



REVIEW OF A META-ANALYSIS OF THE LITERATURE ON THE EFFECT OF CHARTER SCHOOLS ON STUDENT ACHIEVEMENT

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Summary of Review

This report attempts to examine whether charter schools have a positive effect on student achievement. From a review of 52 studies that the authors considered superior (40 of them used in an earlier report), it finds charters are serving students well, particularly in math. This conclusion is overstated; the actual results are not positive in reading and are not significant in high school math; for elementary and middle school math, effect sizes are very small, ranging from 0.03 to 0.08 s.d. The report does a solid job describing the methodological limitations of the studies reviewed, then seemingly forgets those limits in the analysis. For example, the authors include lottery-based studies, considering them akin to random assignment, but lotteries only exist in charter schools that are much more popular than the comparison public schools from which students are drawn. This limits the study's usefulness in broad comparisons of all charters versus public schools. The report also seeks to examine whether the effects of charter schools have changed over time. Despite finding no change, the authors inexplicably assert that there is a positive trend. Claims of positive effects when they are not statistically significant, exaggeration of the magnitude of effects, reliance on simple vote-counts from a selected sample of studies, and unwarranted extrapolation of the available evidence to assert the effectiveness of charter schools further render the report of little value for informing policy and practice.

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REVIEW OF A META-ANALYSIS OF THE LITERATURE ON THE EFFECT OF CHARTER SCHOOLS ON STUDENT ACHIEVEMENT

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I. Introduction

The University of Washington’s Center on Reinventing Public Education (CRPE) released a working paper in August 2014 titled *A Meta-Analysis of the Literature on the Effect of Charter Schools on Student Achievement*.¹ The authors of the report are Julian R. Betts, Professor of Economics at the University of California San Diego, and Yuan Emily Tang, a lecturer at the University of California San Diego. This report is an update of prior CRPE reports² by the same authors examining the achievement of charter schools compared with traditional public schools.

The report states, “The most important question to ask about charter schools is whether their students benefit academically and increasingly, to understand whether the impact of charters schools is changing as the sector matures” (p. 1). The authors used meta-analysis, a technique that combines the results from various studies, to examine the overall magnitude of achievement in reading and mathematics associated with charter schools. Results are reported in terms of effect sizes for mathematics and reading achievement, disaggregated by grade span, ethnicity, study, and inclusion/exclusion of KIPP school studies. Attention is also given to the prominent studies from the Center for Research on Education Outcomes (CREDO). The authors compared results from this latest study with those from a 2011 report; displayed the magnitude of the effects found for each of the 52 studies; and used a vote-counting method to determine the number of studies that found significant effects in favor of each type of school. The report also presents, in an analysis described as “non-achievement indicators,” a brief review of literature that the authors claim suggests overall positive effects for charter schools.

II. Findings and Conclusions of the Report

The report identified 52 studies (40 of which were used in the 2011 report) that had used either lottery-based methods or methods that were able to consider students’ prior achievement to examine the differences in student achievement. In the study’s primary analysis, there were no differences in reading achievement between charter and traditional

public schools across grade spans. There were small differences (with effect sizes, expressed in standard deviations, ranging from 0.03 to 0.08) in mathematics achievement for elementary and middle school, but not for high school.

Results of the meta-analysis were presented for various sub-populations (e.g., subsamples of students disaggregated by ethnicity). Despite the fact that there were no differences in most of the comparisons and that the few that found charter schools outperforming traditional public schools had only modest effect sizes, the report nevertheless asserts that charter schools are outperforming traditional schools:

- “...we demonstrated that on average charter schools are serving students well, particularly in math” (p. 36).
- “One conclusion that has come into sharper focus since our prior literature three years ago is that charter schools in most grade spans are outperforming traditional public schools in boosting math achievement” (p.53).
- “...there is stronger evidence of [charter school] outperformance than underperformance, especially in math” (p. 56).

The report displays the varying magnitude of the effects found for each of the 52 studies and used a vote-counting method to determine the number of studies that found significant effects in favor of each type of school “to give a fuller picture of the distribution of effect sizes” (p. 36). The authors conclude that “charter schools appear to be serving students well, and better in math than in reading” (p. 47) even though the report finds

... that a substantial portion of studies that combine elementary and middle school students do find significantly negative results in both reading and math—35 percent of reading estimates are significantly negative, and 40 percent of math estimates are significantly negative (p. 47).

The report compared the findings with those found in the 2011 report “as a simple way of looking for changes in the impact of charter schools over time” (p. 2). Although reading differences were no longer significant, the report states that effect sizes for reading “are roughly about the same” (p. 14). The overstatement is continued in the summary of the report,³ which asserts the report found “...that the effect sizes for math have increased and the effect sizes for reading have stayed roughly the same” (p. 1). The summary continues:

Based on the findings, and even in light of the variation in results, there is reason to believe that charter schools constitute an important and effective policy tool for raising student achievement—particularly for math (p. 1).

The report states that longitudinal analysis “is particularly relevant for policy because it addresses whether over time charter schools’ impact on achievement has risen” (p. 47). The results of this analysis are described as “suggesting a positive time trend” (p. 47).

However, since the trend was not significantly different from zero, a more accurate statement would have been that there has been no improvement over time. Contrary to the claim of an increasingly positive trend, the effects for mathematics (and certainly for reading) have *not* increased.

The last section detailing results is described as an “[introduction] to a new analysis of the relationship between charter school attendance and student outcomes other than math and reading achievement” (p. 2), but the section does *not* present findings from an analysis. Instead, it extrapolates descriptive results from selected studies to claim favorable outcomes for charter schools.

III. The Report’s Rationale for Its Findings and Conclusions

The authors only included lottery-based and value-added modeling studies in the primary analyses, contending these “represent the best methods available” (p. 8).

The primary rationale is that a simple tally of conclusions based on positive and negative results accurately and adequately represents the universe of findings without regard to study size, scope or significance. In the secondary findings, selected narrative reporting is deemed by the authors to be valid, although no rationale for inclusion or exclusion is provided.

IV. The Report’s Use of Research Literature

Throughout the report, the authors report findings asserting favorable outcomes for charter schools that are not well-supported in the literature. The work that was included in this “new analysis” (p. 2), for example, briefly reviews prior work on educational attainment (college matriculation), and despite “a smattering of other findings, with varying statistical significance” (p. 52), the authors claim, “Again, the general picture that emerges is one suggestive of what the authors contend are large positive impacts of charter schools on high school graduation and eventual college enrollment” (p. 52).

The report also ignores conflicting research. For example, in explaining the findings of the disaggregation of the meta-analysis for urban schools, the report states:

There could be multiple reasons for the larger effects in urban settings. One obvious possibility is that charter schools have more value to add in large urban districts if the traditional schools in these areas are underserving their students to a greater extent than are their nonurban counterparts (p. 35).

The “obvious possibility,” however, is contested in research that is not mentioned in the report.⁴

The authors excluded studies focused exclusively on KIPP schools from the main meta-analysis (although results with KIPP schools are presented for comparison in the report) because the “schools account for only about 2 percent of all charter schools” (p. 28). Nevertheless, CREDO studies, which were 12 of the 20 elementary/middle school studies, remained in the main analyses because “they include extremely large samples of charter schools” (p. 7). The authors reported that CREDO studies, however, introduced biases that favored charter schools because of the matching approach used.

V. Review of the Report’s Methods

To support the methods used in the report, the authors elaborate on the limitations associated with various methodologies and assert that the studies identified for inclusion “represent the best methods available” (p. 8). Only two types of studies met criteria for inclusion in the analyses. The first type of study uses a lottery approach that the authors considered to be “essentially an experimental method” (p. 1) because it compares students who “win and lose lotteries to attend charter schools” (p. 1). The report asserts:

The primary advantage of lottery studies is that, subject to some straightforward data checks, the studies will produce unbiased estimates of the impact of winning a lottery. This approach is useful because the only difference between those who are admitted and those who are not admitted is the luck of the draw (p. 5).

This is an “internal validity” argument, essentially saying that the research can determine the specific over-enrolled charter school’s effect by identifying any differences between the two groups. In a perfect world, that would be true. But charter schools have been able to use weighted lotteries (e.g., certain minority students may have two to three times the likelihood of being selected) that increase the chances students will be selected to attend charter schools “under the public school choice provisions of ESEA Title I.”⁵ Because some students may have a higher chance of admission to the charter school than others, a lottery approach does not address issues with differences between students who are and are not admitted to the school. In addition, as the report itself notes, the comparison—the families who lose the lottery—tend to behave in ways unbecoming a good control group: Many find other choice options, such as private schools or other charter schools.

The exaggeration of claimed effects render the strongly stated conclusions untenable.

Lottery studies also introduce an external validity threat “because only in areas where charter schools outperform traditional public schools are charter schools popular enough to be oversubscribed” (p. 50). In other words, lottery studies are a comparison of very popular, high-achieving charter schools with the less popular public schools the students

attempted to leave. This provides no more insight into a broad “charters versus publics” debate than would a study that compares the most sought-after public schools with less popular charter schools.

The second approach that met inclusion criteria for the report is referred to as value-added modeling because the technique compares students’ improvement with their prior achievement. Included here are studies that used propensity score matching and student fixed-effect models. Whereas propensity score matching attempts to capture the estimated probability that a student attends a particular kind of school and uses these estimates in analyses, student fixed-effect models examine the growth in achievement for students while they were in charter schools, compared with the growth they made when they were not enrolled in charter schools. The authors of the report describe some of the very real limitations with these approaches, yet these limitations did not assuage the strongly stated conclusions the report presents. This is particularly problematic given that the authors found both lottery-based and propensity score matching studies to be significantly related to the effect sizes in the meta-analysis for mathematics, which thereby interjects systemic bias in the analysis. These inherent design weaknesses were apparently not considered in the authors’ presentation of their conclusions.

VI. Review of the Validity of the Findings and Conclusions

The report compared the effect sizes resulting from the meta-analysis with those found in the 2011 report “as a simple way of looking for changes in the impact of charter schools over time” (p. 2). Although reading was no longer significant, the report minimizes this and states that effect sizes for reading “are roughly about the same” between 2011 and 2014 (p. 14). The disappearance of significance that we see with the larger pool of studies in 2014, however, merits attention—and a rethinking of the earlier results—not provided in the report.

The report also claims that the examination of changes of effect sizes from 2011 to 2014 point to “a positive time trend” for mathematics (p. 47). Yet from the report itself, we know that this so-called trend was not significantly different from zero. This discrepancy undermines the assertion that effect sizes in mathematics have increased since the last report.

The report presents some of the results of the meta-analysis in both effect sizes (standard deviations) and percentiles. Although the authors claim that “many readers may find it more understandable to read the results in term of predicted changes in percentile rank” (p. 9), the explanation provided is misleading and diverts attention from the fact that the *largest* effect size is only 0.08 s.d. The report states:

Consider the largest effect size, for middle school math, one more time. If the student began the year ranking 50th out of 100, after one year at a charter middle school he or she is predicted to rise to tie or outrank 53 out of 100

students. This is a meaningful change, and over several years of such gains, a student's gains could be quite large. For example, if a student experienced the same gains during all three years of middle school, he or she would move from the 50th percentile to just below the 60th percentile (p. 13).

The explanation pays no heed to the fact that gains across grades are *not* linear, but instead are more pronounced in the earlier grades.⁶ Further, assuming the gains will be cumulative across years on a relative scale is unwarranted. In addition to ignoring these considerations, the authors' explanation is misleading because percentiles, unlike effect sizes, do not have the same distance from each other. This is never mentioned, and indeed is obscured, in the report's assertions:

Our conclusion that has come into sharper focus since our prior literature three years ago is that charter schools in most grade spans are outperforming traditional public schools in boosting math achievement. . . In the middle school studies, which produce the largest estimates, charter school students are predicted to gain 3.3 percentile points in a single year (p. 53).

Percentiles in the middle of the distribution are much closer together than percentiles at the ends. Even setting aside all the other problems with this analysis, a 3.3 percentile point gain would be found *only* at the 50th percentile. Gains for students who are below or above the 50th percentile would be progressively *smaller*. For example, a 0.08 s.d. gain for a student who started at the 25th or 75th math percentile would be only about 2.3 percentile points; for the 10th or the 90th percentile, only about 1.3 points. Given that the purported math gains are on average only between 0.03 to 0.08 s.d., the conclusions in the report are exaggerated.

Notably, the authors of the report found that both lottery and propensity score matching studies were significantly related to the effect sizes in the meta-analysis for mathematics, but the biasing influence these types of studies had on the outcomes was not considered or described in the results. Despite multiple allusions to the positive effects of charter schools, an honest reading of the report's findings only supports the consensus in the literature: charter schools are neither any more nor any less effective than traditional public schools.

The other methods used involved visual inspection of histograms detailing the disaggregated effects for each of the studies, and then vote-counting the number of studies with positive and with negative effects. While the authors report the limitations of these approaches, they are, nevertheless, employed—which is problematic mainly because the clear intent is to bolster the above-described conclusions. That is, a presentation of histograms or vote-counts along with an explanation of their limitations would not be problematic if the conclusions reached were more reserved and responsible.

A limited narrative overview was also found in the last section of the report, where the authors cite work that, despite presenting mixed results, is asserted to demonstrate a positive effect of charter schools on students' college matriculation, earnings, attendance, and behavior. It is not clear what a more developed and careful analysis of this additional

literature would tell us about these outcomes; as set forth in the report, however, it reads as an add-on that is insufficiently developed to support the authors' conclusions.

VII. Usefulness of the Report for Guidance of Policy and Practice

The report's stated purpose is to determine whether students benefit from attending charter schools and whether there are changes in the impacts that charter schools have on student achievement over time. The report does a solid job describing the limitations with some of the methods used in the work they rely upon. But those limitations are then seemingly forgotten, and the conclusions are thus overstated. Further, some findings are reported as positive when they were actually not significant. The exaggeration of claimed effects render the strongly stated conclusions untenable. A more honest reading of the results would be that they are consistent with the large body of charter school studies: the overall test-score outcomes for charters and public schools are largely indistinguishable.

Notes and References

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