. Anna J. Egalite and Jonathan N. Mills, “The Louisiana Scholarship Program,” *Education Next* 14, no. 1 (Winter 2014), pp. 66-69, http://educationnext.org/files/ednext_XIV_1_egalite.pdf.
55. Anna J. Egalite, Jonathan N. Mills, and Patrick J. Wolf, *The Impact of the Louisiana Scholarship Program on Racial Segregation in Louisiana Schools*, Louisiana Scholarship Program Evaluation Report 3 (Fayetteville: Univ. of Ark., School Choice Demonstration Project; New Orleans: Tulane Univ., Education Research Alliance, 2016), <http://educationresearchalliancena.org/files/publications/Report-3-LSP-and-Racial-Segregation.pdf>.
56. Patrick J. Wolf, “Civics Exam: Schools of Choice Boost Civic Values,” *Education Next* 7, no. 3 (Summer 2007), p. 66-72, http://educationnext.org/files/ednext_20073_66.pdf. The author is aware of one additional study more recent than Wolf’s literature review; see Russell S. Sobel and Kerry A. King, “Does School Choice Increase the Rate of Youth Entrepreneurship?” *Economics of Education Review* 27, no. 4 (Aug. 2008), pp. 429-38, doi:10.1016/j.econedurev.2007.01.005.
57. Paul E. Peterson, David E. Campbell, and Martin R. West, *An Evaluation of the BASIC Fund Scholarship Program in the San Francisco Bay Area, California*, PEPG 01-01 (Cambridge, MA: Harvard Univ., Program on Education Policy and Governance, 2001), <https://www.hks.harvard.edu/pepg/PDF/Papers/BasicReport.PDF>.
58. See note 18 above.
59. David J. Fleming, William Mitchell and Michael McNally, “Can Markets Make Citizens? School Vouchers, Political Tolerance, and Civic Engagement,” *Journal of School Choice* 8, no. 2 (2014), pp. 213-36, doi:10.1080/15582159.2014.905397.
60. Jonathan N. Mills, Albert Cheng, Collin E. Hitt, Patrick J. Wolf, and Jay P. Greene, *Measures of Student Non-Cognitive Skills and Political Tolerance After Two Years of the Louisiana Scholarship Program*, Louisiana Scholarship Program Evaluation Report 2 (Fayetteville: Univ. of Ark., School Choice Demonstration Project; New Orleans: Tulane Univ., Education Research Alliance, 2016), <http://educationresearchalliancena.org/files/publications/Report-2-LSP-Non-Cog-and-Political-Tolerance.pdf>. The authors suggest interpreting these results as merely descriptive, because of the problems identified here; this raised the question of whether the study should be included in this review given that we exclude descriptive studies from our review of research on civic values and practices. However, this study was not designed to be descriptive; it was an attempt to study causal factors; the admonition to give the data only descriptive force is an *fortiori* suggestion from its authors rather than an integral part of the study design. It therefore seemed appropriate to include it here and let the reader draw his own conclusions.
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Greg Forster, Ph.D., is a senior fellow with the Friedman Foundation for Educational Choice. He conducts research and writes on school choice policy. Forster has conducted empirical studies on the impact of school choice programs in Milwaukee, Ohio, Florida, and Texas, as well as national empirical studies comparing public and private schools in terms of working conditions for teachers, racial segregation, and teacher and staff misconduct. He also has conducted empirical studies of other education topics, including charter schools, accountability testing, graduation rates, student demographics, and special education.

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The interpretations, views, and recommendations expressed in this report are solely those of the author(s).

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If research adheres to proper scientific and methodological standards, its findings can be relied upon no matter who has conducted it. If rules and methods are neither specified nor followed, then the biases of the researcher or an organization may become relevant, because a lack of rigor opens the door for those biases to affect the results.

The author welcomes any and all questions related to methods and findings.

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